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* * It is requested that all Communications for this Work may be addressed, post-paid, to the Care of Messrs. Taylor and Francis, Printing Office, Red Lion Court, Fleet Street, London.

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THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
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[FOURTH SERIES.]

OCTOBER 1868.

XXXII. *On Some Effects of a Chemically Clean Surface.*

By CHARLES TOMLINSON, F.R.S.*

THE supersaturated solutions of a number of salts contained in chemically clean vessels can be kept during a long time without crystallizing, and even be reduced to temperatures much below the freezing-point of water, provided they are protected from the motes and dust of the air, and other chemically unclean bodies, which act as nuclei. For this purpose it is sufficient to close the mouths of the tubes and flasks with cotton-wool, which filters the air as it enters the vessel during the period of a rising barometer. When, however, the barometer falls, a portion of the air escapes from the tube, and carries with it some of the aqueous molecules of the solution; and the effect of this action often repeated is to lower the surface of the liquid and to deposit all along the width of the depression a crystalline crust of the salt. This crust is formed with singular facility in supersaturated solutions of magnesian sulphate; it may often be noticed in similar solutions of sodic sulphate, ammonia phosphate, and some others.

Many persons will think it a remarkable fact that this crystalline crust does not act as a nucleus to the solution itself when this is brought into contact with it. Nor does the solution dissolve it, even though the tube be kept inclined for days together so as to immerse a portion of the crust. It cannot do so, since its high state of saturation has more than satisfied its solvent powers. I say *more* than satisfied, because the water contains a much larger quantity of salt than it can take up at the temperature of the air.

* Communicated by the Author.

Phil. Mag. S. 4. Vol. 36. No. 243. Oct. 1868.

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The reason why this crystalline crust does not act as a nucleus to the rest of the solution is, that, being chemically clean, there is perfect adhesion between it and the solution as a whole, and hence there can be no separation of the salt from the water of the solution.

An objection to this theory has been put :—In ordinary crystallization, as in nursing a crystal of alum, are we not dealing with chemically clean surfaces ?

My answer is a decided negative. The evaporating-dish that contains the solution is not chemically clean ; the solution itself, exposed as it is to the air, is not chemically clean ; nor is the hair by which the crystal is suspended ; nor is the crystal itself, for this is frequently taken out and exposed to the air and handled, and abnormal growths chipped off with the thumb-nail. Under such conditions of chemical impurity the crystal acts as a powerful nucleus ; and if the fresh crystalline deposits made upon it are chemically clean, they do not long remain so. A chemically clean octahedron of alum is transparent ; a nursed crystal is opaque, (as I believe) in consequence of the multitude of minute crystals formed upon the impurities of the surfaces.

The general opinion is that the most powerful nucleus for the crystallization of a saline solution is a crystal of the salt itself. It is nevertheless possible, with strict attention to chemical purity, to introduce a crystal into a highly supersaturated solution of the same salt, and yet the crystal shall not act as a nucleus, the solution remaining perfectly liquid as before.

For this purpose a solution of two parts by weight of magnesian sulphate and one of water was boiled and filtered into a flask made clean by means of strong sulphuric acid, and rinsing with water ; the solution was again boiled in the second flask ; and while steam was issuing from the neck, a short tube, suspended by a wire, full of crystals of magnesian sulphate (all previously dipped in sulphuric acid and rinsed with water), was held in the neck, and secured there by putting in the plug of cotton-wool at the same time that the spirit-lamp was removed. The flask was left to cool during about fifteen hours, and then, without disturbing the cotton-wool, the tube with the crystals was lowered into the solution : there was no crystallization ; and during forty hours, though the flask was shaken several times, the solution remained unchanged.

A similar solution was filtered into clean tubes, which were closed with cotton-wool, and when nearly cold were put into strong sulphuric acid, covered with a receiver, and the air exhausted. In the course of twenty minutes a crystalline crust formed on the surface ; and this, during the shaking of the pump, fell to the bottom of the solution ; but it did not induce crystal-

lization. After some hours the tubes were taken out, the cotton-wool removed, when the solution in one tube immediately became solid, and that in the other tube did the same the moment it was touched with a bit of wire.

I sent in a note of these experiments to the Royal Society by way of addendum to a paper "On Supersaturated Saline Solutions" which I had the honour of submitting to that body on the 28th of May last*. I also brought the substance of that note before the Chemical Section of the British Association at Norwich on the 20th ult. The subject excited some interest, and several gentlemen who spoke on the occasion called for a definition of a chemically clean, in contradistinction to a dirty surface, one gentleman quoting the adage that dirt is something in the wrong place.

As a suggestion from any one of the distinguished observers who spoke on this occasion must be highly prized by me, and as all of them seemed to think I had not sufficiently defined the basis of my theory, I adopt the course recommended by no less a man than Descartes—that is, not to answer an objection at the time it is made, but to carry it home, consider it well, and then reply to it in writing. I therefore suspend for a short time the further prosecution of this subject in order to define more precisely what I mean by chemical purity, and to illustrate that definition by briefly recapitulating the work that has been done by means of chemically clean surfaces. The work that remains to be done seems to me to be as important as any that has yet been accomplished. There are several sets of phenomena at present wrapped up in vague molecular theories, which I am presumptuous enough to think admit of simple explanation under the theory I am advocating.

In the first place, what is a chemically clean surface?

A chemically clean surface is one that has on it no film or coating of any substance whatsoever foreign to its own composition. As oxidation by the air, organic matter, and floating motes are the most usual forms of films, we might say loosely, that any substance which has been exposed for some time to the air is chemically unclean; but speaking strictly, a film of any foreign matter will render a surface unclean for some conditions or other in the experiments in hand.

A chemically unclean surface, then, may be generally defined as anything that is exposed to the products of respiration or of combustion, or to the touch, or to the motes and dust of the air, and so becomes covered with a film more or less organic. So

* Proceedings of the Royal Society, vol. xvi. p. 403. See also 'Chemical News' for July 3rd, 1868, and the Philosophical Magazine for September 1868.

also any vessel or surface wiped with a cloth that has been exposed to the air is chemically unclean. In some cases bodies that have been exposed to the oxidizing influences of the air become chemically unclean.

A good test of the chemical purity of the surface of water is afforded by camphor. If a few fragments be scraped from a fresh cut surface with the point of a knife, and be allowed to fall upon water, they rotate with amazing velocity, and sweep over the surface, if the water be chemically clean; but if not, the fragments lie on the surface perfectly motionless.

The morning of September 1st was sunny and bright and the air dry, conditions highly favourable to the camphor-motions, which depend as much on evaporation as on solution. On this occasion I washed four shallow glasses, A, B, C, D, each $3\frac{1}{2}$ inches in diameter, with strong sulphuric acid, rinsed them out with water, and then filled them with water from the cistern-tap. Camphor was very active on all four surfaces. I now put my finger into A, my tongue into B. Fresh fragments were inert upon A, but as active as before upon B—showing that the finger was chemically unclean, and that the tongue was clean, or rather that, instead of depositing a film on the water, it absorbed water and any possible film with it. The water was emptied from C, and the glass filled up from a so-called clean jug from the kitchen filled from the same tap that had been used for the glasses; but the camphor fragments on C were motionless, showing that the jug had imparted a film to the water in C which prevented adhesion. The water from D was also thrown out and the glass rubbed and polished with a dry cloth. On again filling up the glass from the tap, the camphor fragments were motionless on the surface of the water, showing that the glass-cloth, commonly called clean, had imparted a film to the water.

It will be seen from these experiments how easily a chemically clean glass or a water-surface may become contaminated and rendered chemically unclean. In some cases mere exposure to the air during a few minutes is sufficient to convert a clean into an unclean surface, and to arrest processes which appear to be going on with great vigour. In other cases these very processes provide for and maintain the conditions of chemical purity, as, for example, in the camphor-wheels and currents which I have elsewhere described*. A stick of camphor, held by forceps, is lowered so as just to dip below the surface of water which has been very lightly dusted with lycopodium powder. As soon as the camphor touches the surface, a clear

* 'Experimental Essays,' published in Weale's Series, p. 38, figs. 9, 10, and 11.

space of about two inches in diameter is formed as by a flash, arising from the production of a film of camphor, which, being instantly disposed of by solution and evaporation, makes way for another film, which disappears in like manner; and by this very process the surface is kept clean for further action. The solution travels on in lines corresponding with the faces of the stick of camphor, and as the lines reach the side of the glass they divide right and left, and are reflected inwards in curves, the effect of which is to form the lycopodium into pairs of revolving wheels, the two members of each pair revolving in opposite directions. I have had this effect in active operation during fifty hours, merely taking the precaution to lower the camphor as it became cut through at the surface of the water.

Another remarkable continuous action is maintained by camphor in what I have named "camphor pulsations"*. If a stick of camphor be lowered into shallow water, contained in a flat glass dish 6 or 7 inches in diameter, so that the camphor may touch the bottom of the vessel, the water will be thrown into rapid pulsations. On a fine bright day I have counted as many as 240 per minute; but the number rapidly declines to about 80, and the pulsations may continue at that number during some hours. So regular is the beating, that on one occasion a medical friend who came in while the experiment was going on instinctively took out his watch and counted the beats, which he said were exactly like those of the pulse. The effect is, indeed, merely a case of capillarity: the water rises some way up the camphor and detaches a film, which spreads by the adhesion of the water-surface; while spreading, the water sinks about the camphor-stick; but as soon as the film is disposed of by solution and evaporation the water rises again, detaches another film, which is disposed of as before; and thus the surface is kept clean for continuing the action. As the water becomes more and more impregnated with the camphor; the action slackens; and as successive films are detached all round the camphor-stick both above and below the level of the water, the action becomes more and more striking.

Of course in all these experiments, if the finger or other chemically unclean body be made to touch the water, the actions are brought to an end.

But it has been said, during the last century and longer, that if, while the camphor is rotating, the surface of the water be touched with an oil or anything oily or greasy, the camphor motions are instantly and permanently arrested. This statement requires considerable limitations. I have already shown† that

* *Chemical News* for 1863, vol. viii. p. 37.

† *Phil. Mag.* for September 1863.

if turpentine and other essential oils be newly distilled, a film of any one of them on the surface of water does not arrest the motions of the camphor; the fragments skate through the film and cut it up in all directions. Turpentine and similar oils by exposure or long keeping become oxidized; and such products of oxidation, being neither soluble nor volatile, form over the surface of the water a delicate but permanent network which prevents the adhesion of the camphor fragments.

Creosote behaves in many respects like camphor on the surface of water. I described its motions some years ago*; but in order to observe them the surface of the water must be chemically clean. If not, there is no adhesion, and the drop of creosote delivered to the surface from the end of a glass rod forms an inactive double-convex lens with well-defined edges. I have more recently shown† how strong is the adhesive force of creosote for the surface of clean water, as measured by its power of displacing the films of various oils, fixed as well as volatile, and of resuming its activity when it has been temporarily invaded by an essential-oil film. For example, a drop of castor-oil on water forms a beautiful cohesion-figure; but if a drop of creosote be placed by the side of it, the active creosote figure shuts up the oil figure into a lens and chases it about. When the creosote has disappeared, the oil flattens out into a large film covering nearly the whole surface. A second drop of creosote will plough through it, turning over and thickening the edges of the severed portions. The creosote will in like manner repel and cut up films of whale-oil, fish-oil, nut-oil, and several other fatty oils, showing how superior is its adhesion to the water as compared with theirs. The effects of many of the essential oils also afford some remarkable illustrations of adhesion. While the creosote figure is in rapid vibration, sailing about over the surface of the water, if this be touched with a drop of turpentine-oil, oil of cajeput, &c., the drop will flash into a film and cover the surface, suddenly arresting the motions of the creosote. But as the oil-film becomes thinner by evaporation, the creosote starts into life and repels or cuts through what may be left of the oil-film. When the volatile oil has disappeared, a second drop will form a film that will again arrest the creosote; but as the film gets thinner, the creosote begins to show twitches at the edge, and suddenly it becomes active and begins to sail about. This remarkable effect may be repeated a number of times on the same water-surface before the creosote has entirely disappeared.

In like manner a comparative Table of the adhesion of liquids to liquids may be formed by showing what films will displace

* Phil. Mag. for August 1861.

† Ibid. for June 1867.

other films. For example, oleine of fish-oil will displace castor-oil, pale seal-oil will displace oil of nutmegs, and so on.

In all these cases, although dealing with essential and fatty oils, strict attention must be paid to chemical purity. If the vessel or the water-surface be not chemically clean, the oils will form lenses and not films. But if clean, the oils, in being shut up or in moving about, leave no trails on the surface, and the creosote is as free and active in its motions as if no fatty oil were present. It is, in fact, a case of adhesion, in which the attraction between water and creosote is superior to that between water and the oils: even where an essential oil like turpentine arrests the motions of the creosote for a time, it does not displace the creosote; for this retains its hold on the water, and, when the adhesion of the oil-film has become weakened by evaporation &c., the creosote reassumes its rule over the whole surface.

There are, however, some liquids which carry with them certain purifying influences of their own, and impart them to the water and the vessel. Such are ether, absolute alcohol, wood-spirit, &c., which most readily form cohesion-figures, as noticed in my first paper on this subject*. There is also a phenomenon that may be noticed every time a glass of wine is drunk—known, I believe, as the “weeping of the wine,” or “tears in the cup,” and may possibly explain a passage in the Book of Proverbs (xxiii. 31) as to when wine “moveth itself aright” in the cup. When a glass is filled with wine (the stronger the better), the alcohol and vinic ether make the glass chemically clean; a sip is taken and the glass one third or one half emptied. The part of the glass between the original level and the reduced level is completely wetted by the wine, and there is a strong capillary action between the reduced level and the plate of liquid left on the glass. But the supply of liquid above the surface of the wine in the glass is kept up by a twofold action. In the spaces between the tears will be seen an ascending wavy current of liquid which rises (1) by the adhesive action of capillarity, (2) by the formation of a back current in consequence of the downward flow of the tears, just as a backwater is formed at the place where two currents of a river meet; and this action, in the case of a glass of wine, will be rendered more apparent, in the same way as we see it in a river, if there are any specks or floating particles moving on the surface to show its direction.

The motions of eugenic acid on the surface of water† form a kindred phenomenon on a horizontal plane, to the tears in the wine-glass on a vertical one. But in some cases tears are due

* Phil. Mag. for October 1861.

† See Phil. Mag. for July 1864 (Suppl.). This experiment also requires a clean water-surface.

to the evaporation and condensation of the liquid in the glass. This effect may be well shown by putting a little pyroligeneous ether or wood-spirit into a deep glass, covering it with a receiver and exhausting the air. Under these circumstances it readily boils, and the vapour condenses at various points of the surface so as to form tears. The same effect may be shown by filling a long tube with spirits of wine, and then nearly emptying it, so as to clean the surface. If the tube be now fixed vertically and the flame of a spirit-lamp be applied below for a short time, tears will form during the cooling at different heights above the liquid. If spirits of wine be poured into a glass and a portion be then turned out, tears will form, but not nearly so well as in the case of wine. A little ether added to the spirit improves the effect. The explanation in this case is the same as in that of wine. Ether alone, or bisulphide of carbon alone, will not form tears, as the plate of liquid instantly evaporates. Benzole does not act well; naphtha acts tolerably well.

I have noticed tears formed by a very strong solution of the sodic acetate (5 parts salt to 2 water), which was boiled at 234° F., and filtered into a chemically clean flask, in which it was boiled again. It was then plugged with cotton-wool and left to cool. At about 90° the flask was put into water at 40° , and then into water at 32° containing snow. The glass above the solution now began to weep, the ascending and descending currents being well marked on the chemically clean surface.

The care bestowed by me upon chemically clean surfaces in studying the phenomena of supersaturated solutions seems to have greatly simplified what was a very complicated subject. Take the case of supersaturated gaseous solutions, such as soda-water, Seltzer-water, champagne, &c.* If any one of these be poured into a chemically clean glass no bubbles of gas will form on the sides, because the adhesion between the sides and the solution is perfect, and the sides may be regarded as a continuation of the liquid itself. A clean glass rod placed in the liquid does nothing more than form new sides, as it were, to the vessel, and its effect is merely that of the sides. In such case the rod will form no bubbles around it, and hence it is "inactive," because its adhesion is perfect. If dirty, the surface of the liquid surrounding it will be free; that is, there will be little or no adhesion between the water of the solution and the dirty surface; but there will be an adhesion between the gas of the solution and the unclean surface, and hence there will be a liberation of gas. All bodies that have been exposed to the air or to the touch are chemically unclean: they are covered with an organic film, as already

* Phil. Mag. for August and September 1867.

noticed, and act as nuclei in liberating gas, because such film prevents the adhesion of water, but does not prevent the adhesion of gas.

A precisely similar explanation may be applied to supersaturated saline solutions. In chemically clean vessels many of them may be preserved unchanged during a long time, and their very existence as supersaturated solutions depends in many cases on the absence of nuclei. Previous observers remark how sensitive some of these solutions are to cold; solutions that could be kept during the summer months begin to deposit crystals when the first touch of winter's cold is felt. I have kept such solutions unchanged during winter, and have put them into freezing-mixtures at 20° , 10° , and even at 0° F., and have frozen some of them without any separation of the salt. Previous observers are constantly referring to the supposed molecular changes undergone by the solution, or by the sides of the vessel containing it, when many of the results thus obtained are traceable to unclean tubes and flasks and thermometers. It seems to me to be impossible to study fairly or with profit the phenomena of supersaturation without reference to chemically clean conditions; and I believe the time is not far distant when such conditions will be considered essential in various chemical and physico-chemical inquiries.

Take a case of clean as compared with unclean. Five parts of sodic acetate and one part of water were boiled, filtered, and boiled again at 240° F. in a flask containing a thermometer which passed through a plug of cotton with which the flask was closed when the lamp was removed. The solution was left in a cold room during twenty hours, and then placed in a freezing-mixture during some hours. The solution had been some time at 14° , when, the thermometer being gently lifted up without being taken from the flask, the solution ran off from the bulb like a thickish syrup. On touching the solution with a wire it immediately solidified, and the thermometer rose from 14° to 104° F.

Such a solution as is here described crystallizes at 150° in an open evaporating-dish; and yet by observing the conditions required for chemical purity it may be cooled down to 14° , and probably to 0° , without crystallizing. It may be cooled at least 140° below its point of crystallization and yet not crystallize, simply because there is no nucleus to start the action. In the open evaporating-dish, on the contrary, no sooner is the boiling solution in contact with the air than its surface becomes dotted here and there with some of the specks and filaments that are floating about. If these be watched, one or more of them will be seen to increase in size by the aggregation of saline particles

about it, and suddenly from this point fans of crystals begin to radiate. In the meantime the sides of the vessel are active in separating the saline molecules, and the more so in proportion to the absence of chemical purity.

If the thermometer be taken out of the flask that contains a supersaturated solution and held in the air for a moment, it will catch some of the aerial motes, so that, on returning it to the solution, crystallization immediately sets in.

Air is not a nucleus; for it may be drawn through or shaken up with a highly supersaturated solution without any separation of the salt. The "mysterious action of the air," that several observers refer to, resolves itself into this,—that it acts the part of a carrier of some unclean mote or speck of dust which is the real nucleus. It had been noticed that supersaturated solutions can be kept longer in the liquid state in narrow-necked vessels than in wide ones; and the reason is obvious: the floating nuclei have a less chance of entering a narrow than a wide aperture. But even in such a case the locality of the experiment is of importance. If a flask containing a supersaturated saline solution be opened in a room, the solution will most likely crystallize the moment it is opened; but if taken into a field in the country, it may be kept for hours uncovered without crystallizing. Even so sensitive a solution as that of sodic sulphate I have kept in an open flask in the garden of my house without its crystallizing.

It is to my present purpose to notice in this place a fact communicated to me by Mr. Wentworth Scott after the reading of my paper at the last Meeting of the British Association, namely, that, on crystallizing saline solutions on a large scale in chemical manufactories (chlorate of barium for example), the workmen sometimes stretch clean white strings across the large enamelled vessels into which the solution is to be poured; and they find, practically, that the strings act best as nuclei when they draw them through their hands, "which," as my informant remarked, "are not particularly clean."

I might here introduce a number of facts respecting the boiling of water in vessels of different material, and the action of solid fragments in preventing jumping ebullition; but the subject is so extensive that I have included all I have to say respecting it in a separate memoir. I may merely mention here what is, I believe, a matter of common experience, that in introducing fragments of platinum, glass, &c. into retorts to prevent bumping, the fragments soon cease to be of any use. The reason is that they become chemically clean, and the liquid adheres to them with as much force as to the sides of the retort.

Dr. Faraday's experiment of firing the mixed gases by means

of clean platinum-foil is one of a large class of facts which also require special treatment.

There is a curious example of the production of chemically clean lines on the surface of glass by the discharge of a Leyden jar; a square of window-glass, four inches to the side, that has been exposed to the air so as to contract the organic film so often referred to, or dipped into a strong aqueous solution of soap and rubbed dry with a cloth so as to leave an invisible film of soap on the surface, will answer the purpose. If such a pane be presented to the knob of a charged Leyden jar (not a large one) and the discharging rod be placed with one knob on the outer coating and the other on the glass pane, the discharge will be made first on the glass next the knob of the jar, and it will pass round the edge and so get to the discharging rod. On holding the glass up to the light no electrical effect will be seen; but on breathing upon the glass, a remarkable tree-like figure will be traced out by the breath condensing in liquid lines wherever the electricity has burnt away the film and left the glass chemically clean, whereas on other parts of the glass, not touched by the discharge and where the glass is not clean, the breath will be deposited in minute beads of dew*.

I have already pointed out in this Journal† that the texture of dew depends greatly on the condition of the surface which receives it as to chemical purity. If chemically clean bottles, containing water and air or some gas, be placed near the window, we do not get deposits of dew, but weeping tears trickling down over sheets of water and leaving no furrows. In the article in the English Cyclopædia just referred to, I wind up with the following remarks:—"It is curious to notice the various modes in which the breath condenses on the glass plates, according to the temperature of the air and the amount of humidity or dryness, the tension of the electricity, and the state of health of the experimentalist. During the warm humid weather which occurred on certain days at the end of May and the beginning of June 1859, it was almost impossible to produce these lightning-figures. Then, again, in breathing on the plates a quantity of organic matter is deposited, which varies with the kind of food, the time that it has been taken, and the state of health of the individual. The breath, moreover, is differently deposited when projected by different individuals. The state of the breath is known to have a considerable influence in photography, and it

* I have named these figures "Lightning-figures:" see *Edinb. New Phil. Journal* for October 1861 and January 1862. See also the article "Breath-figures" in the *English Cyclopædia*, Arts and Sciences division, vol. i. col. 313. See also letter to the 'Times' newspaper, September 10, 1866.

† *Phil. Mag.* for May 1863.

has been noticed in other arts. In an old French treatise on enamel-printing (*Dict. des Arts et Métiers*, Lyons, 1801) the artist is cautioned not to let any one approach his work who has been eating garlic or taking mercurial medicines."

Variations in the amount of adhesive force, and distinctions between chemically clean and unclean surfaces, are constantly being met with on the great scale of nature. Water does not adhere to an oily or greasy surface; but air does so abundantly; and it seems necessary that it should do so in order to carry on the fermenting processes by which offensive organic matter is got rid of. For an opposite reason, salt will adhere to fatty matter, and so prevent those chemical changes which produce fermentation. It is seldom that we get perfect adhesion between water and a natural organic solid; mere exposure to the air prevents it more or less from retaining a chemically clean surface, even if it had one on its first appearance, as in the case of the young leaf or blade of grass. If the surface were chemically clean, the rain and the dew would completely wet it, evaporation would be more difficult, and there would be danger, from frequent soaking, of organic injury and even decay. But, in consequence of imperfect adhesion, and the presence also of a downy, powdery, oily, varnished, or rough irregular structure in foliage and flowers, and in the natural coverings of animals, the moisture forms in globules, which refresh the plant &c. while they remain, but are quickly dispersed by the wind or the heat of the sun. Inorganic matter, on the contrary, is rendered more or less chemically clean by every shower, and is more or less completely wetted; so that the roots of plants, or those parts which require most moisture, obtain it by a simple variation in the adhesive force of the constituents of the soil for water as compared with the living structures that grow out of it. The roots also are more readily wetted than the foliage or parts exposed to the air.

It is highly probable that all healthy animal and vegetable secretions are chemically clean, as well as the surfaces that secrete them. I have found that camphor will spin upon two or three such fluids when perfectly fresh; but they almost immediately change by exposure to the air, and then the camphor fragments are inactive.

There are various modes of obtaining chemically clean surfaces in flasks and other apparatus. Washing out with strong sulphuric acid, or with a strong solution of caustic potash, or with spirits of wine, and then rinsing with water, is generally sufficient. Soda-water poured into a test-glass thus prepared will not part with a single bubble of gas to the sides. When flasks are cleaned for highly saturated solutions of salts which

are to be heated or boiled again, it is well to rinse out the flask in which the solution is to be kept fluid with the acid of the salt, such as a little strong acetic acid for an acetate, nitric acid for a nitrate, and so on. For ammoniacal salts, a little solution of ammonia should be used. In this way, not only is the surface cleaned in the best manner, but any loss of acid or of ammonia during the heating is compensated. The flame of a spirit-lamp may often be used in cleaning such surfaces as those of platinum wire and foil, or of a glass rod, &c.

Highgate, N.
September 3, 1868.

XXXIII. On Rhodium. By R. BUNSEN*.

[With a Plate.]

IN working platinum ores on a manufacturing-scale, three products are obtained, which are principally used for the preparation of the other metals occurring along with platinum. These are:—

(1) The residues which remain after extracting the ores with aqua regia; they are rich in osmium and iridium, and are therefore especially fitted for the preparation of these metals.

(2) Osmium-iridium, which is obtained from these first residues by lixiviation, and which serves best for the preparation of ruthenium.

(3) The residues of the mother-liquors, which have been reduced by iron from the aqua-regia solution from which the platinum has been removed; being especially rich in palladium and rhodium, they are most conveniently used for obtaining these metals.

The following investigation was made with a material of this kind, which was supplied to me with the utmost liberality from the Imperial Mint in Petersburg. A kilogramme was used for each operation. These Petersburg residues contain all the platinum-metals with the exception of osmium, and are especially interesting owing to the large quantity of rhodium they contain. Claus, who worked on this subject, has endeavoured, if not to eliminate, at any rate to diminish the great difficulties incident to their preparation. The method he adopted, however, is extremely tedious, and scarcely practicable without sacrificing a great part of the valuable platinum-metals which accompany rhodium. In separating rhodium from iridium he used Wollaston's original process, which is based on the solubility, in chloride of ammonium, of the ammonium and potassium double

* Translated from Liebig's *Annalen*, June 1868.

salts of sesquichloride of rhodium. The circumstance that, as I have found, the bichloride of potassium and iridium is dissolved to a considerable extent by a solution of sal-ammoniac, or chloride of potassium saturated with rhodium-salt, must excite a reasonable doubt whether the metal thus prepared, and which has hitherto been regarded as the purest rhodium, and to which Claus, like Berzelius, assigned the atomic weight 52, does not contain considerable quantities of iridium. Hence it seemed necessary to forsake the old way and attempt a more exact method, so as to settle the doubts which the various and frequently discordant statements about rhodium still leave.

1. *Separation of Platinum and Palladium.*

The separation from platinum and palladium of rhodium, iridium, and ruthenium, which are insoluble in aqua regia, by digestion with this mixture, does not succeed with the residues in question; for a considerable portion of the latter metals is present partly in a finely divided state, partly in the form of hydrated sesquioxides, and consequently is dissolved in large quantities with the former metals—apart from the circumstance that the residue is filtered with extreme difficulty. But it is easy to extract platinum and palladium almost completely from the other metals by mixing the original material with one-third to one-half its weight of sal-ammoniac and heating the mixture gently in a Hessian crucible until the sal-ammoniac is *completely* volatilized and only vapours of chloride of iron are seen; the residue is then heated in a large porcelain dish with two to three times its weight of crude commercial nitric acid, and evaporated to the consistence of syrup. By ignition with sal-ammoniac, the metals not belonging to the platinum group are partially converted into chlorides; iridium, rhodium, and ruthenium are rendered insoluble, and the gelatinous silica present in the original material transformed into a pulverulent condition, so that it can be readily filtered. The chlorides formed from the sal-ammoniac yield, when digested with nitric acid, just as much hydrochloric acid as is sufficient for dissolving the platinum as bichloride; at the same time the copper and iron present reduce the palladium dissolved in nitric acid to such an extent that it is contained in the solution, not as bichloride, but as monochloride, which is not precipitated by chloride of potassium. Hence the mass, after treatment with nitric acid, need only be digested with water, filtered, and saturated with chloride of potassium, to obtain at once the greater part of the platinum as very pure bright yellow potassiochloride of platinum. This is washed with chloride of potassium and then with spirit, and the latter must not be mixed with the solution. The platinum precipitate weighed

62 grammes. The filtrate is introduced into a large stoppered flask, which must not be more than half full; if chlorine is passed into the flask which is shaken from time to time until no more absorption of the gas in the space above the liquid ensues, all the palladium is separated as a vermilion-red precipitate consisting of bichloride of palladium and potassium contaminated by platinum and some rhodium and iridium. This weighed 157 grammes. The liquid from which these precipitates are obtained is evaporated with hydrochloric acid not quite to dryness. On the addition of as much water as was necessary, by rubbing with a pestle, just to dissolve the chloride of potassium and the other soluble salts, a dirty chamois-yellow precipitate was left; this was separated by filtration, boiled for a short time with soda-lye and a few drops of alcohol, then mixed with hydrochloric acid until the precipitate at first formed was redissolved. After saturating the filtered solution with chloride of potassium, 13.5 grammes of chemically pure bichloride of platinum and potassium were obtained. The mother-liquor from which all these metals were separated, when tested with zinc and hydrochloric acid, indicated the presence of copper, but no platinum-metals. The separation of the vermilion-red palladium precipitate was effected as follows:—It was dissolved in boiling water, by which part of the bichloride of palladium was reduced to protochloride with disengagement of chlorine. After evaporating the solution with 60 grammes of oxalic acid and redissolving it in solution of chloride of potassium, 42 grammes of bichloride of platinum and potassium remained, which were yellow, and almost completely free from foreign admixtures. The washing was effected as before. The brown filtered liquid, which was somewhat evaporated in the water-bath, deposited on cooling 19 grammes of large and very beautifully formed transparent pea-green crystals of protochloride of palladium and potassium, which contained an admixture of chloride of potassium, but were found, on testing, to be free from other platinum-metals.

The liquid poured off from these crystals, carefully neutralized with soda, gave a slight precipitate of sesquioxide of iron and oxide of copper, which was removed. On the addition of iodide of potassium in the cold, all the palladium was separated as iodide. As iodide of palladium is soluble in iodide of potassium, an excess of the precipitant must be carefully avoided. This is readily effected if from time to time a little drop is taken from the outer edge of the liquid by means of a capillary tube*: as long as the precipitation is not complete, the drops brought against a white ground appear brown; when the precipitation is just complete,

* By this little device almost every constituent of a liquid which gives an insoluble precipitate may be volumetrically determined in a short time.

they are colourless ; the least excess of iodide of palladium makes them appear wine-red. 77 grammes of iodide of palladium were obtained.

A specimen of the precipitate left on ignition pure metallic palladium, which dissolved readily and completely in nitric acid. The whole of the iodine used in the precipitation may be readily obtained in the form of chemically pure hydriodic acid, and be used for fresh precipitations. For this purpose the iodide is introduced into a tubulated retort, the neck of which is drawn out and bent down at right angles, and is fitted into a flask close over a layer of water. Into the tubulure is fitted a glass tube bound round with asbestos and cemented by gypsum, through which a slow current of hydrogen passes from a Döbereiner's apparatus to the iodide placed on the bottom of the retort. The retort is heated in a magnesia-bath (to be afterwards described) so strongly that almost only hydriodic acid and very little free hydrogen emerges from the drawn-out end. As the metal thus reduced retains very small particles of subiodide of palladium with great persistence, which render it partially insoluble in nitric acid, it must be strongly heated once more in a current of hydrogen.

The mother-liquor of this vermilion precipitate, which has thus been worked up for platinum and palladium, may still contain rhodium and iridium. It is evaporated to dryness with a little iodide of potassium, whereby a mixture of iodide of rhodium and iodide of iridium is separated ; this is dissolved in aqua regia, and is separated by bisulphite of soda, as will afterwards be described—or, still better, is mixed with the material to be afterwards worked by this process.

2. *Separation of Ruthenium, and parting of Iridium and Rhodium.*

The residue, after treating with nitric acid a kilogramme of material, weighed 0.4 kilogramme. The platinum-metals had to be extracted in a form suitable for further operations. Hess's method of extraction by fusion with zinc alone is very imperfect, and attended with considerable loss. The following method, which depends on the deportment of zinc and chloride of zinc, is, I think, preferable to all others previously used.

If a small piece of zinc be melted in a porcelain crucible over a lamp, we know that it becomes covered with a coating of oxide. If iridium or any of the platinum-metals be added, this coating of oxide prevents any contact of the metal with the fused zinc, even if the metal be dipped in ; but if a few grains of sal-ammoniac be thrown on the fused zinc, ammonia, hydrogen, and chloride of zinc are formed, and the latter at once dissolves the layer of oxide with the formation of basic chloride. The zinc at the same time closely resembles pure mercury in its lustre and

mobility, and in the readiness with which the small globules coalesce to larger ones. When the chloride has taken up as much of the oxide as it can, the layer of oxide reappears; but by being again sprinkled with sal-ammoniac this disappears. The fused zinc strewed with sal-ammoniac has another property in common with mercury—that of instantaneously adhering to many other metals, and, in case they form alloys with zinc, of rapidly dissolving them. When the sal-ammoniac is strewed on the surface, the chloride of zinc bubbles up, in consequence of the ammonia and hydrogen liberated by the zinc. Several oxides and chlorides, especially those of the platinum-metals, on coming into this layer of chloride, which is kept in rapid agitation by reducing gases, are reduced and rapidly dissolved by the zinc. On this reaction a very simple process is based of quantitatively separating platinum-metals from all substances which are neither reduced nor dissolved by zinc. If, for instance, osmium-iridium is immersed in the layer of chloride of zinc, in a few minutes it is completely dissolved by the zinc regulus at a temperature not much above that of the fusing-point of zinc, especially if the porcelain crucible, which is being heated by a Bunsen's burner, is kept for a few minutes in rotation. The gangue mixed with the grains of osmium-iridium is left in the basic chloride of zinc. If the regulus, after it has just solidified, be lifted out of the still liquid layer of chloride of zinc, and if together with the contents of the crucible it be rinsed in a porcelain dish with acetic acid just sufficient to dissolve the basic chloride, the gangue can be separated by filtration and quantitatively determined. In this operation the crucible is not in the least attacked; but if the regulus be not removed immediately after solidifying, the crucible is generally broken in cooling, owing to the unequal contraction of the metal and of the porcelain. When the operation is properly conducted, there is no trace of metallic globules in the slag. In order that the experiment may be made at as low a temperature as possible below the boiling-point of chloride of zinc, the alloy of zinc formed must not be too difficultly fusible, which is attained by having from twenty to thirty parts of zinc to one of the platinum-metals to be expected.

For the extraction of platinum residues which have been previously exhausted with nitric acid this method is admirably suited. By one fusion for two or three hours all the platinum-metals were extracted to the last trace. The details of the process are as follows:—From 3 to 3·5 kilogrammes of commercial zinc waste are fused in a Hessian crucible holding 2 litres, sal-ammoniac being occasionally added; the residue, weighing 0·4 kilog., which has previously been gently ignited with sal-ammoniac, is projected into it, and the mixture kept for two to

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three hours at a temperature not much above the melting-point of the alloy to be formed, while from time to time, as the mass becomes too viscous, some dry sal-ammoniac is thrown upon it. The contents of the cooled crucible consist of three layers: the higher one, which is readily removed by the blow of a hammer, contains no platinum-metals; the second one (which is inconsiderable in quantity) contains a few porous difficultly fusible crumbs of an alloy of zinc and platinum-metals imbedded in the slag of chloride of zinc; the bottom one consists of what is often a very beautifully crystallized regulus. The second layer is coarsely powdered, softened with water, and the metallic crumbs separated by levigation and united with the principal one. To free this as much as possible from all adherent and enclosed impurities, it is once more fused with 0.5 kilog. of zinc, sal-ammoniac being added, granulated in water, and dissolved in hot fuming hydrochloric acid, which takes place with violent disengagement of gas, and scarcely requires half an hour. The chloride of zinc is used for the next operation. The platinum-metals, which settle as a heavy powder like metallic gold, may be washed by decantation without loss in the course of a few minutes. They are not pure, but contain considerable quantities of lead, copper, and iron, with traces of zinc, arising from the zinc as well as the platinum-metals used. They cannot be separated by means of nitric acid or of aqua regia; for a considerable portion of the platinum-metals dissolves up, while another is converted into insoluble oxides which are filtered and washed with difficulty. But, curiously enough, the platinum-metals may be almost completely freed from those impurities by digestion with hydrochloric acid. Not merely iron and zinc, but also *lead and copper are thereby easily dissolved with disengagement of hydrogen*. This is due to the electric current which is set up from the positive metals of the precipitate to the negative platinum-metals through the hydrochloric acid, by which electrolytic hydrogen is liberated on the platinum-metals, while the positive metals combine with the liberated chlorine. This reaction may be easily recognized by adding a few drops of solution of iridium to hydrochloric acid in which is a strip of copper, and which is heated to 100° C. The disengagement of hydrogen and solution of the copper commence as soon as it begins to be covered with iridium. The solubility of many metals is increased under these circumstances to a remarkable extent, as indicated by the following experiments made with moderately concentrated hydrochloric acid. The second column gives the volume of hydrogen which the same surface of the metals of the first column disengages alone, in twenty minutes, at a temperature of 100° C. The third column gives the quantities of hydrogen which are

given off under the same circumstances after the addition of potassiochloride of iridium.

I.	II.	III.
Palladium	0·0	1·5
Copper	0·0	18·4
Lead	23·5	58·0
Nickel	70·8	242·4
Tin	80·0	920·0
Cadmium	9·1	1040·0

The metallic mass separated from the zinc regulus, and alternately digested with hydrochloric acid and water, forms a fine dusty black powder, and, together with some admixed grains of sand arising from the crucible, weighed 65 grms*. It has the property of softly exploding with the appearance of fire when gently heated. The ignited and cooled powder, when brought in contact with cold unexploded powder, at once explodes the latter. If the explosion is effected by heating in an hermetically closed tube, either exhausted or filled with carbonic acid, neither hydrogen, nor chlorine, nor nitrogen, nor oxygen, nor aqueous vapour is liberated. From the method of preparation, these are the only bodies which could be present absorbed or in combination; it must therefore be assumed that all, or part, of the metals separated from the zinc are in a peculiar allotropic condition, which by heating, or by contact with non-allotropic metals, is got rid of with development of heat. The black powder which exhibits these explosive properties consists essentially of rhodium and iridium, with more or less considerable quantities of the platinum-metals originally present, as well as traces of lead, copper, iron, and zinc.

It is most intimately mixed with three to four times its weight of finely powdered perfectly dehydrated chloride of barium, and heated in a current of chlorine at a temperature which effects no disengagement of the gas.

The apparatus used for this purpose was arranged as follows. In the sheet-copper capsules, *c, c*, Plate II., provided with slides, *d*, and of which five or six could be arranged in a series, flasks of from 160 to 250 cubic centims. capacity were placed half full of the above mixture, and surrounded on all sides by ignited magnesia. Each flask is provided with a double-bored caoutchouc stopper, *ee*, and with two glass tubes; one of these tubes, through which the gas enters, passes to the bottom of the flask, the other, by which the gas emerges to enter the next flask, terminates just below the cork. The glass tubes are connected by india-rubber tubes, and are provided with bulbs

* A kilogramme of residues from other parts of the Petersburg stocks gave more than 120 grammes of this powder.

for the reception of any moisture that may pass over. In the flask *a* chlorine is disengaged, and is dried by passing through sulphuric acid in *b*; after passing through the series of flasks, it terminates in the chlorine-condenser, *f*, which is filled with wood-charcoal and solid hydrate of lime. Under each copper capsule is a single burner with a good draught, or, what is better, a three-jet Bunsen's burner. When all atmospheric air is expelled, all the burners are lit, and a brisk, but not violent, disengagement of chlorine is kept up for about three hours. The chlorine is at first so completely absorbed, that no bubble of gas escapes from the tube leading into the condenser when it dips under water. The successive action in the individual flasks is manifested by a deposition of perchloride of iron in the neck, and may be considered finished when this sublimate has about the same amount and appearance in each. On taking the apparatus to pieces, there is found in the flasks (the necks are cleaned from perchloride by bibulous paper) a dark rusty-brown mass not much agglomerated, which dissolves on the addition of water with great disengagement of heat. In six of the flasks, which may be used six or eight times for the same purpose, 100 to 120 grms. of platinum-metals may be attacked at once. The residue which remained unattacked weighed 13.7 grms., and after reduction with hydrogen 11.4 grms. By means of zinc, 4.5 grms. platinum-metals was extracted, which were insoluble in nitric acid, and which contained all the ruthenium present in the original material. Hence, of 65 grms. of platinum-metals in three hours, and by means of four ordinary non-luminous three-jet burners, 57 grms., or 93 per cent., was attacked by a current of chlorine, in the production of which 415 grms. pyrolusite of 85 per cent. was dissolved in hydrochloric acid.

The mass attacked by chloride of barium and dissolved in water contains, besides rhodium and iridium, chiefly copper, lead, iron, zinc, and small quantities of the other platinum-metals. The liquid is boiled, and the baryta removed by means of sulphuric acid. If specimens are taken by means of a capillary tube from the edge of the heated liquid in the manner mentioned under palladium, in a few minutes, by alternate addition of sulphuric acid and chloride of barium, any excess, either of barium or of acid, may be removed to within a hundred-thousandth of the entire saline mass.

In order to obtain the platinum-metals free from any impurities present in the solution, they are precipitated by hydrogen. For this purpose about a litre of the liquid is brought into a flask of twice the capacity, boiled for a time, and, after removing the lamp, the aqueous vapour is displaced by a rapid current of hydrogen; for this purpose it enters from a Döbereiner's apparatus

through a tube just projecting below a perforated cork, and emerges through a delivery-tube which is just over the layer of liquid. When the air has been completely expelled the exit-tube is closed, and the flask filled with hydrogen is connected, by means of a piece of thick caoutchouc tubing, with the Döbereiner's apparatus for the whole duration of the reduction. The temperature of the flask is kept just below 100° C. by placing it in a large iron pot which is heated by a small flame day and night. A water-bath at a constant level is better. The reduction begins at once, and is completed in five or six days if the quantity of metal amounts to about 100 grms. The first deposit consists principally of platinum and palladium; in the succeeding one rhodium preponderates; and the last parts are particularly rich in iridium. It is best to stop the reduction as soon as the liquid has assumed a greenish-yellow colour. The last traces of iridium which it still contains are obtained by evaporating the contents of the flask, igniting them with carbonate of soda, and afterwards exhausting the fused mass with aqua regia, so as afterwards to add them to another operation with chloride of barium. The reduction may be still more accelerated, if the liberated hydrochloric acid is removed from time to time by evaporation, care being taken to displace each time the hydrogen by aqueous vapour or carbonic acid before allowing air to enter, so as to avoid dangerous explosions. The palladium-metals separated consist of lustrous spangles, foil, and dendrites. Platinum and palladium are extracted by aqua regia, and, after treatment with soda, separated by chloride of potassium and iodide of potassium in the manner indicated above. The mother-liquor still contains some rhodium and traces of iridium, which are precipitated as iodide by repeated boiling with solution of iodide of potassium. These are dissolved in aqua regia and added to the subsequent separation of these metals.

The metals thus treated with aqua regia, and partially oxidized, are reduced by hydrogen at a temperature just below redness, and again converted into chlorides by being treated with chloride of barium in a current of chlorine. If from the solution of these metals the last traces of platinum and palladium are also removed by boiling with soda, and precipitation with chloride and iodide of potassium, there only remains the separation of iridium and rhodium. For this purpose the reddish-brown liquid is evaporated with hydrochloric acid, filtered, mixed with a great excess of bisulphite of soda, and left to stand in the cold for several days. Rhodium slowly deposits as an amorphous lemon-yellow soda double sulphite, while the solution becomes yellow and finally decolorized. With this change of colour, the rhodium precipitate also becomes brighter. The precipitate, well washed

out with water, contains the rhodium almost entirely free from all impurities. If the solution be warmed on the water-bath in a loosely stoppered flask, a fresh precipitate is formed, which is yellowish white and consists mainly of the above-mentioned rhodium-salt mixed with a greater or less quantity of the corresponding iridium salt. If, after filtering off this precipitate, the filtrate be evaporated to a small volume, two precipitates are formed: one is flocculent and of a yellowish white, which contains almost exclusively iridium with small traces of rhodium; the other consists of heavy crystalline scales which speedily fall to the bottom. This latter may be readily separated by washing and decantation; it weighed 16 grms. Whether this salt, which in general behaves like an iridium compound, contains a new metal I shall afterwards attempt to decide. The precipitates produced by sulphite of soda in the cold and on warming weighed, excluding that 16 grms., altogether 99.5 grms. In the mother-liquor, after adding hydrochloric acid and partial evaporation with zinc, traces only of platinum-metals could be detected.

The complete separation of iridium from rhodium is readily effected by merely treating the yellow precipitates with concentrated sulphuric acid. The precipitate is added in small portions to concentrated sulphuric acid, heated in a platinum crucible until sulphurous acid is disengaged, and the crucible heated in a sand-bath until all free sulphuric acid is volatilized and bisulphate of soda has been formed. On boiling out the contents with water, iridium is dissolved out as a sulphate with a deep chrome-green colour, while sulphate of rhodium is left in combination with sulphate of soda as a flesh-coloured double salt. It rapidly settles as a heavy powder, which must be boiled out with water and aqua regia, and washed by decantation. When I come to the determination of the atomic weight of rhodium, I shall dwell more fully on the constitution of this remarkable double salt, which is insoluble in hydrochloric or nitric acid or in aqua regia, can be heated to 250° without any change, and on ignition decomposes into metallic rhodium and sulphate of soda. The total quantity of this double salt obtained from a kilogramme of material amounted to 33.2 grms.

The first precipitates obtained by sulphite of soda in the cold yielded the rhodium almost entirely pure when treated in this way; the subsequent precipitates are very rich in iridium, and yield rhodium which is far purer than that prepared by previous processes, but not quite free from iridium. Hence these products obtained by sulphuric acid, which are not yet quite pure, and are recognized by their brownish tint, are collected, the rhodium separated by ignition, and the metal obtained heated once more with chloride of barium in a current of chlorine to purify it a

second time by the method already described. The green solution free from rhodium, but containing all the iridium, is freed from sulphuric acid by heating it, first in a platinum dish over the open fire, and then at a gradually increasing temperature in a porcelain crucible in a sand-bath; the porcelain crucible with its contents is finally strongly heated in a charcoal fire, by which neutral sulphate of soda and sesquioxide of iridium are formed. The latter remains as a black powder, after boiling out the fused mass, and is easily washed by decantation. It weighed 9.1 grms.

According to these experiments, there were thus obtained from a kilogramme of residue:—

	grms.
Chloride of potassium and platinum . .	117.5
Iodide of palladium	77.0
Chloride of potassium and palladium . .	19.0
Sulphate of rhodic oxide and soda . . .	33.2
Sesquioxide of iridium	9.1
Iridium containing ruthenium	4.5

The whole of the mother-liquors and residues from these preparations gave, when tested by zinc, inconsiderable traces only of platinum-metals.

In a subsequent memoir I shall recur to the constitution of the new compounds on which the method described is based, as well as to the atomic weight and properties of the chemically pure metal and some of its compounds. I will merely remark that working up 5 or 6 kilogrammes of crude material requires very little time, especially if all the filtrations are effected by a method which I introduced some time ago into my laboratory, and which consists in filtering in ordinary glass funnels with ordinary filter-paper under the pressure of a column of water of 25 to 30 feet. This pressure is produced by means of a water air-pump, on Sprengel's principle, which works day and night with a jet of water scarcely as thick as a straw, when by simply opening a tap it is connected with the water supply of the laboratory. The length of time required for washing precipitates is diminished to $\frac{1}{4}$ or $\frac{1}{8}$, especially in quantitative work, and the quantity of wash-water required is not more than from $\frac{1}{10}$ to $\frac{1}{4}$; immediately after filtering, the paper can be removed (even from gelatinous precipitates) like the skin of a boiled potato, so clean and completely, that the same filter may be used a second time; and the precipitates are obtained in such a form that they may be directly ignited without any further drying.

A minute description of this method, which, as regards certainty, precision, and rapidity, brings analysis by weight very near volumetric methods, I must defer to another communication.

XXXIV. *Note on the Resolution of Algebraic Equations.*

By THOMAS P. KIRKMAN, M.A., F.R.S.*

THE function Σ_p of my communication in the preceding Number is needlessly complex. If H_1 be any asymmetric function of the $n+1$ variables $x_0, x_1, \dots, x_n$, which is invariable by the cyclical permutation of all those variables, and which consequently receives all its values, suppose Πn values, by the permutations of $x_1, x_2, \dots, x_{n-1}, x_n$, and if Γ_r denote the group of r cyclical permutations of the first r of $1, 2, 3, \dots, n$, the remaining $n-r$ being undisturbed, and if

$$\Gamma_n H_1 = H_1^i + H_2^i + H_3^i + \dots + H_n^i = J_i$$

be the n values of H_1^i made by the substitutions of Γ_n , then I say that

$$\Sigma_p = \Gamma_n Z_p^\beta = \Gamma_n \Gamma_{n-1}^\gamma \Gamma_{n-2}^\delta \dots \Gamma_3^\lambda \Gamma_4^\mu \Gamma_5^\nu \Gamma_2^\xi J_i^a$$

is a rational and symmetrical function of $x_1, x_2, \dots, x_n$, whatever be the *positive* integers $\beta, \gamma, \delta, \dots, \nu, \xi, a, i$. Here, as before, the exponent over Γ_r is intended to be written over the sum of r values made by its substitutions. This theorem is sufficiently proved in my paper of last month, and can be made further evident thus.

If $P = G + G_1 + G_2 + \dots + G_m$

be any group consisting of a group G of any order followed by $m-1$ derivatives of G , and if θ be any substitution of P , the operation θP can change nothing in the right member except the order of the terms $G, G_1, \dots, G_m$. If (P) be any rational function invariable by the substitutions of the group P , we can write it

$$(P) = (G) + (G_1) + (G_2) + \dots + (G_m),$$

where (G_r) is the value of (G) which is formed on G_r . It follows that if $(P)_i$ denote the sum of the i th powers of the $m+1$ values (G_r) , we have

$$(P)_i = (G)^i + (G_1)^i + (G_2)^i + \dots + (G_m)^i,$$

a function invariable by the substitutions of the group P . For as the operation $\theta(P)$ can do nothing more than change the order of the $m+1$ functions (G_r) , and as the substitution which changes (G_n) into (G_m) will change

$(G_n)(G_n)$ into $(G_m)(G_m)$, $(G_n)(G_n)(G_n)$ into $(G_m)(G_m)(G_m)$, &c., the operation $\theta(P)_i$ can make no algebraic change in the right member of $(P)_i$; that is,

$$\{(G)^i + (G_1)^i + (G_2)^i + \dots + (G_m)^i\}^k = (P)_i^k,$$

* Communicated by the Author.

whatever be the integers i and k , is invariable by the substitutions of the group P .

Now the function

$$\Gamma_2^\xi J_i^a = (J_i^a + {}_2J_i^a)^\xi = K_a^\xi$$

is invariable by the substitutions of the group Γ_2 ; and as

$$P = \Gamma_3 \Gamma_2 = \Gamma_2 + {}_1\Gamma_2 + {}_2\Gamma_2 = G + G_1 + G_2,$$

the function

$$\Gamma_3^\nu \Gamma_2^\xi J_i^a = (K_a^\xi + {}_1K_a^\xi + {}_2K_a^\xi)^\nu = L_\xi^\nu$$

is invariable by the six substitutions of the group $\Gamma_3 \Gamma_2$, whatever be the integers ν , ξ , a , i . Again, writing $G = \Gamma_3 \Gamma_2$,

$$P = \Gamma_4 \Gamma_3 \Gamma_2 = G + G_1 + G_2 + G_3;$$

whence

$$\Gamma_4^\mu \Gamma_3^\nu \Gamma_2^\xi J_i^a = (L_\xi^\nu + {}_1L_\xi^\nu + {}_2L_\xi^\nu + {}_3L_\xi^\nu)^\mu = M_\nu^\mu$$

is a function invariable by the twenty-four substitutions of $P = \Gamma_4 \Gamma_3 \Gamma_2$. Precisely in the same way, writing $\Gamma_4 \Gamma_3 \Gamma_2 = G$, we prove that N_μ^λ , the λ th power of the sum of five values of M_ν^μ , is invariable by the $\Pi 5$ substitutions of $P = \Gamma_5 \Gamma_4 \Gamma_3 \Gamma_2$, and finally we see that

$$\Sigma_\beta = \Gamma_n Z_\gamma^\beta$$

is invariable by the Πn substitutions of

$$P = \Gamma_n \Gamma_{n-1} \Gamma_{n-2} \dots \Gamma_3 \Gamma_2.$$

Hence the sum of β th powers Σ_β is a rational and symmetrical function of the n variables $x_1, x_2, x_3, \dots, x_n$; the sum of γ th powers Z_γ is the issuing root $\{Z_\gamma\}$ of a given equation of the n th degree; the sum of δ th powers Y_δ is the issuing root $\{Y_\delta\}$ of a given equation of the degree $n-1$, ... the sum of ν th powers M_ν is the issuing root $\{M_\nu\}$ of a given quintic; the sum of ξ th powers L_ξ is the root $\{L_\xi\}$ of a given quartic obtained by using $\nu=1, \nu=2, \nu=3, \nu=4$; the sum of a th powers K_a is the issuing root $\{K_a\}$ of a given cubic, obtained by using $\xi=1, \xi=2, \xi=3$; the sum of i th powers J_i is the issuing root $\{J_i\}$ of a given quadratic, got by using $a=1$ and $a=2$; and from $\{J_i\}$, using n values of i , we obtain, by solving a given equation of the n th degree, the required expression

$$\{H_1\}, \{H_2\}, \dots, \{H_n\}$$

of $H_1, H_2, \dots, H_n$, as irrational functions symmetrical in the n variables $x_1, x_2, x_3, x_4, \dots, x_n$.

This restatement and proof of the theorem of my former paper is, I believe, all true, and in part, I hope, new. The application that I made of it to the solution of equations of the $(n+1)$ th

degree, if $n > 3$, is, alas! all new and not true. My friend Professor Harley has shown to me that the irrationals $\{H_1\} \dots \{H_n\}$ are not symmetrical functions of the $n+1$ variables $x_0, x_1, x_2, \dots, x_n$, although the values of H_1 are invariable by their cyclical permutation. I hope the scientific reader will pardon the nonsense of the latter part of my preceding communication, and I wish that he may live to confess, like myself, that his mathematical powers are the worse for wear.

P.S.—Let me venture one word more. All that is required for the solution of $\Theta=0$ of the $(n+1)$ th degree, is the transformation of $H_1, H_2, \dots, H_n$ in the expression for x_0 in my preceding paper, into symmetrical functions of the $n+1$ variables Σx . By writing H_1^a instead of J_1^a in Σ_β first above written, we can obtain, by solution of a given quadratic, $\{H_1\}$ instead of $\{J_1\}$, and we can write this

$$H_1 = \{ {}_0 H_1 \},$$

showing that H_1 is transformed into an irrational symmetric in the n variables $\Sigma x - x_0$. But the Πn values of H_1 are obtainable by the permutations of any n of the $n+1$ variables. We have got $\{ {}_0 H_1 \}$ by permuting all but x_0 , which is undisturbed in the above process. We can repeat the process, permuting the $n, \Sigma x - x_1, x_1$ being now undisturbed. The result will be

$$H_1 = \{ {}_1 H_1 \},$$

an irrational symmetric in the n roots $\Sigma x - x_1$. In the same way we can transform H_1 into the irrational $\{ {}_2 H_1 \}$, symmetric in the n roots $\Sigma x - x_2$. And thus we transform H_1 into $\{ {}_r H_1 \}$, symmetric in the n roots $\Sigma x - x_r$, whatever r may be, obtaining the $n+1$ results

$$H_1 = \{ {}_0 H_1 \}, H_1 = \{ {}_1 H_1 \}, H_1 = \{ {}_2 H_1 \}, \dots H_1 = \{ {}_n H_1 \}.$$

From these it follows that

$$H_1^{n+1} = \{ {}_0 H_1 \} \{ {}_1 H_1 \} \{ {}_2 H_1 \} \{ {}_3 H_1 \} \dots \{ {}_n H_1 \} = [H_1],$$

which is symmetrical in all the $n+1$ roots Σx ; and

$$H_1 = [H_1]^{\frac{1}{n+1}}$$

is the required transformation of the rational asymmetric H_1 into an irrational symmetric in all the roots, and therefore a function of the coefficients of $\Theta=0$.

If the reader will assist me so to handle the ambiguities of the implicated surds that we may be quite sure of all this (and if we can do it with H_1 we can do it with all the n H 's in the expres-

sion for x_0), then, after enjoying his laugh at my battle with the demonstrated impossibilities, he may come to think that we have beaten them after all.

The n groups of Πn which we use for the irrationals $\{_1H_1\} \dots \{_nH_1\}$ are obtained from that which gives $\{_0H_1\}$ by the cyclical permutations of $x_0x_1\dots x_n$, and complete with it the whole group of $\Pi(n+1)$. The $n+1$ tactical values of $\{_0H_1\}$, being numerically equal, depend on a binomial $(n+1)$ tic.

XXXV. On the Capillary Constants of Solid Bodies.

By Professor G. QUINCKE*.

THE phenomena presented by liquids in contact with capillary surfaces may either be referred to so-called molecular forces (that is, to attractive forces which the smallest particles of the bodies exert on each other at infinitely small distances), or they may be deduced from a tension which the surface of a liquid exerts like a stretched membrane, and which has the same value over the entire surface if the liquid is bounded by the same body.

Though the first point of view may be better suited for the accurate mathematical treatment to which it has been subjected by Laplace† and Poisson‡—to whom Gauss may perhaps be added, for his treatment § combines both points of view—yet the second has the advantage of introducing a definite physical idea instead of the unknown molecular forces or molecular functions. With its aid Young||, by developing Séguin's¶ theory, has found the three fundamental principles of capillarity. MM. Hagen** and Plateau†† have more recently repeatedly called attention to the tension of the surfaces of liquids; and the experiments of the latter‡‡, as well as those of Van der Mensbrugghe§§, on thin liquid membranes are especially suited for demonstrating this tension in the surface of liquids.

Young||| first showed that the attractive forces exerted between

* Translated from the *Berliner Berichte*, February 1868.

† *Mécanique Céleste*, vol. iv. p. 389 (1805).

‡ *Nouvelle Théorie de l'Action Capillaire*, 1831.

§ *Principia Generalia Theoriæ Figuræ Fluidarum in statu Æquilibrii*, 1830.

|| *Phil. Trans.* 1804. Lecture on Natural Philosophy, vol. ii. p. 649.

¶ *Comment. Soc. Gott.* vol. i. p. 301 (1751).

** *Abh. der Berl. Akad.* 1845 & 1846.

†† *Mém. de l'Acad. de Belgique*, vol. xxxiii. (1861). *Phil. Mag.* S. 4. vol. xxii. p. 286.

‡‡ *Ibid.* vols. xvi., xxiii., xxx., xxxi., xxxiii., xxxvi. (1842–66).

§§ *Bull. de l'Acad. Roy. de Belgique*, vol. xxii. No. 11 (1866), vol. xxiii. No. 5 (1867). *Phil. Mag.* vol. xxxiii. p. 270, and vol. xxxiv. p. 192.

||| *Miscellaneous Works*, vol. i. pp. 455 *et seq.* & p. 463 (1816).

the particles of a liquid produce an increase in the density on the surface of a liquid, and at the same time a tension on the free surface where it is bounded by a vacuum. He has then extended these considerations to the other surfaces of a liquid, which are bounded by other liquids or solids, and has shown that here also, for all points of the same limiting surface, there must be a constant tension, which, however, is smaller than at the free surface of the liquid.

On account of his extraordinary conciseness he is frequently obscure; and he has changed in the course of years; for from time to time he assigns the difference in the deportment of various substances solely to their different density.

But, from whatever point of view we start, we are always led to the conclusion that *the phenomena of capillarity must occur not only with liquids but also with solids, provided they are not absolutely rigid but permit a displacement of their smallest particles.* The author was originally led to this principle by the observation that fused substances, such as cement or glass, which when soft could form a surface of equilibrium, exhibit an unusual stability; and it seems in fact that a series of surprising phenomena may be unconstrainedly explained by assuming a tension on the surface of solids, whether they have a free surface (that is, bounded by air), or a surface in common with some other solid or liquid.

If the free surface of a solid is the same as that of a liquid upon which the same external forces act, its free surface must have a definite tenacity, measured by the pull exerted upon the unit of surface.

It must therefore be possible to express the tenacity of round metal wires by two numbers, the first of which is proportional to the section, and the second to the periphery of the wire. As the author was about to make experiments as to the validity of this law, his attention was drawn to a memoir by Karmarsch, which appears but little known*. Karmarsch, by numerous experiments on the tenacity of metal wires, arrived at exactly the same law which the author, without knowing those experiments, had deduced by theoretical calculations.

Karmarsch expresses the tenacity of metal wires thus,

$$F = aD^2 + bD; \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where D is the diameter of the wire, and a and b are constants. The validity of this expression has been demonstrated in wires of gold, steel, iron, German silver, silver, brass, copper, platinum, and zinc from the most various sources, and in both annealed and unannealed condition. The difference between the observed tenacities and those which can be calculated from the values

* *Mittheilungen des gew. Ver. für Hannover*, 1858, pp. 138-155.

found for a and b does not, with individual exceptions, exceed a few per cent., which may be considered surprising when the possible sources of error are taken into account, and we remember the difficulty of obtaining constant results for similar determinations in the case of liquids.

As the formula holds for annealed wires, the author does not think with Karmarsch that the metal is first condensed on the surface by the pressure in the draw-plate and its texture advantageously altered. It does not follow thence that the increase in the density of the metal necessitates an increase of those constants a and b . On the contrary, the theory says that this increase must be greater for b than for a , in accordance with experiment.

Karmarsch gives the thickness of wires in millimetres, the tenacity in pounds. Taking f as the tenacity in grammes for wires of the diameter r in millimetres, or

$$f = a2\pi r + \beta\pi r^2,$$

we have

$$\alpha = \frac{b}{\pi} 500, \quad \beta = \alpha \frac{4}{\pi} 500; \quad (2)$$

and the values of the constants α and β are then easily deduced from the values of a and b given by Karmarsch. The following Table gives some values of α , where α is the constant of capillarity of solid metals at ordinary temperature (15°), and measures the pull exerted upon 1 millim. of surface. The numbers may be readily compared with the ordinary capillarity-constants of liquids, which are usually given in milligrammes. (Vide *Fortschritte der Physik*, 1863, vol. xix. pp. 68–74.)

Capillarity-constants of solid Metals.

	Hard-drawn.	Annealed.
	gr.	gr.
Iron	5731	1592
Platinum	3025	2388
Copper	2388	
Silver	2388	478
Gold	1592	478
Zinc	557	
<hr/>		
Steel	6685	955
Argentan	6685	1114
Silver	5253	2547
Gold (14 carats) .	3661	2228
Brass (wire) . . .	2547	1751
Brass (string) . .	1751	637

The values thus found are very great if compared with the

constants for liquids like water (0.008 gr.) or mercury ($\alpha = 0.055$ gr.). Yet, as Laplace has shown, these constants must increase with the square of the density if the function of attraction remains the same; and it is even possible that they experience a sudden increase near the change of condition, just as the density or the electrical conductivity suddenly changes. Experiment has also shown that, as the temperature of water and other liquids sinks, the constant increases. The author has observed the same fact in the case of mercury, in opposition to the statement of M. Frankenheim*.

Near their melting-points the capillarity-constants have comparable values, as will presently be shown. Hence metals with a high melting-point, apart from other differences, must indicate a high value for ordinary temperatures, as is seen from the above Table.

As a layer of liquid near the surface acts like a stretched membrane, the surface must offer to external impressions a resistance which is greater the greater the capillarity-constant. Hence the metals must be ranged according to the value of α in the same order as that of their hardness. Karmarsch's experiments ‡, in fact, as well as those of Calvert and Johnson †, on the hardness of metals, give values in accordance with this relation which agree as well as can be expected in such determinations.

It might be thought that wires of the same section with a larger surface must exhibit greater tenacity. The author has made experiments in this direction with round and flattened silver and copper wires, and he has found that the tenacity with a flattened wire is almost the same as in the case of a round wire of the same section and the same material. It must at the same time be remembered that in flattening, not merely the density but the continuity of the surface is altered, as microscopic inspection shows, and the wire acquires cracks like flattened dough. Moreover the surface of such a flattened wire is not a surface of equilibrium, and other forces (the difficult displaceability of the particles) prevent the formation of the surface of equilibrium towards which the capillary forces tend. In the different parts of the surface there will be different tension. As, moreover, the section of the flattened wire is very irregular, a more accurate calculation would only afford interest in case we could make experiments between drawn wires having an elliptical and those having a circular section.

Gold and silver leaf have not a continuous surface, but are per-

* Pogg. Ann. vol. lxxv. p. 26.

† *Mit. des gew. Vereins für Hannover*, 1858, p. 178.

‡ Ibid. p. 175, taken from the Memoirs of the Literary and Philosophical Society of Manchester.

forated by holes; otherwise the tenacity of a piece of gold leaf must be greater than experiment shows to be the case. If we take care that these holes are wanting, or are present in small quantity, the tenacity is very great. The author has deposited thin layers of platinum, silver, or gold upon plane glass surfaces, and is astounded at the tenacity possessed by layers of 0.01 millim. thickness. A suitably formed piece of steel planes regular shavings in such a layer. Copper deposited upon the silver layer gives, together with the silver, a very firm membrane. On tearing from the glass, the side turned towards the glass is convex, the other is concave. The author thinks it probable that it is chiefly the tension on the free metallic surface (which, according to theory, must be greater than at the limit of glass) that produces this curvature. Hence a metal with a high capillarity-constant, like platinum, does not adhere to glass surfaces when it is deposited upon them in the cold, but acquires cracks and peels off. It is only after the tension on the surface has been diminished by strong heating that the metal adheres more firmly. If by the pressure of polishing the density and therewith the capillary tension of the metallic surface are increased, metallic layers become detached from the glass, which would otherwise adhere firmly. Measurements of the tenacity of such thin layers were not made by the author, owing to the difficulty of fastening and loading them.

If thin metallic laminæ like gold leaf be raised to a higher temperature, by which the mobility of the individual particles is increased, they curve in consequence of the superficial tension (just as liquid drops form from a thin layer of liquid), and the pores of the thin metallic laminæ become larger. This explains the phenomena observed by Faraday in thin metallic laminæ*.

The principle adduced above, that in the common surface of two solids there is a capillary tension, renders it possible to foresee the explanation of a few other known but hitherto unexplained phenomena.

Two plates adhere more firmly the thinner the layer of liquid between them. In like manner, two solids stick the more closely the thinner the layer of cement or glue between them.

The tenacity of damask steel or rolled iron depends on the circumstance that in the manufacture care is taken to produce a great capillary-surface—the junction of heterogeneous substances. Different kinds of iron are packed over each other and joined by rolling and welding; it is not improbable that they remain separated by an extremely thin layer of oxide or of slag. Heated afresh between each welding, the soft masses may arrange their

* Phil. Trans. 1847, p. 145. Faraday, 'Experimental Researches,' vol. iv. pp. 401 *et seq.*

common surface in such a manner that the capillary tension on them is constant and attains the maximum value which is possible for the common limiting surface of the two bodies.

If this arrangement be changed, by the force of crystallization for instance, the fibrous structure changes into a crystalline one, and the solidity must become much smaller.

In drawing wire, hollow cylinders of metal of differing density glide over each other. The heat developed by the drawing contributes to render the metal particles more readily displaced, so that on the common surface of two such hollow cylinders the capillary tension is a maximum. The tenacity of hard-drawn wires must be greater than that of annealed ones. In physical mixtures of heterogeneous substances (alloys, steel) wire-drawing must favour the formation of capillary surfaces more even than in the case of homogeneous metals; so that in these the decrease of the capillarity-constant is most marked. All this agrees with observation.

The great tenacity of bodies which are built up of thin systems of tubes, like bones, the tenacity of organic substances which consist of individual cells with large capillary surface, are explained by this capillary tension. If these solids have been originally formed from soft masses, on their solidifying there has been a constant tension in the entire capillary surface as great as was possible under the given circumstances. The changes which this tension of the surface subsequently undergoes in consequence of alterations in temperature or of the form of the surface, will in general be small in comparison with the whole value on solidification.

The alteration in the measures used for geodetic purposes, which have recently been demonstrated by M. Bæyer*, may partially arise from the usual prismatic form, which agrees so little with a surface of equilibrium. In spheres, solid as well as hollow, there is little reason to apprehend changes of this kind.

The difficulty of assuming forces near the surface of solids which depend on the form of the surface, in opposition to traditional ideas, is not in fact so great as might at first sight appear. Displacements of the molecules of what are called solids have long been known. The plasticity of ice†, the phenomena of regelation‡, the experiments of M. Tresca on the efflux of solids§, the different deportment of the same substance according as it is in the crystalline or the colloid condition, indicate a more or less prominent community of properties in solids and liquids.

A rigidly theoretical treatment of the capillary phenomena of solids is rendered more difficult by the circumstance that the

* *Berliner Monatsberichte*, 1867, p. 1.

† Forbes, *Phil. Trans.* 1846, p. 143. Tyndall, *Phil. Trans.* 1857, p. 327.

‡ Faraday, *Phil. Mag.* vol. xvii. p. 162 (1859), vol. xxi. p. 146.

§ *Comptes Rendus*, vol. lix. p. 754 (1864); vol. lx. pp. 398 & 1226 (1865).

surfaces are not surfaces of equilibrium, as in the case of liquids, that therefore, as in these too, we are ignorant of the nature of the molecular forces and the distance at which they act.

For this reason the author has directed his attention for the present to actual liquids, and has endeavoured to determine the capillarity-constants of fused bodies, especially metals, of which hitherto little was known. His expectation, that the capillarity-constants of metals near their solidifying-point must have comparable values, he has found confirmed. From the nature of the case these determinations must be considered not more than approximations to the true value, and indicate greater discrepancies than is the case with the determinations of liquids at ordinary temperature, like water, mercury, alcohol, &c.

The noble metals, in the form of vertical wires, were melted in a flame at as low a temperature as possible, until the suspended drop fell. Neglecting the small quantity of fused metal adhering to the thin wire, the weight P of the drop is the greatest weight which the fused vertical mass can support, or

$$P = \alpha 2r\pi; \quad \dots \dots \dots (3)$$

where $2r$ is the diameter of the wire in millimetres, α the capillarity of the fused metal.

In the same manner were heated glass threads which had been drawn out of the same glass rod at the blowpipe-table.

The flame used was generally a small coal-gas one, of 10 milims. height and 3 millims. diameter. Only in the case of platinum was a current of oxygen passed into this flame from an ordinary blowpipe. Special experiments with gold wires showed that flames at high temperature give drops which are comparatively smaller only because the highest layer of the surface of the drop touches solid metal, and must therefore be near the fusing-point of the metal. In thick wires the influence of the high temperature of the flame must have least play, and hence these give in general the highest values of the constants α . In this arrangement of the experiment the author thought the highest values of α to be the most trustworthy.

The following are measurements on a series of platinum wires in confirmation of the relation expressed by equation (3):—

Diameter of the wire. $2r$. millim.	Weight of the drop. P . gr.	Capillarity-constant. α . mgr.
0.5675	0.2912	163
0.3689	0.2055	177.4
0.1921	0.0996	165.1
0.0993	0.0530	169.8
0.0767	0.0410	169.9
		<hr/> 169.04

The other metals dropped out of a glass funnel which terminated below in a vertical tube of the diameter $2r$ in the clear. The magnitude of the drop is then also determined by equation (3); yet it is far more difficult to obtain concordant results than by the above method, even if the velocity of efflux of the fused metal (which determines the size of the drop) is made as small as possible.

The capillarity-constants of fused metals α thus determined are collated in the following Table, and, as a comparison, the values found for mercury* and water† are added. The values of α are given in milligrammes. The constant a , by which Poisson designated the height to which a liquid rises against a vertical side that it moistens, may be calculated by the aid of the equation

$$\alpha = \frac{a^2}{2} \sigma,$$

in which σ is the specific gravity of the metal in question as compared with water.

	Capillarity- constant. α . mgr.	Fusing- point.
Platinum	169·04	> 1600°
Gold	100·22	1200
Tin	59·85	230
Lead	(56·14)	330
Mercury	55·21	— 40
Silver	40·94	1000
Glass	18·09	(1100?)
Water	8	0

XXXVI. *On certain Silicic Acids and the Anhydrous Mineral Silicates.* By J. EMERSON REYNOLDS, *Member of the Royal College of Physicians, Keeper of the Minerals and Analyst to the Royal Dublin Society*‡.

THE recent changes which have taken place in the modes of viewing chemical reactions and the constitution and metamorphoses of the products of our laboratories, have hitherto exercised but little influence upon the study of mineralogy. Efforts have been made, as will presently appear, to replace the old equivalent formulæ of minerals by the atomic notation now

* Pogg. *Ann.* vol. cv. p. 33 (1858).

† *Fortschritte der Physik*, vol. xxxi. (1867) p. 107.

‡ Communicated by the Author.

generally employed in expressing chemical changes. But little attention has hitherto been given to these attempts to bring mineral chemistry into accordance with other branches of the science. This indifference may have resulted, on the one hand, from a dislike to alter conventional modes of expression, and on the other from the consciousness that the new formulæ were less simple, though perhaps more accurate, than those written according to the old plan. Whatever the causes may have been, the fact is that few use the "new system" in writing the formulæ of minerals. That a change is most desirable I think few will deny; the question really is how such can be advantageously effected. The adaptation of the old formulæ of the mineral oxides, hydrates, sulphates, carbonates, phosphates, sulphides, arsenides, &c. to those of the new system is a very easy matter. But the alteration in the case of the silicates is very considerable; and since these bodies constitute nearly two-thirds of the solid crust of the globe, it is of great importance that any formulæ by which their composition may be represented should be capable of exhibiting the natural relations of individuals.

We have already had the advantage of ascertaining the views of Dr. Odling* on this subject, of Dr. Frankland†, Mr. Watts‡, M. Adolphe Wurtz§, M. Naquet||, and others; but it must be confessed that we have not yet arrived at a satisfactory mode of expressing the constitution of the silicates, notwithstanding the amount of ingenuity and chemical ability which has been brought to bear upon the question.

Professor Dana, with his usual care and the clear appreciation of scientific truth which marks his works, has entered largely into the subject of the unitary formulæ of minerals in the new edition of his 'Descriptive Mineralogy,' just issued from the press. In most cases he gives the formulæ of minerals on both systems; but in writing those of the silicates he departs to some extent from the ordinary rules even of the new system, and adopts the notation proposed by him in his recent papers on the subject¶. Whether the change is an advantageous one or the reverse remains to be decided; but since the new formulæ, as given by

* Philosophical Magazine, 1857.

† Lecture Notes, 1866, pp. 97 & 175.

‡ Watts's 'Dictionary of Chemistry,' 1863, especially vol. v. pp. 240-263.

§ Modern Chemical Philosophy. The valuable translation of this work by Mr. William Crookes, F.R.S. 1867, p. 157.

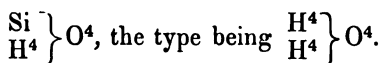
|| Principles of Chemistry: founded on modern theories, translated by W. Cortis: 1868, p. 135. In Naquet's work, only the hydric silicates are treated of; but these are regarded as types of the native metallic silicates; hence the views of this chemist on the constitution of most of the latter salts can be inferred from his statements in reference to the former bodies.

¶ American Journal of Science, vol. xlv. pp. 145, 252, 398.

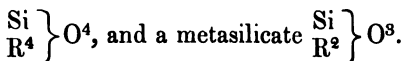
Dana, do not appear to contrast favourably as regards simplicity with the older ones, I venture to bring forward some views which I have long held on the symbolic notation for the anhydrous silicates and on some points in their constitution. These views were expressed in substance at one of the meetings of the Royal Geological Society of Ireland last session; and the present juncture seems a favourable one for discussing them and exhibiting some advantages derivable in practice from their application.

In the following observations it will be understood that the new atomic weights for the elements are those employed, unless otherwise specified.

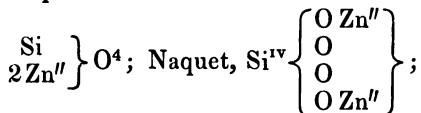
It is usual at the present time to represent the silicates as constructed on the type of water; thus the normal* hydrate of SiO_2 is



A normal silicate is therefore



This, as we all know, is the general plan adopted by modern chemists for expressing the composition of these salts, variously modified, however, by each author to suit his idea of the mode of representing the constitution of the molecule. Thus Odling or Wurtz would represent *Willemite* as



Frankland, $\text{Si ZnO}^{2''}$; and Dana, $\text{Si} \parallel \text{O}^4 \parallel \text{Zn}^2$.

On ordinary grounds there can be no objection to the use of the formulæ just given; but practically they all have the very serious defect of hiding the "oxygen ratio" of the compound, and we shall see presently that this defect is exaggerated as we ascend in the series. It is difficult to understand how mineralogists can do without the aid which this ratio affords in the determination of species; and it appears to me to be of the utmost importance for the interests of practical mineralogy, that any formulæ now employed should be capable of exhibiting the above ratio as clearly as those written in the old way did. I am well aware that in order to do this it would *appear* to be necessary to surrender typical formulæ altogether; but it will be

* In the following pages I have adopted, for the sake of harmony, the nomenclature and order of description of the silicates which Professor Dana employs in the new edition of his 'Mineralogy.' Dana calls the normal salt a *unisilicate*, and the second compound a *bisilicate*.

presently seen that no such sacrifice is really needed. I have little doubt that if a reasonable compromise could be effected, the transition from the notation of the old to that of the new system would become so easy that many mineralogists might be induced to adopt the latter who are now opposed to the change. I hope to show presently that this may be done in a satisfactory manner.

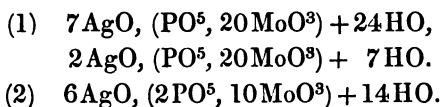
It might be objected that the exhibition of the oxygen ratio of a silicate by a formula is not an accurate statement of fact—that the oxygen acts merely in binding together the acidific and basic elements of the compound, and that the amount of oxygen employed in this way can be only measured by the atomicities of the principal constituents of the salt. To this it might be fairly replied that but little is *really* known of the function of the oxygen contained in a silicate; and though the above statements may be true, all the decompositions with which the mineralogist is acquainted lead him to the general conclusion that each atom of silicon ($=28$) in a silicate has two of the atoms of oxygen more intimately associated with it than the next one. As a striking example of such a decomposition, we might instance the direct liberation of SiO^2 on fusion of an anhydrous silicate with the residue of the ignition of microcosmic salt. It appears to me to be quite as reasonable to separate the oxygen of a silicate into two sections in accordance with such a decomposition, as to divide the oxygen into portions according as part may be supposed to bind together the molecular edifice, and the residue not to be so engaged*. In the absence of distinct proof to the contrary, the utility of the former course would appear to render its adoption preferable.

Accepting SiO^2 ($\text{Si} = 28$) instead of SiO^3 ($\text{Si} = 21$) for silica as the constitution of that body, which has been established beyond a doubt by the recent admirable researches of MM. Friedel and Crafts†, I take, *in a restricted sense*, the view now generally admitted by chemists, viz. that silicic anhydride is capable of almost indefinite polymeric modification; that is to say, we may have not only SiO^2 , but Si^2O^4 , Si^3O^6 , Si^4O^8 , &c., all being simple multiples of the primary SiO^2 . The chief evidence in favour of this theory is afforded by the composition of some of the silicic hydrates, the constitution of which can only be satisfactorily explained on the above view.

* It is necessary to state distinctly here, that in taking this view of the matter I in no way seek to represent that the molecule, in its normal state, is made up of a metallic oxide and silica according to the plan of Berzelius; and it will be seen that I guard against such an idea in the construction of the formulæ adopted, which shows the mutual dependence of the elements constituting a compound.

† *Ann. de Chim. et de Phys.* (4) vol. ix. p. 5.

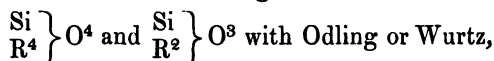
In addition to this, a simple explanation is also afforded of the causes of the great variation in the characters of silica, whether obtained native or prepared artificially. I would further suggest that the recent researches of M. Debray* on the phospho-molybdic acids bear very strongly upon the same point. But very recently some particularly well-defined and crystallized compounds of phosphoric and molybdic acids have been discovered by M. Debray, the composition of which shows that molybdic acid is capable of undergoing an amount of molecular condensation of which we had previously no conception. This is best illustrated by the formulæ of the silver-salts of two of these phospho-molybdic acids. The old notation is that employed by the author.



It would thus appear that we have not only MoO^3 , but at least two other polymers of this— Mo^5O^{15} and $\text{Mo}^{20}\text{O}^{60}$. It is well worthy of remark that the first of these two bodies can be obtained only from alkaline solutions, and the second only from acid liquids, and one modification can be speedily changed into the other by simply realizing either condition. Tungstic acid has been found to possess similar power of atomic condensation.

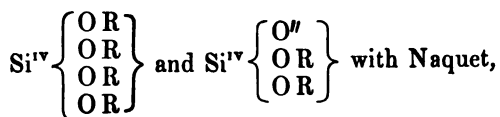
Molybdic, tungstic, and silicic anhydrides and acids are well known to present many chemical analogies; the proof of this power of condensation in the cases of molybdic and tungstic acids, combined with our previous knowledge of the silicic hydrates and of silica itself, warrants us in attributing the same power to silicic acid, more particularly when we shall find the composition of the silicates and their relations to be easily explicable on such a view. In reality, however, this is nothing more than the adaptation of one of the well-known principles of organic chemistry relating to the condensation of radicals—the mode of generation of compounds of this order being thus precisely stated by M. Wurtz† to be “based on the one hand upon the principle of the *accumulation of polyatomic radicals*, and on the other hand upon the *successive dehydration* which may be undergone by hydrates containing polyatomic radicals.”

The plan upon which I construct the formula of the silicates is as follows :—Instead of writing a unisilicate or a bisilicate

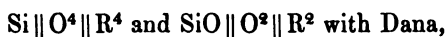


* Chemical News, vol. xvii. p. 183.

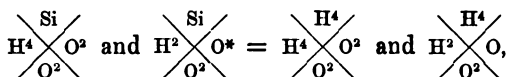
† Crookes's translation of Wurtz's 'Modern Chemical Philosophy.'



or



I employ the following form, constructed, as will be readily seen, on a modified water-type,



1 : 1 and 1 : 2 being the oxygen ratios.

The use of this construction enables me to avoid the anomaly already referred to. This plan is also adopted in order to show the mutual dependence of the elements constituting the molecule of a silicate, the mode in which it is capable of breaking up under the influence of certain reagents, and the oxygen ratio of the compound, and, further, for the purpose of clearly exhibiting the relations of the salt of a condensed acid to its analogues amongst the lower terms of the series. It appears to me that these advantages are gained by the above construction, without involving our acceptance of any particular theory as to the *absolute structure* of the molecule.

In adopting this plan in the cases of the silicates the formulæ of which are given below, I seek to show much more than the mere composition of some of the minerals belonging to the several groups. If we disregard the minor subdivisions, it is shown that all the anhydrous mineral silicates may be divided into two great classes—(1) the silicates which contain in each molecule the elements of SiO^2 only, and (2) those which contain but $3(\text{SiO}^2) = \text{Si}^3 + \text{O}^6$, or a multiple of this quantity.

When the force of this division is realized, I believe that much of the difficulty at present attending the study of the mineral silicates will be found to have disappeared; moreover the relations of groups of minerals become quickly evident when their formulæ are constructed in accordance with the principles above stated.

* It is obvious that this is equivalent to writing the first hydrate $\frac{\text{Si}}{\text{O}^4}$ and the second $\frac{\text{H}^2}{\text{O}^3}$, but by the construction adopted in the text it

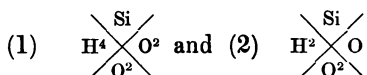
becomes possible to express the "harmonies of variations" of the molecule.

† Hereafter in the text Si (=28) is used to represent the silicon as it occurs in SiO^2 , and Si (=84) as in SiO^3 , the condensed anhydride. The convenience of this distinction is very considerable.

I am well aware that the mode of regarding the native silicates proposed in this paper is not likely to be considered with much favour by those chemists who, with Von Kobell*, dislike the idea of innovation, or others who, with Naquet†, appear to be disposed to place no limit to the power of condensation and successive dehydration of the polysilicic acids. I have therefore sought for such experimental evidence as might be expected to throw any light upon the subject, and hope soon to be able to communicate the results of some experiments in a new direction which promise to give a clue to the modes to be adopted in distinguishing the different conditions of SiO^2 throughout some of their compounds. Much independent evidence, however, is forthcoming; and I will now proceed to show that the composition of the natural and artificial hydrates of SiO^2 can be easily and simply explained in accordance with the principles already stated—and then, that the relations of the native anhydrous salts become apparent when their formulæ are treated in a similar manner.

SILICIC HYDRATES.

1. *Hydrates of SiO^2* .—Frankland‡ states that the normal hydrate (1) is the body separated from a soluble silicate by treatment with a stronger acid. Professor Graham's compound (2) corresponds well to the formula of the dibasic acid§.



In the absence of distinct evidence to the contrary, we may regard the above as silicic hydrates uncondensed. All the salts of the uncondensed SiO^2 are formed on one or the other of the above types.

We have now the second great division of the silicic hydrates and metallic salts, *i. e.* that comprising the hydrates of $\text{SiO}^6 = 3(\text{SiO}^2)$, or a simple multiple of this.

2. *Hydrates of SiO^6* .—(a) First amongst these hydrates stands that prepared and analyzed by Doveri||, and which may be well

represented by the formula $\begin{array}{c} \text{Si} \\ \diagup \quad \diagdown \\ \text{H}^2 \quad \text{O} \\ \diagdown \quad \diagup \\ \text{O}^6 \end{array}$. This corresponds precisely

to Graham's hydrate of the uncondensed acid.

* *Journal für praktische Chemie*, 1859, p. 168.

† *Op. cit.*

‡ Lecture Notes, p. 101.

§ Philosophical Transactions, 1861, p. 204.

|| *Ann. de Chim. et de Phys.* (3) vol. xvi. p. 129.

(b) The next hydrates are those of 2SiO^6 ; and Salvétat's mineral, Randanite*, $\text{H}^2 \frac{\text{Si}^2}{\text{O}^{12}} \text{O}$, exactly corresponds to one hydrate of this body. Fuchs's compound† is probably another hydrate of the same acid, viz. $\text{H}^6 \frac{\text{Si}^2}{\text{O}^{12}} \text{O}^3$; and we may have Ebelmen's compound‡ as a third of the series, $\text{H}^{12} \frac{\text{Si}^2}{\text{O}^{12}} \text{O}^9$.

(c) In this subdivision we have the hydrates of 3SiO^6 , which are few in number. Some varieties of semiopal appear to consist of the first hydrate, $\text{H}^2 \frac{\text{Si}^3}{\text{O}^{18}} \text{O}$. It will be seen further on that orthoclase can be best represented as constructed on the type of the hydrate $\text{H}^{12} \frac{\text{Si}^3}{\text{O}^{18}} \text{O}^6$.

It is a singular fact, and one well worthy of remark in this connexion, that the proportion of water found in all the native hydrates of silica hitherto analyzed is such that the specimens containing most water would be best represented by the formula

$\text{H}^2 \frac{\text{Si}}{\text{O}^6} \text{O}$, and those containing least water fall into the *c* series

above, as $\text{H}^2 \frac{\text{Si}^3}{\text{O}^{18}} \text{O}$, while Randanite, $\text{H}^2 \frac{\text{Si}^2}{\text{O}^{12}} \text{O}$, is the intermediate term required. It is quite true that we know little of the function of the water in these compounds; but the above proportion would appear to be the result of more than mere accident, especially when viewed in connexion with the relations exhibited below between some of the silicic hydrates and their corresponding metallic salts.

* Quoted in Dana's 'Descriptive Mineralogy,' new edition, 1868, p. 200. The above is the formula of the mineral dried at 100°C .

† *Ann. der Chem. und Pharm.* vol. xxxii. p. 119.

‡ *Ann. de Chim. et de Phys.* (3) vol. xvi. p. 129.

GENERAL CLASSIFICATION OF THE ANHYDROUS SILICATES.

Acid or Bisilicates.

	New atomic weights.	Old atomic weights*.	Oxygen ratio.
Class 1. Type of class .	$\begin{array}{c} \text{Si} \\ \text{H}^2 \times \text{O} \\ \text{O}^2 \end{array}$	$\begin{array}{c} \text{Si} \\ \text{H} \times \text{O} \\ \text{O}^2 \end{array}$	1 : 2.
Class 2. „	$\begin{array}{c} \text{Si} \\ \text{H}^6 \times \text{O}^3 \\ \text{O}^6 \end{array}$	$\begin{array}{c} \text{Si} \\ \text{H}^3 \times \text{O}^3 \\ \text{O}^6 \end{array}$	1 : 2.
Class 3. „	$\begin{array}{c} \text{Si}^2 \\ \text{H}^6 \times \text{O}^3 \\ \text{O}^{12} \end{array}$	$\begin{array}{c} \text{Si}^2 \\ \text{H}^3 \times \text{O}^3 \\ \text{O}^{12} \end{array}$	1 : 4.

All the acid silicates can be well represented on either of the above three types.

Normal or Unisilicates.

Class 1. Type . . .	$\begin{array}{c} \text{Si} \\ \text{H}^4 \times \text{O}^3 \\ \text{O}^2 \end{array}$	$\begin{array}{c} \text{Si} \\ \text{H}^2 \times \text{O}^2 \\ \text{O}^2 \end{array}$	1 : 1.
Class 2. „ . . .	$\begin{array}{c} \text{Si} \\ \text{H}^{12} \times \text{O}^6 \\ \text{O}^6 \end{array}$	$\begin{array}{c} \text{Si} \\ \text{H}^6 \times \text{O}^6 \\ \text{O}^6 \end{array}$	1 : 1.
Class 3. „ . . .	$\begin{array}{c} \text{Si}^2 \\ \text{H}^{12} \times \text{O}^6 \\ \text{O}^{12} \end{array}$	$\begin{array}{c} \text{Si}^2 \\ \text{H}^6 \times \text{O}^6 \\ \text{O}^{12} \end{array}$	1 : 2.
Class 4. „ . . .	$\begin{array}{c} \text{Si}^3 \\ \text{H}^{12} \times \text{O}^6 \\ \text{O}^{18} \end{array}$	$\begin{array}{c} \text{Si}^3 \\ \text{H}^6 \times \text{O}^6 \\ \text{O}^{18} \end{array}$	1 : 3.

Basic Silicates.

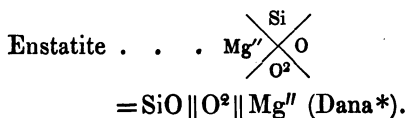
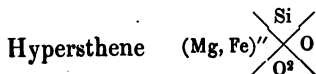
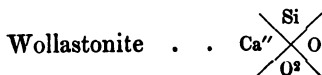
Class 1. Type . . .	$\begin{array}{c} \text{Si} \\ \text{H}^{16} \times \text{O}^9 \\ \text{O}^6 \end{array}$	$\begin{array}{c} \text{Si} \\ \text{H}^9 \times \text{O}^9 \\ \text{O}^6 \end{array}$	4 : 3.
Class 2. „ . . .	$\begin{array}{c} \text{Si} \\ \text{H}^{18} \times \text{O}^9 \\ \text{O}^6 \end{array}$	$\begin{array}{c} \text{Si} \\ \text{H}^9 \times \text{O}^9 \\ \text{O}^6 \end{array}$	3 : 2.
Class 3. „ . . .	$\begin{array}{c} \text{Si} \\ \text{H}^{24} \times \text{O}^{12} \\ \text{O}^6 \end{array}$	$\begin{array}{c} \text{Si} \\ \text{H}^{12} \times \text{O}^{12} \\ \text{O}^6 \end{array}$	2 : 1.

* By contrasting the old and new type-formulæ, when constructed on the plan proposed in this paper, it will be seen how very easy the transition from the equivalent to the atomic notation really is.

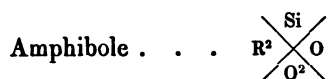
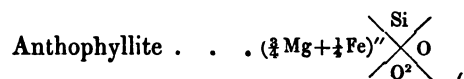
I am well aware that modern views on the molecular construction of condensed compounds would oblige us to deny the possibility of obtaining *some* of the above hydrates in the free state. It need scarcely be remarked that though this is admittedly the case with such unstable bodies as the polysilicic hydrates, yet it is well known that the metallic salts of condensed acids may exist though the isolation of their corresponding hydrates would be a matter of doubt, or (according to present ideas) an impossibility. Whatever views on the subject may ultimately obtain acceptance, they are not likely to alter the above general modes of expression for the atomic ratios of the best-known native silicates. In the cases of the basic or subsilicates, it is shown above that they correspond to one or other of three hydrates of the first stage of condensation of SiO^2 dealt with in this paper, viz. SiO^6 . In this way their relations can be clearly brought out, though the cause of the peculiar ratio of $R(=H)$ to Si is difficult to understand. The normal or unisilicates ought to be the most highly basic of these salts; and yet we find the subsilicate of Class 3 containing twice as much basic element as an unisilicate. According to Dr. Odling this anomaly may be explained thus:—“If we consider alumina as always basic, then there are silicates” (such as those above typified) “in which the basicity exceeds that of the tetrabasic group; but the basic or acidulous function of alumina is yet *sub judice*; and if we exclude aluminous minerals, we shall find that the tetrabasic are the most highly basic of all known silicates, natural or artificial.” These remarks are specially directed to the proof of the conformity of silicic to other acids; but they obviously apply forcibly to the question of the constitution of the subsilicates on the types I have given above.

Bisilicates.

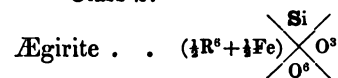
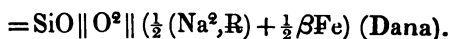
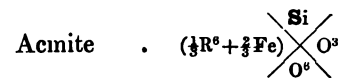
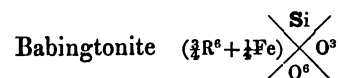
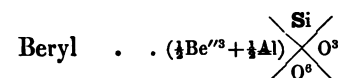
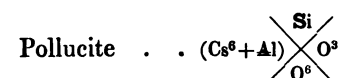
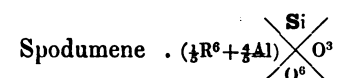
Class 1.



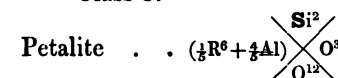
* With each class I give Dana's new formula for the first member of the group.



Class 2.



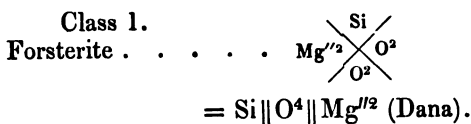
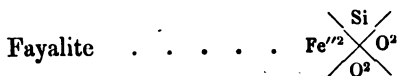
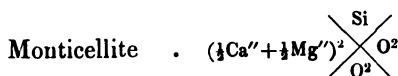
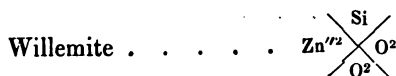
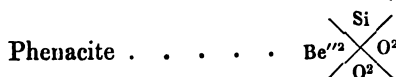
Class 3.



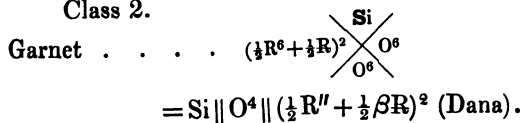
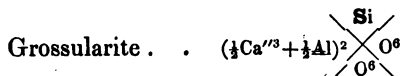
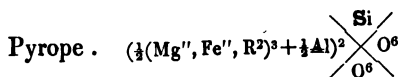
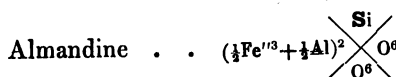
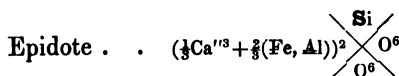
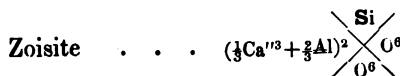
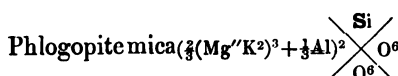
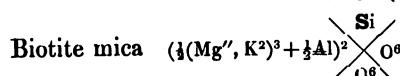
The last is the only member of Class 3 occurring in this division. The relation of petalite to spodumene and other members of the section is thus clearly shown by the formulæ.

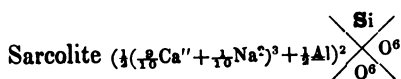
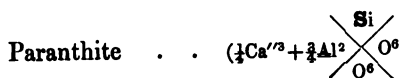
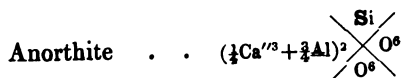
Normal or Unisilicates.

Class 1.

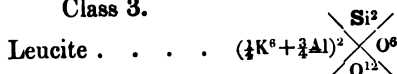
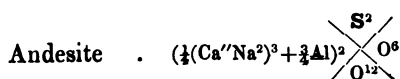
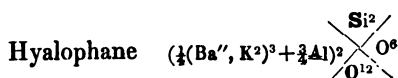
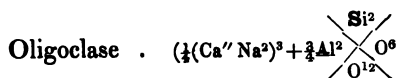


Class 2.

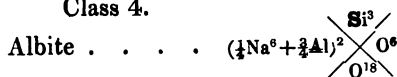
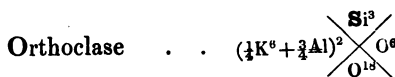




Class 3.



Class 4.



As these lists of minerals are only intended to illustrate the application of the principle of classification proposed in this paper, I have carefully avoided entering upon doubtful questions by expressing the constitution of silicates which have been hitherto insufficiently examined.

The above arrangement of the formulæ renders remarkably evident the relations existing between the members of the felspar and other families. This group of minerals illustrates the mode of accumulation of the anhydride SiO^6 upon itself without otherwise disturbing the arrangement of the molecule; and the composition of these silicates appears to me forcibly to testify that *the normal rate of condensation of the molecule of SiO^2 is three, or some simple multiple of that number.*

It is a well-known fact that anorthite and leucite are two im-

portant felspars of the volcanic rocks, just as albite and orthoclase are of the granitic rocks. Of these four (and other intermediate compounds), however, leucite and orthoclase are the most directly comparable, being both potassium and aluminium felspars, evidently formed on precisely the same type, yet differing by the compound molecule SiO^6 . It now becomes a matter of much interest to attempt an explanation of this remarkable and yet methodic variation in the acidific portion of these felspars. With the assistance derived from some known properties of the silicic hydrates, and the Rev. Professor Haughton's valuable translation of Durocher's memoir on Comparative Petrology*, I think it is possible to understand the conditions under which one or other of these felspars can be formed, and the mode of production of either. We are already well aware of the fact that leucite is convertible into orthoclase by removal of potash and alumina by aqueous action; but this information gives little aid in the direction we require.

It has been already stated that the silicic hydrates are very unstable bodies, desiccation of a hydrate, $\text{H}^2 \overset{\text{Si}}{\underset{\text{O}^6}{\times}} \text{O}$, at 100°C .

being sufficient to convert it into the polysilicic hydrate $\text{H}^2 \overset{\text{Si}}{\underset{\text{O}^6}{\times}} \text{O}$.

Again, the mineral Randanite of Salvétat, already several times referred to, when dried completely at ordinary temperatures has

its composition represented by the formula $\text{H}^2 \overset{\text{Si}}{\underset{\text{O}^6}{\times}} \text{O}$; on drying at 100°C . for a long time, however, its composition then is

$\text{H}^2 \overset{\text{Si}^2}{\underset{\text{O}^{12}}{\times}} \text{O}$. Possibly, on still further heating, another compound would be obtained. It is easy to understand, therefore, how a silicic acid may yield a polysilicic acid by the simple loss of water. But the metallic base of a silicate cannot be got rid of so easily. Heating of the compound alone cannot alter it, notwithstanding the marked tendency of a silicate of a low type to pass into a polysilicate; for as the temperature increases, this tendency likewise grows. Assuming that a typical silicate is capable of existing in a semifused magma, and that its formula is

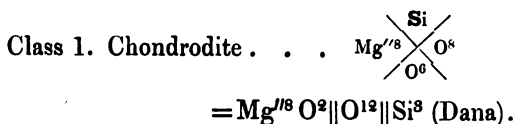
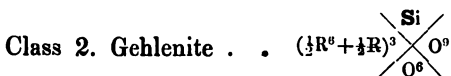
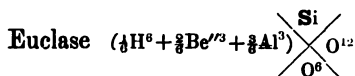
$(\frac{1}{4}\text{K}^6 + \frac{3}{4}\text{Al})^2 \overset{\text{Si}}{\underset{\text{O}^6}{\times}} \text{O}^6$, it obviously cannot lose its basic elements,

* Haughton's 'Manual of Geology,' 1866, p. 16. Durocher's important essay is here given in full, and contributes probably the best aid to be found to the study of the paragenesis of minerals.

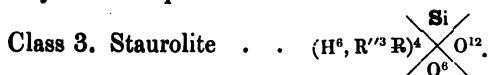
unless some stronger affinity than that of the SiO^6 comes into play; yet its tendency is to pass into leucite, which mineral would result from the addition of SiO^6 to the above group. Though it cannot undergo the change *by loss of base*, it can, and no doubt does, *by increment of acidulous radical*; it will seize any silica which the necessity of the compound molecule enables it to displace from feeble combinations. The result of this is that the SiO^6 is, or its elements are, condensed within the molecule of the silicate, the tendency to accumulation of the polyatomic radical is *partially* satisfied, and *leucite* is the result. Yet this is supposed to take place in the magma which on cooling gives the volcanic rocks; and Durocher tells us, and numerous analyses prove amply, that these rocks are essentially *basic*, and comparatively poor in silica. It would appear, then, that the tendency of the acid radical of the ideal silicate to reach the second stage of condensation is sufficient to enable it to overcome the opposing influences around it in order that leucite may be produced; but the cumulative power is not enough to enable it to pass into orthoclase by the appropriation and condensation of the silica of other compounds. When we come to the granitic rocks, however, no such paucity of silica is observed; on the contrary, we have this body largely in excess, and the necessary consequence is, that in these acid rocks we meet with orthoclase, a mineral in which the acid radical appears to have reached its greatest proportion, and the silica to have completely satisfied its tendency to accumulation within the molecule.

In concluding this section, I may remark that the third stage of condensation of SiO^6 is the highest which appears to have been reached in any mineral. Dana* regards the presence of potassium and sodium in a felspar as the condition which determines the greater or less proportion of silica in such a compound. There appears to be much truth in this; but I think the genesis of a mineral, as instanced above in examining the relations of leucite and orthoclase, should also receive full attention. When this important point is weighed in connexion with the explanation above offered of the causes which have probably operated in the production of one or other mineral, we see still more clearly than before the profound isotypism of the felspar family; and not only this, but we have distinctly brought out the peculiar rate of condensation of silicon within the molecule of some silicates which it has been the object of the present paper to call attention to as affording a simple means of classifying the salts of normal and polysilicic acids.

* American Journal of Science, vol. xlv. p. 401.

Basic or Subsilitates.

Probably the composition of datholite and tourmaline can be also best exhibited on a similar plan, if we regard the boric anhydride as a basic constituent. It need scarcely be said that this is a very doubtful point.



It will have been already noticed that I have rarely dealt with the formulæ of hydrous metallic silicates. So little is really known of the function of the water contained in them, that it seemed undesirable to dwell upon their atomic formulæ at present; at the same time it is necessary to say that the composition of large numbers of them can be easily represented in accordance with the plan proposed in this paper. This is especially the case with the interesting family of the zeolites, the relations of which to that of the felspars can be then clearly traced.

In the preceding pages I have done little more than note down the main points which I sought to give prominence to. In every section numerous cases occur which admit of extended treatment, yet these have not been dwelt upon, nor have more formulæ been given than were absolutely necessary to facilitate explanation. This course seemed preferable, as it then became possible to give an outline of the subject without extending this paper beyond due limits.

In conclusion, I may sum up the main points of this paper
Phil. Mag. S. 4. Vol. 36. No. 243. Oct. 1868. U

in the following terms. Some evidence has been brought forward to show :—

(1) That by an alteration in the construction of formulæ, the atomic ratios of the silicates may be easily expressed in accordance with the principles of modern chemistry, while some practical advantages are gained, and those attending the use of the old formulæ are retained.

(2) That the constitution and relations of the silicic hydrates and salts are best understood by regarding them as (*a*) containing the elements of the primary SiO^2 , or (*b*) a polymer of this.

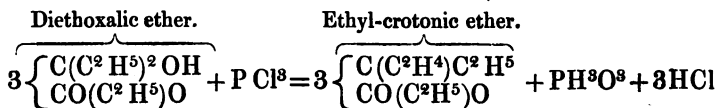
(3) That the rate of condensation of SiO^2 within the molecule of a native anhydrous salt is represented by the number 3, or a multiple of this by 2 or 3 *only* = SiO^2 , SiO^6 , 2SiO^6 , 3SiO^6 ; that this alteration can take place without interfering with the general type of the compound, and that the passage from one stage of accumulation to another can be easily explained by reference to the known properties of normal and polysilicic hydrates.

It is also shown that the silica, hitherto regarded either as *basic* or as *accessory* in such minerals as the feldspars, is present really as the result of the *partial or complete satisfaction* of the tendency of SiO^6 to pass successively into 2SiO^6 and 3SiO^6 , the chemical conditions of the mass in which the mineral may have been formed determining the degree of condensation within the molecule.

XXXVII. *Limited Oxidation of Ethyl-crotonic Acid.*

By ERNEST THEOPHRON CHAPMAN and MILES H. SMITH*.

THE ethyl-crotonic acid employed in the following experiments was prepared from diethoxalate of ethyl in the manner described by Frankland and Duppa. The process consists in adding trichloride of phosphorus to the diethoxalic ether†. The ether loses one atom of water, thus :—



At the same time red amorphous phosphorus is deposited. This, we suppose, owes its origin to an action between the phosphorous

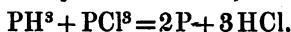
* Communicated by the Authors.

† For the preparation of this ether see "Synthetical Researches on Acids of the Lactic Series," by Frankland and Duppa, Phil. Trans. for 1866, p. 309; and also Journal of the Chemical Society, April 1867, where an improved method of preparing the ether is described by us.

acid and the excess of trichloride of phosphorus. The phosphorous acid has a tendency to split up into phosphuretted hydrogen and phosphorous acid, thus:—



And P H^3 , acting on trichloride of phosphorus, is known to yield phosphorous acid and hydrochloric acid, thus:—



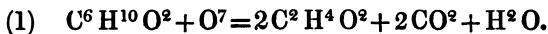
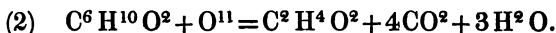
At any rate more than the theoretical quantity of trichloride is required to convert the ether into the new compound.

Ethyl- or ethocrotonic* ether and acid have been so fully described that we have little or nothing to remark with regard to them. We titrated the ether with the following result:—

4.263 grms. required alcoholic potash equivalent to .96423 of potassium. Therefore 100 parts of the ether neutralized 27.31 parts of potassium; theory requires 27.53. The ether was therefore pure.

The acid was prepared from the ether, and subjected to the action of bichromate of potash and sulphuric acid. An experiment on an unknown quantity of substance proved that acetic and carbonic acids were the sole products of the oxidation. This determination was made by methods which have been fully described elsewhere†.

The only point remaining to be ascertained, therefore, was, Does the ethyl-crotonic acid split up into two atoms of acetic acid and two of carbonic, or into one atom of acetic acid and four of carbonic? To ascertain this, and at the same time to check the non-quantitative portion of the work, a determination of the amount of oxygen required to effect the change was made by the method already described by one of us‡. If the decomposition took place so as to yield two atoms of acetic acid and two of carbonic acid, seven atoms of oxygen only would be required, whereas the other decomposition would require eleven atoms.



A known weight of the acid was sealed up with a known quantity of a standard solution of bichromate in dilute sulphuric acid. The tube was heated for some hours in the steam-bath. It was then cooled and opened, and its contents transferred to a small flask. The tube was washed out, and the washings also poured into the same flask. The flask was heated in the water-bath, and

* Frankland and Duppa, Chem. Soc. Journ. 1865, p. 133.

† Chapman and Thorp, Journ. of Chem. Soc. p. 477 (1866).

‡ Chapman, Journ. of Chem. Soc. 1867, p. 227.

then, whilst still hot, placed under the receiver of the air-pump. Of course this caused the contents of the flask to boil, and thereby effectually expelled any carbonic acid that might be dissolved in the liquid. The strength of the standard chromic solution had previously been ascertained by causing it to act on excess of oxalic acid. The carbonic acid so liberated was dried, absorbed by potash, and weighed. Two equivalents of carbonic acid correspond to one of available oxygen.

The chromic acid which had not been reduced by the ethyl-crotonic acid was now estimated in the same way, and the carbonic acid so obtained deducted from the amount which the known quantity of chromic solution would have liberated had no portion of it been reduced. From the difference it is easy to calculate how much oxygen the acid had consumed.

The details of the operation are as follows :—

Strength of chromic solution. 100 grms. of the solution liberated 7·5182 grms. of carbonic acid from oxalic acid.

Amount of chromic solution employed 51·02 grms.

Ethyl-crotonic acid employed 4316 grm.

Carbonic acid obtainable from unaltered chromic solution	} 3·8358
--	----------

Carbonic acid obtained after digestion	1·4496
--	--------

Difference, due to oxygen consumed	2·3862
--	--------

Two equivalents of carbonic acid correspond to one of oxygen.
Therefore $2\text{CO}^2 = 88 : \text{O} = 16 :: 2·3862 : x$,

$$x = 433854.$$

Dividing this by the substance taken, we learn the percentage of oxygen consumed, viz. 100·47

Theory for seven equivalents 98·24

Theory for eleven equivalents 154·38

Therefore equation (1) is evidently correct.

The above work was the first step in a proposed investigation, by oxidation, of the acids of the acrylic series. It was intended to collect these acids and treat them in a similar manner; but the difficulty of obtaining any of these acids, excepting those which could be formed synthetically from the corresponding secondary lactic acids, proved so great that the investigation was temporarily abandoned. Should opportunity offer we intend to resume it.

Laboratory, London Institution.

XXXVIII. *On the Polarization of Heat radiated from the Surfaces of Liquids.* By Professor MAGNUS*.

SINCE my communication of March 5 last†, containing the results of an investigation on the polarization of the heat radiated by solid bodies when heated to 100° C., I have been occupied with the examination of the heat radiated by liquids. In these experiments it was impossible to employ the same form of apparatus as for solids, because it was necessary for the radiating surface to be horizontal. The whole portion of the apparatus containing the reflecting mirror, and the box containing it, was therefore placed upon a plane inclined at an angle of 35° to the horizon. The radiating vessel consisted of a completely closed box 12 centims. broad, 21 centims. long, and 4 centims. high, which was maintained at a temperature of 100° C. by a current of steam. This box was placed in a horizontal position; and upon it was fastened a tray of sheet iron of equal length and breadth, but only 1 centim. deep. The radiating liquid was placed in this tray.

In order to test this arrangement, a plate of polished tin plate was first introduced into the tray instead of the liquid. The polarization of the heat radiated by this plate was found to be almost exactly the same as when the former arrangement was employed‡, namely 25·8 and 26·0 per cent. in two experiments. Also when a plate of transparent glass was placed in the tray instead of a liquid, almost the same value for the polarization was found as previously§, namely 11·9, 11·7, and 9·82 per cent. in three experiments. Consequently this modification of the apparatus gave results as trustworthy as those given by the former one. Such verification of the apparatus was essential, because it was not possible to give the radiant surface various azimuths as with radiating solids.

Only such liquids could be used as boil at a temperature above 100° C.; and there are only a few of these which do not, at this temperature, evaporate rapidly and give rise to clouds in such a manner as to obstruct the radiation. Consequently the number of liquids which could be employed was exceedingly small. Neither amyl-alcohol, boiling at 120° C., nor aniline, boiling at 180° C., could be used, on account of the formation of these clouds. The

* Translated by Frederick Guthrie, F.R.S.E., from the *Monatsbericht der königlich preussischen Akademie der Wissenschaften zu Berlin*. April 1868, p. 249.

† Phil. Mag. September 1868.

‡ Ibid.

§ Ibid.

experiments were therefore confined to mercury, fatty oils, glycerine, melted resins, paraffine, and such like.

Liquids present also an especial difficulty in consequence of their surfaces, when the liquids are warmed, not being so flat as they are at ordinary temperatures. In all cases when we examine a liquid which is of a somewhat higher temperature than the surrounding air, we find the image reflected from it to be no longer completely well defined. Heat, in fact, is continually given off from the surface, the cooler portions sink to make room for the warmer portions which rise; and hence are produced movements, small indeed, but which, in consequence of their taking place to a greater extent at one part than at another, give rise to different motions of the surface at different places, and do not allow it to retain its smoothness.

Consequently too much reliance must not be placed upon the numbers. Nevertheless it was found that a portion of the heat radiated from all the liquids examined was polarized. Mercury showed a polarization of 32·0 and 32·2 per cent., a value at least as great as that found for solid metals*. Other liquids, on the contrary, gave less values.

Rape-oil . . .	{ 6·17 per cent.
	{ 5·11 „
Colophony . . .	7·26 „
White wax . . .	7·3 „
Glycerine . . .	5·61 „
Paraffine . . .	5·0 „

In order to see if the diathermancy of the liquid had influenced the results, the bottom of the tray containing the liquid was covered with a bright metal plate, with a blackened metal plate, and with lampblack, in succession, before pouring in the liquid. Under each of these conditions the same values were found for the same liquids. It hence appears that the radiating-power of the bottom of the tray has no influence in these experiments, and that consequently the liquids examined are athermanous for heat of the low temperature here employed.

The following is a Table of the values for all the substances hitherto examined :—

* Phil. Mag. *loc. cit.*

Substance.	Polarized portion of the heat radiated at an angle of 35° .
Tinned iron	27·6 per cent.
Copper	22·4 „
Aluminium	28·5 „
Mercury	32·0 „
Transparent glass . .	10·4 „
Black glass	12·4 „
Rape-oil	5·64 „
Colophony	7·26 „
White wax	7·3 „
Glycerine	5·61 „
Paraffine	5·0 „

From these results we are justified in concluding that all substances, whether solid or liquid, radiate from their flat surfaces heat which is partly polarized when it makes an angle of about 35° with the surface.

XXXIX. *Note on Successive Involutes to a Circle* *.

By J. J. SYLVESTER †.

IT is surprising that the families and groups of families of forms capable of being educed by successive involution from a circle should not have attracted the attention of geometers. I find, if any, not more than a passing allusion to their existence in Dr. Salmon's 'Higher Plane Curves,' in the 'Integral Calculus' of Mr. Todhunter, or in the memoirs of the late Dr. Whewell in the Cambridge Philosophical Transactions (vols. viii and ix.), although these latter are exclusively devoted to the study of various curves of mechanical and kinematical origin by aid of the so-called intrinsic equation, which is in fact the natural expression of, and key to, the properties of such like curves. And yet this form of equation almost instantaneously furnishes the general polar equation to the entire system of circular involutes, and exhibits at once their leading properties ‡.

* The germ of this Note was communicated to the Mathematical Section of the British Association at the Norwich Meeting.

† Communicated by the Author.

‡ Professor Rankine and Mr. Merrifield have made a useful application of the *second* involute of the circle to the calculation of the stability of the finite solution of vessels. In the Turkey carpet under my eyes whilst this is being written, I perceive graceful and complicated figures of winding and intersecting scrolls and convolutions, which render it, I think, not at all improbable that the successive involutes of the circle would furnish or suggest many patterns available for decorative purposes: the enormous variety of each kind of involute, which of course increases with the order of derivation, adds to the probability of this conjecture.

Foremost amongst these stands the algebraical form of equation (and that a quantic), which connects not only the arc, but also the squared radius vector with the angle of contingence, and consequently the two former with one another. In a marvellous and, so to say, transcendental fashion, these curves participate in the nature of algebraical curves—their apses, cusps, and points of retrocession being counted by the order of the involution, and becoming imaginary in pairs.

I need hardly say that by a second involute I mean an involute of the first by a third, an involute of a second, and so on in continual progression. To any given curve all its first involutes form a system of parallel curves, so that in general the number of *form*-parameters to a curve will be augmented by i when we pass to its general involute of the i th order. In the case of the circle, however, owing to its homogeneity, the first involute, like the curve itself, contains only one *form*-parameter (it being, in other words, a property of the first involute, that when rotated round a certain point, the curves so generated continue always parallel to each other); and so the number of *form*-parameters in the general i th involute will contain i , and not $i+1$ parameters, as the general formula would require.

I shall use ϕ , s , r , θ to denote the angle of contingence, arc, radius vector, and vectorial angle of the curves under consideration.

Starting from the circle $s = a\phi$, a set of corresponding successive involutes will, as is well known, be represented by

$$s_1 = a\phi^2 + b\phi; \quad s_2 = \frac{a\phi^3}{6} + \frac{b\phi^2}{2} + c\phi,$$

and so on, according to the obvious law

$$s_i = \int d\phi \cdot s_{i-1}.$$

Now in general for any curve whatever, if we call p the perpendicular on the tangent from an arbitrary pole, q the projection of the radius on the tangent, we have

$$q = -\frac{dp}{d\phi}; \quad \dots \dots \dots (1)$$

also

$$p + \frac{d^2p}{d\phi^2} = \frac{ds}{d\phi}, \quad \dots \dots \dots (2)$$

$$p^2 + \left(\frac{dp}{d\phi}\right) r^2 \dots \dots \dots (3)$$

These equations are of course not new*; they are given by

* We have only to take P , P' , two consecutive points, and on PP' , $P'T$,

Mr. Todhunter in the later editions of his 'Integral Calculus,' accompanied with a reference to another English treatise, from

the tangents at P, P', draw perpendiculars from an arbitrary point O, and we obtain at once, by inspection,

$$\delta p = -q\delta\phi, \quad \delta q + \delta s = p\delta\phi,$$

whence

$$q = -\frac{dp}{d\phi}, \quad p + \frac{d^2p}{d\phi^2} = \frac{ds}{d\phi}.$$

Or, again, proceeding analytically, we have

$$x - A = \int ds \cos \phi, \quad y - B = \int ds \sin \phi;$$

whence, integrating by parts,

$$x - A = G \cos \phi - G' \sin \phi,$$

$$y - B = G \sin \phi + G' \cos \phi,$$

where

$$G = s' - s''' \dots; \quad G' = s'' - s'''' \dots;$$

whence

$$r = G^2 + G'^2 \text{ and } G + G'' = \frac{ds}{d\phi}.$$

From which also we may deduce

$$q = r \frac{dr}{ds} = G', \quad p^2 = r^2 - q^2 = G^2.$$

This last demonstration would at first sight seem to be only valid for the case of the G series coming to an end, i. e. of $\frac{ds}{d\phi}$ being a rational integral function in ϕ ; but it would be quite legitimate to infer at once from it the *universality* of the equations above written connecting r^2 with s and ϕ ; for we may write down the general differential equation of the second order

$$d \cos^{-1} \frac{dr}{ds} - d\theta = d\phi,$$

i. e.

$$\frac{d \cdot \frac{dr}{ds}}{\sqrt{(ds)^2 - (dr)^2}} - \frac{\sqrt{ds^2 - dr^2}}{r} = d\phi,$$

in which $\frac{ds}{d\phi}$ may be considered as given, and r or r^2 to be determined.

The equations in question, having been shown to be true for a form $\frac{ds}{d\phi}$ containing an indefinite number of arbitrary constants, evidently can only amount to a transformation of, and may be used in supersession of the equation last written. It may be worth while to set out this latter under a more familiar form of notation. If, then, we use y for r and x for ϕ , and call $\frac{ds}{d\phi} = X$ (any function of x), it becomes

$$\frac{-Xy'' + X'y'}{X\sqrt{X^2 - y'^2}} + \frac{\sqrt{X^2 - y'^2}}{y} = 1,$$

an apparently very complicated form of equation, but admitting of the simple solution $y'^2 = u^2 + u'^2$, where u satisfies the linear differential equation $u + u'' = X$.

which he has taken them; but in themselves, easily as they can be obtained, they contain the whole theory of the remarkable curves to which this note refers. In the case before, $\frac{ds}{d\phi} = F\phi$, where F is (a quantic in, *i. e.*) a rational integral function of ϕ . Hence we have for the solution of (1),

$$p = F - F'' + F''' \dots + A \cos \phi + B \sin \phi,$$

$$p = F'' - F''' + \dots - A \sin \phi + B \cos \phi;$$

wherefore r^2 is known in terms of F and the arbitrary constants A and B , whose values depend on the position of the origin from which r is reckoned, by a due choice of which they may be made to vanish. One will readily suppose that this eligible position of the pole must be the centre of the generating circle; and the proof is as follows:—

If $r_1, r_2, \dots r_i$ be any radii vectores corresponding to $s_1, s_2, \dots s_i$, it is well known, and follows from the definition of the involute, that

$$r_{i+1}^2 = r_i^2 - 2r_i s_i \frac{dr_i}{ds_i} + s_i^2. \dots \dots (4)$$

Now, suppose that for any number i the origin has been so chosen that

$$r_i^2 = (s_{i-1} - s_{i-3} + \dots)^2 + (s_{i-2} - s_{i-4} + \dots)^2,$$

then

$$r_i \frac{dr_i}{ds_i} = \frac{1}{s_{i-1}} \left\{ (s_{i-1} - s_{i-3} \dots)(s_{i-2} - s_{i-4} \dots) \right\} \\ + (s_{i-2} - s_{i-4} \dots)(s_{i-3} - s_{i-5} \dots) \Big\} \\ = s_{i-2} - s_{i-4} + \dots$$

Hence

$$r_{i+1}^2 = (s - s_{i-2} + s_{i-4} \dots)^2 + (s_{i-1} - s_{i-3} + \dots)^2;$$

and the supposed relation, if true for any value i , is true for all superior values; but when the origin is at the centre, $s_1 = a\phi$, $r_1^2 = a^2$; and consequently the equation (4) is true universally*.

* The above result might have been deduced more directly from the equation $\frac{dp_i}{d\phi} = p_{i-1}$, which is true for any curve and its evolute. In fact this paper need never have been written (for all that it contains is a straightforward inference from four equations which may be found scattered up and down in elementary treatises), had it been the custom to regard those equations as forming collectively a connected apparatus. I mean the four following, where the unaccented and accented s and p refer to any curve and its evolute respectively, ϕ being the angle of contingence common in mag-

Thus, then, when the origin is at the centre, whilst for the i th involute to a circle the arc is any quantic $\int d\phi F$ of degree $(i+1)$ in ϕ , r^2 is a quantic in ϕ of degree $2i$ of the particular form $(G\phi)^2 + (G'\phi)^2$, where $G(\phi)$ may be supposed, if we please, to be any quantic of the order i or ϕ^* ; and then $\frac{ds}{d\phi}$ the radius of curvature at s , ϕ , is expressed by $G\phi + G'\phi^\dagger$.

It may be here noticed that substituting for ϕ , $\phi + \lambda$, where λ is arbitrary, amounts only to a rotation of the curve through the angle λ , so that, as regards the intrinsic form of the curve, no generality is sacrificed by imposing one condition upon the coefficients in $G\phi$, or, if we please, in making any of the coefficients in it except the first to vanish.

nitude to the two :—

$$\begin{aligned} (1) \quad \frac{ds}{d\phi} &= p + \frac{d^2p}{d\phi^2}, & (2) \quad r^2 &= p^2 + \left(\frac{dp}{d\phi}\right)^2, \\ (3) \quad s' &= \frac{ds}{d\phi}, & (4) \quad p' &= \frac{dp}{d\phi}, \end{aligned}$$

the last of them more familiarly known under the form

$$p'^2 = r^2 - p^2.$$

The third and fourth equations show respectively that s and p are each quantics in ϕ ; the first gives the connexion between the constants which enter into these quantics, and the second, combined with the first, the relation between s and r (in other words, the rectification of the curve), that between r and θ (where θ is the vectorial angle) being contained in a fifth equation,

$$\theta = \phi + \sin^{-1} \frac{p}{r}.$$

* Accordingly we see that the spiral of Archimedes, as is well known, is the locus of the feet of the perpendiculars upon the tangents to the first involute from the centre of the circle; and, much more generally, if we substitute for each radius vector in this spiral any given quantic thereof, we obtain the corresponding first pedal to an involute whose order of derivation is the degree of the quantic. *Ex. gr.* by squaring or cubing the radius vector of the spiral of Archimedes (of course leaving the vectorial angle unchanged), we may form the pedal to particular species of the second and third involutes respectively.

† Since the radius of curvature, radius vector, and perpendicular on the tangent arc are all known rational integral functions of the same quantity, it becomes a simple problem of elimination to determine the central force competent to make a body describe an involute of any order to a circle. Thus it will be found that the half-pitch second involute may be described under the action of a central force varying as the inverse cube of the shortest distance from the generating circle. So, again, the first involute may be described under the action of a central force, the component of which in the direction of the tangent to the generating circle (or say the centrifugal force) varies as the inverse cube of this tangent, the centre of force in each case being of course situated at the centre of the circle.

From what precedes, and from the general theory of elimination, it follows that in general the relation between r^2 and s is expressed by a rational integral equation of the degree $(i+1)$ in the former and $2i$ in the latter. But this is subject to an obvious exception in the case of $i=1$; for then, calling $G\phi=a\phi$, we have

$$\frac{ds}{d\phi} = a\phi, \quad s = \frac{a\phi^2}{2} + b, \quad \text{and} \quad r^2 = a^2\phi^2 + a^2 = 2as + (a^2 - 2ab);$$

so that the degrees in r^2 and s are here 1 and 1 in lieu of 2 and 2, as given by the general rule.

As regards the polar equation to the general involute,

$$r^2 = (G\phi)^2 + (G'\phi)^2,$$

it is obvious that, agreeable to the well-known case for the first involute,

$$\theta = \sin^{-1} \frac{dr}{ds} + \phi = \sin^{-1} \frac{G'}{r} + \phi^*,$$

where G' and ϕ are given by the solution of an algebraical equation of the $2i$ th degree, and which will therefore *usually* be incapable of expression in finite terms beyond the second involute. In the case of this involute the reducing equation is not a general biquadratic, but a form involving only square and no cube roots—being in fact reducible to a quadratic in ϕ^2 , as will at once be seen from the fact that we may write $(\alpha\phi^2 + \gamma)^2 + 4\alpha^2\phi^2 = r^2$.

Since $\frac{ds}{d\phi}$, *i. e.* $G + G'$, is a quantic of the degree i or ϕ , we

* ϕ and G' will form $2i$ systems of values. Will they be all applicable to the true involute, and how about the sign to be given to r ? It must, I think, be a matter of some delicacy and difficulty to answer these ques-

tions. For take even the first involute, where $\theta = \sin^{-1} \cdot \frac{a}{r} + \sqrt{\frac{r^2 - a^2}{a^2}}$,

we know, as a matter of fact, that if the first term is made to decrease as r increases, the positive sign of the square roots only must be employed, and of course the negative sign if the first term increases with r . Were we to reverse this rule, instead of the involute we should obtain what may be termed the counter-involute; *i. e.* a figure formed by points, each the *opposite* of every point in the involute in respect to its centre of curvature. Or, again, if a pair of parallel rulers were made always to touch a circle at opposite points, and the under parallel to *roll* round the circle, whilst each point in this line describes an involute, each point in the one above would describe a counter-involute. Or, again, if a string, by aid of a pin, were unwrapped *back* upon itself from a circle, the extremity would describe the extraneous curve. From this last observation it would seem as if the forced intrusion of a foreign curve into the polar equation of the involute resulted from the impossibility of affixing an absolute sign to the length of an arc—the condition of drawing a tangent always equal in length to the varying arc of a curve admitting of satisfaction without breach of continuity in two distinct modes.

learn that there may be i cusps to the i th involute, or any less number differing from i by an even integer. Also, since $\frac{db^2}{ds} = G'$, the number of apses (in regard to the centre) may be any number inferior to and differing from i by an odd integer*. Also, since G represents the perpendicular on the tangent, the number of points where the tangent passes through the centre will follow the same law (although, of course, the two numbers need not be equal) as the number of the cusps; at such points the curve, after winding round the centre (it may be through one or more complete revolutions and a part of a revolution, or through only a part), will change its curve and wind round in the opposite direction; for it is clear that there can be no points of inflexion, since $\frac{ds}{d\phi}$ can never become infinite. These points last named may be termed points of retrocession. The cusps, of course, can only exist at points where the involute meets the parent curve.

Between any two cusps of an involute evidently must be comprised an odd number of the cusps of its parent curve; but, of course, not *vice versâ*; thus, *e. g.*, in the second involute, if there are no cusps, it will easily be seen that the curve possesses a simple loop enclosing the cusp of the first involute (its evolute), and consequently cutting the two branches of the latter, and so in general the disappearance of consecutive cusps in any involute will give rise to loops enclosing those cusps of the parent curve on the branches adjoining to which (on each side) cusps of the derived curve are wanting; (by a branch, I mean, of course, the portion of curve included between any two cusps, or between either of the two terminal cusps and infinity;) whether the absence of cusps of the involute on $2i$ consecutive branches of the parent curve implies the necessary existence of i distinct loops, one round every alternate one of the $2i-1$ cusps in which those branches meet, requires further consideration. It is clear that in an analytical sense the length of the arc of the parent curve included between any two cusps of the second curve must be taken as zero; the correct view (at least, for the purposes of this theory) being that the angle of convergence *continually* increases or decreases up to positive or negative infinity as we pass in one

* Thus we see that the apsidal distances from the centre are the arithmetical magnitudes of the roots of the equation formed by equating to zero the *discriminant* of $G+r=0$, which is of course of the degree $(i-1)$ in r .

If we consider the apses and cusps of any involute to form a combined group, an odd number of the points of this group will always be included between any two intersections of the curve with a circle concentric with the parent circle; for the limiting equation to $G^2 + G'^2 - r^2 = 0$ is $G'(G+G') = 0$. Every point in this combined group is a point of maximum or minimum elongation from the centre.

direction from point to point in a curve. Accordingly we ought not to say, as is usually done, that at a cusp the tangent is suddenly reversed in direction, but, rather, that the increment of the arc on passing through a cusp changes sign, as it ought to do according to first principles; for the *flow* of the incremental arc, from being concurrent with, becomes opposite to that of the rotating tangent line which carries it, or *vice versâ*. Thus in the common cycloid (a curve of infinite length to the eye and with an infinite number of cusps) we have $s = c \cos \phi$, which, subject to this interpretation, is perfectly true and self-consistent for the whole extent of the curve from infinity to infinity. In that case we have a visible representation of *quantity* undergoing an infinite number of periodic changes, although the *subject matter* of the quantity is continually changing and never recurs. In the case of the involutes of the circle, the number of those periodic changes is of course finite and equal to the number of the cusps. If A, B, C, D, . . . , L be the cusps in natural order on the curve whose involute is to be found, and if we call x the radius of curvature of the point of the involute corresponding to A (x being taken positive when this radius is in the position into which it would be brought by unwinding a string from the infinite branch adjacent to A), and if we form the series $x - AB + BC - CD \dots \pm KL$, where AB, BC, . . . are the arithmetical lengths of the branches, it is clear that at each term of this series in which a change of sign in the sum takes place the involute will have a cusp; if the number of branches is odd and x is negative, the sum may remain negative at whatever term we stop, and then there will be no cusp in the involute so engendered; but when the number of points A, B, C, . . . , L is even, then it is easy to see that one of the infinite branches must contain a cusp of the involute and the other be vacant. The second involute, whether cusped or not, manifestly consists of two parts symmetrically arranged about its apse. If we form a third involute by unwinding from this apse as origin, the figures so formed will again be symmetrical, and the cusps will lie at the vertices of an isosceles triangle; and now *every* involute of this symmetrical third involute will again be symmetrical, and so on continually, the number of conditions imposed on the parameters in order to ensure symmetry in the involute of the i th order being thus the integer part of $\frac{i-1}{2}$. When this

symmetry obtains, the algebraical equation requisite for determining the polar equation depends on the solution of an equation of only the i th instead of the $2i$ th degree; for it is clear that in this case the functions G^2 and G'^2 may be made to contain only powers of ϕ^2 . Thus we may very easily write down the general polar equation to the absolutely general second involute, and

might, if it were worth while, do as much for the symmetrical class of third and fourth involutes, of which the former will contain two and the latter three arbitrary parameters, by solving a cubic and biquadratic equation in these two cases respectively.

It is easy to see also that the *arco-radial* equation to the symmetrical involute of an odd order is of only half the degrees in s and r^2 that it is of in the general case, and for the symmetrical involute of an even order, although of the same degrees in r^2 and s as in the general case, involves only the even powers of s .

A few words upon the second involute, and I have done; for it is difficult to deal with theory in any detail so as to be intelligible, or even safe, without the suggestive and regulative aid of drawn figures, which I have not yet been able to obtain in a form fit for use.

The two principal classes to distinguish in the second involute are the cusped and uncusped species. The cusped second involute winds round the parent curve upon which the extremities of its finite branch rests. The uncusped species crosses itself, and intersects each branch of the first involute of which it encloses the cusp, its node being on one side of it and its apse on the other. The transition case is when the unwinding begins from the cusp of the first involute; the second involute so obtained has a very singular point at that cusp, which may be regarded either as an abortive loop, or as a coincident pair of cusps*.

The general connecting equations for this involute may be put under the form

$$r^2 = \left(\frac{a}{2} \phi^2 + b \right)^2 + a^2 \phi^2,$$

$$\frac{ds}{d\phi} = \frac{a}{2} \phi^2 + (a+b),$$

$$\theta = \sin^{-1} \frac{a\phi}{r} + \phi,$$

where a is the radius of the circle; and there will be a loop or cusps according as $a+b$ is positive or negative; when $b=-a$,

* Mr. Crofton has noticed, in an ingenious paper published in the 'Mathematical Messenger,' that this involute is the locus of the centres of all the circles cutting orthogonally the originating circle and the parent first involute. This is seen very easily as follows.

$$p = a \left(\frac{\phi^2}{2} - 1 \right), \quad s' = p + p'' = a \frac{\phi^2}{2},$$

$$r^2 = p^2 + p'^2 = a^2 \frac{\phi^2}{4} + a, \text{ or } r^2 - a^2 = s'^2,$$

showing that the tangents to the circle and first involute from any point in the second are equal to one another.

we have the transition case, for which

$$r^2 = \frac{a^2}{4} \phi^4 + a^2, \quad s = \frac{a}{6} \phi^3;$$

and the arco-radial equation becomes

$$(r^2 - a^2)^3 = \frac{81}{4} a^2 s^4.$$

Another and more remarkable case occurs when $G^2 + G'^2$ becomes a perfect square, for then the degrees in s and r will sink to half their usual values: this occurs when $b = -\frac{a}{2}$, which is the case of a looped curve bisecting at its apse the radius drawn from the centre of the circle to the generating first involute.

We have in that case

$$G = \frac{a}{2} (\phi^2 - 1),$$

$$r = \frac{a}{2} (\phi^2 + 1) \text{ or } \phi = \sqrt{\frac{2r-a}{a}},$$

$$s = \frac{a}{2} \left(\frac{\phi^3 + 3\phi}{3} \right),$$

whence

$$9as^2 = (2r-a)(r+a)^2,$$

and

$$\theta = \sin^{-1} \sqrt{\frac{2ar-a^2}{r^2}} + \sqrt{\frac{2r-a}{a}} = \text{vers.}^{-1} \frac{a}{r} + \sqrt{\frac{2r-a}{a}},$$

a form even simpler than that of the first involute.

We may write this equation under the form

$$\theta = -2 \cos^{-1} \cdot \sqrt{\frac{\frac{1}{2}a}{r}} + \sqrt{\frac{r-\frac{1}{2}a}{\frac{1}{2}a}};$$

or, turning round the line from which θ is reckoned through a quarter of a revolution,

$$\theta = 2 \sin^{-1} \sqrt{\frac{\frac{1}{2}a}{r}} + \sqrt{\frac{r-\frac{1}{2}a}{\frac{1}{2}a}}.$$

Let now

$$\theta = 2\mathfrak{s}, \quad \frac{a}{2} \cdot r = \rho^2$$

(which is the same thing as if for x and y we substituted $x^2 - y^2$ and $2xy$), then

$$\mathfrak{s} = \sin^{-1} \cdot \frac{\frac{1}{2}a}{\rho} + \frac{\sqrt{\rho^2 - \left(\frac{a}{2}\right)^2}}{a}.$$

This is the polar equation to a known curve (of the kind used by Captain Moncrieff in his barbette gun-carriage). It is of the class of curves generated by a fixed point on a wheel rolling on a plane. Such a curve may be termed the *convolute* of a circle of a *pitch* denoted by the ratio of the distance of the fixed point *below* the centre to the radius of the revolving circle; thus a convolute of zero-pitch is the spiral of Archimedes, a convolute of unit pitch the first involute to the circle: the general equation to a convolute, when the distance below the centre is d and the radius a , is given by the Rev. James White in the last September Number of the Educational Series, and is easily shown to be

$$s = \sin^{-1} \cdot \frac{d}{\rho} + \frac{\sqrt{\rho^2 - d^2}}{a}.$$

Similarly, we may define the pitch of the second involute to be the ratio of the distance of its apse from the centre to the radius; and then we are conducted to the observation that whilst the convolute of full pitch is the first involute, the convolute of half pitch, on applying to it one of the simplest forms of M. Chasles's or Mr. Roberts's method of transformation (given in Dr. Salmon's 'Higher Plane Curves,' p. 236), viz. doubling the vectorial angle and squaring the radius vector, becomes converted into the second involute of half pitch. Since for this curve

$$r = \frac{a}{2} (\phi^2 + 1) = \frac{ds}{d\phi},$$

we see that it may be completely defined, without reference to any theory of involutes, as the curve whose radius of curvature at any point is equal to its radius vector reckoned from a given origin. It is the curve which completely satisfies the equation

$rd \cos^{-1} \frac{dr}{ds} = s$, the two arbitrary parameters which the complete integral of this equation should contain being furnished by the linear magnitude and angle of swing of the curve round the given origin*.

* This evolute possesses the property, which serves to characterize it completely, of cutting the originating circle (its second evolute) orthogonally. For when $r^2 = a^2$, $G^2 = 0$, i. e. the tangent to the curve passes through the centre. Moreover, since $G = 0$ gives $\phi = 1$, it follows that the curve cuts out of the circle an arc equal in length to the diameter. Summarizing such of its principal properties as have fallen in our way, we see that it bisects the line joining the centre of the originating circle and the cusp of the first involute; that it cuts the said circle orthogonally; that its radius

I conclude with the remark that if we regard the s and r^2 of the successive involutes as *rectilinear* coordinates to a variable point, the arco-radial equation will represent a peculiar class of unicursal algebraical curves. Thus the first involute will represent a pair (one for each branch of the curve) of coincident right lines, and the general second involute (taking r^2 and s^2 as the coordinates) a pair of coincident semicubical parabolas.

In making s vary continuously on passing a cusp, the corresponding abscissa from increasing must begin to decrease, or *vice versâ*, according to the principles previously noticed.

Thinking of the recovery of the cusps and apses from the arco-radial equation, I have been led to consider a morphological property of a more general class of unicursal equations, which I think is likely to bear valuable fruit, and may possibly form the subject of another communication.

Athenæum Club,
September 1868.

of curvature is everywhere equal to its elongation from the centre; that it is a trajectory to a central force varying as the inverse cube of the shortest distance from the periphery of the originating circle; that its arco-radial equation is of only half the number of dimensions of the general involute of the same order; and that by the simplest form of quadratic transformation (viz. that which leaves unaltered the inclination of the tangent to the radius vector) it changes into the half-pitch circular convolute; not to add that its polar equation is even simpler than that of the first involute. Certainly, then, as it seems to me, it ought to take permanent rank among the spirals which have a specific name on the geometrical register; and for want of a better, with reference to the place where its properties first came into relief, it might be termed the *Norwich spiral*. Where it meets the first involute we have

$$\left(\frac{\phi^2+1}{2}\right)^2 = \frac{r^2}{a^2} = \phi^2+1,$$

or

$$(\phi^2+1)(\phi^2-3)=0;$$

so at the real intersections the radius vector is $2a$, and the perpendicular on the tangent, viz. $\left(\frac{\phi^2-1}{a} a\right)$, is a , showing that the tangent and radius vector at those points are inclined to each other at an angle of 30° .

XL. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 232.]

May 28, 1868.—Lieut.-General Sabine, President, in the Chair.

THE following communication was read :—

“Observations of the Spectra of some of the Southern Nebulæ.” By Lieutenant John Herschel, R.E.

[Lieutenant Herschel, to whom the observations of the solar eclipse of August next have been entrusted by the Royal Society, has already employed the instruments, placed in his hands by the Royal Society for the observations of the eclipse, to good account by commencing an examination of some of the brightest of the nebulae of the southern heavens. The first results of this examination, which are contained in the present paper, were obtained at Bangalore, Madras Presidency, during the months of March and April 1868. The instruments consist of an equatorially mounted telescope of 5 inches aperture, driven by a clock furnished with a pendulum governor by Cooke and Sons, and a spectroscope by Messrs. Simms furnished with one prism of dense flint glass, and with a micrometer-screw and photographic scale for measuring the lines. The nebulae No. 4390 and No. 2102 have been described by me, Phil. Trans. 1864, p. 439, and Phil. Trans. 1866, p. 383. —W. H.]

No. 3531. [$\mathcal{R}$ $13^h 19^m$: N.P.D. $136^\circ 37'$: !!; globular cluster of stars; ω Centauri.]

March 25. A large cluster visible to the naked eye, oval-shaped, brighter towards the central part. *Spectrum an indefinable haze; no lines.*

No. 2197. [$\mathcal{R}$ $10^h 40^m$: N.P.D. $148^\circ 57'$: great nebula in Argus.]

Spectrum: *lines distinctly visible*, but not clear enough to be separated; approximate position $D+1.8 \pm .3$ ($D=2.30$, $E=3.68$, $b=3.97$, $F=5.03$). An unsatisfactory observation: to be looked for again.

No. 2017. [$\mathcal{R}$ $10^h 1^m$: N.P.D. $129^\circ 47'$: !!; planetary nebula, very bright, very large, little extended, * 9M.]

March 31. Found with difficulty in the spectroscope. After a minute or two's examination the tube was accidentally disturbed; and before direction could be again obtained, clouds had gathered and work was stopped. Appearance in telescope a nebulous-looking star; under a higher power a nebulous-looking object with a much brighter nucleus or centre. In spectroscope *a continuous streak with a blotch of light nearly in the middle of its length two-fifths*, by estimation, from the red end; slit quite wide.

No. 2581. [$\mathcal{R}$ $11^h 44^m$: N.P.D. $146^\circ 27'$: a planetary nebula, small, round; blue = * 7M.]

X 2

April 2. *A pretty well defined and bright short line* was distinctly visible in the spectroscope, accompanied by a considerably fainter and more refrangible companion. Principal line measured with the wires and found to be $D + 2.1 = 4.4$ ($b = 3.97$, $F = 5.03$), i. e. $b + 0.4$.

No. 4083. [$R\ 15^h\ 12^m$: N.P.D. $87^\circ\ 25'$: !!; globular cluster, very bright, large, extremely compressed in the middle.]

April 5. Seen in telescope as a slightly oval nebulous ball, easily seen but not very bright (perhaps owing to moon, nearly full); found with some difficulty in spectroscope; *a faint continuous spectrum* of considerable width; *no trace or suspicion of lines*.

No. 4173. (Seen easily in telescope; but looked for in spectroscope for two hours in vain.)

No. 4390. [$R\ 18^h\ 6^m$: N.P.D. $83^\circ\ 10'$: planetary nebula; very bright, very small, little hazy.]

April 6. Scarcely recognized as a nebula in the telescope. Seen in spectroscope: *a short bright line* with a fainter one on the more refrangible side, and a third strongly suspected. (Knowing so well the relative positions of the "usual" lines, it is impossible that an unprejudiced corroborative opinion can be offered on such slight foundation as I have.) A very slight extension laterally was given in this instance with the cylindrical lens.

No. 2102. [$R\ 10^h\ 18^m$: N.P.D. $107^\circ\ 59'$: !!; planetary nebula, very bright, little extended.]

April 9. Seen at once in telescope with low power; and seen distinctly in the spectroscope *as a bright and a faint line* (the third line not seen); principal line measured with the wires and found

$$= D + \left\{ \begin{matrix} 2.14 \\ 2.16 \end{matrix} = 2.15 \right\}, \text{ or } b + 0.48, F = b + 1.06.$$

No. 1179. Nebula in Orion. Examined for comparison. The spectrum of this nebula *shows the three lines distinctly, and three only*; they were measured (with the wires), and the results were:—

$$D + \left\{ \begin{matrix} 2.17 \\ 2.20 = 2.19 \\ 2.21 \end{matrix} \right\} = b + 0.52,$$

$$D + \left\{ \begin{matrix} 2.31 \\ 2.40 \end{matrix} = 2.36 \right\} = b + 0.69,$$

and

$$D + 2.78 = b + 1.11.$$

The places and descriptions of the objects enclosed within brackets are taken from Sir John Herschel's "General Catalogue of Nebulæ" in the Phil. Trans. for 1864.

P.S.—The other day a storm passed over us. As there was a good deal of lightning, I took the opportunity to examine its spectrum. I saw, as I expected, numerous bright lines; the blue nitrogen one,

I suppose, much the brightest. A suspicion also of the red hydrogen-line C. I was much surprised at the brightness of the continuous spectrum, in which all the principal prismatic colours were brilliant.

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

The following communication was read:—

“On the Combustion of Hydrogen and Carbonic Oxide in Oxygen under great pressure.” By E. Frankland, F.R.S., Professor of Chemistry in the Royal Institution and in the Royal School of Mines.

In a former communication to the Royal Society* I described some researches on the effect of a diminution of pressure on some of the phenomena of combustion, and deduced therefrom the law that *the diminution in illuminating-power is directly proportional to the diminution in atmospheric pressure.*

Further experiments, made more than a year ago, on the nature of the luminous agent in a coal-gas flame†, led me to doubt the correctness of the commonly received theory first propounded by Sir Humphry Davy‡, that the light of a gas-flame and of luminous flames in general is due to the presence of solid particles. In reference to gas- and candle-flames, it is now well known that the fuliginous matter produced when a piece of wire-gauze is depressed upon such flames, and the sooty deposit which coats a piece of white porcelain placed in a similar position, are not pure carbon, but contain hydrogen, which is only completely got rid of by prolonged exposure to a white heat in an atmosphere of chlorine. On pursuing the subject further, I found that there are many flames possessing a high degree of luminosity which cannot possibly contain solid particles. Thus the flame of metallic arsenic burning in oxygen emits a remarkably intense white light; and as metallic arsenic volatilizes at 180° C., and its product of combustion (arsenious anhydride) at 218° C., whilst the temperature of incandescence of solids is at least 500° C., it is obviously impossible here to assume the presence of ignited solid particles in the flame. Again, if carbonic disulphide vapour be made to burn in oxygen, or oxygen in carbonic disulphide vapour, an almost insupportably brilliant light is the result. Now fuliginous matter is never present in any part of this flame, and the boiling-point of sulphur (440° C.) is below the temperature of incandescence, so that the assumption of solid particles in the flame is here also inadmissible. If the last experiment be varied by the substitution of nitric oxide gas for oxygen, the result is still the same; and the dazzling light produced by the combustion of these compounds is also so rich in the more refrangible rays, that it has been employed in taking instantaneous photographs, and for exhibiting the phenomena of fluorescence.

Many other similar cases of the production of brilliant light from incandescent, gaseous, or vaporous matter might be cited; but I will

* Phil. Trans. vol. cli. p. 629 (1861).

† Lectures on Coal-gas, delivered at the Royal Institution in March 1867. Journal of Gas-lighting.

‡ Phil. Trans. for 1817, p. 75.

mention only one other. Amongst the chemical reactions celebrated for the production of dazzling light, there are few which surpass the active combustion of phosphorus in oxygen. Now phosphoric anhydride, the product of this combustion, is volatile at a red heat; and it is therefore manifestly impossible that this substance should exist in the solid form at the temperature of the phosphorus-flame, which far transcends the melting-point of platinum. For these reasons, and for others stated in the lectures above quoted, I consider that incandescent particles of carbon are not the source of light in gas- and candle-flames, but that the luminosity of these flames is due to radiations from dense but transparent hydrocarbon vapours. As a further generalization from the experiment above mentioned, I was led to the conclusion that dense gases and vapours become luminous at much lower temperatures than æriform fluids of comparatively low specific gravity, and that this result is to a great extent, if not altogether, independent of the nature of the gas or vapour, inasmuch as I found that gases of low density, which are not luminous at a given temperature when burnt under common atmospheric pressure, become so when they are simultaneously compressed. Thus mixtures of hydrogen and carbonic oxide with oxygen emit but little light when they are burnt or exploded in free air, but exhibit intense luminosity when exploded in closed glass vessels, so as to prevent their expansion at the moment of combustion.

I have recently extended these experiments to the combustion of jets of hydrogen and carbonic oxide in oxygen under a pressure gradually increasing to twenty atmospheres. These experiments were conducted in a strong iron vessel, furnished with a thick plate of glass of sufficient size to permit of the optical examination of the flame. The results are so remarkable that, although still far from being complete, I venture to communicate them to the Royal Society before the close of the Session. The appearance of a jet of hydrogen burning in oxygen under the ordinary atmospheric pressure is too well known to need description. On increasing the pressure to two atmospheres, the previously feeble luminosity is very visibly augmented, whilst at ten atmospheres' pressure the light emitted by a jet about 1 inch long is amply sufficient to enable the observer to read a newspaper at a distance of 2 feet from the flame, and this without any reflecting surface behind the flame. Examined by the spectroscope, *the spectrum of this flame is bright and perfectly continuous from red to violet.*

With a higher initial luminosity, the flame of carbonic oxide in oxygen becomes much more luminous at a pressure of ten atmospheres than a flame of hydrogen of the same size and burning under the same pressure. The spectrum of carbonic oxide burning in air is well known to be continuous; burnt in oxygen under a pressure of fourteen atmospheres, the spectrum of the flame is very brilliant, and perfectly continuous.

If it be true that dense gases emit more light than rare ones when ignited, the passage of the electric spark through different gases ought to produce an amount of light varying with the density

of the gas; and this is in fact the case, for electric sparks passed as nearly as possible under similar conditions through hydrogen, oxygen, chlorine, and sulphurous anhydride emit light the intensity of which is very slight in the case of hydrogen, considerable in that of oxygen, and very great in the case of chlorine and sulphurous anhydride. When liquefied sulphurous anhydride is sealed up in a strong tube furnished with platinum wires, and the temperature then allowed to rise until the internal pressure amounts to three or four atmospheres, the passage of induction-sparks through the enclosed gas is attended with very brilliant flashes of light. Further, if a stream of induction-sparks be passed through air confined in a glass tube connected with a condensing syringe, and the pressure of the air be gradually augmented to two or three atmospheres, a very marked increase in the luminosity of the sparks is observed, whilst on allowing the condensed air to escape, the same phenomena are observed in the reverse order.

The electric arc from fifty cells of Grove's battery is incomparably more brilliant when mercury vapour, instead of atmospheric air, is interposed in the path of the discharge between the carbon points. The gases and vapours just mentioned have the following relative densities :—

Hydrogen	1·0
Air	14·5
Oxygen	16·0
Sulphurous anhydride	32·0
Chlorine	35·5
Mercury	100·0

It is obvious that the above results have a very direct bearing upon the views now generally held regarding the constitution of the sun, stars, and nebulae; but I refrain from making any such application of them until I have the honour of laying before the Royal Society a complete account of these experiments.

June 18.—Lieut.-General Sabine, President, in the Chair.

The following communications were read :—

“Researches on Refraction-equivalents.” By J. H. Gladstone, Ph.D., F.R.S.

Since the paper of the Rev. T. Pelham Dale and myself “On the Refraction, Dispersion, and Sensitiveness of Liquids”*, our researches have been continued from time to time, and a good deal of attention has been paid to the subject in Germany. The permanence of the specific refractive energy of a body, notwithstanding change of temperature, aggregate condition, solution, or even chemical combination, has been confirmed, and upon this has been built the doctrine of Refraction-equivalents.

Our “specific refractive energy” is the refractive index of any substance minus unity, divided by the density—in symbolic language

* Philosophical Transactions, 1863, p. 317.

$\frac{\mu-1}{d}$. Professor Landolt's "Refraction-equivalent" is the same multiplied by the chemical equivalent, or $P \frac{\mu-1}{d}$.

The largest generalization arrived at is that the refraction-equivalent of a compound is the sum of the refraction-equivalents of its constituents. This has been sufficiently proved in a multitude of instances among the compounds of carbon, hydrogen, and oxygen, and it has been shown, or assumed to be the case, in the combinations of many other elementary bodies*.

My more recent researches have branched off into several lines; but an especial attempt has been made to answer the following questions. Have any of the elements more than one definite refraction-equivalent? and what are the refraction-equivalents of the metallic elements? A large mass of observations bearing on these points has been gathered together, and more or less collated; but it is yet imperfect, and my present object is rather to indicate the principal method of inquiry than to publish the actual results.

As the metals are opaque, their refractive indices cannot be determined in a direct manner as those of gaseous hydrogen, liquid phosphorus, crystallized carbon, and other transparent elements have been. An attempt must therefore be made to determine their effect on the rays of light by examining their compounds; but their crystalline salts are very frequently doubly refracting, owing to some peculiarities of internal structure, and, where they give only one spectrum, there are practical difficulties about the experiment that are not encountered in dealing with liquid bodies. The solutions of these salts have only one refraction; and it occurred to me that they might afford an easy means of determining the refraction-equivalents, first, of the compounds themselves, and, secondly, of their metallic and other constituents. In practice, many sources of error presented themselves, all of which tell upon the ultimate result, and which necessitated improved apparatus, and great care both in preparing the solutions and in taking the observations.

The method generally adopted was as follows:—An amount of salt representing the chemical equivalent was dissolved in n equivalents of water, and the refractive index and density of the solution were taken. From these was reckoned the refraction-equivalent; and subtracting from this n times the refraction-equivalent of water for the solar line A, there remained the refraction-equivalent of the dissolved salt for that part of the spectrum. That this fairly represents the action exerted on light by the chemical compound itself is supported by several considerations. 1st. In the few cases (such as chloride of sodium and sugar) where the refraction-equivalent of the

* See Brit. Association Report, 1863, Transactions of Sections, p. 12. Ibid. 1866, p. 37. Journal of the Chemical Society, ser. 2, vol. iii. p. 108. Landolt, Pogg. Annalen der Physik und Chemie, vols. cxvii., cxxii., and cxxiii. Ketteler, Ueber die Farbenzerstreuung der Gase, 1865. Haagen, Pogg. Annalen, vol. cxxiii. p. 125. Schrauf, Pogg. Annalen, vol. cxxvii. p. 344. Wüllner, Pogg. Annalen, vol. cxxxiii. p. 1.

substance has been obtained both in the solid and dissolved condition, it is found to be the same. 2nd. Solutions of several organic substances, such as formic and citric acids, give the theoretically correct equivalent for these substances. 3rd. The refraction-equivalent of a salt seems to be the same, whether it be dissolved in water or in alcohol. 4th. The refraction-equivalent of a salt in solution is not affected by altering the amount of water in which it is dissolved. 5th. The numbers reckoned for these salts in solution bear such a remarkable relation to one another as to force the conviction that they are made up of two components, the one depending on the metal, the other on the substance combined with it. To exhibit the nature and force of this argument, it would be necessary to tabulate a long series of results; but for the present I shall confine myself to the salts of potassium and sodium, with the corresponding hydrogen compounds.

Dissolved compound.	Common formula.	Refraction-equivalents.			Difference between potassium and sodium compounds.	Difference between potassium and hydrogen compounds.
		Potassium compound.	Sodium compound.	Hydrogen compound.		
Chloride.....	MCl	18.44	15.11	14.44	3.3	4.0
Bromide	MBr	25.34	21.70	20.63	3.6	4.7
Iodide	MI	35.33	31.59	31.17	3.7	4.2
Cyanide.....	MNC	17.12				
Sulphocyanide	MSNC	33.40				
Nitrate	MNO ₃	21.80	18.66	17.24	3.1	4.5
Metaphosphate	MPO ₃		19.48	18.68		
Hydrate.....	MHO	12.82	9.21	5.95	3.6	6.8
Alcoholate.....	MC ₂ H ₅ O	27.68	24.28	20.89	3.4	6.8
Formiate	MCHO ₂	19.93	16.03	13.40	3.9	6.5
Acetate	MC ₂ H ₃ O ₂	27.65	24.05	21.20	3.6	6.5
Tartrate.....	M ₂ C ₄ H ₄ O ₆	57.60	50.39	45.18	3.6	6.2
Carbonate	M ₂ CO ₃	34.93	28.55		3.2	
Sulphate	M ₂ SO ₄	30.55	26.20*	22.45	2.2	4.1
Bichromate	M ₂ Cr ₂ O ₇	79.9	72.9		3.5	
Hypophosphite.....	M ₂ PH ₂ O ₂	26.94	20.93		3.0	

From the above numbers several conclusions may be safely drawn. First, it is evident that the refraction-equivalents of the compounds of potassium differ very widely according to the nature of the electro-negative constituents; again, that the refraction-equivalents of the compounds of sodium differ *pari passu* with those of the potassium compounds, being always less by a number varying from 3.0 to 3.9. From this it may be fairly concluded that the electro-negative constituent has the same effect on light, whichever metal it is united with, and that the refraction-equivalent of potassium exceeds that of sodium by 3.4, or thereabouts. But does the Table afford the data for de-

* This number seems to be too high, but it is the mean of fairly accordant results. It is rejected in the calculation of average difference between potassium and sodium.

termining the absolute equivalent of one or other of these metals? It was at first thought that this would be arrived at by a comparison of the metal with hydrogen, the refraction-equivalent of which has hitherto been estimated at 1·3 (or 1·5 in the case of water); but the last column shows that the difference between potassium and hydrogen is not always the same, the differences being greater than can be attributed to errors of observation. Indeed the numbers seem to fall into two groups: with the mineral acids the differences lie between 4·0 and 4·7, while with water, alcohol, and the organic acids, they are always upwards of 6, varying indeed from 6·2 to 6·8, the average being 6·55. But it is in these last-mentioned compounds that the equivalent of hydrogen is believed to be 1·3. Assuming this, we may reckon the refraction-equivalent of potassium to be about $6·55 + 1·3$, that is, 7·85. We have, however, other means of arriving at an estimation. Chlorine, in such bodies as chloroform or tetrachloride of carbon, is represented by 9·8. Again, cyanogen, from the experiments of Dulong on the gas itself, may be taken at 9·2. Sulphur has a refraction of 16·0; hence sulphocyanogen may be reckoned as $16·0 + 9·2$, that is, 25·2. Subtracting these numbers from those of the respective potassium salts, we obtain the equivalent of the metal. Thus from different sources we may calculate for the value of potassium :—

From the chloride	8·6
„ cyanide	7·9
„ sulphocyanide	8·2
„ hydrate	8·3
„ alcoholate	8·1
„ formiate	7·8
„ acetate	7·7
„ tartrate	7·5

These numbers are tolerably close, though the equivalent of potassium deduced from its inorganic, would, on the whole, be higher than that deduced from its organic compounds. The mean of the first four computations is 8·2, that of the last four 7·8. Perhaps, pending further researches, it will be best to assume the mean of these numbers,

Potassium = 8·0 ;

and since a sodium salt has a refraction-equivalent generally 3·4 lower than the corresponding potassium salt, we may reckon

Sodium = 4·6*.

If, instead of taking the refraction-equivalent $P \frac{\mu-1}{d}$, we reckon the specific refractive energy $\frac{\mu-1}{d}$, we obtain the following values:—

Potassium	0·205
Sodium	0·200

This implies that equal quantities of these two analogous metals

* Haagen, from the crystalline chloride, determined the refraction-equivalent of sodium for the hydrogen line d at 4·89.

exert very nearly, if not precisely, the same effect on the velocity of the rays of light.

Another deduction from the above Table is that already alluded to in regard to hydrogen; while in the organic acids it probably has the known refraction-equivalent 1·3, it would seem that in the others, viz. hydrochloric, hydrobromic, hydriodic, nitric, metaphosphoric, and sulphuric acids, it has a very much higher refraction-equivalent, one in fact which is little less than sodium, and falls short of potassium by only about 4·3. Hence we may deduce—

Hydrogen in organic compounds = 1·3

Hydrogen in mineral acids = 3·7

How far this conclusion may hold good throughout, and whether one number should be an exact multiple of the other number, must remain to be determined by future observations. It appears, however, to answer in the affirmative the question whether an element can have more than one definite refraction-equivalent.

Of course, from the Table given above, it would be easy to deduce values for each of the electro-negative constituents; but it would be safer to generalize from a larger number of instances.

The series of observations on potassium and sodium salts are the most complete and the most carefully revised of any which have yet been made; but if their refraction-equivalents are fixed, it becomes a much simpler matter to determine those of most other metals. Thus, of lead the nitrate and acetate have alone been examined; but as the refraction-equivalents reckoned for these salts differ from those of the corresponding potassium compounds in each instance by 4·1, the presumption is great that the refraction-equivalent of lead is very near $8\cdot0 + 4\cdot1$, or $12\cdot1$.

The following are numbers deduced from two or more salts of each metal. They must be looked upon only as approximately true; and any subsequent modification of the value $8\cdot0$ for potassium must lead to a corresponding modification of the whole series.

Metal.	Chemical equivalent.	Refraction-equivalent.	Specific refractive energy.
Potassium	39	8·0	0·205
Sodium	23	4·6	0·200
Lithium.....	7	3·9	0·557
Magnesium	12	3·7	0·308
Barium	68·5	7·8	0·114
Strontium	43·8	6·5	0·148
Calcium.....	20	5·2	0·260
Zinc	32·6	4·8	0·147
Nickel	29·5	5·1	0·173
Cobalt	29·4	5·2	0·177
Lead	103·5	12·1	0·117
Mercury	100	9·8	0·098
Ammonium	18	11·4	0·633

These numbers are suggestive in many ways; but I will only remark the very high refractive energy of lithium, the practical identity of nickel and cobalt, and the remarkable fact that the specific refractive energy of the metals are (with one or two exceptions) in the inverse order of their atomic weights.

“On a Method of making a Direct Comparison of Electrostatic with Electromagnetic Force; with a Note on the Electromagnetic Theory of Light.” By J. Clerk Maxwell, F.R.SS.L. & E.

The experiments described in this paper were made in the laboratory of Mr. Gassiot, who placed his great battery of 2600 cells of bichloride of mercury at the disposal of the author. Mr. Willoughby Smith lent his resistance-coils of 1,102,000 Ohms; Messrs. Forde and Fleeming Jenkin lent a sensitive galvanometer, a set of resistance-coils, a bridge, and a key for double simultaneous contacts; and Mr. C. Hockin undertook the observation of the galvanometer, the adjustment of the resistances, and the testing of the galvanometer, the resistance-coils, and the micrometer-screw. The electrical balance itself was made by Mr. Becker.

The experiments consisted in observing the equilibrium of two forces, one of which was the attraction between two disks, kept at a certain difference of potential, and the other was the repulsion between two circular coils, through which a certain current passed in opposite directions. For this purpose one of the disks, with one of the coils attached to its hinder surface, was suspended on one arm of a torsion-balance, while the other disk, with the other coil behind it, was placed at a certain distance, which was measured by a micrometer-screw. The suspended disk, which was smaller than the fixed disk, was adjusted so that in its position of equilibrium its surface was in the same plane with that of a “guard-ring,” as in Sir W. Thomson’s electrometers, and its position was observed by means of a microscope directed on a graduated glass scale attached to the disk. In this way its position could be adjusted to the thousandth of an inch, while a motion of much smaller extent was easily detected.

An exactly similar coil was placed at the other end of the torsion-balance, so as to get rid of the effects of terrestrial magnetism.

It was found that though the suspended disk and coil weighed about half a pound, a very slight want of equality between the opposing forces could be detected, and remedied by means of the micrometer.

The difference of potential between the disks was maintained by means of Mr. Gassiot’s great battery. To measure this difference of potential, it was made to produce a current through Mr. Willoughby Smith’s resistance-coil, and the primary coil of the galvanometer shunted with a variable resistance.

The current in the coils was maintained by a Grove’s battery, and was led through the secondary coil of the galvanometer.

One observer, by means of the micrometer-screw, altered the distance of the disks till the suspended disk was in equilibrium at zero. At the same time the other observer altered the shunt till the gal-

vanometer-needle was also in equilibrium. The micrometer-reading and the resistance of the shunt were then set down as the results of the experiment.

The mean of twelve satisfactory experiments, at distances varying from .25 to .5 inch, gave for the ratio of the electromagnetic to the electrostatic unit of electricity—

$$\begin{aligned} v &= 27.79 \text{ Ohms, or B. A. units.} \\ &= 277,900,000 \text{ metres per second.} \\ &= 174,800 \text{ statute miles per second.} \end{aligned}$$

This value is considerably lower than that found by MM. Weber and Kohlrausch by a different method, which was 310,740,000 metres per second. Its correctness depends on that of the B. A. unit of resistance, which, however, cannot be very far from the truth, as it agrees so well with Dr. Joule's thermal experiments.

It is also decidedly less than any estimate of the velocity of light, of which the lowest, that of M. Foucault, is 298,000,000 metres per second.

In a note to this paper the author gave his reasons, in as simple a form as he could, for believing that the ratio of the electrical units and the velocity of light, are one and the same physical quantity, pointing out the difference between his theory and those of MM. Riemann and Lorenz, which appear to lead to the same conclusion.

XLI. Intelligence and Miscellaneous Articles.

SECOND NOTE ON THE LAWS OF INDUCTION.

BY MM. JAMIN AND ROGER.

IN a previous communication we announced that the heat developed in the external circuit of a magneto-electrical machine, working with a constant velocity, was governed by the same law as in the case of an ordinary battery. Our machine consisted of six rotating plates, each provided with sixteen coils mounted in *tension*. The plates themselves were united in *quantity*; that is, the similar poles of each of them terminated in two common points, with which were connected the external circuit. Since then the machine has been arranged afresh, so as to combine the various plates in all possible manners. We will now give the results obtained by associating 1, 2, . . . 6, . . . n plates in quantity.

By joining in the same manner n piles, each having the electromotive force A and the resistance r , we have a single electromotor of the force A and resistance $\frac{r}{n}$; the intensity i of the current with an external resistance x will be

$$i = \frac{A}{\frac{r}{n} + x},$$

and the quantity of heat regenerated under these circumstances will be

$$C = \frac{A^2 r}{\left(\frac{r}{n} + x\right)^2}.$$

Now, working with 1, 2 ... 6, ... n plates, we have found that the machine verified this formula, taking for A^2 and r the values 813.12 and 100.

The following are some of the results :—

Resistance x .	Thermal units reproduced in the circuit.									
	Two plates.		Three plates.		Four plates.		Five plates.		Six plates.	
	Obs.	Calc.	Obs.	Calc.	Obs.	Calc.	Obs.	Calc.	Obs.	Calc.
7.52	1.27	1.56	2.72	3.14	4.75	4.98	6.20	7.01	8.35	8.36
15.04	2.30	2.48	4.55	4.58	6.65	6.77	9.05	8.91	10.85	11.00
22.56	2.86	3.05	5.18	5.24	7.45	7.32	9.57	9.24	10.71	10.99
30.08	3.26	3.37	5.62	5.50	7.35	7.37	9.30	9.02	10.51	10.45
46.40	3.69	3.67	5.66	5.47	7.00	6.91	8.20	8.02	9.05	9.01
69.60	3.81	3.65	5.33	5.02	5.90	6.00	6.76	6.74	7.40	7.32
92.08	3.70	3.41	4.49	4.50	5.09	5.21	5.60	5.81	5.60	6.13

The laws we have established are not to be considered special to the instrument which we have used ; we consider that they apply to all electromotors of the same kind, and express the general laws of induction.

For the future it will be possible to calculate the work of these machines just like that of batteries, by which their use may be better regulated ; it will be necessary for this purpose to determine the constants A and r . We have obtained an insight into the nature of these constants, on which we will make a remark.

In the case of a constant battery, the internal resistance is that of the liquid in the troughs. This is not the case with our machine : r is merely a coefficient which satisfies the formula, it is equal to 110 turns of the rheostat ; and this resistance is only equal to 16.

Hence, for the very short and inverted currents which are developed in the plates when the induced currents pass, the bobbins have a very great resistance, much greater than that found with prolonged currents—than that, in short, which enters into Ohm's formula.

This circumstance alone characterizes induction, since it is the only change which is introduced into the formula ; but it alone is sufficient to explain the observed effects. If, in fact, the magneto-electrical machine had no other resistance than that of its wires, six plates together would represent more than six metres of normal copper wire ; it would act like a thermo-electric pile, and, having no resistance, would produce effects neither of light nor tension.

Tension, on the other hand, is the essential feature of induction ; it can only be produced by an electromotor of great resistance ; and by finding that this great resistance exists in the bobbins at the mo-

ment they become the seat of induction, we explain effects which are otherwise inexplicable, and show how dynamic electricity may be changed into electricity of tension.—*Comptes Rendus*, June 22, 1868.

THIRD NOTE ON THE LAWS OF INDUCTION.

BY MM. JAMIN AND ROGER.

If n' batteries, of the force A and the resistance r , are united in tension, they constitute a single one having the force $n'A$ and the resistance $n'r$; and, finally, if n of these mixed batteries were joined in parallel order, they would constitute a single one the constants of which would be $n'A$ and $\frac{n'r}{n}$. The heat reproduced in the external circuit x will be

$$C = \frac{n'^2 A^2 x}{\left(\frac{n'r}{n} + x\right)}.$$

We have connected our plates in couples of two, or in groups of three, or we have joined them all in tension; the results of experiment have confirmed the formula, as is proved by the following Table:—

Plates grouped.

Resistances.	One couple.		Two couples.		Three couples.		Two groups of three.		Six plates in tension.	
	Thermal units.		Thermal units.		Thermal units.		Thermal units.		Thermal units.	
	Obs.	Calc.	Obs.	Calc.	Obs.	Calc.	Obs.	Calc.	Obs.	Calc.
58	2.25	2.44	6.80	6.67	10.52	10.93	7.27	8.05		
92	2.92	3.06	7.97	7.32	10.99	10.93	9.12	9.51		
144	3.35	3.51	7.43	7.25	9.80	9.99	11.05	10.98		
173	3.98	3.64	7.05	6.99	9.22	9.24	10.93	11.08		
239	3.86	3.69	6.27	6.37	7.90	7.97	10.35	10.88		
303	3.63	3.60	6.30	5.76	7.19	6.96	8.93	9.51		
452	3.38	3.25	4.59	4.65	5.26	5.33	7.40	8.63		
612	2.86	2.86	3.87	3.81	4.25	4.23	7.02	7.32	11.32	11.08

Our experiments may be thus generalized:—

Whenever bobbins in any number pass with a constant velocity at the same distances in front of magnets, they act like the elements of a constant battery. Notwithstanding interruptions or inversions of the current, the quantity of electricity produced is governed by Ohm's law, and the heat regenerated in the external circuit by that of Joule. These laws apply to all modes of grouping bobbins. The electromotive force of each bobbin varies with the velocity, and with all the circumstances of the construction.

The same is the case with the resistance ρ of this couple; it is always greater than the resistance of the wires composing the bobbin:

α and ρ had still to be measured—that is, compared with the constants of a known element. For this purpose we measured with twenty Bunsen's elements the heat produced in the external circuit r ;

it is given by the formula

$$C = \frac{(20a')^2 x}{(20\rho' + x)^2}.$$

By these experiments a' and ρ' could be measured. We had

For one bobbin	$a = 1.78$	$\rho = 6.87$
For a Bunsen	$a' = 0.753$	$\rho' = 1.00$
Ratio	$\frac{a}{a'} = 2.37$	$\frac{\rho}{\rho'} = 6.87$

With its 96 plates in tension our machine was equal as force to 225, and as resistance to 655 Bunsen's elements. In quantity it is only equal to 38 elements of the resistance 18. With other groupings we had the following results :—

	Value of the machine in Bunsen's elements.	
	Electromotive force.	Resistance.
Tension	226	655
Two groups of three . . .	113	163.6
Three groups of two . . .	75	72.3
Quantity	37.9	18.0

In all cases the effect of the machine and its arrangement might be so modified as to produce in a given position of the circuit the maximum calorific effect.—*Comptes Rendus*, July 1, 1868.

ON THE DIATHERMANCY OF CHLORIDE OF POTASSIUM.

BY G. MAGNUS*.

There are now found at Stassfurt large crystals of perfect transparency to which mineralogists have given the name *sylvine*. I have ascertained that sylvine has the same properties as rock-salt as regards its transparency for heat. A plate of sylvine 26 millims. in thickness allows 76 per cent. of the incident heat to traverse it, which is the same proportion as a plate of rock-salt of the same thickness from Stassfurt.

Melloni found that a plate of rock-salt of 2.6 millims. (that is, a tenth of the thickness of my plate) allowed 92.3 per cent. of the incident heat to traverse it; another plate, which he calls imperfectly transparent (*louche*), only allowed 62 per cent. to pass. Thus sylvine and Stassfurt rock-salt, with ten times the thickness, possess a diathermancy which is the mean of those which Melloni found for plates of rock-salt.

I had occasion to try a plate of rock-salt from Würtemberg, which gave 92.6 for a thickness of 26 millims.

The diathermancy of sylvine resembles that of rock-salt in another respect: it does not vary with the temperature of the source. I have found it the same for heat emitted by a lamp and for that arising from a vessel heated to 100°. Rock-salt has hitherto been the only substance known to be endowed with this property.—*Comptes Rendus*, June 29, 1868.

* Extract from a letter to M. Regnault.

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149 Strand, London, W.C., September, 1868.

* Mr. TENNANT held the appointment of Lecturer on Geology and Mineralogy at Woolwich for seventeen years; the Lectures were discontinued in December last, Lectures on Military History being substituted.

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THE
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[FOURTH SERIES.]

NOVEMBER 1868.

XLII. *Notes on the Chemical Geology of the Gold-fields of California.* By J. ARTHUR PHILLIPS*.

DURING three separate visits to the Pacific coast of North America I have had numerous opportunities of studying the geology of the Californian gold-fields, and of investigating the circumstances attending the chemical and physical changes which have formerly occurred and which, to a certain extent, are still taking place in those regions. Whilst carrying out these researches, various facts have come under my notice which appear to throw some light on the formation of auriferous veins, as well as on the distribution of the precious metal in the rocks in which it is found.

I have endeavoured to embody in the following paper some of the results of my inquiries, in the hope that, should the conclusions to which I have arrived be shown to be fallacious, the facts which have been collected may nevertheless assist other investigators in arriving at a correct interpretation of the phenomena attending the formation of auriferous veins. In doing this, I propose to first give a short description of the gold regions, and subsequently to treat of their quartz veins, alluvial deposits, hot springs, and salt lakes, all of which are intimately connected with the chemical geology of the districts in which they occur.

Rocks of the Gold-Region of California.

The great sedimentary metalliferous belt of California lies on the western slope of the Sierra Nevada, beginning in the neigh-

* Communicated by the Author, being the substance of a paper read before the Royal Society, March 12th, 1868.

bourhood of the Téjon Pass, and extending through the State to its northern limit. In consequence, however, of various local circumstances, different portions of this band are of very unequal importance as gold-producing districts.

The principal auriferous region may be said to occupy the western portions of the several counties of Mariposa, Tuolumne, Calaveras, Amador, El Dorado, Placer, Nevada, Sierra, and Plumas, with portions of the eastern sides of Yuba and Butte counties.

The apparently limited extent of the auriferous belt towards the north, in the counties of Plumas and Butte, is, according to the State Geological Survey, not owing to the thinning out of the gold-bearing formation in these localities, so much as to its being here, as well as in Shasta and Siskiyou counties, in a great measure covered by large masses of lava of very recent origin. This has been poured forth from Lassen's Butte and other volcanic cones in its vicinity; and, overflowing the older slates, it has covered them to a great depth with a non-metalliferous and almost indestructible capping.

Beyond Mariposa, in the southern portion of the gold-region, the slates are narrower and subject to interruption, and, from being more frequently and more extensively encroached on by the granite, they almost cease to form a continuous belt. This gradual decrease in the width of the auriferous formation from north to south, and the continuously increasing amount of metamorphism displayed, are very marked, since the granite progressively occupies a relatively larger portion of the Sierra, and by degrees descends lower down its flank.

The slates of the auriferous belt of California have been satisfactorily shown by Professor Whitney * to belong, for a great extent, to the Jurassic period, although the occurrence of numerous Triassic fossils in the gold-bearing rocks of Plumas county and elsewhere renders it more than probable that no inconsiderable portion of the slates in the heart of the gold-region are of that age. The sedimentary rocks of the great auriferous belt lying on the western slope of the Sierra Nevada principally consist of various slates and schists, sometimes containing nodules of white felspar, which is generally more or less decomposed. Among them are also found sandstones of various degrees of fineness (often transformed into quartzites), black talcose schists, with slates exhibiting a well-defined cleavage and silky structure, together with bands of crystalline limestone.

The rock constituting the principal mass of the Sierra Nevada is a granite containing only a small proportion of quartz, and in which but one species of felspar (oligoclase) is generally found.

* Geological Survey of California.

In addition to quartz, felspar, and mica, the latter of which is well crystallized and often of a brilliant black colour, this rock encloses titaniferous iron, and occasionally crystals of sphene of a light yellow colour. Magnetic oxide of iron is also present in notable quantities, and consequently imparts this property to the enclosing granite. The granite of the Sierra appears to retain its peculiar characteristics throughout a great portion of its extent; but towards the southern extremity of the chain hornblende begins to make its appearance as one of the constituents of the rock; the mica at the same time becoming less plentiful, and losing its brilliancy and dark colour.

Lying between the band of metamorphic slates and the great central granitic mass forming the more elevated portions of the chain, are found various eruptive rocks, such as syenites, diorites, and porphyries. The first of these pass into granites by almost imperceptible gradations, and appear to belong to the same geological period. The diorites, on the contrary, are of more recent origin, and contain neither sphene nor titaniferous iron, although they enclose a considerable amount of magnetic oxide of iron. In many places diorite is observed to traverse the granite in the form of distinct dykes. The porphyries are comparatively of small extent, and contain crystals showing the usual striæ, indicating the presence of plagioclastic felspars.

Quartz Veins.

All the crystalline rocks in the vicinity of the sedimentary deposits contain numerous veins of quartz, which traverse the granite and diorites near their point of junction with the slates, and, in addition to gold, enclose crystals of iron pyrites and other metallic sulphides.

These quartz veins of the crystalline rocks are comprised within a narrow zone running from south to north, along the western flank of the mountains above the great band of metamorphic slates forming the most productive portion of the gold-region, and extend, in the vicinity of the line of junction, nearly throughout its whole extent.

The quartz veins of the band of metamorphosed slates occupying the western slope of the Sierra, and lower on its flank than the crystalline rocks above described, are numerous and important. They are not, however, by any means equally distributed throughout the region of slates, but are chiefly concentrated in a belt having a width from east to west of some twelve or fifteen miles, and extending from south to north throughout the whole length of the formation. These veins, for the most part, follow the general direction of the strata in which they are enclosed. This parallelism, however, is not

absolute, since in many instances a vein, besides having a somewhat different direction from that of the bedding of the enclosing rock, throws off branches cutting the slate at considerable angles.

One of the most remarkable gold veins in California is that extending from Mount Ophir, in Mariposa county, to Mokelumne Hill in Calaveras, a distance of over seventy miles. This lead*, which frequently crops boldly out above the surface of the ground, and varies in thickness from six to sixty feet, may in some places be traced for many miles across the country, and often presents an outcrop like an immense white wall. Although by no means continuous, this may be considered as an axis with regard to the other veins of the region, which have generally an almost similar direction, and are most frequently grouped at no very considerable distances from it.

The gangue of the auriferous veins of California is invariably quartz, which is generally crystalline in its structure, or partially vitreous and semitransparent. In the majority of cases the quartz constituting an auriferous veinstone is ribboned in such a way as to have the appearance of a succession of layers parallel with the walls of the lead; and some one or more of these laminæ are not unfrequently far more productive of gold than the others.

In some instances these parallel bands are separated from each other by a thin layer of quartz, slightly differing, either in colour or structure, from that forming the seams themselves; or they may be only distinguished by a difference of the colour or structure of two adjoining members of the series.

In many cases, however, laminæ of the enclosing slates divide the vein into distinct bands; and in such instances it will be observed that the thickness of the interposed fragments of slate is sometimes not greater than that of a sheet of the thinnest paper. Cavities or druses containing crystals of quartz sometimes occur in all the auriferous veins of the country; and a certain amount of crystallization may also not unfrequently be remarked along the lines of junction of the several bands of which a vein is composed. In such cases crystallization appears to have been set up on the surface of the last-deposited stratum, which has induced the formation of a similar crop of crystals on the layer subsequently formed on its surface. Quartz crystals, however, rarely occur in notable quantities in any of the most productive veins; and when the structure of a lead is highly crystalline, and the quartz more than ordinarily transparent, it is con-

* In California a quartz vein is called a "lead," and in Australia a "reef." In the gold-regions of the latter country the term *lead* is applied to the deposits of the "deep placers."

sidered an unfavourable indication with regard to its auriferous character.

The quartz considered by miners as most "favourable for gold" is seamy, stained by oxide of iron arising from the decomposition of pyrites, mottled, and somewhat marble-like in appearance. In addition to ordinary quartz in a more or less crystalline form, amorphous hydrated silica, or semi-opal, and chalcedony are occasionally met with : and in some instances the opal is interfoliated between layers of true quartz, and is sufficiently auriferous to repay the expenses of treatment.

Generally the walls of auriferous veins are smooth and well defined, often affording evidence of a considerable amount of dynamic action ; and in the case of the lead being divided into bands by interfoliations of slate, these, to a less extent, are sometimes marked by groovings indicative of mechanical motion. Between the vein and the enclosing rock there is sometimes, but not always, a thin stratum of clay, which occasionally encloses small particles of gold.

The metallic minerals enclosed in the gangue of auriferous veins are ordinary iron pyrites, blende, and galena, and, less frequently, arsenical pyrites, magnetic and copper pyrites, and cinnabar. These sulphides invariably contain gold ; and veins in which some one or more of them does not occur in considerable amounts, are not regularly and lastingly productive. In the earlier days of quartz-mining these sulphides were allowed to escape, and the "free gold" was alone obtained ; but at the present time they are all carefully collected, and form an important addition to the profits of the miner.

Near the surface the iron pyrites and other sulphides become decomposed by the action of air and the percolation of meteoric water through the mass, staining the quartz of a red or brown colour, and leaving the gold in a form highly favourable for amalgamation. Under such circumstances numerous cubical moulds of iron pyrites are found in the veinstone ; and although this mineral has been entirely removed by chemical action, the cavities left contain finely divided gold, obviously liberated by the decomposition of pyrites.

Beneath the line of natural drainage of the country the sulphides remain undecomposed, and the extraction of gold becomes more difficult ; but if "rock" containing crystals of pyrites be placed in nitric acid and allowed to remain for a few hours in a warm place, the sulphide becomes dissolved and finely divided, or filiform gold will partially occupy the resulting cavities.

In addition to the gold thus enclosed in the metallic sulphides, grains and small plates of that metal are disseminated throughout the veinstone ; and this is particularly the case in the vic-

nity of certain coloured streaks, generally nearly parallel with the apparent lines of deposition of the quartz. When gold is found lining a cavity in a lode, or is enclosed in plastic clay, it is often in the form of crystals, which are usually either octahedrons or cubes modified by planes of the octahedron or rhombic dodecahedron. When crystallization takes place in a narrow fissure, the crystals are much flattened.

In one of the detrital beds in the vicinity of the village of Volcano, in the county of Amador, a distinctly marked quartz vein may be observed cutting through the gravel, and evidently formed by the action of water holding silica in solution. The mass of this lode consists of agate and chalcedony, portions of which are coloured by ferruginous stains. In speaking of this vein, Professor Whitney says, "This is not by any means an isolated case; other localities have been noticed where all the conditions necessary to the formation of quartz veins, similar in their general features to those in the auriferous slates, must have existed during the most recent geological epoch."

With reference to this subject, Dr. Oxland* writes me as follows:—"The recent quartz vein containing argentiferous pyrites, of which I spoke to you when we met in California, is situated at Sulphur Springs, Bear Valley, about ten miles north-east of Borax Lake. This locality is resorted to by invalids for the purpose of bathing in the hot waters, which are supposed to possess medicinal properties, and which flow in very considerable quantities out of the foot of a hill on the north side of the creek.

"This water, which contains carbonate of soda, chloride of sodium, and a little borax, is perfectly bright and clear, but so hot, that it has to be conducted into a large reservoir to cool before it can be admitted into the tubs provided in the wooden bathing-sheds.

"The hill out of which this hot water flows rises about 400 feet above the cañon through which the waters make their escape, and near its summit a deposit of sulphur has been opened upon. This has been covered to a depth of several feet by vegetable mould, and Manzanita bushes are now growing luxuriantly upon it. In cutting a roadway from the valley to the sulphur deposit, a vein of friable quartz, evidently due to the action which has produced the solfatara, was cut through. This silica is banded in the direction of the vein, and contains finely divided iron pyrites. I took what I considered a fair sample of this quartz, which afforded me 16 ounces of silver per ton. Many of the nuggets of gold obtained in this immediate neighbourhood have been found with large pieces of cinnabar adhering to them."

* Formerly of the Toland Medical College, San Francisco, but now of Plymouth.

Some attention has also been recently directed to bands of auriferous slate found in the copper-bearing band west of the main gold-belt of the State, and in the foot hills of the Sierra. Of the deposits of this description, those near Lincoln, in Placer county, and at Quail Hill, in Calaveras, are the most remarkable.

In these localities the gold, instead of being obtained from well-defined veins chiefly composed of ordinary quartz, is enclosed in bands of siliceous slaty rock, extending north-west and south-east, and dipping in conformity with the other strata of the district. At the surface the Lincoln deposit is chiefly distinguished from the other slates of the neighbourhood by being, in places, deeply stained by oxide of iron, and forms a low outcrop of about 3000 feet in length and 200 feet in width.

Where this has been cut into, it has been found to consist of alternating bands of clay-slate stained by oxide of iron, and of a friable sandy quartz still retaining distinct evidences of stratification, and which under the microscope presents the appearance of a siliceous skeleton from which the more soluble constituents have been removed by aqueous agencies.

Through these bands, and running parallel with their stratification, dykes of felspathic rock make their appearance, and contain small opaque white crystals too much decomposed to admit of identification. These, like the slate itself, contain gold, but, in the case of the felspathic rock, in very small quantities only. In the slate are also found lenticular masses of blende and iron pyrites, both auriferous, with stains of carbonate of copper. The whole mass of the stratified rock contains a certain amount of "free gold," although the seams vary considerably in their yield, some of them affording mere traces, whilst others assay as much as 3 oz. per ton. The whole of the gold in the portions of the deposits as yet worked is in a "free" state; but it is probable that below the water-level it will be found associated with various sulphides, by the decomposition of which in the upper portions the precious metal has become liberated. The pyrites and blende, in addition to gold, afford by assay a small quantity of silver per ton; and particles both of native silver and native copper may be extracted by washing the decomposed slates.

As a general rule, the quartz lodes of California have not been found to be more subject to impoverishment in depth than other mineral veins, some of them having been worked on their dip for a distance of above 1200 feet without any diminution of their yield having taken place.

In order to determine the chemical and physical composition of quartz forming the gangues of some of the principal auri-

ferous veins of California, I have made analyses of the veinstones from several of the most productive mines, and have examined under the microscope thin sections from a great number of others. My experiments have, however, been chiefly confined to various lodes in the vicinity of Grass Valley, in the county of Nevada, with which I have had numerous opportunities of making myself acquainted, and among which are comprised some of the most productive veins of the country.

In preparing thin sections for examination I have carefully avoided the use of any kind of polishing-powder; and the arrangement made use of for examining them has generally been a $\frac{1}{4}$ objective, by Ross, with draw-tube and eyepiece magnifying about 400 linear. In some cases, however, a magnifying-power of above 1000 linear was employed.

Examination of Quartz from the Kate-Hayes Vein.—Fragments of quartz taken from near the surface at the Kate-Hayes vein, Grass Valley, and having a specific gravity of 2.59, gave the following results:—

Water lost at 212° F.	0.26	
Water lost by subsequent ignition	0.70	
Silica	93.32	
Alumina	2.03	
Iron	1.29	} Iron pyrites.
Sulphur	1.45	
Potash	0.40	
Soda	0.21	
Lime and magnesia	traces	
	99.66	

This vein is composed of parallel bands, some of which contain small quantities of chlorite and iron pyrites. The quartz of which it consists is of a greyish colour, not very distinctly crystallized, has a peculiar greasy appearance, and contains cavities, on the surfaces of which have been deposited guttate silica of a chalcedonious description. This rock contains gold in the form of large scales and plates.

When examined under the microscope with a low power, this quartz presents in some places a cloudy appearance, and is found to contain a few small cubical crystals of iron pyrites, together with cavities filled by chalcedonious matter. Under a higher power it is seen to contain fluid-cavities of about one-thousandth of an inch in diameter, and in which the ratio of the vacuity to the cubical content of the cavity is about 1 : 10.

This veinstone also encloses small cavities which appear to be lined with clay, and in which no vacuities can be seen. It likewise contains gas-cavities and *markings* very similar in appearance to the "glass-cavities" described by Mr. Sorby as occurring

in porphyritic pitchstone, in the leucite of the lava of Vesuvius, and in the augite of some trappean rocks. In the case of quartz from the auriferous veins of California, I am, however, inclined to believe that they are due to crystallization only, since I have never been enabled to discover globular vacuities in any of them, and precisely similar markings may be observed on many quartz crystals on examining their surfaces by the aid of an ordinary lens. Should a fresh deposit of silica immediately take place on the planes of a crystal thus marked, an appearance not unlike glass-cavities would be the result; and if the depressions on the original crystal should become coated with clay, by deposition from turbid water, before the formation of the succeeding stratum of quartz, a certain degree of opacity, like that observed in the quartz of many auriferous veinstones, would be caused. It is also obvious that crystallizations of other minerals might take place in such depressions; and these, becoming covered by the succeeding deposit of quartz, would have, to a great extent, the appearance of having been formed from a slag or glass in the act of cooling from a state of fusion.

That crystalline quartz is frequently produced by successive depositions we have abundant evidence from specimens in which a crystal is seen to enclose others, each successively smaller than that which immediately surrounds it. Clay and oxide of iron are also in this way deposited on the surfaces of crystals during the interval which elapses between the formation of two distinct layers, as may be observed at the tin mines of Schlackenwald and in other localities, where crystals are obtained of which the various layers are so separated from each other by a clayey deposit as to admit of being readily divided into a series of cup-like envelopes, finally enclosing a central nucleus.

Examination of Quartz taken from the Norambagua Mine.—Specimens of quartz taken from a depth of 500 feet at the Norambagua mine, situated about four miles south of Grass Valley, and having a specific gravity of 2.60, were selected for analysis and afforded the following results:—

Water lost at 212° F.	0.19
Water lost by subsequent ignition	0.39
Silica	96.29
Alumina	1.42
Iron	0.55
Sulphur	0.57
Arsenic	traces
Potash	0.36
Soda	traces
Lime and magnesia	traces
	<hr/> 99.77

The Norambagua vein is enclosed in a crystalline rock locally known as syenite; but in some parts of the hill its composition would rather entitle it to be classed as a diorite. This vein, which is seldom more than a foot in thickness, is divided by headings into thin laminæ parallel with its walls, and is chiefly composed of greasy-looking quartz, showing rather faint indications of crystallization. Between the several laminæ of silica there are not unfrequently thin deposits of blue slate, which have evidently been subjected to considerable pressure, and which cause the veinstone to be easily separated into parallel bands. In some places iron pyrites and arsenical pyrites are abundant; but in selecting specimens for analysis those portions of the veinstone containing a visible amount of sulphides &c. were avoided.

A microscopical examination of sections cut from this vein showed that it contains fluid-cavities of about $\frac{1}{5000}$ of an inch in diameter, in most of which vacuities could be observed. It also encloses a few gas-cavities and some apparently containing clay, together with markings like those observed in the quartz from the Kate Hayes.

Examination of Quartz from the North-Star Mine.—Some of the ordinary white quartz from the North-Star mine, near Grass Valley, having a specific gravity of 2.61, was next subjected to analysis, and gave the following results:—

Water lost at 212° F.	0.07
Water lost by subsequent ignition	0.03
Silica	98.61
Alumina	0.82
Peroxide of iron	traces
Potash	0.24
Soda	traces
	<hr/> 99.77

This vein, which sometimes thins down to a mere seam, and at others has a width of 6 feet, is one of the most productive in the district, and has been worked to a depth, on the inclination of the lode, of 750 feet. All the specimens were taken from near the bottom of the workings.

The quartz, of which it is principally composed, is of a milky colour, and presents indistinct crystalline faces in whatever direction it may be broken. Like nearly all the auriferous veins of the country, it exhibits a tendency to an arrangement according to bands parallel to the walls of the enclosing fissure, and, wherever it is most productive, contains iron pyrites and other sulphides.

Thin sections of this veinstone, examined under a high power, show a few minute fluid-cavities with vacuities, numerous small

cavities apparently containing clay, some larger ones in which the clay appears coloured by oxide of iron, and a great number of the markings before referred to, and which are believed to be due to crystallization.

The most remarkable circumstance in connexion with this vein is the occurrence within it of large quantities of hydrated silica of a bluish-grey colour: this has a resinous lustre, and possesses a distinct conchoidal fracture, but exhibits well-defined lines showing layers of deposition parallel with the other bands of which the lode is made up. It is found in the form of a series of lenticular deposits, often several fathoms in length, interlaminated with the ordinary crystalline quartz, from which it is separated by a thin heading of unctuous clay. This substance, which has the appearance of semi-opal, is frequently 6 or 8 inches in width in its thickest part, and gradually tapers off in all directions in such a way as to assume a lens-like form. This hydrated silica sometimes encloses iron pyrites, and contains a sufficient amount of the precious metal to render its treatment commercially advantageous.

Examination of Semi-opal from the North-Star Mine.—A specimen of this substance submitted to analysis had a specific gravity of 2·01, and afforded the following results:—

Water	8·08
Silica	86·69
Alumina	1·92
Protoxide of iron	0·99
Lime	1·44
Magnesia	0·23
Potash	0·26
Soda	0·05
	<hr/>
	99·66

This substance, at a temperature of 212° F., lost water slowly during several days, and, placed under a bell-glass over fused chloride of calcium, continued to lose weight after 300 hours, when the loss amounted to 5·11 per cent. Under these circumstances no attempt was made to determine separately the water lost at 212° F.; but the amount of hygrometric moisture present must have been exceedingly small, as this specimen was taken at the same time and kept in the same cabinet with the crystallized variety, which lost only 0·07 per cent. of moisture at 212° F.

Examined by polarized light this mineral does not show any evidence of crystallization; but when a high power is employed, it appears full of small elliptical cavities in which no vacuities were observed.

Examination of undecomposed Auriferous Slate from Lincoln.—A specimen of the undecomposed auriferous slate from the

neighbourhood of Lincoln in Placer county, had a specific gravity of 2·55, and yielded the following results:—

Water lost at 212° F.	0·31
Water lost by subsequent ignition	1·88
Silica	72·05
Alumina	17·87
Peroxide of iron	1·98
Lime	1·65
Magnesia	0·38
Potash	1·71
Soda	2·40
	<hr/>
	100·23

When examined under a low power, this slate is seen to be made up of angular fragments of quartz connected together by clayey matter, their largest planes being parallel to the lines of cleavage. It also contains numerous spots of peroxide of iron, which in many instances still retain the cubical form of iron pyrites. By the aid of a high power the quartz fragments are seen to enclose a few small gas-cavities; and one or two fluid-cavities, with distinctly visible vacuities, were observed.

Examination of decomposed Auriferous Slate from Lincoln.—The decomposed siliceous matter before referred to, somewhat resembling chalk in appearance, was found to have a specific gravity of 2·50, and on being analyzed gave the following results:—

Water lost at 212° F.	0·11
Water lost by subsequent ignition	traces
Silica	93·33
Alumina	4·46
Peroxide of iron	traces
Lime	0·75
Magnesia	0·43
Potash	0·52
Soda	traces
	<hr/>
	99·60

When examined under the microscope, this substance appears to consist of the siliceous skeleton of the auriferous slate from which the aluminous and ferruginous matters have been almost entirely removed. The angular fragments of quartz, being thus deprived of the cementing material which originally bound them together and gave solidity to the mass, remain in a disintegrated and friable state, although still retaining distinct indications of stratification.

The number of well-defined fluid-cavities in the veinstones of the auriferous lodes of California is exceedingly limited; and in

order to obtain sections affording good examples, even of small size, it is necessary to select such bands as may be more than ordinarily crystalline, or to operate on thin fragments of crystals sometimes found lining the interior of drusy cavities.

In the more opaque and generally most auriferous portions of veins, the cavities are numerous but exceedingly small, and are often so opaque (apparently rendered so by being internally coated by a lining of clay) that no vacuities can be distinguished. Some of the bubbles, contained in cavities requiring an arrangement magnifying a thousand linear to be distinctly seen, were observed to move freely about.

Out of more than sixty sections of veinstone examined, only some six or eight were found to contain fluid-cavities of sufficient size to admit of any attempt at accurate measurement; but in all cases there appeared to be considerable differences in the relative dimensions of the vacuities and the enclosing cavities, and I consequently ascertained by direct experiment the temperatures at which they severally became filled. For this purpose a small bath, made of sheet brass with a piece of microscopic glass fitted into each side, was employed. The section to be examined was clipped on the inside of one of the small panes of thin glass, and the bath filled with spermaceti into which the bulb of a thermometer was inserted. By attaching this apparatus to the stand of a microscope, of which the body was fixed horizontally, heating the bath, and throwing the light of an oil-lamp on the object by means of a condenser, it was easy to study the appearance of the different cavities under various conditions of temperature.

The magnifying-power employed was in this case about 275 linear, and consequently only such sections as contained tolerably large cavities could be satisfactorily examined. In every instance, however, they were found to require very different degrees of temperature to become full, since in the same specimens some of the vacuities disappeared at 180° F., others filled at temperatures slightly above that of boiling water, whilst many, though much reduced in size, remained perfectly visible at 365° F.*

Alluvial Deposits.

Although a very large amount of the gold annually obtained was no doubt originally derived from auriferous veins, not more than about one-third of the precious metal now collected is procured directly from that source. The larger proportion of the gold brought into the market is derived from alluvial diggings,

* As an illustration of this fact, the results obtained by heating the section of a crystal of quartz from the great Mariposa vein may be given. Six several vacuities disappeared at the following temperatures, 250°, 260°, 280°, 290°, 310°, and 320° F. Others were not nearly full at 362° F.

in which it is associated with clay, sand, and gravel, from which it is separated by washing.

These gold-bearing drifts belong to at least two geological epochs, both comparatively modern, although the later period is distinctly separated from the earlier, its materials being chiefly derived from the disintegration and redistribution of the older deposits.

In California the more ancient deposits, or *deep placers*, are probably referable to a river-system different from that which now exists, flowing at a higher level or over a then less elevated continent, and frequently nearly at right angles to the direction of the main valleys of the present period.

The sources from which the supply of gold is derived are therefore the following:—

(1) Auriferous veins, most frequently enclosed in metamorphic slates.

(2) The distribution of gold by ancient river-systems, giving rise to *deep diggings*.

(3) The redistribution of placer gold by the present river-system, forming accumulations known as *shallow diggings*.

The deep placers are in many localities covered by a thick capping of lava; and in some places the eruptive matters covering the auriferous deposits occur in the form of basaltic columns, beneath which are found the layers of sand, gravel, and boulders with which gold is associated. The wood which occurs in these gravel-beds is either beautifully silicified, or is replaced by iron pyrites. In such localities it not unfrequently happens that a piece of wood will be observed of which one end had been converted into lignite, whilst the other remained unaltered; but the whole having subsequently become silicified, now presents the appearance of a combination of alabaster and jet, each portion still retaining the structure of the original wood.

In the more clayey strata of these sedimentary deposits, leaf-beds and impressions of leaves are not unfrequently found; and an examination of these made by Dr. Newberry authorizes the conclusion that the auriferous deposits lying beneath the lava are of tertiary age, and that in all probability they generally belong to the later Pliocene epoch*.

In many localities, and particularly between the south and middle forks of the Yuba River, these auriferous gravels have frequently, where exposed to denudation, a thickness of 120 feet, and of more than 250 feet where they have been protected by a volcanic capping. These vast auriferous beds are composed of rounded masses of all the eruptive and metamorphic rocks which occur above them in the Sierra. As a general rule, the lower

* Geological Survey of California, pp. 250 & 251.

portions consist of larger boulders than the upper; but this does not exclude the occasional appearance of large rounded masses of rock among the middle or upper members of the series.

Water-worn gold is, to a greater or less extent, disseminated throughout the whole mass of these deposits; not, however, with uniformity, but always with greater abundance near the bottom, and more particularly in direct contact with the *bed-rock*, which is invariably grooved and worn by the action of water. The materials of which these deep placers are composed are frequently consolidated into a sort of hard concrete by being firmly bound together by crystalline iron pyrites; and sometimes this cementing material partially consists of carbonate of lime and amorphous silica. In many cases, as at some places in the county of Nevada, the sand, gravel, and boulders are firmly held together by a material consisting almost entirely of silica, in such instances giving rise to an exceedingly hard conglomerate.

When in the deep placers this consolidation of the materials of which they are composed has taken place, it is known by the name of *cement*, and often necessitates the employment of large quantities of gunpowder for its removal.

The silica forming one of the ingredients of the auriferous conglomerates is rarely met with in a crystalline form; but near Kenebeck Hill I found a cavity, resulting from the junction of several pebbles, completely lined with well-defined crystals of quartz. One of these, on being sufficiently reduced in thickness by grinding, did not show under the microscope the usual fluid-cavities of the quartz of the ordinary veins of the country. In some localities the silicified wood is found in the form of large trees, of which the entire mass has become replaced by a siliceous deposit; whilst in others this transformation has taken place to a limited extent only, and on breaking up a large trunk the central portion still retains the structure and properties of the original wood.

Where the cementing material of the conglomerate chiefly consists of pyrites, the enclosed trunks of trees are usually replaced by that mineral, although, of two pieces of wood lying in close proximity to each other, one may have become silicified, whilst the other is replaced by iron pyrites. On examining under the microscope the leaves which often form thin beds in the sands and clays of the tertiary auriferous river-beds of the gold-regions, they are seen to be covered by small guttate deposits of semitranslucent silica; and on burning some leaves found in these drifts in the county of Nevada, I found that, after long digestion in hydrochloric acid, they afforded 25.21 per cent. of a siliceous ash.

A specimen of silicified wood (coniferous) from French Corral

lost in the water-bath 0.59 per cent. of moisture; and by subsequent ignition the further loss amounted to 8.46 per cent., thus making the total loss 9.05 per cent. Another piece of wood from the same locality lost 0.56 per cent. at 212°, and 8.43 per cent. by subsequent ignition; in this case, therefore, the total amount of water contained was 8.99 per cent.

The assay of several specimens of the cementing pyrites showed that it invariably contained a small but very variable amount of gold. In order to ascertain whether this exists in the form of water-worn grains mechanically enclosed within the sulphides, or in the form of spongy and filamentary particles similar to those met with in the pyrites of auriferous veins, I dissolved various samples in nitric acid, and afterwards subjected the residues to microscopical examination. In this way I detected granules of the precious metal which had evidently been worn by the action of water, whilst others appeared not to have been subjected to such attrition. However, on examining pyrites forming the substance of various fossilized trees, I was unable, when the samples were taken from the inside of the trunks in order to avoid the possibility of accidentally attached particles, to detect an appreciable amount of gold, even when the assays were made on several hundred grains of the material; but my experiments were conducted on specimens from one locality only (French Corral), and in Australia very different results appear to have been obtained. Mr. Ulrich states that "in the gold-drifts pyrites is often found incrusting or entirely replacing roots and driftwood; such specimens very quickly decompose on exposure to the atmosphere, and samples have, on assay by Messrs. Daintree, Latta, and Newberry, yielded from a few pennyweights to several ounces of gold per ton." According to Mr. H. A. Thompson, "a beautiful specimen of crystallized iron pyrites, deposited on a piece of wood taken from the drift immediately below the basalt at Ballarat, gave, by assay, 40 oz. of gold per ton; and in another case, where only the pyrites from the centre of an old tree-trunk was examined, the yield was over 30 dwt. of gold per ton. Some of the fine dust obtained in washing out the gold at the Royal Saxon claim, Ballarat, yielded, by assay, over 15 oz. of gold per ton. When placed under the microscope, this dust was seen to be composed of minute crystals of pyrites aggregated into round pellets from $\frac{1}{300}$ to $\frac{1}{100}$ of an inch in diameter, the surfaces being roughened by the projecting angles of the crystals, and unwater-worn"*.

[To be continued.]

* Notes on the Physical Geography, Geology, and Mineralogy of Victoria. By Alfred R. C. Selwyn and George H. F. Ulrich. Pp. 36. (Melbourne, 1866.)

XLIII. *On Discontinuous Movements of Fluids.*

By Professor HELMHOLTZ*.

IT is well known that the hydrodynamic equations give exactly the same partial differential equation for the interior of an incompressible fluid free from friction, and the particles of which do not rotate, as obtains for stationary currents of electricity or heat in conductors of uniform conductivity. It might be expected, therefore, that, for the same shape of the space traversed and for like conditions of boundary, the form of the stream for fluids would be the same as that for electricity and heat, with the exception of trifling differences caused by secondary conditions. In fact, however, the differences in many cases are very evident and profound between the arrangement of the current of a fluid and that of the above-mentioned imponderables.

Such differences are especially obvious if the current enters through an opening with sharp edges, into a wider space. In such cases the radiant lines of electricity separate from one another in all directions immediately on passing the opening; while a fluid current, whether water or air, moves forwards at first from the orifice in a compact stream, which is then usually dissipated in gyrations at a greater or less distance. The parts of the fluid of the larger vessel which lie in the neighbourhood of the orifice, but at the sides of the current, may, on the contrary, remain completely at rest. Every one is acquainted with this sort of motion, which is shown clearly by a current of air impregnated with smoke. In fact in these phenomena the compressibility of the air is of little influence; and air shows, with very few deviations, the same forms of motion as water.

Such great differences between what actually takes place and the deductions from the theoretical analysis hitherto accepted, must cause physicists to regard the hydrodynamical equations as a practically very imperfect approximation to the truth. The cause of this discrepancy might be supposed to lie in the internal friction of the fluid, although the divers strange and saltatory irregularities which every one has encountered who has experimented upon the motions of fluids can in no wise be accounted for by the continuous and uniform action of friction.

The investigation of the case where periodical motions result from a continuous air-current, as, for instance, in organ-pipes, convinced me that such an action could only arise by a discontinuous motion of the air, or at least by an approximately discontinuous one. Hence I was led to the discovery of a condi-

* Translated by Frederick Guthrie, F.R.S.E., from the *Monatsbericht der königlich preussischen Akademie der Wissenschaften zu Berlin*. April 1868, p. 215.

tion which must be taken into account in the integration of the hydrodynamic equations, and which has, as far as I am aware, been hitherto neglected. By taking this condition into account, on the other hand, where the calculation can be carried out, forms of motion are obtained such as are observed in reality. The circumstance in point is the following:—

In the hydrodynamic equations the velocity and the pressure of the current-particles are treated as continuous functions of the coordinates. But, on the other hand, if we consider a perfect liquid (that is, one not subject to friction), there is nothing essential in its nature to prevent two immediately neighbouring liquid layers from slipping past one another with finite velocity. At all events, those properties of fluids which are considered in the hydrodynamic equations, namely the constancy of the mass in all units of volume and the equality of the pressure in all directions, clearly offer no obstacle to the occurrence of tangential motions of finite difference of magnitude on both sides of a plane passing through the interior. The components perpendicular to the surface of the velocity and the pressure must, on the other hand, of course be equal on both sides of such a surface. In my work on gyratory movements*, I have already pointed out that such a case must occur if two previously separated masses of water come into superficial contact when they have different motions. In my above-mentioned work I was led to the idea of such a *surface of separation*, or, as it was then called, “*gyration-surface*,” by the conception of lines of gyration arranged continuously along a surface, the mass of which may be vanishingly small, while its moment of rotation remains finite.

Now, in a fluid which at first is at rest, or in continuous motion, a finite difference of motion between immediately neighbouring particles can only be brought about by a moving force which acts discontinuously. Of external forces, we here only consider the force of impact.

But there exists also in the interior of the fluids a cause which may give rise to discontinuity in the motion. Pressure, indeed, may assume any positive value, and the density of the fluid will vary continuously with the pressure. But the moment the pressure passes zero and commences to become negative, a discontinuous change in the density takes place, the fluid is broken asunder.

Now the magnitude of the pressure in a moving fluid depends upon the velocity, and, indeed, in incompressible fluids the diminution of the pressure under otherwise similar circumstances is directly proportional to the *vis viva* of the moving particles. If the *vis viva* exceeds a certain amount, the pressure must in fact

* *Journal für reine und angewandte Mathematik*, vol. lx.

become negative, and the fluid must be torn apart. At such a place the accelerating force which is proportional to the differential coefficient of the pressure becomes, of course, discontinuous, and consequently the condition is fulfilled which is necessary to bring about a discontinuous motion in the fluid. The motion of the fluid in passing by such a place must be of such a nature that a surface of separation is there formed.

The velocity necessary to bring about the rupture of the fluid is that which the fluid would assume if it were to flow into *vacuum* under the pressure which the fluid would have at the same place when at rest. This is certainly a comparatively considerable velocity; but we must bear in mind that if fluids are to flow continuously like electricity, the velocity at every sharp edge around which the current turns must be infinitely great*. It consequently follows that *every perfect geometrically sharp edge by which a fluid flows must tear it asunder and establish a surface of separation, however slowly the rest of the liquid may move*. When the edges are imperfectly formed, and rounded, such a separation requires a certain increased velocity. Pointed projections on the side of the orifice must act in the same manner.

In regard to gases, the same condition obtains with them as with liquids, with the exception that the *vis viva* of the motion of a particle is not directly proportional to the diminution of the pressure p , but to the quantity p^m , where, taking into account the cooling of the air by expansion, $m = 1 - \frac{1}{\gamma}$, γ being the ratio between the specific heat with constant pressure and that with constant volume. For atmospheric air, the exponent m has the value 0.291. Since this is positive and real, p^m and p can only decrease to nothing, and not become negative as the velocity becomes high. It would be otherwise if gases simply followed Mariotte's law, and did not suffer any change of temperature. The value $\log p$ would then enter instead of p^m ; and this value may become infinite and negative without p becoming negative. Under this condition, a tearing asunder of the mass of air would not be necessary.

We may satisfy ourselves of the actual existence of such discontinuities by allowing a stream of air impregnated with smoke to flow through a round hole or cylindrical tube with moderate velocity so that no hissing ensues. Under favourable circumstances thin streams may in this way be obtained of a line in diameter and several feet long. Within the cylindrical

* At the very small distance ϵ from a sharp edge the faces of which are inclined at an angle α , the velocity becomes infinite with ϵ^{-m} , where $m = \frac{\pi - \alpha}{2\pi - \alpha}$.

surface the air is then in motion with constant velocity ; but outside the stream, even quite close to it, the air is not at all, or scarcely at all, moved. This sharp separation may also be seen very distinctly if a calmly flowing cylindrical current of air be led through the apex of a flame. A sharply bordered piece of the flame is cut out, while the rest of it remains quite undisturbed, or at most a very thin sheet, corresponding to the boundary layers caused by the friction, is carried along a little way with the air-current.

With regard to the mathematical theory of these motions, I have already given the limiting conditions for an internal surface of separation of the liquid. These conditions are, that the pressure on both sides of the surface must be the same, as also must be the components of the velocity in a direction perpendicular to the surface of separation. Since, now, the motion throughout the interior of an incompressible fluid, the particles of which have no rotary motion, is fully known when the motion of its entire surface and its interior discontinuities are given, it follows generally that when a fluid is rigidly enclosed, we have only to consider the motion of the surface of separation and the changes in the discontinuity.

Such a surface of separation can be treated mathematically exactly as if it were a surface of gyration—that is, as if it were completely covered with lines of gyration (*Wirbelfäden*) of infinitely small mass, but finite moments of rotation. In every surface unit of such a surface of gyration there is one direction in which the components of the tangential velocities are equal. This gives at once the direction of the lines of gyration at the corresponding place. The moment of these lines must be made proportional to the difference shown by the perpendicular components of the tangential velocity on both sides of the surface.

The existence of such lines of gyration for an ideal fluid without friction is a mathematical fiction which facilitates the integration. In an actual liquid subjected to friction this fiction is quickly realized, because the bordering particles are set in rotation by the friction, and then immediately are formed lines of gyration of finite gradually increasing mass, while the discontinuity of the motion is thereby at the same time equalized.

The motion of a surface of gyration, and of the lines of gyration lying in it, are to be determined according to the rules established in my work on gyratory motion. The mathematical difficulties of this problem can only, it is true, be overcome in a few of the more simple cases. But in many other cases we may at least draw conclusions concerning the direction of the changes by means of the method of viewing the matter just described.

It is especially to be remarked that, according to the laws

established for gyratory motions, the lines, and with them the surfaces of gyration, can neither originate nor disappear in the interior of a fluid free from friction, but that, on the contrary, every line of gyration must retain the same moment of rotation constant. It is further clear that the lines of gyration upon a gyration surface must advance with a velocity which is the mean of the velocities existing on both sides of the surface. It hence follows that a surface of separation can only be elongated in that direction in which the stronger of the two contiguous currents in it moves.

I sought, in the first place, for examples of persistent surfaces of separation in stationary currents which would allow of integration, in order to test whether the theory gives forms of currents which correspond better with experiment than when the discontinuity of the motion is neglected. If a surface of separation which divides flowing from still water has to remain stationary, the pressure along the surface must be the same in the moving layer as in the stationary one; whence it follows that the tangential velocity of the particles of water throughout the whole extent of the surface must be constant. So also must be the density of the fictitious lines of gyration. The beginning and end of such a surface can only lie at the side of the vessel, or at an infinite distance. In the first case they must be tangential to the side of the vessel, provided that the latter be continuously bent, because the component of the velocity perpendicular to the side of the vessel must be equal to nothing.

The stationary forms of the surfaces of division are distinguished, as experiment and theory alike indicate, by a remarkably high degree of alterability when subjected to the least disturbance, so that they comport themselves in some degree like bodies in unstable equilibrium. The remarkable sensitiveness to sound of a cylindrical current of air impregnated with smoke has already been described by Dr. Tyndall. I have verified Dr. Tyndall's experiments. This is clearly a property of the surfaces of separation, which is of the greatest importance in sounding musical pipes.

Theory points out that, wherever an irregularity is formed on the surface of an otherwise stationary current, this must give rise to a progressive spiral unrolling of the corresponding part of the surface; the corresponding portion, moreover, advances along the current. This endeavour towards spiral unrolling at every interruption is also easily recognizable on observing the currents. According to theory, a prismatic or cylindrical current could be infinitely long. But in practice such a current cannot be formed, because in an element which is so easily moved as air it is impossible entirely to avoid small disturbances.

It is easy to see that the conditions of the state of rest are satisfied by such an infinitely long cylindrical current which, issuing out of a tube of the same diameter, enters into a still external fluid, and contains throughout fluid which moves parallel to its axis with uniform velocity.

I shall here merely give a sketch of the mathematical treatment of a case of the inverse kind, where the current passes out of a wide space into a narrow channel, in order at the same time to give an example of a method by which certain problems can be solved in the doctrine of potential functions which have hitherto presented difficulty.

I confine myself to the case where the motion is "stationary," and depends alone upon two rectangular coordinates x, y , and where, further, in the frictionless fluid there are no rotating particles, and consequently none such can arise. For the particle of fluid at the point (x, y) , let us denote by u the component velocity parallel to x , and by v that parallel to y . Then two functions of x and y may, as is well known, be found such that

$$\left. \begin{aligned} u &= \frac{d\phi}{dx} = \frac{d\psi}{dy}, \\ v &= \frac{d\phi}{dy} = -\frac{d\psi}{dx}. \end{aligned} \right\} \dots \dots \dots (1)$$

By means of these equations the condition is at once fulfilled in the interior of the fluid, that the mass remains constant for every unit of volume, namely

$$\frac{du}{dx} + \frac{dv}{dy} = \frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} = \frac{d^2\psi}{dx^2} + \frac{d^2\psi}{dy^2} = 0. \dots (1a)$$

With the constant density h , and when the power of the external force is represented by V , the pressure in the interior of the fluid is given by the equation

$$\left. \begin{aligned} V - \frac{p}{h} + C &= \frac{1}{2} \left[\left(\frac{d\phi}{dx} \right)^2 + \left(\frac{d\phi}{dy} \right)^2 \right] \\ &= \frac{1}{2} \left[\left(\frac{d\psi}{dx} \right)^2 + \left(\frac{d\psi}{dy} \right)^2 \right] \end{aligned} \right\} \dots \dots (1b)$$

The curves

$$\psi = \text{const.}$$

are the current lines of the fluid, and the curves

$$\phi = \text{const.}$$

are orthogonal to them. The latter are the curves of equal power in the case of electricity, or of equal temperature in the case of heat, flowing in a stationary current in conductors of constant conductivity.

From equation (1) we get as integral equation that the value $\phi + \psi i$ is a function of $x + yi$ (where $i = \sqrt{-1}$). The solutions hitherto found generally express ϕ and ψ as a sum of terms which are themselves functions of x and y . But, inversely, we may regard $x + yi$ as a function of $\phi + \psi i$ and develop. In problems dealing with currents between two rigid walls, ψ is constant along the borders; so that if ϕ and ψ be laid down as rectangular coordinates in a plane, we have, in a strip of this plane bounded by two parallel straight lines $\psi = c_0$ and $\psi = c_1$, to seek the function $x + yi$ so that it may correspond at the edge to the equation of the wall, and in the interior assume the given discontinuities.

A case of this kind is when we put

$$x + yi = A \{ \phi + \psi i + e^{\phi + \psi i} \}, \quad (2)$$

or

$$x = A\phi + Ae^{\phi} \cos \psi,$$

$$y = A\psi + Ae \sin \psi :$$

for the value $\psi = \pm \pi$, y is constant, and

$$x = A\phi - Ae^{\phi}.$$

If ϕ varies from $-\infty$ to $+\infty$, x changes at the same time from $-\infty$ to $-A$, and then back again to $-\infty$. The current-curves $\psi = \pm \pi$ correspond, therefore, to the current along two straight walls, for which $y = \pm A\pi$, and x varies between $-\infty$ and $-A$.

If, therefore, we use ψ to express the current-curves, equation (2) corresponds to a current which flows into infinite space from a canal bordered by two parallel planes. At the edge of the canal, however, where $x = -A$ and $y = \pm A\pi$, and, further, where $\phi = 0$ and $\psi = \pm \pi$,

$$\left(\frac{dx}{d\phi} \right)^2 + \left(\frac{dy}{d\phi} \right)^2 = 0,$$

$$\left(\frac{d\phi}{dx} \right)^2 + \left(\frac{d\phi}{dy} \right)^2 = \infty.$$

Electricity and heat may flow in this manner; liquids would be torn asunder.

If from the edge of the canal there should proceed stationary lines of division, which of course would be continuations of the current-lines $\psi = \pm \pi$ passing along the wall, and if exteriorly to these lines of division which border the current fluid there should be rest, then the pressure must be the same on both sides of the lines of division. That is, along those portions of the

lines $\psi = \pm \pi$ which correspond to the independent divisional lines we must have, according to (1b),

$$\left(\frac{d\phi}{dx}\right)^2 + \left(\frac{d\phi}{dy}\right)^2 = \text{const.} \quad . \quad . \quad . \quad (3)$$

In order now to maintain the chief features of the motion given in equation (2), let us add to the above expression for $x + yi$ another term $\sigma + \tau i$, which is also a function of $\phi + \psi i$.

We have then

$$\left. \begin{aligned} x &= A\phi + Ae^{\phi} \cos \psi + \sigma, \\ y &= A\psi + Ae^{\phi} \sin \psi + \tau; \end{aligned} \right\} \quad . \quad . \quad . \quad (3a)$$

and we must determine $\sigma + \tau i$ so that, along the free portion of the divisional surface $\psi = \pm \pi$, we may have

$$\left(A - Ae^{\phi} + \frac{d\tau}{d\phi}\right)^2 + \left(\frac{d\tau}{d\phi}\right)^2 = \text{const.}$$

This condition is fulfilled if, in the same place, we make

$$\frac{d\sigma}{d\phi} = 0, \text{ or } \sigma = \text{const.}, \quad . \quad . \quad . \quad (3b)$$

and

$$\frac{d\tau}{d\phi} = \pm A \sqrt{2e^{\phi} - e^{2\phi}}. \quad . \quad . \quad . \quad (3c)$$

Since ψ is constant along the wall, we can integrate the last equation in regard to ϕ , and convert the integral into a function of $\phi + \psi i$ by putting $\phi + i(\psi + \pi)$ everywhere in place of ϕ . We thus, after properly determining the constants of integration, get

$$\sigma + \tau i = Ai \left\{ \sqrt{-2e^{\phi + \psi i} - e^{2\phi + 2\psi i}} + 2 \arcsin \left[\frac{i}{\sqrt{2}} e^{\frac{1}{2}(\phi + \psi i)} \right] \right\}. \quad (3d)$$

The points of divergence of this expression are where $e^{\phi + \psi i} = -2$, that is, where $\psi = \pm (2a + 1)\pi$ and $\phi = \log 2$; so that none of them lie in the interval from $\psi = +\pi$ to $\psi = -\pi$. The function $\sigma + \tau i$ is here continuous.

Along the wall

$$\sigma + \tau i = \pm Ai \left\{ \sqrt{2e^{\phi} - e^{2\phi}} - 2 \arcsin \left[\frac{1}{\sqrt{2}} e^{\frac{1}{2}\phi} \right] \right\}.$$

If $\psi < \log 2$ the whole value is purely imaginary, so that $\sigma = 0$, while $\frac{d\tau}{d\phi}$ assumes the value given in (3c). This portion of the lines $\psi = \pm \pi$, therefore, corresponds to the free portion of the stream.

If $\phi > \log 2$, the whole expression as far as $+A\pi$ becomes real, which latter belongs to the value of τi through its relation to $y i$.

The equations (3a) and (3d) correspond, therefore, to a pouring out of a boundless trough into a canal bordered by two planes, the breadth of the canal being $4A\pi$, and the length of its walls being from $x = -\infty$ to $x = -A(2 - \log 2)$. The free divisional line of the current liquid is at first bent from the edge of the orifice a little towards the side of the positive value of x , where for $\phi = 0$, $x = -A$, and $y = \pm A(\frac{3}{2}\pi + 1)$ it attains its greatest x -value. It then enters the interior of the canal, and at last approaches the two lines $y = \pm A\pi$ asymptotically; so that, finally, the breadth of the stream as it flows out is only equal to half the breadth of the canal.

The velocity along the divisional surface and at the straight end of the stream flowing out is $\frac{1}{A}$. Along the rigid wall, and in the interior of the liquid, the velocity is always less than $\frac{1}{A}$, so that this form of motion may take place with all values of velocity of efflux.

One especially remarks in these examples how it is shown that the form of the liquid current in a tube may be determined for a long way by the shape of the first portion.

Addition, relative to electrical dispersion.—If in equation (2) we look upon the value ψ as the power of the electricity, we get the distribution of the electricity in the neighbourhood of the edge of two plane and almost touching screens under the condition that the distance between the two may be considered vanishingly small compared with the radius of curvature of their curved edges. This is a very simple solution of the problem which Clausius* has discussed. The same distribution of electricity is obtained as was found by Clausius, as far, at least, as it is independent of the curvature of the edge.

I may add that the same method suffices to determine the distribution of the electricity on two parallel infinitely long plane strips whose four corners (in section) form the points of a rectangle. The potential function ψ is given by an equation of the form

$$x + yi = A(\phi + \psi i) + B \frac{1}{H(\phi + \psi i)}, \quad . \quad . \quad . \quad (4)$$

where $H(u)$ represents the function developed by Jacobi in the *Fundamenta Nova*, p. 172, of the numerator of $\sin am u$. The

* Pogg. *Ann.* vol. lxxxvi.

strips correspond, according to the system there adopted, to the values $\phi = \pm 2K$, where $x = \pm 2AK$ gives half the distance of the strips, while the breadth of the strips depends upon the relation between the constants A and B.

From the form of the equations (2) and (4) we may see that ϕ and ψ can only be expressed as functions of x and y by exceedingly complicated serial development.

XLIV. On Unitation ; a novel Arithmetical Operation.

By W. H. WALENN, F.C.S.*

IT has long been known that the remainder to the division of any number by 9 may be determined by dividing the sum of its digits by 9. The following theorem is a generalization and explanation of this property, and establishes a similar operation for all other divisors less than 10.

The theorem is, that if t be the tens' and u the units' digit of a two-figure number, and δ be any integer less than 10, then

$$\overline{10 - \delta t + u}$$

has the same remainder to δ as $10t + u$.

For $\overline{10 - \delta t + u} = 10t + u - \delta t$, and the latter expression is only different from $10t + u$ by an exact number of times δ .

Further, if the digits be s, t, u , the same is true of

$$\overline{10 - \delta^2 s + 10 - \delta t + u},$$

and so on. For, each time 10 occurs as a factor in any term, it must be treated in the way above indicated, and

$$\overline{10 - \delta^2 s + 10 - \delta t + u} = \overline{10 - \delta} \overline{10 - \delta s + 10 - \delta t + u}.$$

These formulæ show the means of determining the remainder to any digit without knowledge of any multiple of that digit, inasmuch as the operation shown by the formula $\overline{10 - \delta t + u}$ only involves multiplication and addition (when $\delta = 9$, addition only); instead of dividing the result of the said operation by δ to obtain the final result, the operation is repeated until only one digit, necessarily one or more units, is obtained. The fitness of regarding this method of obtaining the remainder to δ as an operation totally distinct from that of division, and the fact of single digits only being used, make it convenient to call the said operation "unitation," the remainders being "unitates," and the divisor the "base."

In practice, unitation itself may be simplified by commencing

* Communicated by the Author.

at the left hand and reducing the result of the operation to a single figure as soon as any number higher than 9 arises; for instance, to unitate 34567 to the base 8, or, as it might be expressed, to find $U_8 34567$, the work is $6+4=10$, $4+5=9$, $2+6=8$, 7; $\therefore U_8 34567=7$.

Regarding unitation in connexion with the well-known fact that all direct operations upon the remainders to divisors are respectively analogous to the same operations upon the dividends, also taking into account the periodicity of series of unitates—the verification of direct arithmetical operations and of formulæ involving them (with a certain degree of accuracy) becomes very easy; also the unitate of any unknown number may be calculated from a known number with which it is connected by a known direct operation, or by some inverse operations.

In the following examples, Ux = the unitate of x to the base 9.

Examples:—I. $x=25(2558^2-187^3)=104025$.

$$Ux = U\{7(2^2 - U7^3)\} = U\{7(4-1)\} = U(7 \times 3) = 3.$$

$$\text{II.} \quad x = 537 \cdot 2 \times 23 = 12355 \cdot 6.$$

$$Ux = U(8 \times 5) = 4.$$

$$\log x = 2 \cdot 7301360 + 1 \cdot 3617278 = 4 \cdot 0918638.$$

$$U(\log x) = U(4+8) = 3.$$

$$\text{III.} \quad x = \frac{36326599}{1342} = 27069 + \frac{1}{1342}.$$

$$Ux = 7 = 6 + 1;$$

or, proving by multiplication,

$$x_1 = 36326599 = (27069 \times 1342) + 1.$$

$$Ux_1 = 7 = (6 \times 1) + 1.$$

$$\text{IV.} \quad x = 58^{247}; \text{ find } Ux, U_8x, \text{ and } U_7x.$$

In this case $Ux^y = U\{Ux^{Uy}\}$, for the series of the powers of $U_8 58$, = 4, has 3 (a divisor of 9) as its repeating period;

$\therefore Ux = U4^4 = 4$; i. e. $\frac{58^{247}}{9}$ gives 4 as a remainder. $U_8 x^y =$

$U_8\{Ux^{Uy}\}$ being true for $U_8 58^{247}$, $U_8 x = U_8 2^7 = 8$, that is, 58^{247} is divisible by 8 without a remainder. $U_7 x = 2^1 = 2$, for the series of the powers of $U_7 58$ has 6 for its repeating period.

V. $x = 47^{85}$; find Ux , U_8x , and U_7x . The series Ux_1^n , Ux_1^{n+1} , Ux_1^{n+2} , &c. repeats after 6 terms; except when $Ux_1 = 2$, 5, or 8, the same series repeats after 9 terms. $U47 = 2$, it therefore repeats after 6 terms, and $Ux = 2^1 = 2$, $U_8 x = U_8 7^5 = 7$, $U_7 x = 5^1 = 5$.

VI. The series Ux^2 , giving x the successive values 1, 2, 3, 4, 5, 6, 7, 8, 9, is 1, 4, 9, 7, 7, 9, 4, 1, 9. This series enables Tables of squares to be verified within certain limits of correctness.

VII. A similar series for Ux^3 is 1, 8, 9, 1, 8, 9, 1, 8, 9.

VIII. A similar series for Ux^{-1} is (excluding the values of $x=3, 6$, and 9) 1, 5, —, 7, 2, —, 4, 8, —. This series enables unitates to be assigned to such circulating decimals as $\frac{1}{7}$.

The operations U_8x , U_9x , $U_{10}x$, $U_{11}x$, $U_{99}x$, and $U_{999}x$ are the most useful and easily performed.

When any series of numbers, the powers of natural numbers for instance, is to undergo examination by unitation, it is useful to make a rectangular Table with the unitates of the powers in vertical columns; each vertical column belonging to one power—the square for instance. The unitates of the powers of the roots are in horizontal columns, each horizontal column referring to the powers of the same root—2 for instance.

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XLV. On Riccati's Equation. By A. CAYLEY, F.R.S.*

THE following is, it appears to me, the proper form in which to present the solution of Riccati's equation.

The equation may be written

$$\frac{dy}{dx} + y^2 = x^{2q-2},$$

which is integrable by algebraic and exponential functions if $(2i+1)q = \pm 1$, i being zero, or a positive integer. To effect the

integration, writing $y = \frac{1}{u} \frac{du}{dx}$, we have

$$\frac{d^2u}{dx^2} = x^{2q-2}u.$$

The peculiar advantage of this well-known transformation has not (so far as I am aware) been explicitly stated; it puts in evidence the form under which the sought-for function y contains the constant of integration. In fact if $u=P$, $u=Q$ be two particular solutions of the equation in u , then the general solution is $u=CP+DQ$; and denoting by P' , Q' the derived functions, the value of y is

$$y = \frac{CP' + DQ'}{CP + DQ},$$

* Communicated by the Author.

showing the form under which the constant of integration $C+D$ is contained in y . To complete the solution, assume

$$u = ze^{\frac{1}{q}x^q};$$

we find

$$\frac{d^2z}{dx^2} + 2x^{q-1} \frac{dz}{dx} + (q-1)x^{q-2}z = 0;$$

considering first the particular integral of the form

$$z = A + Bx^q + Cx^{2q} + Dx^{3q} + \&c.,$$

we find that the equation will be satisfied if

$$\begin{aligned}(q-1)A + q(q-1)B &= 0, \\ (3q-1)B + 2q(2q-1)C &= 0, \\ (5q-1)C + 3q(3q-1)D &= 0, \\ (7q-1)D + 4q(4q-1)E &= 0, \\ \&c.\end{aligned}$$

If $A=1$, this is

$$\begin{aligned}A &= 1, \\ B &= -\frac{q-1}{q(q-1)}, \\ C &= +\frac{(q-1)(3q-1)}{q(q-1)2q(2q-1)}, \\ D &= -\frac{(q-1)(3q-1)(5q-1)}{q(q-1)2q(2q-1)3q(3q-1)} \\ \&c.,\end{aligned}$$

where it is to be noticed that the series may be considered to stop so soon as there is in the numerator a factor $=0$. For instance, if $5q-1=0$, then if the particular integral had been assumed to be $z=A+Bx^q+Cx^{2q}$, the only conditions to be satisfied by the coefficients are the first and second equations giving the foregoing values of A, B, C . It is immaterial that the analytical expressions of F and the subsequent coefficients contain in the denominators the evanescent factor $5q-1$; the coefficients after C do not ever come into consideration.

Thus if $(2i+1)q = +1$, the series terminates, and we have for u the finite particular solution

$$u = P = \left(1 - \frac{q-1}{q(q-1)}x^q + \frac{(q-1)(3q-1)}{q(q-1)2q(2q-1)}x^{2q} - \&c.\right)e^{\frac{1}{q}x^q}:$$

and it is easy to see that we may herein change the sign of x^q , thereby obtaining another finite particular solution,

$$u = Q = \left(1 + \frac{q-1}{q(q-1)} x^q + \frac{(q-1)(3q-1)}{q(q-1)2q(2q-1)} x^{2q} + \&c. \right) e^{-\frac{1}{q} x^q}.$$

Reverting to the equation in z , we have next a particular solution of the form

$$z = Ax + Bx^{q+1} + Cx^{2q+1} + Dx^{3q+1} + \&c.,$$

giving between the coefficients the relation

$$\begin{aligned} (q+1)A + (q+1)q B &= 0, \\ (3q+1)B + (2q+1)2qC &= 0, \\ (5q+1)C + (3q+1)3qD &= 0, \\ (7q+1)D + (4q+1)4qE &= 0, \\ &\&c. \end{aligned}$$

If $A=1$, we have

$$\begin{aligned} A &= 1, \\ B &= -\frac{(q+1)}{(q+1)q}, \\ C &= +\frac{(q+1)(3q+1)}{(q+1)q(2q+1)2q}, \\ D &= -\frac{(q+1)(3q+1)(5q+1)}{(q+1)q(2q+1)2q(3q+1)3q}, \\ &\&c., \end{aligned}$$

where, as in the former case, the series is considered to terminate as soon as there is an evanescent factor in the numerator, without any regard to the subsequent coefficients which contain in the denominators the same evanescent factor.

Hence if we have $(2i+1)q = -1$, the series terminates, and we have for u the finite particular solution,

$$u = P = x \left(1 - \frac{q+1}{(q+1)q} x^q + \frac{(q+1)(3q+1)}{(q+1)q(2q+1)2q} x^{2q} - \&c. \right) e^{\frac{1}{q} x^q},$$

from which, changing the sign of x^q , we deduce the other finite particular solution,

$$u = Q = x \left(1 + \frac{q+1}{(q+1)q} x^q + \frac{(q+1)(3q+1)}{(q+1)q(2q+1)2q} x^{2q} + \&c. \right) e^{-\frac{1}{q} x^q}.$$

Hence, in the equation

$$\frac{dy}{dx} + y^2 = x^{2q-2},$$

where $q(2i+1) = \pm 1$, we have (writing $D=1$)

$$y = \frac{CP' + Q'}{CP + Q},$$

where C is the constant of integration, P, Q are finite series as above, and P', Q' are the derived functions of P and Q . Writing successively $i=0, i=1, i=2$, &c., we may tabulate the solutions

$$\frac{dy}{dx} + y^2 = 1, \quad P = e^x, \quad Q = e^{-x},$$

$$\frac{dy}{dx} + y^2 = x^{-4}, \quad P = xe^{-\frac{1}{x}}, \quad Q = xe^{\frac{1}{x}},$$

$$\frac{dy}{dx} + y^2 = x^{-\frac{1}{2}}, \quad P = (1 - 3x^{\frac{1}{2}})e^{3x^{\frac{1}{2}}}, \quad Q = (1 + 3x^{\frac{1}{2}})e^{-3x^{\frac{1}{2}}},$$

$$\frac{dy}{dx} + y^2 = x^{-\frac{3}{2}}, \quad P = x(1 + 3x^{-\frac{1}{2}})e^{-3x^{-\frac{1}{2}}}, \quad Q = x(1 - 3x^{-\frac{1}{2}})e^{3x^{-\frac{1}{2}}},$$

$$\frac{dy}{dx} + y^2 = x^{-\frac{5}{2}}, \quad P = \left(1 - 5x^{\frac{1}{2}} + \frac{25}{3}x^{\frac{3}{2}}\right)e^{5x^{\frac{1}{2}}}, \quad Q = \left(1 + 5x^{\frac{1}{2}} + \frac{25}{3}x^{\frac{3}{2}}\right)e^{-5x^{\frac{1}{2}}},$$

&c.

It is hardly necessary to make the final step of calculating P' and Q' and substituting in y ; but, as an example, take the above equation $\frac{dy}{dx} + y^2 = x^{-\frac{1}{2}}$: we have

$$y = \frac{-3x^{-\frac{1}{2}}(Ce^{3x^{\frac{1}{2}}} + e^{-3x^{\frac{1}{2}}})}{C(1 - 3x^{\frac{1}{2}})e^{3x^{\frac{1}{2}}} + (1 + 3x^{\frac{1}{2}})e^{-3x^{\frac{1}{2}}}},$$

which is readily identified with the solution, p. 98 of Boole's 'Differential Equations' (Cambridge, 1859).

Cambridge, September 29, 1868.

XLVI. *On the New Electromotive Force in the Voltaic Arc.*
By E. EDLUND*.

WHEN a luminous arc is formed between two charcoal points by means of a powerful voltaic battery, and the circuit is broken in any place on one side of the arc so that the current ceases, and if the circuit be completed after a brief interval, the luminous arc is not extinguished, though the current was interrupted for a short time. The time during which the current may be thus interrupted without extinguishing the arc depends upon the intensity of the current. With a strong current the time may be longer; with a weak one the time must be reduced if the arc is to continue. If the same experiment be made with a luminous arc between silver instead of carbon poles, the light is extinguished at the moment of interruption, and does not spontaneously reappear when the circuit has again been restored. This deportment of the light between carbon poles doubtless indicates that the conduction in the arc lasts a short time after the cessation of the current, by which it is possible that the current, when the break on the side of the luminous arc has been restored, can reproduce the arc. If the conduction had been quite destroyed during the short break, the incipient current would not be able to traverse the path between the carbon poles and renew the light. But the conduction in the luminous arc is principally produced by detached particles which are carried by the current from one pole to the other. It must hence be assumed that in the first moment after the break in the conduction the particles of carbon are being continually detached and carried from one pole to the other.

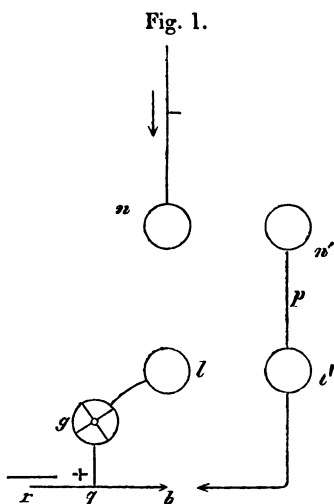
In a previous investigation I have proved that the mechanical work which the current consumes in order to disintegrate the poles gives rise to an electromotive force which sends a current in the opposite direction to the principal current†. If, now, this disintegration lasts for a short time after the cessation of the principal current, it may be possible to separate the current resulting from this electromotive force from the principal one, and measure it free from all action of the latter. With this design I have made the following experiments in common with Dr. Lemström, of Helsingfors.

2. To break the circuit on the side of the arc, a commutator

* Translated from a separate copy communicated by the Author, having been read before the Royal Academy of Sciences at Stockholm, January 1868.

† Phil. Mag. S. 4. vol. xxxv. p. 103.

of the following construction was used. In fig. 1, n , n' and l , l' represent four mercury-cups bored in a mahogany base, in which the various parts of the commutator were fastened. In the middle was a horizontal axis to which a lever was fastened, both whose ends were forked, and these forks were bent downwards at right angles to the longitudinal direction of the lever. When the one half of the lever was depressed, the cups n and n' were connected with each other, because the forked points at the end of the depressed lever were dipped in these cups. If the other half of the lever was lowered, the cups l and l' were



connected, and the connexion between n and n' ceased. By means of a copper wire, p , the cups n' and l' were connected. The negative pole of the battery was connected with the cup n , and a wire went from l' to b , where the arc was formed. The wire bqr led to the positive pole of the battery. From q a wire led to the magnetometer g ; and the other end of the magnetometer-wire was in connexion with the cup l . When n and n' were connected with each other by the forked lever, a luminous arc was formed at b . If the lever was reversed, the connexion between n and n' was broken and the current ceased, upon which the cups l and l' were connected. By this means, one end of the magnetometer-wire was connected with each side of the luminous arc b . Hence if, after the cessation of the principal current, there were any residual electromotive force, this would send a current through the magnetometer and produce a deflection. The magnetometer was the same as that I had used in previous researches. If a thermo-couple consisting of a German-silver and an iron wire was connected with the magnetometer, an increase of 1 degree in the temperature of the soldering produced a deflection of 54 divisions.

It is of course important that the connexion between l and l' ensue as rapidly as possible after the cessation of the connexion between n and n' . But, on the other hand, care must be taken that, in reversing the lever, n and n' are not connected with each other for a brief space as well as l and l' . If this is the case, part of the principal current passes during the simultaneous closing through the galvanometer-wire, and the magnetic needle gives in consequence a deflection which is due to the principal

current. When in reversing the lever the fork-points n and n' are raised, the surface of the mercury rises about the points, owing to adhesion; and therefore the contact between them lasts longer than if the surface of the mercury remained unchanged. If the current is passing, a formation of sparks ensues as soon as contact between the mercury and the fork-points ceases; and as long as the formation of sparks lasts, the current continues. This retardation in the cessation of the current, due to the formation of sparks, increases with the intensity of the current. The reversal of the lever was effected by means of a falling weight, in order that it might always take place with the same speed. The time which elapsed from the moment in which the forked points left the mercury in the cups n and n' to the moment in which the points at the other end of the lever touched the mercury in the cups l and l' , was calculated to amount to $\frac{1}{80}$ of a second. Yet, owing to the formation of sparks and the adhesion of the mercury, the time that elapsed between the cessation of the current and junction between l and l' was much shorter. That in this rapid reversal the junction between n and n' was not simultaneous with that between l and l' , was proved in the following manner. The pole-points at b were pushed together, the conducting-wire p taken away, and in its place a German-silver wire of considerable resistance was interposed between n' and l' . This resistance, however, was so arranged as to produce a smaller diminution in the intensity of the current than the luminous arc when this existed and n' was connected with l' by p . Thereupon the magnetometer-wire g g was detached from q and joined with n' . If, now, the closing had been simultaneous on reversing the lever, part of the principal current must have gone through the magnetometer and have produced a deflection; in the opposite case the needle must have remained at rest. In this experiment the intensity of the current was greater than in the following, and accordingly the formation of sparks more intense. Yet notwithstanding this, there was no simultaneous closing. In order to diminish in any possible way the formation of sparks, the mercury in the cups n and n' was covered with a thin layer of oil. That there was no simultaneous closing follows also from another experiment, which will be mentioned afterwards.

3. We pass now to the actual observations, which, however, are not adduced in the order in which they were made. As the resistance in the luminous arc is great and increases with its length (other things remaining the same), the deflection must have been smaller in proportion as the length of the arc increased. Hence it was important to have the same length of arc in the various experiments. For this purpose one carbon point was moveable, and might be pushed backwards and forwards by means of a

micrometer-screw. Notwithstanding this, the length of the arc was not the same in all the experiments, in consequence of which the deflections varied not inconsiderably. It was necessary, therefore, to make a greater number of experiments and take the mean. To give an idea of the magnitude of the variations, with the mean numbers adduced below are given both the probable errors and the number of observations.

Experiment 1. The battery consisted of fifty Bunsen's elements with carbon and nitric acid. The trough described in my former investigations was placed in the circuit, and, as before, was partially filled with solution of sulphate of copper. The distance between the copper disks was 5 inches. By this resistance the intensity of the current was materially diminished. The luminous arc was formed between carbon points. When the magnetometer-wires were connected in the above manner with the path of the circuit, on reversing the lever there was always a deflection in the galvanometer, which indicated a current between the points in the opposite direction to the principal current. As the mean of ten observations, this deflection amounted to 18.7 divisions, with a probable error of ± 1.24 .

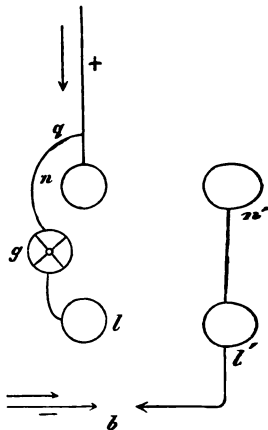
When the carbon points were replaced by similar ones of silver and the same experiment repeated, the magnetic needle remained completely at rest. When, therefore, the luminous arc was formed between silver poles, there was no subsequent action after the arc was extinguished. This deportment proves that the cause of the current which produces the deflection is to be sought in the luminous arc itself, because the deflection depends upon the quality of the pole-points. This proves at the same time that there was no simultaneous closing between n, n' and l, l' .

The mean deflection, = 18.7, adduced above gives of itself no idea of the magnitude of the electromotive force by which the current is produced. The deflection depends not merely on the delicacy of the instrument and the resistance between the poles, but also on the time during which the current acts, and on the electromotive force. Without information on this point, it cannot be determined beforehand whether the electromotive force is most nearly equal, say, to that of 0.1 of an element, or of from ten to fifteen such elements. In order to determine the force in question the following method was pursued.

Experiment 2. The connexion between the principal current, the magnetometer, and the commutator was altered, as represented in fig. 2. The mercury-cup n was joined with the wire from the positive pole of the battery, and the magnetometer-wire gq connected with the same wire. The conducting-wire from the negative pole of the battery was directly connected with one carbon point. When n and n' were connected by the

fork-shaped lever, the current formed a luminous arc at b ; when, on the contrary, the lever was reversed by the falling weight, contact between n and n' ceased, the luminous arc was extinguished, and the current passed through the galvanometer so long as the conduction lasted through the extinguishing luminous arc. The deflection obtained was produced by the electromotive force of the battery, diminished by the electromotive force in the extinguishing luminous arc. The battery consisted of the same fifty elements as in experiment 1, and with the same resistance in the solution of sulphate of copper. When this resistance was not introduced, the current was so strong that the luminous arc, as mentioned before, was not extinguished when the lever was reversed.

Fig. 2.



As the mean of ten observations, a deflection of 27.9 divisions was obtained, with a probable error of ± 1.43 . With a luminous arc between silver points no deflection was obtained, the needle was quite stationary. Hence between the silver poles there was no conduction when the luminous arc was extinguished.

From the deflections obtained (18.7 and 27.9) the electromotive force in the disappearing arc cannot be directly calculated, because the resistance is unequal in the two cases. In the first case it consists of that in the arc, together with that of the magnetometer. In the latter case there is in addition the resistance of the battery and the solution of sulphate of copper. Calling the first m and the latter m' , the electromotive force of the battery E , and the electromotive force in the extinguishing luminous arc x , we have

$$\frac{E-x}{m} : \frac{x}{m'} = 27.9 : 18.7.$$

In the following experiments a determination is obtained of the ratio between m' and m , by the aid of which the calculation can be executed.

Experiment 3. The battery consisted of twenty-six elements, and the sulphate solution was not interposed in the circuit. When the observations were made as in experiment 1, a deflection of 40 divisions was obtained as the mean of fifteen observations, with a probable error of ± 3.5 . When afterwards a German-silver wire 18.8 feet long was introduced, a deflection

of 34 divisions was obtained as the mean of fifteen observations, with a probable error of ± 3.7 . From this the resistance in the disappearing luminous arc may be calculated. If the resistance in the German-silver wire is ρ , and that in the luminous arc and the magnetometer together is called B , we have

$$B : B + \rho = 34 : 40,$$

from which $B = 5.67\rho$.

Experiment 4. With thirty elements, but without introducing the sulphate of copper into the circuit, fifteen observations were made as in experiment 2. The battery was increased from twenty-six to thirty elements, because the luminous arc was so feeble with twenty-six elements that it was often spontaneously extinguished, in consequence of which the experiments had to be frequently repeated. The mean of these fifteen observations was 64 divisions, with a probable error of ± 6.5 . The resistance in the battery was found on measurement to amount to 1.7ρ . The resistance in the luminous arc with the use of twenty-six elements was found to be 5.67ρ ; when thirty elements are used the resistance becomes somewhat smaller, for it diminishes as the intensity increases. The resistance in the entire circuit was somewhat smaller than $1.7\rho + 5.67\rho = 7.37\rho$. If the resistance had been equal to that in experiment 3, the deflection of the magnetometer would at most have been

$$\frac{64 \times 7.37}{5.67} = 83.2 \text{ divisions.}$$

Hence we have

$$83.2 : 40 = E - x : x,$$

from which $x = 9.7$ elements.

We see from this that the electromotive force in the extinguishing luminous arc between carbon points cannot be less than that of 9.7 Bunsen's elements.

The data furnished by this and by previous experiments may be used to calculate the electromotive force of the luminous arc from experiments 1 and 2.

If the resistance in 30 elements is 1.7ρ ,

It may be assumed that in 50 elements it is $\frac{5}{3} 1.7\rho$, or 2.83ρ

If the resistance in one inch of the solution of sulphate of copper be called r , a special investigation showed that $\rho = 2.06r$;

Hence in five inches of the solution of sulphate of }
copper the resistance was } 2.43ρ

Hence the total resistance in the battery and in the rheostat together is 5.26ρ . Assuming that the resistance in the luminous arc and in the magnetometer, as in experiment 3, amounts

to 5.67ρ , the entire resistance in the battery, in the rheostat, in the arc, and in the magnetometer together amounts to 10.93ρ .

If the resistance in experiment 2, as in experiment 1, had been equal to 5.67ρ , the deflection of the magnetic needle must have been

$$\frac{10.93}{5.97} \times 27.9 = 53.8 \text{ divisions.}$$

Hence we have

$$53.8 : 18.7 = 50 - x : x,$$

from which $x = 12.9$ elements.

Experiment 5. The battery consisted of fifty elements; and five inches of the solution of sulphate of copper were introduced into the circuit. The resistance of the battery was found to be 3.97ρ , which, together with the resistance in the circuit, makes 6.4ρ . When the galvanometer circuit surrounded the luminous arc, the deflection on reversing the lever was 11.7 divisions as the mean of ten observations, with a probable error of ± 0.85 . When thereupon a resistance $= 12.9\rho$ was introduced into the circuit, the deflection was equal to 6.5 divisions as the mean of ten observations, with a probable error of ± 0.37 . From this it may be calculated that the resistance in the luminous arc and the magnetometer together was equal to 16.1ρ . The great increase in the resistance of the luminous arc was probably due to a smaller intensity of the current.

Experiment 6. The battery and the interposed resistance were the same as in experiment 5. The magnetometer-wire was connected with the commutator and the principal circuit as represented in fig. 2. When the lever was reversed, a deflection was always obtained amounting to 19.2 divisions as the mean of fifteen observations, with a probable error of ± 1.35 . The resistance in this case was $= 22.5\rho$ ($= 6.4\rho + 16.1\rho$). If the resistance had been equal to 16.1ρ , as in experiment 5, the deflection would have been $= 26.83$ divisions. We have thus

$$26.83 : 11.7 = 50 - x : x,$$

from which $x = 15.2$ elements.

Hence the mean of the three determinations adduced of the electromotive force of the vanishing luminous arc amounts to 12.6 elements. The probable errors adduced above show that this determination can lay claim to no great accuracy. It is possible that the true value may be a few units above or below the one adduced; but this is immaterial for our immediate object. The experiments adduced prove that, in the voltaic arc between carbon points, during the first moment after extinction there is an electromotive force comparable in magnitude to that of from ten to

fifteen Bunsen's elements. It is moreover to be remarked that the numbers obtained are the means of the electromotive force. This is doubtless strongest during the first moment after the extinction of the luminous arc, and then diminishes in intensity till it quite ceases. If the lever could be more quickly reversed than has been heretofore possible, and thereby let the closing of the magnetometer circuit follow more rapidly the opening of the principal circuit and the extinction of the luminous arc, a higher value would be obtained for the electromotive force of the luminous arc.

4. The source of electricity in the disappearing luminous arc cannot be of the nature of an electromotive force due to contact. There are indeed two contacts between carbon and gas; but these act in opposite directions and neutralize each other. It is possible that when the experiments are made in air, the moisture present is decomposed and produces a polarization of the carbon points; but even if this were the case, it would only explain an insignificant part of the electromotive force found. Just as little can the force be an induction-phenomenon; for the conditions for the production of such a one do not exist in the luminous arc. It also seems to have nothing in common with the electromotive force discovered by Quincke to accompany the passage of liquids through porous diaphragms. The only supposition which might possibly be made is that the force is of thermoelectric origin. The positive pole is considerably more heated than the negative; and we have thus in the luminous arc two contacts between carbon and gas, of which one has a considerably higher temperature than the other. But it appears improbable that this thermoelectric force could have obtained so considerable a value as that we have observed*. In order to ascertain with certainty the real state of the case, the following experiments were made.

If the force in question is of thermoelectric origin, and caused by the circumstance that the positive pole has a higher temperature than the negative one, the current observed must disappear when the temperature of the negative pole is raised to the same

* Mr. Wilde, in *Pogg. Ann.* vol. cxi., has communicated a paper in which it is demonstrated that a delicate magnetometer gives a considerable deflection if it is connected with a battery of twenty Bunsen's elements as in fig. 1. Mr. Wilde does not mention any precautions which he used to convince himself that in his experiments there was no simultaneous closing on opening the principal current and introducing the magnetometer circuit. If there is a simultaneous closing, deflections of the galvanometer are always obtained; but these are then caused by the principal current itself. Mr. Wilde has made no experiments with the view of measuring the strength of the current or the intensity of its electromotive force. Hence Mr. Wilde did not hesitate to assume that the deflections obtained were caused by a thermoelectric current which resulted from the contact of the pole points (heated to unequal temperatures) with the interjacent gas.

height as that of the positive. If, also, the temperature of the negative pole be only so much raised that the difference between the poles is decreased, the thermoelectric current must be diminished; or it might even be possible that it would sink to zero, and then acquire an opposite direction, before the temperature of the negative pole attained that of the positive. If the statement that the current decreases when the temperature of the negative pole is increased is perfectly correct under all circumstances, the arc must of course retain its conductivity unchanged. Yet this depends principally, if not exclusively, on the quantity and condition of the transported particles, and not on the temperature of the gas. On the contrary, it is by no means certain *à priori* that the current will increase if the temperature of the positive pole be raised, and thereby the difference between the temperature of the poles become greater, because it is possible that the greatest electromotive force corresponded to a certain temperature of the positive pole, and that the force would become less at a higher temperature. It is also important to be able to show that increase in temperature in the negative pole produces no diminution in the intensity of the current.

To effect this, a Bunsen's burner provided with a bellows was used. When the flame was directed against carbon of the same quality as that used in the experiments, the carbon began to ignite, and a thin wire coiled round it melted. The temperature, at any rate on the side of the carbon turned towards the flame, was 1000° or more. Upon this the following experiments were made, partly without heating by the burner, partly with the application of heat to the positive or negative pole as near as possible to the arc.

Experiment 7. The battery consisted of fifty elements; but the current, probably owing to imperfect conduction, was not particularly strong. The magnetometer was connected with the principal circuit as shown in fig. 1; that is, the electromotive force in the luminous arc caused the deflection.

The mean of ten observations, when there was no heating, and when the negative pole was heated to strong ignition, gave :—

Without heating	30.8 divisions.
The probable error	± 2.5 .
Heating the negative pole . .	40.8 divisions.
The probable error	± 3.1 .

On heating the negative pole, therefore, and diminishing thus the difference in temperature between the poles, the current was stronger—quite opposite to what should have been the case if the current had been of thermoelectric origin. In the subsequent

experiments the positive and negative poles were alternately heated.

Experiment 8. With a somewhat stronger current than in the previous experiments, there was obtained as the mean of ten observations, on

Heating the positive pole 40 divisions.
Probable error ± 3.1 .

Heating the negative pole 54 divisions.
Probable error ± 3.8 .

The current was also greater in this case when the difference in temperature was diminished.

Experiment 9. The battery consisted of thirty elements without any introduced external resistance. There was obtained as the mean of fifteen observations,

Without heating 46 divisions.
Probable error ± 2.8

Heating the positive pole 36.5 divisions.
Probable error ± 3.1 .

Heating the negative pole 45.3 divisions.
Probable error ± 2.2 .

Experiment 10. The battery consisted of fifty elements, with no other resistance than that afforded by the thick conducting-wires. The current was strong and the deflections great; but the difficulty of obtaining constant deflections increased in the same proportion. There was obtained as the mean of fifteen observations,

Heating the positive pole 73 divisions.
Probable error ± 7.7 .

Heating the negative pole 73 divisions.
Probable error ± 7.1

Although, as the probable errors adduced indicate, the sources of error are great, the preceding experiments justify the conclusion that the current in question does not decrease when the difference in temperature between the poles is less, owing to the heating of the negative pole, which it ought to do if the current were of thermoelectric origin. To this may be added that this diminution of the difference of temperature is very considerable, although it certainly does not entirely disappear.

When the positive pole is heated, the deflection seems to be somewhat less—though this cannot be considered to be made

out, for the experiments which speak in its favour are not more numerous or more accurate.

Moreover the result which these heatings of the poles give is nothing more than a confirmation of the conclusions which I have drawn from my earlier investigation on the voltaic arc. No one will doubt that the electromotive force which, from the experiments adduced, is met with in the just-extinguished arc is the same as that which, from my former investigations, occurs in the luminous arc during its existence. But it has been shown* in regard to this force, that it is independent of the intensity of the current. It is only when the intensity approaches the minimum with which a luminous arc can be produced, that a decrease in the magnitude of the force begins to be seen†. But the temperature of the pole-points is dependent on the intensity of the current, and the same is also obviously the case with their difference in temperature. The force found is thus independent of the heating of the poles, and therefore cannot be of thermoelectric origin.

XLVII. *On Geological Time, and the probable Date of the Glacial and the Upper Miocene Period.* By JAMES CROLL, of the *Geological Survey of Scotland.*

[Continued from p. 154.]

AS has already been remarked, if the glacial epoch resulted from a high state of excentricity there must, according to the foregoing Tables, have been at least five ice-periods during the last three millions of years, viz. the glacial epoch, two as severe, and two much less severe. The question which now remains to be discussed is whether this conclusion is consistent with the facts of geology or not. Can it be shown from geological evidence that there have not within the past three millions of years been two ice-periods as severe as the glacial period? In other words, do the testimony of geology and the testimony of physics agree in regard to this point?

From what has already been shown respecting the limit to geological time, I presume we may reasonably conclude that three millions of years may probably embrace the greater portion of the Tertiary period. Now, as has been stated, we have good evidence of at least three ice-periods since the beginning of the Tertiary period—one about the middle of the Eocene period, another during the Upper Miocene period, and the third and last the well-known glacial epoch.

* Phil. Mag. S. 4. vol. xxxv. p. 103.

† *Öfversigt af K. Vet. Akad. Förhandl.* December 1867.

Let it be observed that it is not essential to the object in view to be able to show from the facts of geology that former cold periods, such as those of the Eocene and the Miocene, were as severe as that of the glacial epoch. All that is required is simply to show that, for anything that geology can prove to the contrary, those cold periods *may* have been as severe as the glacial epoch. What I wish to show is, that although the conclusions derived from astronomical and physical considerations regarding the severity of former ice-periods may not at present be fully borne out by geological science, the imperfection of geological records upon this point is such that the absence of direct geological evidence cannot reasonably be regarded as sufficient proof that the physical conclusions arrived at are improbable. But this is not all; I shall endeavour to show that the records of geology in regard to former glacial epochs are not only imperfect, but that this imperfection *follows as a necessary consequence from the principles of geology itself*—that there is not simply a want of records, but a *reason* in the very nature of geological evidence why there is such a want.

Geological evidence in reference to past glacial epochs is evidently much more imperfect than many suppose.

It is on a land-surface that the principal traces of the action of ice during a glacial epoch are left; for it is there that the stones are chiefly striated, the rocks ground down, and the boulder-clay formed. But where are all our ancient land-surfaces? They are not to be found. The total thickness of the stratified rocks of Great Britain, according to Professor Ramsay, is nearly fourteen miles. But from the top to the bottom of this enormous deposit there is hardly a single land-surface to be found. True, there are patches of old land-surfaces of a local character, such, for example, as the dirt-beds of Portland; but, with the exception of the coal-beds, every general formation from top to bottom was formed under water, and none but the under-clays *ever existed as a land-surface*. And it is here in such a formation that the geologist has to collect all his information regarding the existence of former glacial epochs. The entire stratified rocks of the globe, with the exception of the coal-beds and under-clays (places where no one would expect to find traces of ice-action), consist almost entirely of a *series of old sea-bottoms*, with here and there an occasional freshwater deposit. Bearing this in mind, what is the sort of evidence which we can now hope to find in these old sea-bottoms of the existence of former ice-periods?

Every one, of course, who has ever reflected on the matter admits that the stratified rocks are not old land-surfaces, but a series of old sea-bottoms formed out of the destruction of old

land-surfaces. And it is true that all land-surfaces once existed as sea-bottoms ; but the stratified rocks consist of a series of old sea-bottoms which never were land-surfaces. Many of them no doubt have been repeatedly above the sea-level, and may once have possessed land-surfaces ; but these, with the exception of the under-clays of the coal-measures, the dirt-beds of Portland, and one or two more patches, have all been annihilated. The important bearing that this consideration has on the nature of the evidence which we can now expect to find of the existence of former glacial epochs has certainly been very much overlooked.

When we examine the matter fully, we find that the *transformation of a land-surface into a sea-bottom* will probably completely obliterate every trace of glaciation that may have existed on that land-surface. For example, we cannot expect to find the polished and striated stones belonging to a former land-glaciation ; for stones are not carried down by our rivers and deposited in the sea. They are first disintegrated by subaërial agency into sand or clay, as the case may be, and then carried down by our rivers and deposited as such on the sea-bottom. But supposing striated stones were carried down by our rivers out of the boulder-clay, they could not retain their ice-markings, for they would soon become waterworn in their passage seawards.

Neither can we expect to find boulder-clay in the stratified rocks, for boulder-clay is not carried down as such and deposited in the sea. The boulder-clay is washed off the land and carried down as soft mud, clay, sand, and gravel. Patches of boulder-clay may have been now and again forced into the sea by means of the ice and become covered up ; but such cases are wholly exceptional, and the absence of examples of this sort in any formation cannot fairly be adduced as a proof that that formation does not belong to a glacial period.

The only evidence which we can now reasonably expect to find in the stratified rocks of the existence of land-ice of former epochs, is the presence of erratic blocks which may have been transported by icebergs and dropped into the sea. But unless the glaciers of that epoch reached the sea or the sea was frozen, we could not possibly have even this evidence. Traces in the stratified rocks of the effects of land-ice of former epochs must, from the very nature of things, be rare indeed. The only sort of evidence which, as a general rule, we may always expect to find is the presence of large blocks of older rocks found imbedded in strata which, we know from their constitution, must have been formed in still water. But this is quite enough ; for it proves the existence of ice at the time that the deposit containing the blocks was being formed as conclusively as though we saw the ice floating with the blocks

upon it. This sort of evidence, when found in low latitudes, ought to be received as conclusive of the existence of former glacial epochs, and, no doubt, would have been so received had it not been for the idea that, if these blocks had been transported by ice, there ought in addition to have been found striated stones, boulder-clay, and other indications of the agency of land-ice.

The reason why we now have, comparatively speaking, so little direct evidence of the existence of former glacial periods will be more forcibly impressed upon the mind, if we reflect on how difficult it would be in a million or so of years hence to find any trace of what we now call the glacial epoch. The striated stones would by that time be all, or nearly all disintegrated, and the till washed away and deposited in the bottom of the sea as stratified sands and clays. And when these became consolidated into rock and were raised into dry land, the only evidence that we should probably then have that there ever had been a glacial epoch would be the presence of large blocks of the older rocks, which would be found imbedded in the upraised formation. We could only infer that there had been ice at work from the fact that by no other known agency could we conceive such blocks to have been transported and dropped in a still sea.

Probably few geologists believe that during the Middle Eocene and the Upper Miocene periods our country passed through a condition of glaciation as severe as it has done during the Post-pliocene period; yet when we examine the subject carefully, we find that there is actually no just ground to conclude that it has not. For in all probability, in the strata formed out of the destruction of the now existing land-surfaces, evidence of ice-action will be as scarce as in the Eocene or Miocene strata.

If the stratified rocks forming the earth's crust consisted of a series of old land-surfaces instead (as they actually do) of a series of old sea-bottoms, then probably dozens of glacial periods might be detected.

Nearly all the evidence which we have regarding the glacial epoch has been derived from what we find on the now existing land-surfaces of the globe. But probably not a trace of this will be found in the stratified beds of future ages, formed out of the destruction of the present land-surfaces. Even the very arctic shell-beds themselves, which have afforded to the geologist such clear proofs of a frozen sea during the glacial epoch, will not be found in those stratified rocks; for they must suffer destruction along with everything else which now exists above the sea-level. There is probably not a single relic of the glacial epoch which has ever been seen by the eye of man that will be found in the stratified rocks of future ages. Nothing but what

lies buried in the deep recesses of the ocean will escape complete disintegration and appear imbedded in those formations. It is only those objects which lie in our existing sea-bottoms that will remain as monuments of the glacial epoch of the Posttertiary period. And, besides, it will only be those portions of the sea-bottoms that may happen to be upraised into dry land that will be available to the geologist of future ages. The point to be determined now is this:—*Is it probable that the geologist of the future will find in the rocks formed out of the now existing sea-bottoms more evidence of a glacial epoch during Posttertiary times than we now do of one during the Miocene, the Eocene, or the Permian period?* Unless this can be proved to be the case, we have no ground whatever to conclude that the cold periods of the Miocene, Eocene, and Permian periods were not as severe as that of the glacial epoch. This is evident; for the only relics which now remain of the glacial epochs of those periods are simply what happened to be protected in the then existing sea-bottoms. Every vestige that lay on the land would in all probability be destroyed by subaërial agency and carried into the sea in a sedimentary form. But before we can determine whether or not there is more evidence of the glacial epoch in our now existing sea-bottoms than there is of former glacial epochs in the stratified rocks (which are in reality the sea-bottoms belonging to ancient epochs), we must first ascertain what is the nature of those marks of glaciation which are to be found in a sea-bottom.

We know that the rocky face of the country was ground down and striated during the glacial epoch; and this is now generally believed to have been done by land-ice. But we have no direct evidence that the floor of the ocean, beyond where it may have been covered with land-ice, was striated. Beyond the limits of the land-ice it could be striated only by means of icebergs. But do icebergs striate the rocky bed of the ocean? Are they adapted for such work? It seems to be almost universally assumed that they are. But I have been totally unable to find any rational grounds for such a belief. Clean ice can have but little or no erosive power, and never could scratch a rock. To do this it must have grinding materials in the form of sand, mud, or stones. But the bottoms of icebergs are devoid of all such materials. Icebergs carry the grinding materials on their backs, not on their bottoms. No doubt, when the iceberg is launched into the deep, great masses of sand, mud, and stones will be adhering to its bottom. But no sooner is the berg immersed, than a melting process commences at its sides and lower surface in contact with the water; and the consequence is, the materials adhering to the lower surface soon drop off and sink to the bottom of the sea. The iceberg, divested of these materials, can now do very

little harm to the rocky sea-bottom over which it floats. It is true that an iceberg moving with a velocity of a few miles an hour, if it came in contact with the sea-bottom, would, by the mere force of concussion, tear up loose and disjointed rocks, and hurl some of the loose materials to a distance; but it would do but little in the way of grinding down the rock against which it struck. But even supposing the bottom of the iceberg were properly shoed with the necessary grinding materials, still it would be but a very inefficient grinding agent; for a *floating* iceberg would not be in contact with the sea-bottom. And if it were in contact with the sea-bottom, it would soon become stranded and, of course, motionless, and under such conditions could produce no effect.

It is perfectly true that although the bottom of the berg may be devoid of grinding materials, yet these may be found lying on the surface of the submarine rock over which the ice moves. But it must be borne in mind that the same current which will move the icebergs over the surface of the rock will move the sand, mud, and other materials over it also; so that the markings effected by the ice will in all probability be erased by the current. In the deep recesses of the ocean the water has been found to have but little or no motion. But icebergs always follow the path of currents; and it is very evident that at the comparatively small depth of a thousand feet or so reached by icebergs the motion of the water will be considerable; and the continual shifting of the small particles of the mud and sand will in all probability efface the markings which may be made now and again by a passing berg.

Much has been said regarding the superiority of icebergs as grinding and striating agents in consequence of the great velocity of their motion in comparison with that of land-ice. But it must be remembered that it is while the iceberg is floating, and before it touches the rock, that it possesses high velocity. When the iceberg runs aground, its motion is suddenly arrested or greatly reduced. But if the iceberg advancing upon a sloping sea-bottom is raised up so as to exert great pressure, it will on this account be the more suddenly arrested, the motion will be slow, and the distance passed over short, before the berg becomes stranded. If it exerts but little pressure on the sea-bottom, it may retain a considerable amount of motion and advance to a considerable distance before it is brought to a stand; but, exerting little pressure, it can perform but little work. Land-ice moves slowly, but then it exerts enormous pressure. A glacier 1000 feet in thickness has a pressure on its rocky bed equal to about 25 tons on the square foot; but an iceberg a mile in thickness, forced up on a sloping sea-bottom to an elevation of

20 feet (and this is perhaps more than any ocean-current could effect), would only exert a pressure of about half a ton on the square foot, or about $\frac{1}{50}$ part the pressure of the glacier 1000 feet in thickness. A great deal has been said about the erosive and crushing-power of icebergs of enormous thickness, as if their thickness gave them any additional pressure. An iceberg 100 feet in thickness will exert just as much pressure as one a mile in thickness. The pressure of an iceberg is not like that of a glacier, in proportion to its thickness, but to the height to which it is raised out of the water. An iceberg 100 feet in thickness raised 10 feet will exert exactly the same pressure as one a mile in thickness raised to an equal height.

To be an efficient grinding agent, steadiness of motion, as well as pressure, is essential. A rolling or rocking motion is ill-adapted for grinding down and striating a rock. A steady rubbing motion under pressure is the thing required. But an iceberg is not only deficient in pressure, but also deficient in steadiness of motion. When an iceberg moving with considerable velocity comes on an elevated portion of the sea-bottom, it does not move steadily onwards over the rock, unless the pressure of the berg on the rock be trifling. The resistance being entirely at the bottom of the iceberg, its momentum, combined with the pressure of the current, applied wholly above the point of resistance tends to make the berg bend forward, and in some cases upset (when it is of a cubical form). The momentum of the moving berg, instead of being applied in forcing it over the rock against which it comes in contact, is probably all consumed in work against gravitation in raising the berg upon its front edge. After the momentum is consumed, unless the berg be completely upset, it will fall back under the force of gravitation to its original position. But the momentum which it acquires from gravitation in falling backwards carries it beyond its position of repose in an opposite direction. It will thus continue to rock backwards and forwards until the friction of the water brings it to rest. The momentum of the berg, instead of being applied to the work of grinding and striating the sea-bottom, will chiefly be consumed in heat in the agitation of the water. But if the berg does advance, it will do so with a rocking unsteady motion, which, as Mr. Couthouy* and Professor Dana† observe, will tend rather to obliterate striations than produce them.

A floating berg moves with great steadiness; but a berg that has run aground cannot advance with a steady motion. If the rock over which the berg moves offers little resistance, it may

* Report on Icebergs, read before the Association of American Geologists. Silliman's Journal, vol. xliii. p. 163 (1842).

† Manual of Geology, p. 677.

do so ; but in such a case the berg could produce but little effect on the rock.

Dr. Sutherland, who has had good opportunities to witness the effects of icebergs, makes some most judicious remarks on the subject. "It will be well," he says, "to bear in mind that when an iceberg *touches the ground, if that ground be hard and resisting, it must come to a stand*, and, the propelling power continuing, a slight leaning over in the water, or yielding motion of the whole mass, may compensate readily for being so suddenly arrested. If, however, the ground be soft, so as not to arrest the motion of the iceberg at once, a moraine will be the result ; but the moraine thus raised will tend to bring it to a stand"*.

There is another cause referred to by Professor Dana, which, to a great extent, must prevent the iceberg from having an opportunity of striating the sea-bottom, even though it were otherwise well adapted for so doing. It is this: the bed of the ocean in the track of icebergs must be pretty much covered with stones and rubbish dropped from the melting bergs. And this mass of rubbish will tend to protect the rock†.

If icebergs cannot be shown *à priori*, from mechanical considerations, to be well adapted for striating the sea-bottom, one would naturally expect, from the confident way in which it is asserted that they are so adapted, that the fact has been at least established by actual observation. But, strange as it may appear, we seem to have little or no proof that icebergs actually striate the bed of the ocean. This can be proved from the direct testimony of the advocates of the iceberg theory themselves.

We shall take the testimony of Mr. Campbell, the author of two well-known works in defence of the iceberg theory, viz. 'Frost and Fire' and 'A Short American Tramp.' Mr. Campbell went in the fall of the year 1864 to the coast of Labrador, the Straits of Belle Isle, and the Gulf of St. Lawrence, for the express purpose of witnessing the effects of icebergs, and testing the theory which he had formed, that the ice-markings of the glacial epoch were caused by floating ice and not by land-ice, as is now generally believed.

The following is the result of his observations on the coast of Labrador.

Hanly Harbour, Strait of Belle Isle :—"The water is 37° F. in July. . . . As fast as one island of ice grounds and bursts, another takes its place ; and in winter the whole strait is blocked up by a mass which swings bodily up and down, grating along the bottom at all depths. . . . Examined the beaches and rocks at the water-line, especially in sounds. Found the rocks ground

* Quart. Journ. Geol. Soc. vol. ix. p. 306.

† Dana's 'Manual of Geology,' p. 677.

smooth, *but not striated*, in the sounds."—Short American Tramp, pp. 68, 107.

Cape Charles and Battle Harbour:—"But though these harbours are all frozen every winter, the *rocks at the water-line are not striated*" (p. 68).

At St. Francis Harbour:—"The water-line is much rubbed, smooth, *but not striated*" (p. 72).

Cape Bluff:—"Watched the rocks with a telescope, and *failed to make out striæ anywhere*; but the water-line is everywhere rubbed smooth" (p. 75).

Seal Islands:—"No *striæ are to be seen at the land-wash in these sounds or on open sea-coasts near the present water-line*" (p. 76).

He only mentions having here found striations in the three following places along the entire coast of Labrador visited by him; and in regard to two of these, it seems very doubtful that the markings were made by modern icebergs.

Murray's Harbour:—"This harbour was blocked up with ice on the 20th of July. The water-line is rubbed, and in *some places striated*" (p. 69).

Pack Island:—"The water-line in a narrow sound was polished and striated in the direction of the sound, about N.N.W. This seems to be fresh work done by heavy ice drifting from Sandwich Bay; *but, on the other hand, stages with their legs in the sea, and resting on these very rocks, are not swept away by the ice*" (p. 96). If these markings were modern, why did not the "heavy ice" remove the small fir poles supporting the fishing-stages?

Red Bay:—"Landed half-dressed, and found some *striæ* perfectly fresh at the water-level, but weathered out a short distance inland" (p. 107). The striations "inland" could not have been made by modern icebergs; and it does not follow that because the markings at the water-level were not weathered they were produced by modern ice.

These are the evidences which he found that icebergs striate rocks, on a coast of which he says that, during the year he visited it, "the winter-drift was one vast solid raft of floes and bergs more than 150 miles wide and perhaps 3000 feet thick at spots, driven by a whole current bodily over one definite course, year after year, since this land was found" (p. 85).

But Mr. Campbell himself freely admits that the floating ice which comes aground along the shores does not produce *striæ*. "It is sufficiently evident," he says, "*that glacial striæ are not produced by thin bay-ice*" (p. 76). And in 'Frost and Fire,' vol. ii. p. 237, he states that, "from a careful examination of the water-line at many spots, it appears that bay-ice grinds rocks, *but does not produce striation.*"

"It is impossible," he continues, "to get at rocks over which heavy icebergs now move; but a mass 150 miles wide, perhaps 3000 feet thick in some parts, and moving at the rate of a mile an hour or more, *appears to be an engine amply sufficient* to account for striæ on rising rocks." And in 'American Tramp,' p. 76, he says, "*striæ must be made* in deep water by the large masses which seem to pursue the even tenor of their way in the steady current which flows down the coast."

Mr. Campbell, from a careful examination of the sea-bottom along the coast, finds that the small icebergs do not produce striæ, but the large ones, which move over rocks impossible to be got at, "must" produce them. They "appear" to be amply sufficient to do so. If the smaller bergs cannot striate the sea-bottom, why must the larger ones do so? There is no reason why the smaller bergs should not move as swiftly and exert as much pressure on the sea-bottom as the larger ones. And even supposing that they did not, one would expect that the light bergs would effect on a smaller scale what the heavy ones would do on a larger.

I have no doubt that when Mr. Campbell visited Labrador he expected to find the sea-coast under the water-line striated by means of icebergs, and was probably not a little surprised to find that it actually was not. And I have no doubt that were the sea-bottom in the tracks of the large icebergs elevated into view, he would find to his surprise that it was free from striations also.

So far as observation is concerned, we have no grounds from what Mr. Campbell witnessed to conclude that icebergs striate the sea-bottom.

The testimony of Dr. Sutherland, who has had opportunities of seeing the effects of icebergs in Arctic regions, leads us to the same conclusion. "Except," he says, "from the evidence afforded by plants and animals at the bottom, we have *no means whatever* to ascertain the effect produced by icebergs upon the rocks*. In the Malegat and Waigat I have seen whole clusters of these floating islands, drawing from 100 to 250 fathoms, moving to and fro with every return and recession of the tides. I looked very earnestly for grooves and scratches left by icebergs and glaciers in the rocks, but always failed to discover any"†.

We shall now see whether river-ice actually produces striations or not. If floating ice under any form can striate rocks, one would expect that it ought to be done by river-ice, seeing that such ice is obliged to follow one narrow definite track.

St. John's River, New Brunswick:—"This river," says Mr. Campbell, "is obstructed by ice during five months of the year.

* Quart. Journ. Geol. Soc. vol. ix. p. 306.

† Journal, vol. i. p. 38.

When the ice goes, there is wild work on the bank. Arrived at St. John, drove to the suspension-bridge. . . . At this spot, if *anywhere in the world*, river-ice ought to produce striation. The whole drainage of a wide basin, and one of the strongest tides in the world, here work continually in one rock-groove; and in winter this water-power is armed with heavy ice. *There are no striæ about the water-line* ”*.

River St. Lawrence :—“ In winter the power of ice-floats driven by water-power is tremendous. The river freezes and packs ice till the flow of water is obstructed. The rock-pass at Quebec is like the Narrows at St. John’s, Newfoundland, in the frontispiece. The whole pass, about a mile wide, was paved with great broken slabs and round boulders of worn ice as big as small stacks, piled and tossed, and heaped and scattered upon the level water below and frozen solid. . . . This kind of ice does NOT *produce striation* at the water-margin at Quebec. At Montreal, when the river ‘goes,’ the ice goes with it with a vengeance. . . . The *piers are not yet striated* by river-ice at Montreal. . . . The rocks at the high-water level have *no trace* of glacial striæ. . . . The rock at Ottawa is rubbed by river-ice every spring, and *always in one direction*, but *it is not striated*. . . . The surfaces are all rubbed smooth, and the edges of broken beds are rounded where exposed to the ice; *but there are no striæ* ”†.

When Sir Charles Lyell visited the St. Lawrence in 1842, at Quebec he went along with Colonel Codrington “and searched carefully below the city in the channel of the St. Lawrence, at low water, near the shore, for the signs of glacial action at the precise point where the chief pressure and friction of packed ice are exerted every year,” but found none.

“At the bridge above the Falls of Montmorenci, over which a large quantity of ice passes every year, the gneiss is polished, and kept perfectly free from lichens, but not more so than rocks similarly situated at waterfalls in Scotland. In none of these places were any long straight grooves observable ”‡.

The only thing in the shape of modern ice-markings which he seems to have met with in North America was a few straight furrows $\frac{1}{2}$ an inch broad in soft sandstone, at the base of a cliff at Cape Blomidon in the Bay of Fundy, at a place where during the preceding winter “packed” ice 15 feet thick had been pushed along when the tide rose over the sandstone ledges §.

The very fact that a geologist so eminent as Sir Charles Lyell, after having twice visited North America, and searched specially for modern ice-markings, was able to find only two or

* Short American Tramp, pp. 168, 174. † Ibid. pp. 239–241.

‡ Travels in North America, vol. ii, p. 137.

§ Ibid. vol. ii. p. 174.

three scratches, upon a soft sandstone rock, which he could reasonably attribute to floating ice, ought to have aroused the suspicion of the advocates of the iceberg theory that they had really formed too extravagant notions regarding the potency of floating ice as a striating agent.

There is no reason to believe that the grooves and markings noticed by M. Weibye and others on the Scandinavian coast and other parts of northern Europe were made by icebergs. Mr. Geikie has clearly shown, from the character and direction of the markings, that they are the production of land-ice*. If the floating ice of the St. Lawrence and the icebergs of Labrador are unable to striate and groove the rocks, it is not likely that those of northern Europe will be able to do so.

It will not do for the advocates of the iceberg theory to assume, as they have hitherto done, that, as a matter of course, the sea-bottom is being striated and grooved by means of icebergs. They must prove that. They must either show that, as a matter of fact, icebergs are actually efficient agents in striating the sea-bottom, or prove from mechanical principles that they must be so. The question must be settled either by observation or by reason; mere opinion will not do.

The transporting of boulders and rubbish, and not the grinding and striating of rocks, is evidently the proper function of the iceberg. But even in this respect I fear too much has been attributed to it.

In reading the details of voyages in the Arctic regions one cannot help feeling surprised how seldom reference is made to stones and rubbish being seen on icebergs. Arctic voyagers, like other people when they are alluding to the geological effects of icebergs, speak of enormous quantities of stones being transported by them; but in reading the details of their voyages, the impression conveyed is that icebergs with stones and blocks of rock upon them are the exceptions. The greater portion of the narratives of voyages in Arctic regions consists of interesting and detailed accounts of the voyagers' adventures among the ice. The general appearance of the icebergs, their shape, their size, their height, their colour are all noticed; but rarely is mention made of stones being seen. That the greater number of icebergs have no stones or rubbish on them is borne out by the positive evidence of geologists who have had opportunities of seeing icebergs.

Mr. Campbell says:—"It is remarkable that up to this time we have only seen a few doubtful stones on bergs which we have passed. . . . Though no bergs with stones *on them or in them*

* Proceedings of the Royal Society of Edinburgh, Session 1865-66, page 537.

have been approached during this voyage, many on board the 'Ariel' have been close to bergs heavily laden. A man who has had some experience of ice has *never seen a stone on a berg* in these latitudes. Captain Anderson of the 'Europa,' who is a geologist, has *never seen a stone on a berg* in crossing the Atlantic. *No stones were clearly seen on this trip*"*. Captain Sir James Anderson (who has long been familiar with geology, has spent a considerable part of his life on the Atlantic, and has been accustomed to view the iceberg as a geologist as well as a seaman) has never seen a stone on an iceberg in the Atlantic. This is rather a significant fact.

Sir Charles Lyell states that, when passing icebergs on the Atlantic, he "was most anxious to ascertain whether there was any mud, stones, or fragments of rocks on any one of these floating masses; but after examining about forty of them without perceiving any signs of frozen matter, I left the deck when it was growing dusk"†. After he had gone below, one was said to be seen with something like stones upon it. The Captain and officers of the ship assured him that they had *never seen a stone upon a berg*.

There is still another point connected with icebergs to which we must allude, viz. the opinion that great masses of the boulder-clay of the glacial epoch was formed from the droppings of icebergs. It is perfectly obvious that *unstratified* boulder-clay could not have been formed in this way. Stones, gravel, sand, clay, and mud, the ingredients of boulder-clay, tumbled all together from the back of an iceberg, could not sink to the bottom of the sea without separating. The stones would reach the bottom first, then the gravel, then the sand, then the clay, and last of all the mud, and the whole would settle down in a stratified form. But, besides, how could the *clay* be derived from icebergs? Icebergs derive their materials from the land before they are launched into the deep, and while they are in the form of land-ice. The materials which are found on the backs of icebergs are what fell upon the ice from mountain tops and crags projecting above the ice. Icebergs are chiefly derived from continental ice, such as that of Greenland, where the whole country is buried under one continuous mass, with only a lofty mountain peak here and there rising above the surface. And this is no doubt the chief reason why so few icebergs have stones upon their backs. The continental ice of Greenland is not, like the glaciers of the Alps, covered with loose stones. It is perfectly plain that clay does not fall upon the ice. What falls upon the ice is stones, blocks of rocks, and the loose débris. Clay and

* Short American Tramp, pp. 77, 81, 111.

† Second visit, vol. ii. p. 367.

mud we know, from the accounts given by arctic voyagers, are sometimes washed down upon the coast-ice; but certainly very little of either can possibly get upon an iceberg. Arctic voyagers sometimes speak of seeing clay and mud upon bergs; but it is probable that if they had been near enough they would have found that what they took for clay and mud was merely dust and rubbish.

Undoubtedly the boulder-clay of many places bears unmistakeable evidence of having been formed under water; but it does not on that account follow that it was formed from the drop-pings of icebergs. The fact that the boulder-clay in every case *is chiefly composed of materials derived from the country on which the clay lies*, proves that it was not formed from matter transported by icebergs. The clay no doubt contains stones and boulders belonging to other countries, which evidently have been transported by icebergs; but the clay itself has not come from another country. But if the clay itself has been derived from the country on which it lies, then it is absurd to suppose that it was deposited from icebergs. The clay and materials which are found on icebergs are derived from the land on which the iceberg is formed; but to suppose that icebergs, after floating about upon the ocean, should always return to the country which gave them birth and deposit their loads is rather an extravagant supposition. And if they did not do that, then how could the boulder-clay always be derived from the country in which it is found?

Boulder-clay, whether formed on dry land or under water, is evidently the product of land-ice. Boulder-clay will be formed under land-ice whether the ice be moving on dry land or along the sea-bottom. In many parts of England, as at Norwich for example, the boulder-clay contains huge blocks which have evidently come from Scandinavia and other distant parts on the back of icebergs. These have been dropped among the clay then being brought down under the land-ice. In fact during the glacial epoch, unless the seas around a country were completely blocked up by land-ice, it would be impossible to prevent icebergs from coming and discharging their loads occasionally amongst the clay forced from under the land-ice. Few Scandinavian or other far transported blocks are found in the lower boulder-clay of Scotland; and this is owing, no doubt, to the fact that the seas surrounding that country during the glacial epoch were so completely blocked up by land-ice that no berg could possibly approach near to the coast.

If we simply admit (what follows from theory) that as the ice began to accumulate on the land the sea began to rise, and that the time of the greatest extension of the ice was also the time of the greatest submergence, many a difficulty will disappear.

The view which at one time prevailed in regard to this point was, that at the period of continental ice the land stood much higher than at present, and that as the cold began to abate, the land began slowly to sink and went down under the sea covered with ice. The land, after remaining for a long course of ages under water, ultimately emerged still covered with ice; but, the cold continuing to abate, the general covering of ice began to break up, and then followed the period of local glaciers.

I have never been able to find any proof given, either geological or physical, that at the time that the country was enveloped in ice the land stood higher. Instead of a greater elevation of the land during the period of ice, geological facts seem to prove the reverse; for in every country where we have evidence of glaciation, we have also evidence of a corresponding submergence of the land. The two so constantly accompany each other, that many geologists have been led to suspect the existence of some physical bond of connexion between them. We have good evidence, no doubt, that the land in relation to the sea must have stood much higher as well as much lower than it does at present, but we have no evidence that the country was at that particular time covered with ice. On the contrary, geological facts go rather to show that the land was then covered with forests and peat-bogs, and possessed an abundance of animal life.

It follows from theory, as has been shown on former occasions*, that during the cold periods of the glacial epoch the sea must have stood much higher than at present, and during the warm periods much lower. When certain considerations are taken into account which were overlooked on former occasions, a submergence to the extent of at least 500 feet may be accounted for. Although the repeated submergence and emergence of the land during Posttertiary times were probably due to oscillations of the sea rather than of the land, still we have evidence that there must have been local elevations and depressions of the land during the glacial epoch; for we find in some places that the land was submerged to the extent of upwards of 1000 feet, while in other parts it remained above water altogether. The oscillations of the sea-level resulting from cosmical causes would not in any way interfere with upheavals and depressions of the land resulting from volcanic or other agencies.

It was evidently not the facts of geology, but the notion that the cold of the glacial epoch must have resulted from a greater elevation of the land, which gave rise to the opinion that the country stood much higher than at present at the time that it was covered with ice. In order to account for the cold of the

* Phil. Mag. April 1866. 'Reader,' September 2 & 14, 1865; November 27, 1865.

glacial epoch and the general covering of ice, the land was first elevated; and then to make matters harmonize with the facts of geology, the land was submerged.

There is no necessity for this hypothesis in order to account for the cold of the glacial epoch; and even supposing that there was, it would not suit. The cold and ice of the glacial epoch were too general to be accounted for upon the hypothesis of elevation of the land. It is the elevation of a part *in relation to the surrounding* continents that can cool the climate of that part and cover it with ice. A general elevation would produce but little effect; for in such a case the atmosphere would to a considerable extent rise along with the land, and no very sensible change would result. We shall have to return to this part of the subject on a future occasion.

From the facts and considerations adduced we are, I would venture to presume, warranted to conclude that, with the exception of what may have been produced by land-ice, very little in the shape of boulder-clay or striated rocks belonging to the glacial epoch lie buried under the ocean—and that when the now existing land-surfaces are all denuded, probably scarcely a trace of the glacial epoch will then be found, except the huge blocks that were transported by icebergs and dropped into the sea. It is therefore probable that we have as much evidence of the existence of a glacial epoch during the Miocene, Eocene, and Permian periods as the geologists of future ages will have of the existence of a glacial epoch during the Posttertiary period, and that consequently we have no warrant whatever to conclude that the glacial epoch was a something unique in the geological history of our globe.

It might be thought that if glacial epochs have been so numerous as Table No. I. represents, we ought to have abundance of palæontological evidence of their existence. I do not know if this necessarily follows. Take the glacial epoch itself, quite a modern affair. Here we do not require to go and search in the bottom of the sea for the evidence of its existence; for we have the surface of the land in almost identically the same state in which it was when the ice left it, with the boulder-clay and all the wreck of the ice lying upon it. But what geologist, with all these materials before him, would be able to find out from palæontological evidence alone that there had even been such an epoch? He might search the whole, but would not be able to find fossil evidence from which he would be warranted to infer that the country had ever been covered with ice. We have evidence in the fossils of the Crag and other deposits of the existence of a colder condition of climate prior to the true glacial period, and in the shell-beds of the Clyde and other places of a cold condition of climate after the

true glacial period. But in regard to the period of the true boulder-clay or till, when the country was enveloped in ice, all is almost, comparatively speaking, a perfect blank so far as palæontology is concerned. "Whatever may be the cause," says Sir Charles Lyell, "the fact is certain that over large areas in Scotland, Ireland, and Wales, I might add throughout the northern hemisphere on both sides of the Atlantic, the stratified drift of the glacial period is very commonly devoid of fossils"*.

In the "flysch" of the Eocenes of the Alps, in which the huge blocks are found which prove the existence of ice-action during that period, few or no fossils have been found. So devoid of organic remains is that formation, that it is only from its position, says Sir Charles, that it is known to belong to the middle or "nummulitic" portion of the great Eocene series. Again, in the conglomerates at Turin, belonging to the Upper-Miocene period, in which the angular blocks of limestone are found which prove that during that period Alpine glaciers reached the sea-level in the latitude of Italy, not a single organic remain has been found. It would seem that an extreme paucity of organic life is a characteristic of a glacial period, which warrants us to conclude that the absence of organic remains in any formation otherwise indicative of a cold climate cannot be regarded as sufficient evidence that that formation does not belong to a cold period.

But if there is a deficiency of direct positive evidence of a general glaciation of the northern hemisphere during the Middle-Eocene, Upper-Miocene, and other periods similar to what we know took place during the Postpliocene period, there is, however, abundance of indirect evidence in favour of it.

Those facts to which I allude that appear to lead indirectly to the conclusion that a general condition of glaciation must have prevailed during the Upper-Miocene and other periods, are, strange to say, the very facts which have all along been adduced as a reason against the possibility of a cold condition of climate at those periods—notwithstanding the positive evidence which we have that the Alps and the Pyrenees† must have possessed enormous glaciers during that period, those of the former reaching the sea-level in the latitude of Italy.

If a cold and glacial condition of climate prevailed at those periods, we may be perfectly certain that a very warm and equable condition of climate must have also prevailed immediately before

* Antiquity of Man, p. 268 (third edition).

† For an account of the evidence of a glaciation of the Pyrenees during the Miocene period, see a paper "On the Glacial Phenomena of the Pyrenees," by Mr. P. W. Stuart Menteath, read before the Edinburgh Geological Society, 1867. See also *Bulletin de la Société Ramond*, 1866.

or immediately after those periods, or perhaps both before and after the cold period. This follows as a necessary consequence from the cosmical theory of climate. For it is physically impossible that we can have a cold and Arctic condition of climate on the one hemisphere, resulting from a great increase of excentricity, without at the same time having a warm, equable, if not an almost tropical, condition of climate prevailing on the other hemisphere. Whether a high state of excentricity will produce in our northern hemisphere a cold and glacial condition of climate or a warm and equable condition, depends solely upon the position of the winter solstice in relation to the perihelion at the time. But if the excentricity continues at a high value during a long course of ages, then, owing to the precession of the equinoxes and motion of the perihelion, the conditions of climate on the two hemispheres will be reversed every ten or twelve thousand years. Consequently when we find, as we do in the Upper-Miocene period and Middle-Eocene period, evidence of a cold condition of climate, we may reasonably expect to find also evidence of a much warmer and more equable condition than now prevails. And this is actually what we do find. "The Upper-Miocene flora and fauna of the whole of Central Europe," says Sir Charles Lyell, "afford unmistakeable evidence of a climate approaching that now only experienced in subtropical regions." "In the present state of the globe the island of Madeira presents the nearest approach to such a flora. The proportion of arborescent as compared to the herbaceous plants is very great; and among the former the predominance of evergreens implies an absence of severe winter cold"*. And in regard to the conglomerates of the Superga, near Turin, in which the erratic blocks are found, "the fauna and flora both of the overlying and underlying rock," says Sir Charles, "have the same subtropical character as that of Miocene date in Switzerland and Central Europe generally." "Hence," he continues, "the hypothesis of the transport of such huge blocks by ice-action has naturally been resorted to most unwillingly; but in the present state of our knowledge it is the only one which appears tenable."

Here is a case which, if found to be general, ought to settle the whole question of geological climate. Here is a bed of conglomerate indicating a cold and arctic condition of things when it was formed, with icebergs floating around the place now occupied by the city of Turin, overlain and underlain conformably by strata indicating a subtropical condition of climate. But this is not all. This warm condition of things was not confined to Central Europe, but extended probably over the entire northern

* 'Principles,' vol. i. p. 200 (10th edition).

hemisphere, even up to the North Pole ; for it has been proved that Greenland at this time must have been free of ice and covered with a luxuriant flora. (See Heer's *Flora Fossilis Arctica*.) The facts are wholly inexplicable on the ordinary theories of climate. The cosmical theory, however, not only explains them, but they follow according to this theory as a necessary consequence ; for if it should actually turn out that there is no such thing as a warm and equable condition of climate somewhere about the time of an ice-period, then the whole theory would have to be given up, because a warm period, according to theory, is just as necessary a result of an increase of excentricity as a cold period.

The occurrence of a warm condition of climate close beside a cold condition is not a mere accidental circumstance which has only been observed during the Upper-Miocene period. For when we go back to the Middle-Eocene period, we find the "flysch," which bears the marks of having been formed during an ice-period, closely associated with the nummulitic strata, indicating a warm condition of climate. "It has always been objected," says Sir Charles Lyell, "to the hypothesis that these huge masses were transported to their present sites by glaciers or floating ice, that the Eocene strata of nummulitic age in Switzerland, as well as in other parts of Europe, contain genera of fossil plants and animals characteristic of a warm climate. It has been particularly remarked by M. Desor that the strata most *nearly associated* with the flysch in the Alps are rich in echinoderms of the *Spatangus* family, which have a decidedly tropical aspect."

Passing back to the Cretaceous period, we find, closely associated with the floating ice in the sea of the White Chalk, fossil evidence of a warm condition of climate. And then, if we go back to the Permian period, we find glaciers reaching the sea-level in the very centre of England, and other indications of an age of ice, as has been clearly proved by Professor Ramsay. But the fossil remains of the Permian period declare emphatically the prevalence of a warm and equable condition of climate also during that age.

Sir Roderick Murchison has done me the honour to refer, in the last edition of the 'Siluria,' page 548, to my views on the occurrence of glacial epochs during past ages. He is opposed to the opinion held by many geologists and expressed in one of my papers, that there probably was ice-action during the Old-Red-Sandstone and other early periods. Supposing that it were proved that there was no ice during the Palæozoic age, this would not affect the cosmical theory in the least degree ; for it is quite possible, as has been already shown*, that the direct heat of the

* Phil. Mag. S. 4. vol. xxxv. p. 373.

sun may have been greater during the earlier periods of the earth's history. It must also be borne in mind that the simple fact of the fauna and flora of that age indicating a warm and equable condition of climate is not a sufficient proof that there were no cold periods; for a warm and equable condition of climate is just as necessary a result of an increase of excentricity as a cold condition; and the warm periods may be represented by organic remains, and the cold periods not.

Returning to the Postpliocene period. If this period afforded no geological evidence of a warmer condition of climate in Europe than now prevails, it would be so far a presumptive evidence against the assumption that the glacial epoch resulted from cosmical causes. But we have undoubted evidence that the climate of Europe during a portion of that period was much warmer and more equable than at present; for we find evidence of animals and shells existing in latitudes where they could not now live in consequence of the cold. The *Cyrena fluminalis* is a shell which does not live at present in any European river, but inhabits the Nile and parts of Asia, and especially of Cashmere. The *Unio littoralis*, extinct in Britain, is still abundant in the Loire. The *Paludina marginata* does not exist in this country, but inhabits the more southern parts of Europe. These shells have been found in Posttertiary deposits at Gray's Thurrock in Essex, in the valley of the Ouse near Bedford, at Hoxne in Suffolk, in the fluviomarine beds of the Norfolk cliffs, in the freshwater formation at Mundesley, and other parts of England. Along with these shells of a southern type have been found the bones of the Hippopotamus, of a species closely allied to that now inhabiting the Nile, and the *Elephas antiquus*, an animal remarkable also for its southern range, and the *Rhinoceros megarhinus*. But what is most remarkable, along with these have been found such animals of an arctic type as the Mammoth, the woolly Rhinoceros, and the Reindeer. How these could all have lived under the same conditions of climate has long been a puzzle to geologists. But many geologists now, as Sir Charles Lyell* and Mr. Boyd Dawkins†, are inclined to believe that these did not all live under the same conditions of climate, but imply oscillations of climate. During a cold condition of climate the Mammoth and other arctic animals would move southwards; and when the climate assumed a warm and equable condition, the animals and shells of a southern type would migrate northwards.

This warm condition of climate was not confined to temperate regions, but extended to high northern latitudes; for we find all over the Arctic regions remains of ancient forests where now the

* 'Principles,' vol. i. p. 193 (tenth edition).

† Quart. Journ. Geol. Soc. May 1867, p. 104.

entire country is covered with snow and ice. The trunk of a white spruce tree was dug up by Sir E. Belcher near Wellington Sound, in lat. $75^{\circ} 32'$ N. The remains of an ancient forest were discovered by Captain M'Clure in Bank's Land in lat. $74^{\circ} 48'$. "This remarkable phenomenon," says Captain M'Clure, "opens a vast field for conjecture; and the imagination becomes bewildered in trying to realize that period in the world's history, when the absence of ice and a milder climate allowed forest-trees to grow in a region where now the ground-willow and dwarf-birch have to struggle for existence." Evidence of ancient forests was found in Prince Patrick's Island and in Melville Island, one of the coldest spots, perhaps, in the northern hemisphere.

These phenomena have excited a very great amount of astonishment, and have been considered very puzzling; but they need not be so regarded if it be the case that the climate of the globe is greatly affected by variations in the excentricity of its orbit; for in such a case there could not possibly be a glacial epoch extending over a long course of time without the frequent recurrence of warm periods. From geological evidence alone, independently of cosmical considerations, we are warranted by analogy from the Posttertiary period to expect the existence of a glacial condition of things during the Upper-Miocene and Middle-Eocene period, from the fact that a warm condition of climate prevailed in Europe and extended to high latitudes during a part of those periods, similar to what we know occurred in the Posttertiary period.

The occurrence of a warm condition of climate always along with a cold condition shows that the two are in some way or other physically connected, and that they are both related to a common cause.

At one time I was under the impression that when the excentricity was near to its superior limit and the winter solstice in the perihelion, the mean annual temperature would not in the temperate regions be higher than it is at the present day, although in all probability at that time the winters would be as warm as the summers, the effect of this condition of things being to produce what Sir John Herschel has called a "perpetual spring." But after making calculations regarding the amount of energy in the form of heat that is being continually transferred from the tropical regions to the temperate and polar regions by ocean-currents, and to what extent the increase in the volume of those currents, which would result from that condition of things, would affect the climate, I felt persuaded that that opinion must be abandoned, and that a "perpetual summer" would better represent the condition of climate which must then prevail.

In the discussion of the subject of geological climate another point has been greatly overlooked, viz. the enormous influence that a mantle of ice continually lying on the continent of Greenland has upon the climate of the northern hemisphere. If we could by some means or other remove this cold mantle off that continent, though all things else should remain the same, the effect that its simple removal would have on the climate of the entire northern hemisphere would be quite magical. On a former occasion we referred to the physical reason of this, but shall have occasion to return again to this part of the question.

In looking over Table I., which, as has already been remarked, probably embraces the greater portion of the Tertiary age, the excentricity several times attained pretty high values, although it is only at the three periods embraced in Tables II., III., and IV. that the values reached would be likely to lead to a condition of things during the past three millions of years that might be properly designated a glacial epoch. But as the excentricity during the greater part of that vast period was greater than it is at the present day (for we are just now much nearer to the inferior limit than we are to the superior limit), its effect on climate would be more marked than it is at present. The effect would be that, as a general rule, during the greater portion of the three millions of years past, the climate would be somewhat colder when the winters occurred in the aphelion than it is at the present day, and a good deal warmer when they occurred in the perihelion than at present.

Judging from geological evidence alone, we naturally conclude that, as a general rule, the climate of the Tertiary period was somewhat warmer than it is at the present day. It is from fossil remains that the geologist principally forms his estimate of the character of the climate during any period. Now, in regard to fossil remains, the warm periods will always be far better represented than the cold; for we find that, as a *general rule, those formations which geologists are inclined to believe indicate a cold condition of climate are remarkably devoid of fossil remains*. If a geologist does not keep this principle in view, he will be very apt to form a wrong estimate of the general character of the climate of a period of such enormous length as the Tertiary.

Suppose that the presently existing sea-bottoms, which have been forming since the commencement of the glacial epoch, were to become consolidated into rock and then elevated into dry land, we should then have a formation which might be properly designated the Postpliocene. It would represent the time which has elapsed from the beginning of the glacial epoch to the present day. Suppose one to be called upon as a geologist to determine from that formation what was the general character

of the climate during the period in question, what would probably be the conclusion at which he would arrive? He would probably find here and there patches of boulder-clay containing striated and ice-worn stones. Now and again he would meet with bones of the mammoth and the reindeer, and shells of an Arctic type. He would likewise find huge blocks of the older rocks imbedded in the formation, from which he would infer the existence of icebergs and glaciers reaching the sea-level. But, on the whole, he would find that the greater portion of the fossil remains met with in this formation implied a warm and temperate condition of climate. At the lower part of the formation, corresponding to the time of the true boulder-clay, there would be such a scarcity of organic remains that he would probably feel at a loss to say whether the climate at that time was cold or hot. But if the intense cold of the glacial epoch was not continuous, but was broken up by one or more warm periods*, during which the ice, to a considerable extent at least, disappeared for a long period of time (and there are few geologists who have properly studied the subject that will positively deny that such was the case), then the country would no doubt during those warm periods possess an abundance of plant and animal life. It is very probable that it was during those periods that the Arctic forests flourished. It is quite true that we may almost search in vain on the present land-surface for the organic remains which belonged to those interglacial periods; for they were nearly all swept away by the ice which followed. But no doubt in the deep recesses of the ocean, buried under hundreds of feet of sand, mud, clay, and gravel, lie multitudes of the plants and animals which then flourished on the land, and were carried down by rivers into the sea. And along with these lie the shells and other marine faunæ which flourished in the warm seas of those periods. A geologist, thus judging from the great abundance of organic remains that this lower portion of the formation would contain indicating a warm condition of climate, and the almost total

* For geological evidence of warm periods during the glacial epoch, see:—Mr. Geikie's memoir "On the Glacial Drift of Scotland;" M. Morlot "On the Posttertiary and Quaternary Formations of Switzerland," Edin. New Phil. Journ., New Series, vol. ii. 1855; Lyell's 'Antiquity of Man,' p. 321, second edition; 'Principles,' vol. i. p. 198, tenth edition; Heer, *Urwelt der Schweiz*; Vogt's 'Lectures on Man,' pp. 318–321; Mr. Edward Hull "On the Drift-deposits in the Neighbourhood of Manchester," Mem. Lit. and Phil. Soc. of Manchester, 1863. For some remarkable facts bearing on the subject, see a valuable memoir by Mr. James Bennie of Glasgow, "On the Surface Geology of the District round Glasgow," Trans. of the Geol. Soc. of Glasgow, vol. iii. part 1. See also a paper by Mr. James Geikie "On the Remains of the *Bos primigenius* found in an Interglacial Bed," Geol. Mag. for September 1868.

absence of fossil remains corresponding to the glacial condition of things which we now know also did prevail at that epoch, would very likely come to the conclusion that the former part of the Postpliocene period was a warm period; whereas we, at the present day, looking at the matter from a different standpoint, declare that part to be a glacial epoch. No doubt, if the beds formed during the cold periods of the glacial epoch could be distinguished from those formed during the warm periods, the fossil remains of the one would indicate a cold condition of climate, and that of the other a warm condition; but still, taking the entire epoch as a whole, the percentage of fossil remains indicative of a warm condition would probably so much exceed that indicative of a cold condition, that we should come to the conclusion that the character of the climate, as a whole, during the epoch in question was warm and equable.

As geologists we have, as a rule, no means of arriving at a knowledge of the character of the climate of any given period but through an examination of the sea-bottoms belonging to that period; for these contain all the evidence upon the subject. But unless we exercise caution, we shall be very apt, in judging of the climate of such a period, to fall into the same error that we have just now seen one would naturally fall into were he called upon to determine the character of the climate during the glacial epoch from the character of the organic remains which lie buried in our adjoining seas.

In conclusion, during the past three millions of years there were three periods when the excentricity attained a high value. These three periods are represented in Tables II., III., and IV. The first began about 2,630,000 years ago, and terminated about 2,460,000 years ago. The second began about 980,000 years ago, and terminated about 720,000 years ago. The third began about 240,000 years ago, and terminated about 80,000 years ago.

The third period, for reasons which have now been considered at some length, I believe to have been that of the Glacial Epoch; the second in all probability was that of the Upper-Miocene period; and the first might probably correspond to the glacial epoch of the Middle-Eocene period.

From the commencement of the glacial epoch, 240,000 years ago, back to the close of the glacial epoch of the Upper-Miocene period, represented in Table III., there is an interval of 480,000 years. Taking the ordinary rate of subaërial denudation, as indicated by the rate at which rivers carry the soil off the land, which we have already seen is not less than about one foot in 6000 years*, it follows that about 80 feet must have been removed off the face of the country during that long interval. It is there-

* *Phil. Mag.* for May 1868, p. 379. *Trans. of Glasgow Geol. Soc.* vol. iii. part. 1.

fore probable that almost every trace of the ice of the Miocene period would be removed before the glacial epoch began. From the close of the glacial epoch of the Middle-Eocene period, represented in Table II., to the commencement of the glacial epoch of the Miocene period, there is the enormous interval of 1,480,000 years. During that time no less than 247 feet would be removed off the general level of the country.

From the close of the glacial epoch of the Miocene period to the present day, 120 feet of rock must have been removed from the surface of the land and carried down in the form of sediment into the sea. And since the glacial epoch of the Eocene period, no less than 410 feet must have been removed. We need not, therefore, wonder that so few traces of the ice of those periods remain. Remove 410 feet of rock off the surface of the present existing continents, and where should we then find our "roches moutonnées," striated rocks, boulder-clay, or in fact any evidence whatever on the land that there had been a glacial epoch during the Posttertiary period?

XLVIII. *Note on the Solvibility of Equations by means of Radicals.* By Professor CAYLEY, F.R.S.*

IN regard to the theorem that the general quintic equation of the n th order is not solvable by radicals, I believe that the proofs which have been given depend, or at any rate that a proof may be given that shall depend, on the following two lemmas:—

I. A one-valued (or symmetrical) function of n letters is a perfect k th power, only when the k th root is a one-valued function of the n letters.

There is an exception in the case $k=2$, whatever be the value of n : viz. the product of the squares of the differences is a one-valued function, a perfect square; but its square root, or the product of the simple differences, is a two-valued function. It is in virtue of this exception that a quadric equation is solvable by radicals; we have the one-valued function $(x_1 - x_2)^2$, the square of a two-valued function $x_1 - x_2$, and thence the two roots are each expressible in the form

$$\frac{1}{2} \{ x_1 + x_2 + \sqrt{(x_1 - x_2)^2} \}.$$

II. A two-valued function of n letters is a perfect k th power, only when the k th root is a two-valued function of the n letters.

There is an exception in the case $k=3$, when $n=3$ or 4: viz. for $n=3$ we have $(x_1 + \omega x_2 + \omega^2 x_3)^3$ (ω an imaginary cube root of unity) a two-valued function, and a perfect cube; whereas its cube root is the six-valued function $x_1 + \omega x_2 + \omega^2 x_3$. And similarly for $n=4$ we have, for instance,

$$\{ x_1 x_2 + x_3 x_4 + \omega(x_1 x_3 + x_2 x_4) + \omega^2(x_1 x_4 + x_2 x_3) \}^3$$

a two-valued function, and a perfect cube, whereas its cube root

* Communicated by the Author.

is a six-valued function. And it is in virtue of this exception that a cubic or a quartic equation is solvable by radicals. But I assume that for $n > 4$ the lemma is true without exception.

The course of demonstration would be something as follows :— Imagine, if possible, the root of an equation expressed, by means of radicals, in terms of the coefficients; the expression cannot contain any radical such as $\sqrt[p]{X}$, $p > 2$, where X is a one-valued (or rational) function of the coefficients, not a perfect p th power, for the reason that, expressing the coefficients in terms of the roots, such function $\sqrt[p]{X}$ is not a rational function of the roots; if it were so, by lemma I. it would be a one-valued (that is, a symmetrical) function of the roots; consequently a rational function of the coefficients, or X expressed in terms of the coefficients, would be a perfect p th power.

The expression may however contain a radical $\sqrt{X}$, X a one-valued (or rational) function of the coefficients, not a perfect square; viz. X may be any square function multiplied into that function of the coefficients which is equal to the product of the squared differences of the roots, or, say, multiplied into the discriminant $X = Q^2\Delta$, or $\sqrt{X} = Q\sqrt{\Delta}$.

We have next to consider whether the expression can contain any radical $\sqrt[p]{X}$, where X , not being a rational function of the coefficients, is a function expressible by radicals. But the foregoing reasoning shows that if this be so, X cannot contain any radical other than the radical $\sqrt{Q^2\Delta}$ or $Q\sqrt{\Delta}$, as above; that is, X must be $= P + Q\sqrt{\Delta}$, where P and Q are rational functions of the coefficients, and where we may assume that $P + Q\sqrt{\Delta}$ is not a perfect p th power of a function of the like form $P' + Q'\sqrt{\Delta}$. But then, expressing the coefficients in terms of the roots, we have $P + Q\sqrt{\Delta}$, a (rational) two-valued function of the roots; and there is no radical $\sqrt[p]{P + Q\sqrt{\Delta}}$, which is a rational function of the roots; for by lemma II., if such radical existed we should have $\sqrt[p]{P + Q\sqrt{\Delta}}$, a (rational) two-valued function of the roots; that is, it would be $= P' + Q'\sqrt{\Delta}$, P' and Q' one-valued (symmetrical) functions of the roots, consequently rational functions of the coefficients; or $P + Q\sqrt{\Delta}$ would be a perfect p th power $(P' + Q'\sqrt{\Delta})^p$.

The conclusion is that for $n > 4$ there is not (besides the function $P + Q\sqrt{\Delta}$) any function of the coefficients, expressible by means of radicals, which, when the coefficients are expressed in terms of the roots, will be a rational function of the roots, and consequently no possibility of expressing the roots in terms of the coefficients by means of radicals.

Cambridge, October 1, 1868.

XLIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 317.]

June 18, 1868.—Lieut.-General Sabine, President, in the Chair.

THE following communications were read :—

“An attempt to apply Chemical Principles in explanation of the Action of Poisons.” By W. H. Broadbent, M.D.

The starting-point in the inquiry has been the two following postulates :—

1. That there must be some relation between the substance administered and the animal organism, on which the effects depend.
2. That, so far as the substance is concerned, the basis of the relation can only be its chemical properties, using the term in its widest sense.

From these postulates follow certain corollaries :—

1. That the physiological and therapeutic action of the same substance must be similar in kind.
2. That the action of food, remedies, and poisons must be capable of explanation on the same principles.
3. That substances chemically allied should have similar physiological and therapeutic actions, or any diversity found to exist should be capable of explanation on chemical grounds.

The second of these deductions is taken as a guide in the present inquiry. Something is known as to the uses of the various classes of foods in the economy, and of the mode in which they subserve these uses; this knowledge may be applied in the endeavour to ascertain the mode of action of poisons.

The operations taking place in the animal organism may be divided into two great classes :—(α) for maintenance of structure, (β) for evolution of force. While mutually interdependent, they are distinct, and in character essentially antagonistic—structural and chemical elaboration on the one hand, oxidation or disintegration on the other.

The two great classes of food, organic and mineral, are in close relation with these. The organic foods build up the tissues, but ultimately undergo oxidation and yield force. The inorganic foods take a subordinate part in the composition of the textures; they do not yield force by oxidation, but they influence the nutritive processes. So also the organic remedies and poisons affect the evolution of force, mineral substances the organic processes.

(The action of mineral matters has been noticed elsewhere.)

The force evolved in the animal organism takes the form of heat, motion, and nervous action; but there are very important points of difference between heat on the one hand and nervo-muscular action on the other, both as to the part they take in the vital processes, and in the conditions of their evolution.

It is through their action on the nervous system that the powerful organic poisons destroy life; and in order to understand this

action, it is necessary to consider closely the evolution of nerve-force, and to endeavour to realize the chemical conditions implied.

In the first place, the source of nerve-force is oxidation, and the seat of the oxidation is the nervous structures. This is generally admitted, and seems to be conclusively established by an analysis of the phenomena observed in experiments with a prepared frog's leg.

This admitted, it is to be noted—

1. That nerve-action is intermittent and of varying intensity, and that, in addition to the presence of the oxygen brought to the nerve-structures by the blood, an impulse from without, or from some other part of the nervous system, is necessary to determine the evolution of the force.

2. Again, there is a storing up of potential energy in the nervous structures; witness the necessity for sleep, &c.

3. A due supply of oxygen is required. The phenomena of asphyxia show that the demand is most urgent in the hemispherical ganglia.

These facts indicate that the constituent of the nervous structures by oxidation of which the force is yielded, possesses what I have ventured to call chemical tension, a property which does not belong to non-nitrogenized matter, or to all nitrogenized matter. It will be further explained later; for the present, it is sufficient to refer to nitroglycerine as an extreme example.

The protagonist of Liebreich, and the neurine recently identified by Wurtz with hydrate of trimethyl-oxethyl-ammonium, have this characteristic in a certain degree.

Turning now to the poisons which kill by their powerful action on the nervous system. They all contain nitrogen, and all possess chemical tension; and these seem to be the only points common to the entire group.

Nitrogen cannot be the poisonous element; it has no great chemical energy, and it is present in large proportion in substances which are inert. It is nevertheless the pivot on which the deadly influence turns. Its affinity for H, O, and especially for C, is only feeble. When, therefore, in a molecule containing C, H, and N, or C, H, N, and O, the elements are not so arranged that the mutual affinities of C, H, and O cooperate to maintain the integrity of the molecule; there may be a more or less powerful tendency on the part of C, H, and O to rearrange themselves without regard to the N, or to combine with O or H₂O if presented. This is what is meant by the term chemical tension. In the example given, nitroglycerine {C₃H₅(NO₂)₃O₃}, the dislocation is of O from N in favour of C and H. Equally striking examples of dislocation of N from C or H cannot be given, and it is not easy in all cases to point out the source of the tension. A very important method in which the balance of affinities is deranged and the condition of tension brought about, is by departure from a stable type, as, for instance, in the nitrite bases which are residues derived from ammonium-salts by dehydration. To this class belong morphia, strychnia, brucia, and most poisonous alkaloids. Additional interest is given to these compounds by the fact that Dr. Crum Brown and Dr. Fraser have shown that, by in-

roduction into the molecule of methyl-iodide, carrying back the constitution a step in the direction of the ammonium type, the poisonous effects are greatly diminished, and entirely altered in character.

Let the deduction as to the evolution of nerve-force be accepted, and we have in the introduction into the blood of substances having varying degrees and directions of tension an intelligible method of influencing its manifestations.

Looking now upon nerve-action as a result of oxidation, in the various methods by which this oxidation may be influenced, analogies may be traced with conditions which affect ordinary combustion. These conditions are :—

1. The supply of oxygen.
2. The character of the combustible.
3. The presence of products of combustion, or of bodies having a similar influence.

It is of course necessary to bear in mind the peculiarities of the oxidation yielding nerve-force, the differences between combustion and oxidation in the moist state, and the special modifying conditions of the animal organism. For example, while in asphyxia the deprivation of oxygen arrests all nervous action, the respiration of undiluted oxygen does not intensify it, either because the blood will only take up a certain proportion of oxygen, or more probably because the effects of the O are expended in altering the blood, which is thus oxidized instead of being oxygenated.

The analogies to the above conditions found in the action of substances on the nervous system are :—

1. The liberation in the nascent state in the nervous structures of C and H, which appropriate the O brought by the blood, and so produce a result equivalent to the exclusion of O. The C and H are set free by the dislocating influence of N; and the example of this mode of action is furnished by prussic acid.

The converse of this, the liberation of O by a similar process, is not likely to occur, as O is never present in an organic body in excess of the proportion which would fully oxidize the other elementary constituents.

2. The analogy to the influence on the energy of combustion by the character of the combustible, is found in the introduction into the blood of substances having chemical tension, holding different relations to the tension of the nervous matter.

3. The action of anæsthetics on the nervous system furnishes a strict parallel to the influence of CO₂ on combustion.

The rationale here given as to the action of anæsthetics is, for the purposes of the present paper, taken as established by the late Dr. Snow. Objections which have been made to it are capable of removal by experiments and considerations which need not here be adduced.

Considerable importance is attached to the establishment of the explanation here given of the action of prussic acid. Stated more explicitly, this explanation is, that the prussic acid is carried by the blood to the nerve-centres, that, under the influence of the affi-

nities thus brought to bear upon it (affinities which normally determine the oxidation by which nerve-force is evolved), its elements are dislocated from each other; and the C and H, liberated in the latent condition, appropriate the O destined for the evolution of nerve-force, which is thus arrested.

This explanation is suggested by the composition of hydrocyanic acid, H Cy. Cyanide of potassium KCy, again, is used as a powerful reducing agent in chemical processes. Its liability to the change which will permit its elements to exercise their individual affinity for O is indicated by its spontaneous decomposition in water, by its position as a nitrile (formio-nitrile). Corroborative evidence that it is by means of such a change that it acts, is furnished when the elements are held together by some supporting affinity, as in ferrocyanogen. But the best example is in sulphocyanogen and hydro-sulphocyanic acid, which of themselves are poisonous (*i. e.* cannot resist the dislocating influences), but reinforced by a base are innocent. [A parallel to this is seen in aniline, which is poisonous, and in sulphate of aniline, which is not. See Lond. Hosp. Reports, Dr. Letheby.]

The phenomena of poisoning by prussic acid are perfectly consistent with this view. All observers have noted their similarity to those of asphyxia.

Still more striking is the fact that artificial respiration, and especially with oxygen, is the great means of neutralizing the effects of this poison.

Probably this chain of facts would be considered conclusive, were it not that the hypothesis as to the mode of death by H Cy is paralysis of the respiratory movements. This hypothesis, however, still leaves unexplained the cause of the paralysis itself, and therefore the real mode of action of the poison. It is, moreover, inconsistent with certain of the phenomena; the respiratory nerve-centres are actually the last to be paralyzed, except those concerned in the action of the heart.

Experiments, nevertheless, have been made for the purpose of ascertaining whether the previous respiration of undiluted oxygen would retard, or in any measure prevent the action of prussic acid. The results have been by no means uniform; but instances have occurred (rats being the animals used) in which, after the injection of an overwhelming dose, the fatal effect has been delayed quite beyond the operation of accidental causes, and, again, in which a dose fatal to two rats, and barely survived (after a long train of symptoms) by one other, produced comparatively little effect on another after the respiration of oxygen.

It has been found also that a proportion of prussic acid diffused in equal volumes of air and of oxygen, has a decidedly less powerful action on the animal in the latter case.

With frogs the results were most contradictory and embarrassing, till it was discovered that prussic acid injected under the skin had scarcely any action on them; but if they were subsequently placed under a glass shade, or in some other confined atmosphere, into which the acid diffused, it would gradually affect them. It seems

probable that the affinities in operation in the nervous structures of the frog are not sufficiently energetic to determine the decomposition of the H Cy , which will then act upon this animal as an anæsthetic.

Nitroglycerine was at first made the subject of experiment, under the idea that possibly oxygen might be evolved from the NO_2 which is substituted for three equivalents of H in the typical molecule. Subsequent reflection showed that this is not likely to occur; but the fact remains, that it is a substance liable to change, and very highly charged with oxygen, as compared with the ordinary constituents of the body.

It is a very powerful poison, having, however, entirely different effects on frogs and rats.

In frogs it very speedily induces powerful tetanic convulsions: a single drop of a solution of one pint of nitroglycerine in four parts of methyl in alcohol placed on the back of a frog is followed in five or eight minutes by stiffness of movement, and in thirteen to sixteen minutes by most violent spasms. In rats an hour or more elapses before any symptom is manifest, and then death is by a gradually increasing feebleness of movement, in two or three hours, without convulsion.

It is unquestionable that this difference in the effects has a relation to the oxygen contained in the nitroglycerine. The contrast with prussic acid in the action on warm and cold-blooded animals is suggested.

A very extended and comprehensive inquiry, both as to the conditions in the nervous system associated with convulsions, tetanus, delirium, &c., and into the relations and constitution of the poisons which give rise to these symptoms, is necessary before the second analogy can be followed out with any confidence. Experiments are being made with substances of known composition and constitution, with a view to elucidate this part of the question.

In conclusion, two points are considered which cannot be passed over in any attempt to apply the principles of physical and chemical science to the case of poisons.

The first is as to the minuteness of the fatal dose. Any explanation, before it can be accepted, must show that the cause is adequate to produce the effect. This is a difficulty in the path of any rational explanation. It is attempted to meet it by showing, on the one hand, that the equivalency of nerve-force is extremely small, by reference to its analogy with electrical currents, and by other considerations, and that therefore the degree of chemical change involved in its evolution is also small; and, on the other hand, the maximum of force to be obtained from an organic body is through the exercise of the affinities of its individual elements.

The second point is as to the special action of certain poisons on particular nervous centres—strychnia on the cord, morphia on the brain, &c., the substances being carried by the blood to all alike. It is as necessary to explain why no effect is produced on those centres or tracts which do not suffer, as to explain the action on the one which does. The explanation is sought in the fact that the differ-

ence in the functional activity of the brain and cord, the need for sleep by the brain, not experienced, at any rate in the same degree, by the cord, point to a difference of tension, and therefore of relation to the substances which act as poisons. This consideration will apply where the differences of susceptibility and of tension are not so marked.

But this is only part of a still wider question—the different action of poisons on different classes of animals. The explanation is still the same. Difference in the functional energy or activity of corresponding nerve-centres implies difference of tension.

The following facts bear strikingly on this point :—

1. Anæsthetics affect all classes of animals alike, *i. e.* when the effect is a general arrest of oxidation.

2. Strychnia, which acts on the cord, affects all animals alike. The spinal system is the centre which is most similar in its endowments in all classes of vertebrates.

3. The poisons which have the most diverse action on different animals are such as in man act on the cerebral ganglia.

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

The author describes the appearance of the comet in the telescope on June 22 to consist of a nearly circular coma, which became rather suddenly brighter towards the centre, where there was a nearly round spot of light. A tail was traced for nearly a degree.

He found the light of the comet, when examined with a spectro-scope furnished with two prisms of 60° , to be resolved into three broad bright bands.

The brightest band commences at about *b*, and extends nearly to F. Another band begins at a distance beyond F rather greater than half the interval between *b* and F. The third band occurs about midway between D and E. In the two more refrangible of these bands the light was brightest at the less refrangible end, and gradually diminished towards the other limit of the bands. The least refrangible of the three bands did not exhibit a similar gradation of brightness.

These bands could not be resolved into lines, nor was any light seen beyond the bands towards the violet and the red.

The measures of these bands are given, and a diagram of their appearance.

The author found this cometic spectrum to agree exactly with a form of the spectrum of carbon which he had observed and measured in 1864. When an induction spark, with Leyden jars intercalated, is taken in a current of olefiant gas, the highly heated vapour of carbon exhibits a spectrum which is somewhat modified from that which may be regarded as typical of carbon. The light is of the same refrangibilities, but the separate strong lines are not to be distinguished. The shading (composed of numerous fine lines) which accompanies the lines appears as an unresolved nebulous light.

On June 23 the spectrum of the comet was compared directly

in the spectroscope with the spectrum of the induction spark taken in a current of olefiant gas.

The three bands of the comet appeared to coincide with the corresponding bands of the spectrum of carbon. In addition to an apparent identity of position, the bands in the two spectra were very similar in their general characters and in their relative brightness.

These observations were confirmed on June 25.

The remarkably close resemblance of the spectrum of the comet to that of the spectrum of carbon, necessarily suggests the identity of the substances by which in both cases the light was emitted.

The great fixity of carbon seems, indeed, to raise some difficulty in the way of accepting the apparently obvious inference from these prismatic observations. Some comets have approached sufficiently near the sun to acquire a temperature high enough to convert even carbon into vapour.

In the case of other comets, the author suggests that the difficulty is one of degree only; for the conditions are not known under which even a gas permanent at the temperature of the earth could maintain sufficient heat to emit light.

The author states that some phosphorescent substances give spectra which are discontinuous, but he gives reasons which would scarcely permit us to consider cometary light to be of a phosphorescent character.

The spectrum shows that the colour of this comet was bluish green. Considerable difference of colour has been remarked in the parts of some comets. Sir William Herschel described the head of the comet of 1811 to be of a greenish or bluish-green colour, while the central point appeared of a ruddy tint. The same colours have been observed in other comets. If carbon be the substance of some comets, this substance, if incandescent in the solid state, or reflecting, when in a condition of minute division, the light of the sun, would afford a light which, in comparison with that emitted by the luminous vapour of carbon, would appear yellowish or approaching to red.

The author refers to the bearing of these results on certain cometary phenomena, and on the apparent identity of the orbits of the periodical meteors with those of some comets.

L. Intelligence and Miscellaneous Articles.

ON STELLAR SPECTRA. BY FATHER SECCHI.

IN one of my previous researches on stellar spectra I pointed out that researches on coloured stars led us to admit a fourth type of stellar spectra, the standard of which might be the spectrum which I have published of the star 12561 of Lalande. Now (after having completed a review of the coloured stars) I am absolutely certain of this fact. This type had escaped me in my previous researches; for none of them exceeds the sixth magnitude; and it would not have been remarked without the introduction of the cylindrical eyepiece, which leaves to the spectra of these stars all their brilliancy.

The essential character of this type is to present a spectrum formed of three luminous bands separated by dark intervals. The brightest band is in the green; it is generally strong, well marked, and very dilated. Another, much feebler band is met with in the blue; but this band is often very difficultly visible. The third band is found in the yellow, and expands towards the red; this, however, is divided into several others.

All these bands have the characteristic that their light increases in the direction of the violet, and then suddenly ceases. On the contrary, towards the red they gradually shade into absolute black. There is thus a complete opposition between this type and the third; for in this not only are the columns double in an equal space, but they present the maximum of light on the side of the red, and the minimum on that of the violet. The two spectra are therefore not a modification of one and the same type; they are evidently due to substances completely different.

The feebleness of their light, which prevents us from using the spectroscope with a slit, hinders a rigorous definition of the substances which produce these phenomena. It may, however, be said that there is great analogy with the reversed carbon spectrum. But I think any conclusion of this kind is premature.

The following is a list of the principal stars of this type, and their position:—

Number in Schellerup's catalogue.	Right ascension.	Declination.	Magnitude.	Notes.
	h m s			
41.	4 36 2	+67° 54'	6½	Beautiful.
43.	4 42 8	+28 16	8	
51.	4 58 1	+ 0 59	6	
78.	6 26 9	+38 33	6½	Beautiful.
89.	7 11 5	—11 43	7½	
124.	9 44 6	—22 22	6½	
128.	10 5 8	—34 38	7	Doubtful.
132.	10 30 7	—12 39	6	Beautiful.
136.	10 44 8	—20 30	6½	Magnificent.
152.	12 38 5	+46 13	6	
159.	13 19 3	—11 59	5½	
163.	13 47 3	+41 2	7	
229.	19 26 5	+76 17	6½	
238.	20 8 6	—21 45	6	
249.	21 25 8	+50 58	9	Beautiful.
252.	21 38 6	+37 13	8½	
273.	23 39 2	+ 2 42	6	

A description of these spectra, with figures, will be inserted in the memoir which is ready for publication; this memoir will comprehend the details of the third type, all the spectra of which present a superb colonnade. I will here restrict myself to giving the curve of the light of the 152nd, which is the most beautiful; the projecting lines resemble dazzling and very brilliant lines near the end of the luminous band.

It is to be presumed that among the smaller stars some other spectrum will be found; but for this purpose I think it is indispensable to use a telescope with a greater power than that of nine inches, or a more delicate eye. Nearly all red stars have spectra with bands; the small ones often present a continuous spectrum: this continuity might be merely apparent, and these stars would belong to the red type without bands, as is the case with Arcturus among the large ones. In my researches I have not limited myself to the stars of the catalogue, but each time I have traversed the surrounding heavens; thus I have discovered some new stars, but in small number, which leads me to believe that the greater number have been noted, at any rate for our hemisphere. A similar study would have to be undertaken for the southern hemisphere.

Having at length been able to put up the complete apparatus for the absolute measurement of the lines and their comparison with chemical elements, I have confirmed that the rays of hydrogen coincide with the four black lines of α Lyræ with surprising accuracy. I say the four lines; for even the most refrangible line of the violet is found in its place in the spectrum of hydrogen. The tube with which the observation was made was sent to me by M. Geissler of Bonn, who prepared it with chemically pure hydrogen. The spectrum thus obtained agrees with that which has been given by M. Morren of Marseilles. Another secondary line of hydrogen appeared in the spectrum of the star, like that of the yellow. After this it can no longer be doubted that this substance forms the absorbing atmosphere of this star, which was already known. But it is also evident that the star α Lyræ has no appreciable motion of its own the velocity of which is comparable to that of light, as I have said in another communication. The spectroscope used was made by M. Hoffmann of Paris. It was formed of two very dispersive prisms; and when it was directed towards the sun, it separated the two lines of sodium and the three lines of magnesium with great sharpness.

The coincidence of the lines of hydrogen with those of the light of the sun reflected by the moon presented the same degree of precision and exactitude as for α Lyræ. At the time of observation this star was almost on the zenith. The observation was made on the 2nd and 3rd of this month (August). It remains to be seen if, with this delicate means, we do not succeed in ascertaining the influence of the motion of the earth. I do not think it is impossible to appreciate differences amounting to one-tenth the magnitude of the line *f*. — *Comptes Rendus*, August 10, 1868.

ON A THERMO-RHEOMETER. BY M. JAMIN.

The instrument which I have the honour of submitting to the Academy is a water-thermometer. The reservoir is a long glass tube with a graduated stem which is bent downwards and terminates in a cup in which water can be placed to fill the instrument. The bottom of the reservoir is fastened in a mercury-cistern arranged as in a Fortin's barometer. The mercury can thus be raised or lowered by a measurable amount, which diminishes or increases the quantity of water in the reservoir; and if its temperature be raised without

heating the mercury, the end of the column in the stem moves. The delicacy of the thermometer varies with the height of the mercury, but according to a simple law which is deduced from calculation or from observation.

A very fine platinum wire is stretched in the reservoir from the top to the cistern; its ends are fused into the glass and connected with the poles of a battery; it transmits the current through the mercury without resistance; offering through the water a resistance x , it develops an amount of heat which only affects the water of the thermometer, which does not heat the metal, and which cannot be transmitted downwards owing to the feeble conductivity of the liquid.

(1) By raising or lowering the mercury, the length and resistance x of the platinum wire are diminished or increased at pleasure. The apparatus thus becomes a rheostat analogous to that of Pouillet, but more convenient and better, inasmuch as the wire, being immersed in water, is but little heated, and serves for currents whether strong or weak.

(2) The heat developed by the passage of a current may be measured. It equals $p(t' - t)$, the product of the weight of water into the increase of temperature; the weight is sxd . The increase of temperature is measured by the variation of the volumes, or the number n of divisions through which the thermometer-column has moved, divided by the volume sx , and by the coefficient of expansion k . Then

$$p(t' - t) = \frac{sxd}{sxk} n = \frac{d}{k} n,$$

which signifies that the heat given up, apart from the corrections, which I here neglect, may be measured by the number of divisions by which the thermometer-column has moved; n is independent of the height of the mercury. A simpler result could not be arrived at. The measurement of n is made with the ordinary precautions.

(3) This heat n is proportional to the resistance x , which is determined by the square of the intensity. We have then

$$i = \sqrt{\frac{n}{x}}.$$

The instrument may thus be used as a galvanometer; the smaller the ratio of the sections of the stem and of the reservoir, the more delicate it is. In fact it is a thermo-rheometer.

(4) Replacing i by its value, we get

$$\frac{A^2 x}{(R + x)^2} = n.$$

Making the determination^s of n with different values of x , A and R may be calculated; that is, the electromotive force and the resistance of a battery may be determined without the aid of any other instrument than the thermo-rheometer.

(5) What has been said applies to induction-currents as well as to the current of batteries. The former have hitherto not been much investigated: as they are alternately contrary, it is impossible to separate them accurately; and in general their effects neutralize themselves. The calorific effect alone is independent of the direction of the currents and unaffected by their interruptions; and, finally, the

sum of the heats observed by the thermo-rheometer is proportional to the square of the electricity put in circulation.

In short, the thermo-rheometer is at once and of itself a rheostat, a galvanometer, and a measurer of electromotive forces; it is the only one of these various instruments which can be applied to induction-currents as easily as to ordinary currents.—*Comptes Rendus*, July 6, 1868.

INVESTIGATIONS ON OBSCURE CALORIFIC SPECTRA.

BY M. DESAINS.

The whole of the researches relative to the calorific spectrum have long ago established that the heat of the luminous part of this spectrum is more transmissible through water than are the mean obscure rays. Melloni has gone further, and, in a memoir presented in 1831 to the Academy, announced that the least-refrangible parts of the obscure solar spectrum are completely destitute of the property of traversing a layer of water a few millimetres thick. These portions, from this point of view, would resemble the radiations from blackened bodies heated to temperatures below 300° .

Whatever since Melloni's time be the results which science has acquired relative to the properties of solar calorific radiations, I have thought it right to investigate again whether, in the spectra formed of these radiations, there really are rays analogous to those of sources at a very low temperature.

The ideas recently acquired on the absorbing action of vapours made it little probable; and in fact I could never find, in the solar spectra I have investigated, rays completely deprived of transmissibility through water. These spectra were so pure that the eye could readily detect the principal lines; the luminous part was about 25 millims. in extent, and their obscure part about the same. I examined the different parts by the aid of a line pile, the breadth of which was scarcely a twenty-fifth of the whole breadth of the spectrum. In the maximum I had frequently a deviation of 30° , and sometimes much more; but on removing the pile to the limits of the obscure, where there was only a deviation of one or two divisions, I found that the heat capable of producing these deviations was transmitted in considerable proportion through a layer of water of 2 millims. It is not, perhaps, superfluous to remark that these latter pencils of obscure heat did not in my experiments exceed the two-hundredth part of the total heat spread over the whole extent of the spectrum.

Entirely different results are obtained on analyzing the spectra of terrestrial luminous sources, such as the flame of a lamp without a glass, or an incandescent platinum wire.

In one series of experiments a wire of this kind was kept at a cherry-red heat in a small gas-flame. Its radiation, confined by a suitable diaphragm, fell on a rock-salt lens with a focus of 15 centims., which formed a well-defined image at a distance a little greater than 30 centims. With a fine prism of rock-salt, the rays were deviated and dispersed. The luminous portion of the spectrum was barely visible, and the calorific effects it produced of feeble intensity. The dark spectrum was at least as expanded as in the case

of the solar rays, and it exhibited a well-defined maximum; in the position relative to this maximum, the pile received so much heat that the deviation obtained often amounted to 20° . On the dark side the spectrum extended to a distance at least equal to that which separated the maximum from the luminous region. The whole of this second part of the spectrum was completely absorbable by a layer of water 2 millims. in thickness; and the rays forming the maximum were almost completely absorbed. Scarcely a third part of the rays escaped absorption, and it was necessary to approach the luminous region to rediscover well-marked transmissions. Thus in a spectrum formed by rays which have not had to traverse an absorbing layer of appreciable thickness, rays are found which are analogous to those ordinarily proceeding from the sides of heated enclosures.

The apparatus I used in these spectrum-studies enabled me to measure to a quarter of a degree the deviation of the pencil by the prism of rock-salt, which was always placed in the position in which the deviation of the red was a minimum. In making these measurements I soon ascertained that pencils of the same breadth, and the same mean refrangibility, were very differently absorbable by 2 millims. of water, according as I took them in the solar spectrum or in a spectrum formed by rays emanating from platinum. In this latter case the absorption is stronger than in the other.

The differences are very precise, and cannot be attributed to the presence of rays quite absorbable coming from the extremities of the spectrum and which, owing to a defective arrangement of the apparatus, were mixed with the rays to be operated upon. If, in fact, there were such a flux of absorbable rays, there ought to be another of rays much more transmissible coming from the most refracted portion of the spectrum, and the effects would almost balance each other. Moreover the following new experiment could leave no doubt. A moderator lamp was taken as source of heat, and a very pure spectrum was formed with the rays proceeding from it. In one of my experiments the deviation of the red rays was $40^{\circ}5$, that of the violet 43° ; and I obtained a few tenths of deviation by placing the index which supported my pile at a distance of 38° from the direct rays. When the alidade was $39^{\circ}9$ from the incident ray, the pile received no luminous ray, but was covered by a luminous band near the extreme red. A trough of fluor-spar containing a layer of water of 2 millims. thickness allowed about a third of the rays forming this band to pass. Thus, for instance, 33° of deviation were obtained with the direct, and 11° with the transmitted. Placing the index at $39^{\circ}2$ from the direction of the incident rays, strong deflections in the direct were still observed; but the transmissibility through the same trough was singularly diminished; it did not amount to more than $\frac{31}{100}$; that is, the mean deviation for the direct amounted to 19° , and for the transmitted to $3^{\circ}1$.

Having made these observations, I modified the nature of the rays by causing them, before falling on the prism, to pass through a layer of water of 0.003 metre in thickness contained between two glass plates. For the two positions which I have mentioned, the trans-

missibility through the trough of spar filled with water was remarkably increased; it became $\frac{4}{3}$ in the first case, and $\frac{1}{2}$ in the other.

From these numbers a complete demonstration of the propositions previously enunciated appears to follow. In fact, if the trough filled with water only allows a third of the rays near the extreme red to pass, and far less of the other and less refrangible obscure pencils, it is impossible to assume that a mixture of rays so little transmissible could form a pencil capable of being transmitted in the proportion of $\frac{1}{2}$ or $\frac{1}{3}$. Yet this consequence must be admitted if it be allowed that the trough of water only modifies the incident pencils in the degree that it clears them by extinguishing rays of mean refrangibility very different from those of the pencils investigated.

It seems to me made out that if in very fine spectra, but of different sources, we isolate two layers of rays of the same mean refrangibility, and the breadth of which (the same in both cases) is a very small portion of the total breadth of the spectrum, these two layers of rays may have very different properties.

Without discussing here the various interpretations which might be proposed of this fact, I will restrict myself to the remark that it may be completely explained by the data which spectrum-analysis furnishes. In fact, if in a solar spectrum, whether luminous or chemical, we consider a band the breadth of which is a fifteenth to a thirtieth of that of the spectrum, we know that this band is filled with a number of lines which correspond to rays that have been absorbed in passing through the solar or terrestrial atmosphere; and, though with considerable differences, absorbents conveniently chosen may produce analogous effects on spectra from terrestrial sources. Now the lines (the bands of absorption in question) being developed in a pencil of definite mean refrangibility, necessarily modify it, both as regards intensity and transmissibility, since obviously the rays which have disappeared were not the same as those which remained, though they had the same degree of refrangibility.

In concluding, I beg to mention a fact which is directly connected with the study of calorific spectra. A body raised to a red heat emits both dark and luminous calorific rays. If it be heated from the point at which it becomes distinctly luminous, is the increase in the energy of its radiation restricted to the luminous part alone, or does it extend to both at once? The simple fact that the maximum heat is greatest in the dark part of the solar spectrum seems to indicate that the second hypothesis is true. It may moreover be verified in the following manner. We take as source of heat a plate of platinum forming a side of a small gas-furnace, in which gradually higher temperatures may be obtained by increasing the pressure of the air. When the plate is at a distinct cherry-red, part of the rays it emits are isolated and dispersed by a prism. The pile, placed in the obscure part of the spectrum at a considerable distance from the luminous part, remains fixed as long as the pressure of the air injected is the same; if the pressure be increased, the plate passes from cherry to white, and at the same time the thermoscopic indication is greatly increased, although the pile cannot receive any luminous ray.—*Comptes Rendus*, August 3, 1868.

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[FOURTH SERIES.]

DECEMBER 1868.

LI. *On the Communication of Vibration from a Vibrating Body to a surrounding Gas.* By G. G. STOKES, M.A., D.C.L., Sec. R.S., Fellow of Pembroke College, and Lucasian Professor of Mathematics in the University of Cambridge*.

IN the first volume of the Transactions of the Cambridge Philosophical Society is a short paper by Professor John Leslie, "On Sounds excited in Hydrogen Gas," in which the author mentions some remarkable experiments indicating the singular incapacity of hydrogen for becoming the vehicle of the transmission of sound when a bell is struck in that gas, either pure or mixed with air. With reference to the most striking of his experiments, the author observes (p. 267), "The most remarkable fact is, that the admixture of hydrogen gas with atmospheric air has a predominant influence in blunting or stifling sound. If one half of the volume of atmospheric air be extracted [from the receiver of the air-pump], and hydrogen gas be admitted to fill the vacant space, the sound will now become scarcely audible."

No definite explanation of the results is given; but with reference to the feebleness of sound in hydrogen the author observes, "These facts, I think, depend partly on the tenuity of hydrogen gas, and partly on the rapidity with which the pulsations of sound are conveyed through this very elastic medium;" and he states that, according to his view, he "should expect the intensity of sound to be diminished 100 times, or in the compound ratio of its tenuity and of the square of the velocity with which it conveys the vibratory impressions." With reference to the effect of the admixture of hydrogen with air he says, "When

* From the Philosophical Transactions for 1868, Part II., having been read June 18, 1868.

hydrogen gas is mixed with common air, it probably does not intimately combine, but dissipates the pulsatory impressions before the sound is rigorously formed."

In referring to Leslie's experiment in which a half-exhausted receiver is filled up with hydrogen, Sir John Herschel suggests a possible explanation founded on Dalton's hypothesis that every gas acts as a vacuum towards every other*. According to this view there is a constant tendency for sound-waves to be propagated with different velocities in the air and hydrogen of which the mixture consists, but this tendency is constantly checked by the resistance which one gas opposes to the passage of another, calling into play something analogous to internal friction, whereby the sound-vibration, though at first produced, is rapidly stifled. Air itself indeed is a mixture; but the velocities of propagation of sound in nitrogen and oxygen are so nearly equal that the effect is supposed not to be sensible in this case.

This explanation never satisfied me, believing, as I always have done, for reasons which it would take too long here to explain, that for purely hydrodynamical phenomena (such as those of sound) an intimate mixture of gases was equivalent to a single homogeneous medium. I had some idea of repeating the experiment, thinking that possibly Leslie might not have allowed sufficient time for the gases to be perfectly mixed (though that did not appear likely), when another explanation occurred to me, which immediately struck me as being in all probability the true one.

In reading some years ago an investigation of Mr. Earnshaw's, in which a certain result relating to the propagation of sound in a straight tube was expressed in terms, among other things, of the velocity of propagation, the idea occurred to me that the high velocity of propagation of sound in hydrogen would account for the result of Leslie's experiment, though in a manner altogether different from anything relating to the propagation of sound in one dimension only.

Suppose a person to move his hand to and fro through a small space. The motion which is occasioned in the air is almost exactly the same as it would have been if the air had been an incompressible fluid. There is a mere local reciprocating motion, in which the air immediately in front is pushed forwards, and that immediately behind impelled after the moving body, while in the anterior space generally the air recedes from the encroachment of the moving body, and in the posterior space generally flows in from all sides, to supply the vacuum which tends to be created; so that in lateral directions the motion of the fluid is backwards, a portion of the excess of fluid in the front going to

* *Encyclopædia Metropolitana*, vol. iv. art. Sound, § 108.

supply the deficiency behind. Now conceive the periodic time of the motion to be continually diminished. Gradually the alternation of movement becomes too rapid to permit of the full establishment of the merely local reciprocating flow; the air is sensibly compressed and rarefied, and a sensible sound-wave (or wave of the same nature, in case the periodic time be beyond the limits suitable to hearing) is propagated to a distance. The same takes place in any gas; and the more rapid the propagation of condensations and rarefactions in the gas, the more nearly will it approach, in relation to the motions we have under consideration, to the condition of an incompressible fluid; the more nearly will the conditions of the displacement of the gas at the surface of the solid be satisfied by a merely local reciprocating flow.

This explanation, when once it suggested itself, seemed so simple and obvious that I could not doubt that it afforded the true mode of accounting for the phenomenon. It remained only to test the accuracy of the assigned cause by actual numerical calculation in some case or cases sufficiently simple to permit of precise analytical determination. The result of calculations of the kind applied to a sphere proved that the assigned cause was abundantly sufficient to account for the observed result. I have not hitherto published these results; but, as the phenomenon has not to my knowledge been satisfactorily explained by others, I venture to hope that the explanation I have to offer, simple as it is in principle, may not be unworthy of the notice of the Royal Society.

For the purpose of exact analytical investigation I have taken the two cases of a vibrating sphere and a long vibrating cylinder, the motion of the fluid in the latter case being supposed to be in two dimensions. The sphere is chosen as the best representative of a bell, among the few geometrical forms of body for which the problem can be solved. The cylinder is chosen as the representative of a vibrating string. In the case of the sphere the problem is identical with that solved by Poisson in his memoir "*Sur les mouvements simultanés d'un pendule et de l'air environnant*"*; but the solution is discussed with a totally different object in view, and is obtained from the beginning, to avoid the needless complexity introduced by taking account of the initial circumstances, instead of supposing the motion already going on.

A. Solution of the Problem in the case of a Vibrating Sphere.

Suppose an elastic solid, spherical externally in its undisturbed position, to vibrate in the manner of a bell, the amplitude of vibration being very small. Suppose it surrounded by a homo-

* *Mémoires de l'Académie des Sciences*, vol. xi. p. 521.

geneous gas, which is at rest except in so far as it is set in motion by the sphere; and let it be required to determine the motion of the gas in terms of that of the sphere supposed given. We may evidently for the purposes of the present problem suppose the gas not to be subject to the action of external forces.

Let the gas be referred to the rectangular axes of x, y, z , and let u, v, w be the components of the velocity. Since the gas is at rest except as to the disturbance communicated to it from the sphere, u, v, w are by a well-known theorem the partial differential coefficients with respect to x, y, z of a function ϕ of the co-ordinates; and if a^2 be the constant expressing the ratio of the small variations of pressure to the corresponding small variations of density, we must have

$$\frac{d^2\phi}{dt^2} = a^2 \left(\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} + \frac{d^2\phi}{dz^2} \right); \quad . \quad . \quad . \quad (1)$$

and if s be the small condensation,

$$s = -\frac{1}{a^2} \frac{d\phi}{dt}.$$

As we have to deal with a sphere, it will be convenient to refer the gas to polar coordinates r, θ, ω , the origin being in the centre. In terms of these coordinates, equation (1) becomes

$$\frac{d^2\phi}{dt^2} = a^2 \left\{ \frac{d^2\phi}{dr^2} + \frac{2}{r} \frac{d\phi}{dr} + \frac{1}{r^2 \sin \theta} \frac{d}{d\theta} \left(\sin \theta \frac{d\phi}{d\theta} \right) + \frac{1}{r^2 \sin^2 \theta} \frac{d^2\phi}{d\omega^2} \right\}; \quad (2)$$

and if u', v', w' be the components of the velocity along the radius vector and in two directions perpendicular to the radius vector, the first in and the second perpendicular to the plane in which θ is measured,

$$u' = \frac{d\phi}{dr}, \quad v' = \frac{1}{r} \frac{d\phi}{d\theta}, \quad w' = \frac{1}{r \sin \theta} \frac{d\phi}{d\omega}. \quad . \quad . \quad (3)$$

Let c be the radius of the sphere, and V the velocity of any point of its surface resolved in a direction normal to the surface, V being a given function of t, θ , and ω ; then we must have

$$\frac{d\phi}{dr} = V \text{ when } r=c. \quad . \quad . \quad . \quad (4)$$

Another condition, arising from what takes place at a great distance from the sphere, will be considered presently.

The sphere vibrating under the action of its elastic forces, its motion will be periodic, expressed so far as the time is concerned partly by the sine and partly by the cosine of an angle proportional to the time, suppose mat . Actually the vibrations will

slowly die away, in consequence partly of the imperfect elasticity of the sphere, partly of communication of motion to the gas; but for our present purpose this need not be taken into account. Moreover there will in general be a series of periodic disturbances coexisting, corresponding to different periodic times; but these may be considered separately. We might therefore assume

$$V = U \sin mat + U' \cos mat;$$

but it will materially shorten the investigation to employ an imaginary exponential instead of circular functions. If we take

$$V = Ue^{imat}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

where i is an abbreviation for $\sqrt{-1}$, and determine ϕ by the conditions of the problem, the real and imaginary parts of ϕ and V must satisfy all those conditions separately; and therefore we may take the real parts alone, or the coefficients of i or $\sqrt{-1}$ in the imaginary parts, or any linear combination of these even after having changed the arbitrary constants which enter into the expression of the motion of the sphere, as the solution of the problem, according to the way in which we conceive the given quantity V expressed.

The function ϕ will be periodic in a similar manner to V , so that we may take

$$\phi = \psi e^{imat}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

As regards the periodicity merely, we might have had a term involving e^{-imat} as well as that written above; but it will be readily seen that in order to satisfy the equation of condition (4) the sign of the index of the exponential in ϕ must be the same as in V .

On substituting in (2) the expression for ϕ given by (6), the factor involving t will divide, and we shall get for the determination of ψ a partial differential equation free from t . Now ψ may be expanded in a series of Laplace's Functions so that

$$\psi = \psi_0 + \psi_1 + \psi_2 + \dots \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

Substituting in the differential equation just mentioned, taking account of the fundamental equation

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

and equating to zero the sum of the Laplace's Functions of the same order, we find

$$\frac{d^2 \psi_n}{dr^2} + \frac{2}{r} \frac{d\psi_n}{dr} - \frac{n(n+1)}{r^2} \psi_n + m^2 \psi_n = 0.$$

This equation belongs to a known integrable form. The integral

is

$$\gamma\psi_n = u_n e^{-imr} \left\{ 1 + \frac{n(n+1)}{2 \cdot imr} + \frac{(n-1)n(n+1)(n+2)}{2 \cdot 4(imr)^2} + \dots \right\} \\ + u'_n e^{imr} \left\{ 1 - \frac{n(n+1)}{2 \cdot imr} + \frac{(n-1)n(n+1)(n+2)}{2 \cdot 4(imr)^2} - \dots \right\},$$

u_n and u'_n being evidently Laplace's Functions of the order n , since that is the case with ψ_n .

It will be convenient to take next the condition which has to be satisfied at a great distance from the sphere. When r is very large, the series within braces may be reduced to their first terms 1, and we shall have

$$r\phi = e^{im(at-r)} \Sigma u_n + e^{im(at+r)} \Sigma u'_n.$$

The first of these terms denotes a disturbance travelling outwards from the centre, the second a disturbance travelling towards the centre, the amplitude of vibration in both cases, for a given phase, varying inversely as the distance from the centre. In the problem before us there is no disturbance travelling towards the centre, and therefore $\Sigma u'_n = 0$, which requires that each function u'_n should separately be equal to zero. We have therefore simply

$$r\psi_n = u_n e^{-imr} \left\{ 1 + \frac{n(n+1)}{2 \cdot imr} + \frac{(n-1) \dots (n+2)}{2 \cdot 4(imr)^2} + \dots + \frac{1 \cdot 2 \cdot 3 \dots 2n}{2 \cdot 4 \cdot 6 \dots 2n(imr)^n} \right\}, \quad (8)$$

or, if we choose to reverse the series,

$$r\psi_n = u_n e^{-imr} \frac{1 \cdot 3 \cdot 5 \dots (2n-1)}{(imr)^n} \left\{ 1 + \frac{2n}{1 \cdot 2n} imr + \frac{(2n-2)2n}{1 \cdot 2(2n-1)2n} (imr)^2 \dots + \frac{2 \cdot 4 \cdot 6 \dots 2n}{1 \cdot 2 \cdot 3 \dots 2n} (imr)^n \right\} \quad (9).$$

Putting for shortness $f_n(r)$ for the multiplier of $u_n e^{-imr}$ in the right-hand member of (8) or (9), we shall have

$$\phi = \frac{1}{r} e^{im(at-r)} \Sigma u_n f_n(r).$$

It remains to satisfy the equation of condition (4). Put for shortness

$$\frac{d}{dr} \left\{ \frac{1}{r} e^{-imr} f_n(r) \right\} = - \frac{1}{r^2} e^{-imr} F_n(r),$$

so that

$$F_n(r) = (1 + imr)f_n(r) - rf'_n(r), \quad . \quad . \quad . \quad (10)$$

and suppose U expanded in a series of Laplace's Functions,

$$U_0 + U_1 + U_2 + \dots;$$

then substituting and equating the functions of the same order on the two sides of the equation, we have

$$U_n = -\frac{1}{c^2} e^{-imc} F_n(c) u_n,$$

and therefore

$$\phi = -\frac{c^2}{r} e^{im(at-r+c)} \sum \frac{U_n}{F_n(c)} f_n(r). \quad \dots \quad (11)$$

This expression contains the solution of the problem, and it remains only to discuss it.

At a great distance from the sphere the function $f_n(r)$ becomes ultimately equal to 1, and we have

$$\phi = -\frac{c^2}{r} e^{im(at-r+c)} \sum \frac{U_n}{F_n(c)}. \quad \dots \quad (12)$$

It appears from (3) that the component of the velocity along the radius vector is of the order r^{-1} , and that in any direction perpendicular to the radius vector of the order r^{-2} , so that the lateral motion may be disregarded except in the neighbourhood of the sphere:

In order to examine the influence of the lateral motion in the neighbourhood of the sphere, let us compare the actual disturbance at a great distance with what it would have been if all lateral motion had been prevented, suppose by infinitely thin conical partitions dividing the fluid into elementary canals, each bounded by a conical surface having its vertex at the centre.

On this supposition the motion in any canal would evidently be the same as it would be in all directions if the sphere vibrated by contraction and expansion of the surface the same all round and such that the normal velocity of the surface was the same as it is at the particular point in which the canal in question abuts on the surface. Now if U were constant the expansion of U would be reduced to its first term U_0 , and seeing that $f_0(r) = 1$ we should have from (11)

$$\phi = -\frac{c^2}{r} e^{im(at-r+c)} \frac{U_0}{F_0(c)}.$$

This expression will apply to any particular canal if we take U_0 to denote the normal velocity at the sphere's surface for that particular canal; and therefore to obtain an expression applicable at once to all the canals we have merely to write U for U_0 . To facilitate a comparison with (11) and (12) I shall, however, write

ΣU_n for U . We have then

$$\phi = -\frac{c^2}{r} e^{im(at-r+c)} \frac{\Sigma U_n}{F_0(c)} \quad . \quad . \quad . \quad (13)$$

It must be remembered that this is merely an expression applicable at once to all the canals, the motion in each of which takes place wholly along the radius vector; and accordingly the expression is not to be differentiated with respect to θ or ω with the view of applying the formula (3).

On comparing (13) with the expression for the function ϕ in the actual motion at a great distance from the sphere (12), we see that the two are identical, with the exception that U_n is divided by two different constants, namely $F_0(c)$ in the former case and $F_n(c)$ in the latter. The same will be true of the leading terms (or those of the order r^{-1}) in the expressions for the condensation and velocity*. Hence if the mode of vibration of the sphere is such that the normal velocity of its surface is expressed by a Laplace's Function of any one order, the disturbance at a great distance from the sphere will vary from one direction to another according to the same law as if lateral motion had been prevented, the amplitude of excursion at a given distance from the centre varying in both cases as the amplitude of excursion, in a normal direction, of the surface of the sphere itself. The only difference is that expressed by the symbolic ratio $F_n(c) : F_0(c)$. If we suppose $F_n(c)$ reduced to the form $\mu_n (\cos \alpha_n + \sqrt{-1} \sin \alpha_n)$, the amplitude of vibration in the actual case will be to that in the supposed case as μ_0 to μ_n , and the phases in the two cases will differ by $\alpha_0 - \alpha_n$.

If the normal velocity of the surface of the sphere be not expressible by a single Laplace's Function, but only by a series, finite or infinite, of such functions, the disturbance at a given great distance from the centre will no longer vary from one direction to another according to the same law as the normal velocity of the surface of the sphere, since the modulus μ_n and likewise the amplitude α_n of the imaginary quantity $F_n(c)$ vary with the order of the function.

Let us now suppose the disturbance expressed by a Laplace's Function of some one order, and seek the numerical value of the

* Of course it would be true if the *complete* differential coefficients with respect to r of the right-hand members of (12) and (13) were taken; but then the former does not give the velocity u' except as to its leading term, since (12) has been deduced from the exact expression (11) by reducing $f_n(r)$ to its first term 1; nor again is it true, except as to terms of the order r^{-1} , of the actual motion of the unimpeded fluid that the whole velocity is in the direction of the radius vector.

alteration of intensity at a distance, produced by the lateral motion which actually exists.

The intensity will be measured by the *vis viva* produced in a given time, and consequently will vary as the density multiplied by the velocity of propagation multiplied by the square of the amplitude of vibration. It is the last factor alone that is different from what it would have been if there had been no lateral motion. The amplitude is altered in the proportion of μ_0 to μ_n ; so that if

$$\frac{\mu_n^2}{\mu_0^2} = I_n,$$

I_n is the quantity by which the intensity which would have existed if the fluid had been hindered from lateral motion has to be divided.

For the first five orders the values of the function $F_n(c)$ are as follows:—

$$F_0(c) = imc + 1,$$

$$F_1(c) = imc + 2 + imc,$$

$$F_2(c) = imc + 4 + \frac{9}{imc} + \frac{9}{(imc)^2},$$

$$F_3(c) = imc + 7 + \frac{27}{imc} + \frac{60}{(imc)^2} + \frac{60}{(imc)^3},$$

$$F_4(c) = imc + 11 + \frac{65}{imc} + \frac{240}{(imc)^2} + \frac{525}{(imc)^3} + \frac{525}{(imc)^4}.$$

If λ be the length of the sound-wave corresponding to the period of the vibration, $m = \frac{2\pi}{\lambda}$; so that mc is the ratio of the circumference of the sphere to the length of a wave. If we suppose the gas to be air and λ to be 2 feet, which would correspond to about 550 vibrations in a second, and the circumference $2\pi c$ to be 1 foot (a size and pitch which would correspond with the case of a common house bell), we shall have $mc = \frac{1}{2}$. The following Table gives the values of the square of the modulus and of the ratio I_n for the functions $F_n(c)$ of the first five orders, for each of the values $4, 2, 1, \frac{1}{2},$ and $\frac{1}{4}$ of mc . It will presently appear why the Table has been extended further in the direction of values greater than $\frac{1}{2}$ than it has in the opposite direction. Five significant figures at least are retained.

$mc.$	$n=0.$	$n=1.$	$n=2.$	$n=3.$	$n=4.$	
4	17	16.25	14.879	13.848	20.177	Values of μ_n^2
2	5	5	9.3125	80	1495.8	
1	2	5	89	3965	300137	
0.5	1.25	16.25	1330.2	236191	72086371	
0.25	1.0625	64.062	20878	14837899	18160 $\times 10^6$	
4	1	0.95588	0.87523	0.81459	1.1869	Values of I_n
2	1	1	1.8625	16	299.16	
1	1	2.5	44.5	1982.5	150068	
0.5	1	13	1064.2	188953	57669097	
0.25	1	60.294	19650	13965 $\times 10^3$	17092 $\times 10^6$	

When $mc = \infty$ we get from the analytical expressions $I_n = 1$. We see from the Table that when mc is somewhat large I_n is liable to be a *little* less than 1, and consequently the sound to be a *little* more intense than if lateral motion had been prevented. The possibility of this is explained by considering that the waves of condensation spreading from those compartments of the sphere which at a given moment are vibrating positively, *i. e.* outwards, after the lapse of a half period may have spread over the neighbouring compartments, which are now in their turn vibrating positively, so that these latter compartments in their outward motion work against a somewhat greater pressure than if each compartment had opposite to it only the vibration of the gas which it had itself occasioned; and the same explanation applies *mutatis mutandis* to the waves of rarefaction. However, the increase of sound thus occasioned by the existence of lateral motion is but small in any case, whereas when mc is somewhat small I_n increases enormously, and the sound becomes a mere nothing compared with what it would have been had lateral motion been prevented.

The higher the order of the function, the greater will be the number of compartments, alternately positive and negative as to their mode of vibration at a given moment, into which the surface of the sphere will be divided. We see from the Table that for a given periodic time as well as radius the value of I_n becomes considerable when n is somewhat high. However, practically vibrations of this kind are produced when the elastic sphere executes, not its principal, but one of its subordinate vibrations, the pitch corresponding to which rises with the order of the vibration, so that m increases with that order. It was for this reason that the Table was extended from $mc = 0.5$ further in the direction of high pitch than low pitch, namely, to three octaves higher and only one octave lower.

When the sphere vibrates symmetrically about the centre, *i. e.* so that any two opposite points of the surface are at a given

moment moving with equal velocities in opposite directions, or (more generally) when the mode of vibration is such that there is no change of position of the centre of gravity of the volume, there is no term of the order 1. For a sphere vibrating in the manner of a bell the principal vibration is that expressed by a term of the order 2, to which I shall now more particularly attend.

Putting, for shortness, $m^2c^2=q$, we have

$$\mu_0^2 = q + 1, \quad \mu_2^2 = (q^{\frac{1}{2}} - 9q^{-\frac{1}{2}})^2 + \left(4 - \frac{9}{q}\right)^2 = q - 2 + \frac{9}{q} + \frac{81}{q^2},$$

$$I_2 = \frac{q^3 - 2q^2 + 9q + 81}{q^2(q+1)}.$$

The minimum value of I_2 is determined by

$$q^3 - 6q^2 - 84q - 54 = 0,$$

giving approximately

$$q = 12.859, \quad mc = 3.586, \quad \mu_0^2 = 13.859, \quad \mu_2^2 = 12.049,$$

$$I_2 = .86941;$$

so that the utmost increase of sound produced by lateral motion amounts to about 15 per cent.

I come now more particularly to Leslie's experiments. Nothing is stated as to the form, size, or pitch of his bell; and even if these had been accurately described, there would have been a good deal of guesswork in fixing on the size of the sphere which should be considered the best representative of the bell. Hence all we can do is to choose such values for m and c as are comparable with the probable conditions of the experiment.

I possess a bell, belonging to an old bell-in-air apparatus, which may probably be somewhat similar to that used by Leslie. It is nearly hemispherical, the diameter is 1.96 inch, and the pitch an octave above the middle C of a piano. Taking the number of vibrations 1056 per second, and the velocity of sound in air 1100 feet per second, we have $\lambda = 12.5$ inches. To represent the bell by a sphere of the same radius would be very greatly to underrate the influence of local circulation, since near the mouth the gas has but a little way to get round from the outside to the inside, or the reverse. To represent it by a sphere of half the radius would still apparently be to underrate the effect. Nevertheless for the sake of rather underestimating than exaggerating the influence of the cause here investigated, I will make these two suppositions successively, giving respectively $c = .98$ and $c = .49$, $mc = .4926$ and $mc = .2463$ for air.

If it were not for lateral motion the intensity would vary from gas to gas in the proportion of the density into the velocity

of propagation, and therefore as the pressure into the square root of the density under a standard pressure, if we take the factor depending on the development of heat as sensibly the same for the gases and gaseous mixtures with which we have to deal. In the following Table the first column gives the gas, the second the pressure p , in atmospheres, the third the density D under the pressure p , referred to the density of air at the atmospheric pressure as unity, the fourth, Q_r , what would have been the intensity had the motion been wholly radial, referred to the intensity in air at atmospheric pressure as unity, or, in other words, a quantity varying as $p \times$ (the density at pressure 1)¹. Then follow the values of q , I_2 , and Q , the last being the actual intensity referred to air as before.

Gas.	p .	D .	Q_r .	$c=.98$.			$c=.49$.		
				q .	I_2 .	Q .	q .	I_2 .	Q .
Air	1	1	1	.2427	1136	1	.06067	20890	1
Hydrogen	1	.0690	.2627	.01674	284700	.001048	.004186	4604000	.001191
Air, rarefied	.01	.01	.01	.2427	1136	.01	.06067	20890	.01
Air filled with H....	1	.0783	.2798	.0190	220600	.001440	.004751	3572000	.001637
Air of same density.	.0783	.0783	.0783	.2427	1136	.0783	.06067	20890	.0783
Air rarefied $\frac{1}{2}$	.5	.5	.5	.2427	1136	.5	.06067	20890	.5
Air filled with H....	1	.5345	.7311	.1297	4322	.1921	.0324	74890	.2039

An inspection of the numbers contained in the columns headed Q will show that the cause here investigated is amply sufficient to account for the facts mentioned by Leslie.

It may be noticed that while q is 4 times as small, and I_2 is 16 or 18 times as large, for $c=.49$ as for $c=.98$, there is no great difference in the values of Q in the two cases for hydrogen and mixtures of hydrogen and air in given proportions. This arises from the circumstance that q is sufficiently small to make the last terms in μ_0^2 and μ_2^2 , namely, 1 and $81q^{-2}$, the most important, so that I_n does not greatly differ from $81q^{-2}$. If this result had been exact instead of approximate, the intensity in different gases, supposed for simplicity to be at a common pressure, would have varied as $D^{\frac{1}{2}}$; and it will be found that for the cases in which $p=1$ the values of Q in the above Table, especially those in the last column, do not greatly deviate from this proportion. But the simplicity of this result depends on two things. First, the vibration must be expressed by a Laplace's Function of the order 2; for a different order the power of D would have been different; and this is just one of the points respecting which we cannot infer what would be true of a bell of the ordinary shape from what we have proved for a sphere.

Secondly, the radius must be sufficiently small, or the pitch sufficiently low, to make q small; at the other extremity of the scale, in which c is supposed to be very large, or λ very small, Q varies nearly as $D^{\frac{1}{2}}$ instead of $D^{\frac{2}{3}}$, whatever be the order of the Laplace's Function. Hence no simple relation can be expected between the numbers furnished by experiment and the numerical constants of the gas in such experiments as those of M. Perolle*, in which the same bell was rung in succession in different gases.

B. Solution of the Problem in the case of a Vibrating Cylinder.

I will here suppose the motion to be in two dimensions only. In the case of a vibrating string, which I have mainly in view, it is true that the amplitude of excursion of the string varies sensibly on proceeding even a moderate distance along it, and that the propagation of the sound-wave produced by no means takes place in two dimensions only. But the question how far a sound-wave is produced at all, and how far the displacement of the gas by the cylinder merely produces a local motion to and fro, is decided by what takes place in the immediate neighbourhood of the string, such as within a distance of a few diameters; and though the sound-wave, when once produced, in its subsequent progress diverges in three dimensions, the same takes place with the hypothetical sound-wave which would be produced if lateral motion were prevented; so that the comparison which it is the object of the investigation to institute is not affected thereby.

Assuming, then, the motion to be in two dimensions, and referring the fluid to polar coordinates, r, θ , r being measured from the axis of the undisturbed cylinder, we shall have for the fundamental equation derived from (1),

$$\frac{d^2\phi}{dt^2} = a^2 \left\{ \frac{d^2\phi}{dr^2} + \frac{1}{r} \frac{d\phi}{dr} + \frac{1}{r^2} \frac{d^2\phi}{d\theta^2} \right\}; \quad \dots \quad (14)$$

and if u', v' be the components of the velocity along and perpendicular to the radius vector,

$$u' = \frac{d\phi}{dr} \quad v' = \frac{1}{r} \frac{d\phi}{d\theta}.$$

If c be the radius of the cylinder, and V the normal component of the velocity of the surface of the cylinder, we must have

$$\frac{d\phi}{dr} = V \text{ when } r = c.$$

As before, I will suppose the motion of the cylinder, and consequently of the fluid, to be regularly periodic; but instead of

* *Mémoires de l'Académie des Sciences de Turin*, vol. iii. (1786-7), *Mém. des Correspondans*, p. 1.

using circular functions directly, I will employ the imaginary exponential e^{imat} , i denoting as before $\sqrt{-1}$, and will put accordingly $V = e^{imat}U$, U being a given function of θ , and $\phi = \psi e^{imat}$. For a given value of r , ψ may by a known theorem be developed in a series of sines and cosines of θ and its multiples, and therefore for general values of r can be so developed, the coefficients being functions of r . If ψ_n be the coefficient of $\cos n\theta$ or $\sin n\theta$, we find

$$\frac{d^2\psi_n}{dr^2} + \frac{1}{r} \frac{d\psi_n}{dr} - \frac{n^2}{r^2} \psi_n + m^2 \psi_n = 0. \quad (15)$$

If we suppose the normal velocity of the surface of the cylinder to vary in a given manner from one generating line to another, so that U is a given function of θ , we may expand U in a series of the form

$$U = U_0 + U_1 \cos \theta + U_2 \cos 2\theta + \dots \\ + U'_1 \sin \theta + U'_2 \sin 2\theta + \dots$$

On applying now the equation of condition which has to be satisfied at the surface of the cylinder, we see that a term $U_n \cos n\theta$ or $U'_n \sin n\theta$ of the n th order in the expression for U will introduce a function ψ_n of the same order in the general expression for ϕ . Now the only case of interest relating to an infinite cylinder is that of a vibrating string, in which the cylinder moves as a whole. The vibration may be regarded as compounded of the vibrations in any two rectangular planes passing through the axis, the phases of the component vibrations, it may be, being different. These component vibrations may be treated separately, and thus it will suffice to suppose the vibration confined to one plane, which we may take to be that from which θ is measured. We shall accordingly have

$$U = U_1 \cos \theta,$$

U_1 being a given constant; and the only function ψ_n which will appear in the general expression for ϕ will be that of the order 1. Besides this we shall have to investigate, for the sake of comparison, an ideal vibration in which the cylinder alternately contracts and expands in all directions alike, and for which accordingly U is a constant U_0 . Hence the equation (15) need only be considered for the values 0 and 1 of n .

For general values of n the equation (15) is easily integrated in the form of infinite series according to ascending powers of r . The result is

$$\psi_n = A r^n \left\{ 1 - \frac{m^2 r^2}{2(2+2n)} + \frac{m^4 r^4}{2 \cdot 4(2+2n)(4+2n)} - \dots \right\} \\ + B r^{-n} \left\{ 1 - \frac{m^2 r^2}{2(2-2n)} + \frac{m^4 r^4}{2 \cdot 4(2-2n)(4-2n)} - \dots \right\} \quad (16)$$

When n is any integer the integral as it stands becomes illusory; but the complete integral, which in this case assumes a special form, is readily obtained as a limiting case of the complete integral for general values of n .

The series in (16) are convergent for any value of r however great, but they give us no information of what becomes of the functions for very large values of r .

When r is very large, the equation (15) becomes approximately

$$\frac{d^2\psi_n}{dr^2} + m^2\psi_n = 0,$$

the integral of which is $\psi_n = R e^{-imr} + R' e^{imr}$, where R and R' are constant. This suggests putting the complete integral of (15) under the same form, R and R' being now functions of r , which, when r is large, vary but slowly, that is, remain nearly constant when r is altered by only a small multiple of λ . Assuming for R and R' series of the form $A r^\alpha + B r^\beta + C r^\gamma \dots$, where $\alpha, \beta, \gamma \dots$ are in decreasing order algebraically, and determining the indices and coefficients so as to satisfy (15), we get for another form of the complete integral

$$\left. \begin{aligned} \psi_n = & C(imr)^{-\frac{1}{2}} e^{-imr} \left\{ 1 - \frac{1^2 - 4n^2}{1 \cdot 8imr} + \frac{(1^2 - 4n^2)(3^2 - 4n^2)}{1 \cdot 2(8imr)^2} \right. \\ & \left. - \frac{(1^2 - 4n^2)(3^2 - 4n^2)(5^2 - 4n^2)}{1 \cdot 2 \cdot 3(8im)^3} + \dots \right\} \\ & + D(imr)^{-\frac{1}{2}} e^{imr} \left\{ 1 + \frac{(1^2 - 4n^2)}{1 \cdot 8imr} + \frac{(1^2 - 4n^2)(3^2 - 4n^2)}{1 \cdot 2(8imr)^2} \right. \\ & \left. + \frac{(1^2 - 4n^2)(3^2 - 4n^2)(5^2 - 4n^2)}{1 \cdot 2 \cdot 3(8imr)^3} + \dots \right\} \end{aligned} \right\} \quad (17)$$

These series, though ultimately divergent, begin by converging rapidly when r is large, and may be employed with great advantage when r is large, if we confine ourselves to the converging part. Moreover we have at once $D=0$ as the condition to be satisfied at a great distance from the cylinder. If mc were large we might employ the second form of integral in satisfying the condition at the surface of the cylinder, and the problem would present no further difficulty. But practically in the case of vibrating strings mc is a very small fraction, the series (16) are rapidly convergent, and the series (17) cannot be employed. To complete the solution of the problem, therefore, it is essential to express the constants A and B in terms of C and D , or at any rate to find the relation between A and B imposed by the condition $D=0$.

This may be effected by means of the complete integral of (15)

expressed in the form of a definite integral. For $n=0$ we know that

$$\psi_0 = \int_0^{\frac{\pi}{2}} \{E + F \log(r \sin^2 \zeta)\} \cos(mr \cos \zeta) d\zeta. \quad (18)$$

is a third form of the integral of (15). It is not difficult to deduce from this the integral of (15) in a similar form for any integral value of n . Assuming

$$\psi_n = r^n \int r^\beta \chi_n dr,$$

and substituting in (15), we have

$$r^{\alpha+\beta} \frac{d\chi_n}{dr} + (2\alpha + \beta + 1)r^{\alpha+\beta-1} \chi_n + (\alpha^2 - n^2)r^{\alpha-2} \int r^\beta \chi_n dr + m^2 r^\alpha \int r^\beta \chi_n dr = 0.$$

Assume

$$\alpha^2 - n^2 = 0, \quad (19)$$

divide the equation by r^α , differentiate with respect to r , and then divide by r^β . The result is

$$\frac{d^2 n \chi_n}{dr^2} + \frac{2\alpha + 2\beta + 1}{r} \frac{d\chi_n}{dr} + (2\alpha + \beta + 1)(\beta - 1) \frac{\chi_n}{r^2} + m^2 \chi_n = 0.$$

This equation will be of the same form as (15), provided

$$\alpha + \beta = 0,$$

which reduces the coefficient of the last term but one to $-(\alpha + 1)^2$. In order that this coefficient may be increased we must choose the positive root of (19), namely n , which I will suppose positive. Hence

$$\psi_n = r^n \int r^{-n} \chi_n dr \quad (20)$$

gives

$$\frac{d^2 \chi_n}{dr^2} + \frac{1}{r} \frac{d\chi_n}{dr} - \frac{(n+1)^2}{r^2} \chi_n + m^2 \chi_n = 0,$$

the same equation as that for the determination of ψ_{n+1} . Hence, expressing χ_n in terms of ψ_n from (20), writing $n-1$ for n , and replacing χ_{n-1} by ψ_n , we have

$$\psi_n = r^{n-1} \frac{d}{dr} r^{-(n-1)} \psi_{n-1},$$

a formula of reduction which, when n is integral, serves to express ψ_n in terms of ψ_0 . We have

$$\psi_n = r^n \left(\frac{1}{r} \frac{d}{dr} \right)^n \psi_0 \quad (21)$$

an equation which when applied to (18) gives the complete inte-

gral of (15) for integral values of n in the form of a definite integral.

Let us attend now more particularly to the case of $n=0$. The equation (16) is of the form $\psi_n = A f(n) + B f(-n)$, $f(n)$ containing r as well as n . Expanding by Maclaurin's theorem, we have

$$\psi_n = (A+B)f(0) + (A-B)f'(0)n + (A+B)f''(0)\frac{n^2}{1.2} + \dots$$

Writing A for $A+B$, $n^{-1}B$ for $A-B$, and then making n vanish, we have

$$\psi_0 = A f(0) + B f'(0),$$

or

$$\psi_0 = (A+B \log r) \left\{ 1 - \frac{m^2 r^2}{2^2} + \frac{m^4 r^4}{2^2 4^2} - \frac{m^6 r^6}{2^2 4^2 6^2} + \dots \right\} + B \left\{ \frac{m^2 r^2}{2^2} S_1 - \frac{m^4 r^4}{2^2 4^2} S_2 + \frac{m^6 r^6}{2^2 4^2 6^2} S_3 - \dots \right\} \quad (22)$$

where

$$S_n = 1^{-1} + 2^{-1} + 3^{-1} \dots + n^{-1}.$$

The integral in the form (17) assumes no peculiar shape when n is integral, and we have at once

$$\psi_0 = C(imr)^{-\frac{1}{2}} e^{-imr} \left\{ 1 - \frac{1^2}{1.8mr} + \frac{1^2 3^2}{1.2(8mr)^2} - \frac{1^2 3^2 5^2}{1.2.3(8imr)^3} + \dots \right\} + D(imr)^{-\frac{1}{2}} e^{imr} \left\{ 1 + \frac{1^2}{1.8imr} + \frac{1^2 3^2}{1.2(8imr)^2} - \frac{1^2 3^2 5^2}{1.2.3(8imr)^3} + \dots \right\} \quad (23)$$

I have explained at length the mode of dealing with such functions, and especially of connecting the arbitrary constants in the ascending and descending series, in two papers published in the Transactions of the Cambridge Philosophical Society*, in the second of which the connexion of the constants is worked out in this very example. To these I will refer, merely observing that while it is perfectly easy to connect A , B with E , F , the connexion of C , D with E , F involves some extremely curious points of analysis. The result of eliminating E , F be-

* "On the Numerical Calculation of a Class of Definite Integrals and Infinite Series," vol. ix. p. 166, and "On the Discontinuity of Arbitrary Constants which appear in Divergent Developments," vol. x. p. 105. A supplement to the latter paper has recently been read before the Cambridge Philosophical Society.

tween the two equations connecting A, B with E, F and the two connecting C, D with E, F is given, except as to notation, in the two equations (41) of my second paper. To render the notation identical with that of the former paper, it will be sufficient to write $A - B \log(im) + B \log(imr)$ for $A + B \log r$, and x for imr . The equations referred to may be simplified by the introduction of Euler's constant γ , the value of which is $\cdot 57721566$ &c., since it is known that

$$\pi^{-1} \Gamma'(\frac{1}{2}) + \log 4 + \gamma = 0,$$

$\Gamma'(x)$ denoting the derivative of the function $\Gamma(x)$. Putting

$$A - B \log im = A',$$

we have by equations (41) of the second paper referred to

$$C = (2\pi)^{-\frac{1}{2}} \{iA' + [(\log 2 - \gamma)i - \pi]B\}, \quad \dots \quad (24)$$

$$D = (2\pi)^{-\frac{1}{2}} \{A' + (\log 2 - \gamma)B\}, \quad \dots \quad (25)$$

i being written for $\sqrt{-1}$. It is shown in that paper that these values of C, D hold good when the amplitude of the imaginary variable x lies between the limits 0 and π , or that of r (supposed to be imaginary) between the limits $-\frac{\pi}{2}$ and $\frac{\pi}{2}$, but in crossing either of these limits one or other of the constants C, D is changed. In the investigation of the present paper r is of course real.

We have now

$$A' = A - B \log im = (\gamma - \log 2)B$$

for the relation between A and B arising from the condition that the motion is propagated outwards from the cylinder; and substituting in (22), we have for the value of N_0 subject to this condition

$$\psi_0 = B \left(\gamma + \log \frac{imr}{2} \right) \left(1 - \frac{m^2 r^2}{2^2} + \frac{m^4 r^4}{2^2 4^2} - \dots \right) + B \left(\frac{m^2 r^2}{2^2} S_1 - \frac{m^4 r^4}{2^2 4^2} S_2 + \dots \right); \quad (26)$$

or expressed by means of the descending series,

$$\psi_0 = -B \left(\frac{\pi}{2imr} \right)^{\frac{1}{2}} e^{-imr} \left\{ 1 - \frac{1^2}{1 \cdot 8imr} + \frac{1^2 3^2}{1 \cdot 2 (8imr)^2} - \frac{1^2 3^2 5^2}{1 \cdot 2 \cdot 3 (8imr)^3} + \dots \right\}. \quad (27)$$

We have from (21)

$$\psi_1 = \frac{d\psi_0}{dr},$$

from which the complete integral of (15) for $n=1$ may be got from that for $n=0$. In the form (17) of the integral the parts arising from differentiation of the parts containing e^{-imr} and e^{imr} respectively will contain those same exponentials; and therefore the complete integral of (15) for $n=1$, subject to the condition that the part containing e^{imr} shall disappear, will be got by differentiating the complete integral for $n=0$ subject to that same condition. The form of the integral in the shape of a descending series is given at once by (17). Hence we get by differentiating (26) and (27), and at the same time changing the arbitrary constant by writing $B_1 m^{-1}$ for B ,

$$\psi_1 = \frac{B}{mr} \left\{ 1 - \frac{m^2 r^2}{2^2} + \frac{m^4 r^4}{2^2 \cdot 4^2} - \dots \right\} - B_1 \left(\gamma + \log \frac{imr}{2} \right) \left(\frac{mr}{2} - \frac{m^3 r^3}{2^2 \cdot 4} + \frac{m^5 r^5}{2^2 \cdot 4^2 \cdot 6} - \dots \right) + B_1 \left(\frac{mr}{2} S_1 - \frac{m^3 r^3}{2^2 \cdot 4} S_2 + \frac{m^5 r^5}{2^2 \cdot 4^2 \cdot 6} S_3 - \dots \right) \quad (28)$$

$$\psi_1 = B_1 \left(\frac{\pi i}{2mr} \right)^{\frac{1}{2}} e^{-imr} \left\{ 1 - \frac{1 \cdot 3}{1 \cdot 8mr} + \frac{-1 \cdot 1 \cdot 3 \cdot 5}{1 \cdot 2 (8mr)^2} - \frac{-1 \cdot 1 \cdot 3^2 \cdot 5 \cdot 7}{1 \cdot 2 \cdot 3 (8mr)^3} + \dots \right\} \quad (29)$$

To determine the arbitrary constants B_1 and B , the first belonging to the actual motion, the second to the motion which would take place if the fluid were confined by an infinite number of planes passing through the axis, we must have, as before, for $r=c$,

$$\frac{d\psi_0}{dr} = U, \quad \frac{d\psi_1}{dr} = U,$$

whence

$$\frac{cU_1}{B} = 1 - \frac{m^2 c^2}{2^2} + \frac{m^4 c^4}{2^2 \cdot 4^2} - \dots - \left(\gamma + \log \frac{mc}{2} + i \frac{\pi}{2} \right) \left(\frac{m^2 c^2}{2} - \frac{m^4 c^4}{2^2 \cdot 4} + \dots \right) + \frac{m^2 c^2}{2} S_1 - \frac{m^4 c^4}{2^2 \cdot 4} S_2 + \frac{m^6 c^6}{2^2 \cdot 4^2 \cdot 6} S_3 - \dots = F_0(mc) + i \frac{\pi}{2} f_0(mc), \text{ suppose; } \quad (30)$$

$$\left. \begin{aligned} \frac{cU_1}{B_1} = & -\frac{1}{mc} - \frac{3mc}{2^2} + \frac{7m^3c^3}{2^2 \cdot 4^2} - \frac{11m^5c^5}{2^2 \cdot 4^2 \cdot 6^2} + \dots \\ & - \left(\gamma + \log \frac{mc}{2} + i \frac{\pi}{2} \right) \left(\frac{mc}{2} - \frac{3m^3c^3}{2^2 \cdot 4} + \dots \right) \\ & + \frac{mc}{2} S_1 - \frac{3m^3c^3}{2^2 \cdot 4} S_2 + \frac{5m^5c^5}{2^2 \cdot 4^2 \cdot 6} S_3 - \dots \\ = & \frac{1}{mc} \left\{ F_1(mc) + i \frac{\pi}{2} f_1(mc) \right\}, \text{ suppose.} \end{aligned} \right\} \quad (31)$$

If I be the ratio of the intensities at a distance in the supposed and in the actual case, we see from (30) and (31) that I will be equal to the ratio of the squares of the moduli of B and B_1 , and we shall therefore have

$$I = \frac{\{4F_1(mc)\}^2 + \pi^2 \{f_1(mc)\}^2}{m^2 c^2 [4\{F_0(mc)\}^2 + \pi^2 \{f_0(mc)\}^2]}. \quad (32)$$

For a piano-string corresponding to the middle C , c may be about $\cdot 02$ inch, and λ is about 25 inches. This gives $mc = \cdot 005027$. For such small values of mc I does not sensibly differ from $(mc)^{-2}$, which in the present case is 39571, so that the sound is nearly 40000 times as weak as it would have been if the motion of the particles of air had taken place in planes passing through the axis of the string. This shows the vital importance of sounding-boards in stringed instruments. Although the amplitude of vibration of the particles of the sounding-board is extremely small compared with that of the particles of the string, yet, as it presents a broad surface to the air, it is able to excite loud sonorous vibrations, whereas, were the string supported in an absolutely rigid manner, the vibrations which it could excite directly in the air would be so small as to be almost or altogether inaudible.

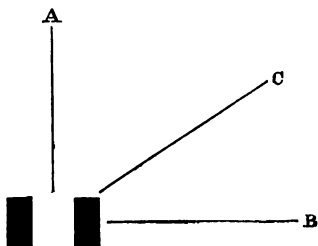
I may here mention a phenomenon which fell under my notice, and which is readily explained by the principles laid down in this paper. As I was walking one windy day on a road near Cambridge, on the other side of which ran a line of telegraph, my attention was attracted by a peculiar sound of extremely high pitch, which seemed to come from the opposite side of the road. On going over to ascertain the cause, I found that it came directly through the air from the telegraph-wires. On standing near a telegraph-post, the ordinary comparatively bass sound with which we are so familiar was heard, appearing to emanate from the post. On receding from the post the bass sound became feebler, and midway between two posts was quite inaudible. Nothing was then heard but the peculiar high-pitched sound, which appeared to emanate from the wires overhead. It had a peculiar metallic ring about it which the ear distinguished from the whistling of the wind in the twigs of a bush. Although the

telegraph ran for miles, it was only at one spot that the peculiar sound was noticed, and even there only in certain states of the wind. The wires seemed to be less curved than usual at the place in question, from which it may be inferred that they were there subject to an unusually great tension.

The explanation of the phenomenon is easy after what precedes. The wires were thrown into vibration by the wind, and a number of different vibrations, having different periodic times, coexisted. As regards the vibrations of comparatively long period, the air around the wires behaved nearly like an incompressible fluid, no sonorous vibrations of sensible amount were produced. These vibrations of the wires, however, were communicated to the posts, which being broad acted as sounding-boards, and thus sonorous vibrations of corresponding period were indirectly excited in the air. But as regards the vibrations of extremely short periodic time, the wires in spite of their narrowness were able, by acting directly on the air, to produce condensations and rarefactions of sensible amount.

The diameter of the telegraph-wire was about $\cdot 166$ inch; and if we take the C below the middle C of a piano for the representative of the pitch of the lower note, and a note five octaves higher for that of the higher, we have in the first case $\lambda = 50$ inches nearly, and in the second $\lambda = 50 \times 2^{-5}$, giving in the former case $mc = \cdot 01043$, and in the latter $mc = \cdot 3338$. The former of these values is so small that we may take $I = (mc)^{-2}$; in the latter case the formula (32) gives for I a value a little less than $(mc)^{-2}$. We find in the two cases $I = 9192$ and $I = 7\cdot 202$ respectively; so that in the former case the sound is more than 9000 times as feeble as that corresponding to the amplitude of vibration of the wire on the supposition of the absence of lateral motion, whereas in the latter case the actual intensity is nearly one-seventh of the full intensity corresponding to the amplitude.

The increase of sound produced by the stoppage of lateral motion may be prettily exhibited by a very simple experiment. Take a tuning-fork, and holding it in the fingers after it has been made to vibrate, place a sheet of paper or the blade of a broad knife with its edge parallel to the axis of the fork, and as near to the fork as conveniently may be without touching. If the plane of the obstacle coincide with either of the planes of symmetry of the fork, as represented in section at A or B, no effect is produced; but if it be placed in an intermediate position, such as C, the sound becomes much stronger.



LII. *Notes on the Chemical Geology of the Gold-fields of California.* By J. ARTHUR PHILLIPS.

[Concluded from p. 336.]

Hot Springs.

HOT and boiling springs are exceedingly numerous throughout California; and considerable accumulations of sulphur, together with evidences of extensive solfatara action, are met with in different sections of the State. To attempt even a mere enumeration of all the various localities where these phenomena have been observed would occupy more space than it is my intention to devote to the subject; and I shall therefore confine myself to a description of two or three of the more remarkable examples, selecting such only as appear to have the most direct bearing on some of the chemical and geological changes which have been produced, and are, to a certain extent, still going on in this portion of the American continent.

Borax Lake.—Lying about a mile beyond the ridge which borders Borax Lake on the north-east is a locality in which solfatara action is still exceedingly active, and where a large amount of sulphur has accumulated. This is called the "Sulphur Bank," and is of some six or seven acres in extent. It consists of a much decomposed volcanic rock traversed by innumerable fissures, through which steam and gas are constantly issuing, and over and through which large quantities of sulphur have been deposited in such a way that, at a little distance, the whole mass appears to consist of this substance. Into some of the cavities a pole may be inserted for several feet; and they are often lined with stalactites and beautiful crystallizations of sulphur.

This sulphur is being constantly deposited, and its deposition is attended by the evolution of aqueous vapour, carbonic and boracic acids; but this apparently takes place without the emission of sulphuretted hydrogen. The gaseous matters issuing from the crevices have usually a temperature of about 95° F., and appear to be the agency by which various mineral substances now deposited in the cavities were brought to the surface. Sulphur is deposited on the sides of the various fissures, either in groups of crystals, as stalactites, or in translucent amorphous masses of a beautiful yellow colour. It is sometimes intermixed with cinnabar, but more frequently with minute crystals of iron pyrites, and with pulverulent silica, blackened by the presence of a hydrocarbon having the appearance of coal-tar. With these, according to Dr. Oxland, are found silver and traces of gold*.

* Mining and Metallurgy of Gold and Silver, by J. Arthur Phillips, p. 11. E. and F. N. Spon: London.

On the sides of the cavities gelatinous silica is found coating chalcedony and opalescent silica in various stages of formation, from the gelatinous state to that of the hardest opal. This indurated silica is sometimes colourless, but is more frequently permeated by cinnabar and iron pyrites, or blackened by the tarry matter before referred to. Cinnabar is also found in striæ, and occasionally even in veins and concretionary masses of considerable thickness; where the bituminous matter occurs in the largest quantity, and the mass consequently becomes black and friable, cinnabar is replaced by globules of metallic mercury.

On the edge of Clear Lake near the sulphur bank is a hot spring, of which the outlet, even when the water is low, is partially beneath the lake, so that the amount which flows from it cannot be ascertained. Hot water, however, rises through the sand at various points, extending over a considerable area; and Mr. Moore, of San Francisco, who analyzed the water from this locality, has found that, in addition to chloride of sodium and carbonate of soda, it contains a large quantity of borax, and above a hundred grains of ammoniacal salts per gallon. Lying immediately at the foot of the sulphur bank are several springs of cold water, some of which contain carbonate and sulphate of soda, whilst others yield boracic acid. All these springs evolve large quantities of carbonic acid.

The chalcedony and semi-opal from this place, when examined under the microscope, are seen to enclose cubical pyrites, together with crystalline cinnabar; but the latter mineral appears to have been generally deposited in an amorphous state. Specimens of chalcedony taken from the fissures in the sulphur bank were, when first broken, so extremely soft as to readily receive an impression of the nail; but on reaching England they had become hardened, and had assumed the ordinary characteristics of that mineral.

Thin sections of these specimens showed a structure like that of fortification-agate, and were traversed by numerous fissures parallel to lines of incipient crystallization, besides which they were in places stained by oxide of iron.

Boiling Springs.—About fifteen miles east of Little Lake, situated in lat. $35^{\circ} 50'$ north and long. $18^{\circ} 10'$ west from Greenwich, 200 miles north of Los Angeles, are numerous boiling springs. The first group covers several acres of the southern declivity of a granitic hill, and is no longer in a state of great activity, although numerous apertures are giving off an abundance of steam and acid vapours, and small quantities of sulphur are being deposited around the various openings. In the immediate neighbourhood of these springs the granite has been reduced to the state of a soft hot mud very dangerous to walk over; and

a large area having become reduced to this pasty condition, it has flowed down the side of the elevation and become deposited in layers on the low ground near its base. It was also observed in one of the ravines that mud resulting from the upper springs had sometimes flowed over and choked those lower in the series; and this appears to have given rise to the formation of fresh openings, and the progressive movement of the area of greatest activity up the side of the declivity.

About a mile to the west of these springs a high granitic mountain is traversed by an immense dyke of obsidian, which, having broken through it, has flowed for a considerable distance into the valley.

At the eastern foot of the hill on which is situated the first group of springs are numerous others in a much more active state. Here the ground is covered over a large extent by innumerable cones of plastic mud, varying from a few inches to three feet in height; these rise above the surface of a barren steaming swamp, and give issue to jets of watery vapour and streams of boiling water. In some cases these springs, instead of issuing from small cones as above described, evolve large quantities of steam and gas under the surface of water contained in basin-shaped reservoirs produced by their agency in the decomposed granite in which they are situated. By these means are formed a multitude of seething caldrons, in which the rapid ebullition of water keeps the mud in a constant state of suspension; and this is either white or of a reddish colour, in accordance with the amount of ferruginous matters present. To the east, at a distance of some three or four miles, were seen high ranges of evidently basaltic hills, the whole district being totally uninhabited except by a few Indians, some of whom were met with collecting oxide of iron to be employed as paint.

The appliances at command in such localities are usually of a limited character; but I was enabled to ascertain by means of litmus-paper that the water of all the springs examined had an acid reaction; and having filled a pint bottle with water from one of the largest of the basins, it was brought to England for analysis.

This amount, although manifestly too small to allow of a very accurate analysis, was sufficient to admit of the estimation of all the principal constituents. The following are the respective quantities of the different bodies present, calculated on an imperial gallon of the filtered water:—

Contents per Imperial Gallon.

Silica	grs. 7·00
Alumina	7·70
Oxide of iron	traces
Lime	1·96
Magnesia	1·41
Potash	0·85
Soda	1·26
Chlorine	2·25
Sulphuric acid	25·86

The foregoing results may be rendered thus :—

Contents per Imperial Gallon.

Silica	7·00
Tersulphate of alumina	25·67
Sulphate of iron	trace
„ lime	4·76
„ potash	1·57
„ soda	2·87
„ magnesia	1·77
Chloride of magnesium	3·01
Free sulphuric acid	1·57
	<hr/> 48·22

It is somewhat remarkable that this water, issuing from the ground in a boiling state, should at the same time contain chlorides and free sulphuric acid ; but it must be remembered that the acid is in an exceedingly dilute state, and that the water was collected immediately at the mouth of the aperture from which it emerged. It is probable that potash and soda may exist in the form of the bisulphates of those bases, since an excess of sulphuric acid is present.

It was observed that where these acid waters flowed away at a temperature of above 200° F. several low forms of vegetation (probably Confervaceæ) appeared to flourish, but to become less abundant and less vigorous where the temperature had become considerably reduced.

Steamboat Springs.—The most remarkable instance on the Pacific coast of the actual growth, on a large scale and at the present time, of mineral veins is probably that afforded by the boiling springs in Steamboat Valley, seven miles north-west of the great Comstock silver vein and Virginia city.

These springs are situated at a height of about 5000 feet above

the level of the sea, at the foot of the eastern declivity of the Sierra Nevada. The rock in this locality presents several parallel fissures, either giving out heated water or simply ejecting steam. The first principal group of crevices comprises five longitudinal openings extending in a straight line for a distance of 3500 feet. Their general direction is north 6° west, and they are comprised within a band of about 600 feet in width. The fissures are separated from one another by intervals of from 40 to 60 feet, have each a thickness of about 12 inches, and are severally connected with each other by lesser openings, which intersect the first nearly at right angles.

These crevices are often full of boiling water, which sometimes overflows and escapes in the form of a rivulet, whilst at others it does not flow over, but violent ebullition is heard to be taking place at a short distance below the surface. The fissures are partially filled by a siliceous incrustation which is being constantly deposited on the sides, whilst a longitudinal central crevice allows of the escape of boiling water or steam. On the most eastern of these lines of fracture are several active centres of eruption, from which boiling water is often ejected to a height of from 8 to 10 feet. These waters are alkaline, and contain, in addition to carbonate of soda, the sulphate of that base, with chloride of sodium. There is also everywhere an escape of carbonic acid, whilst from some places sulphuretted hydrogen is also evolved. These products give rise to the deposition of sulphur, silica, and oxide of iron. The silica forms semicrystalline bands parallel with the walls of the fissures; and spongy deposits accumulate around some of the points of most active emergence in such a way as to form small circular basins, of which the edges go on continually increasing in height, so that each finally presents the appearance of a small volcano of boiling water.

At a considerable distance to the west of these, a second group having the same origin is observed; but this is no longer traversed by hot water, although it still gives off steam and carbonic acid at various points. This large dyke of quartz commences at its southern extremity by a number of fissures which subsequently converge into a single crevice, of which the two walls are of silica, and extend a distance of considerably over a mile. At its northern extremity a central fissure still remains open; but in other localities it is for the most part obstructed by siliceous concretions. Towards the south the siliceous deposits extended beyond the edges of the cleft, and have accumulated on the surface of the ground to a thickness of several feet, and for a distance of from 80 to 90 feet on each side of the fissure. These deposits almost entirely consist of more or less hydrated silica.

This silica presents itself in the form of a compact rock, much resembling in appearance the quartz of ordinary veins, with chalcedony, and occasional nodules of hyalite. This rock is, to a certain extent, metalliferous, and, in addition to oxide of iron, contains oxide of manganese, together with iron and copper pyrites. M. Laur states that he discovered metallic gold in this deposit, although I was myself unable to detect its presence.

These phenomena of the Steamboat Valley appear to indicate that metalliferous veins may be produced by mineral waters, in the fissures through which they circulate. On taking into consideration the principal circumstances connected with these veins, it will be observed that their eruptive activity is by no means equal throughout their longitudinal extent. This activity is concentrated around certain points, which appear to be the sources from which are principally discharged the waters holding various mineral substances in solution, and whence they subsequently become distributed throughout the fissures existing in the rock. It has been observed by M. Laur* that the slowness with which these incrustations are formed in the interior of the cavities, as compared with the great mass of mineral waters which escape over the edge of the various fissures, also shows that a large proportion of the metalliferous emanations may thus have been carried beyond them; so that, should they contain a non-oxidizable metal, such as gold, the superficial deposits may occupy a considerable surface and contain a great amount of wealth, although the vein from which it originally issued may be small and comparatively unimportant.

The rock enclosing the veins of Steamboat Springs is a granite, which in their vicinity is much decomposed, being often reduced to a cavernous skeleton of silica containing only a few scales of mica. The granite in this locality is poor in quartz, and chiefly consists of oligoclase, but contains magnetic oxide of iron, titaniferous iron, &c.

This rock, forming the bottom of Steamboat Valley, is overlain on its eastern and western flanks by a compact basalt containing magnetic oxide of iron together with various zeolitic minerals.

Examination of the siliceous deposit at Steamboat Springs.—A specimen of the siliceous deposit taken from the side of a fissure from which steam and carbonic acid were issuing in considerable quantities had a specific gravity of 1.99, slightly scratched felspar (orthoclase), and afforded by analysis the following results:—

* *Annales des Mines*, sixième série, tom. iii. p. 424.

Water lost at 212° F.	0·48
Water lost by subsequent ignition.	6·16
Silica	92·64
Alumina	0·30
Potash	0·27
Lime, iron, and soda	traces
	<hr/> 99·85

This veinstone, which exhibited numerous parallel bands in the direction of the enclosing walls, was selected for analysis as having been most recently formed. On boiling 35·18 grains of this substance in a finely divided state during eight hours in 4 oz. of a strong solution of caustic potash, only 2·35 grains, or 6·68 per cent., of the silica present was dissolved.

A section of this substance examined under the microscope showed the reniform structure of chalcedony, together with patches of amorphous silica and small crystals of iron pyrites; but no other appearance of crystallization could be detected. This specimen contained a well-defined fluid-cavity, in which the vacuity occupied about one-tenth of its total volume. Another portion of the deposit, which, from being further removed from the sides of the fissure, must have been of older date, afforded only 4·72 per cent. of water, and distinctly showed the presence of crystalline silica. On carefully examining the great western vein before alluded to, considerable quantities of chalcedony were found; and in one of the cavities a few small perfectly defined crystals of quartz were discovered.

Alkaline Lakes.

In that portion of California lying on the east of the Sierra Nevada are Mono Lake and Owen's Lake, both considerable sheets of water highly impregnated with alkaline salts. Owen's Lake, at the southern extremity of which I was for some weeks encamped during the fall of 1865, lies in lat. 36° 20' south, long. 118° west from Greenwich, is about twenty miles in length and eight in width. Owen's Valley is a narrow basin enclosed between high mountains, and has a length of about 140 miles, and an average width of 10 miles. On its western side it is bordered by the Sierra Nevada, which opposite the lake rises in peaks fourteen thousand feet in height. On the eastern side the mountains are more broken, although of nearly equal height, but nothing like a complete exploration of this region has yet been attempted; besides which it is so utterly barren and forbidding that the Indians, whose stronghold it still remains, are not likely for some time to be much interfered with by the indefatigable mining "prospector." These mountains are bare,

and no stream of any importance flows from them into the valley, which is almost exclusively supplied by the melting of the snows which during the winter months accumulate in the upper portions of the Sierra.

This range, on the eastern side of the valley, is towards its southern extremity called the Inyo Mountains, whilst further north they are known as the White Mountains. Owen's River rises not far from the head of the San Joaquin, and near the southern extremity of the valley flows into Owen's Lake, which has no visible outlet, and whose banks are in many parts thickly coated by an alkaline incrustation. No fish inhabit its waters; but at certain seasons of the year it is resorted to by myriads of waterfowl attracted by the grub-like larvæ of a fly which deposits its eggs on the surface.

These grubs are about the length of a small grain of rice, but somewhat less in diameter, and are thrown on the lake shores by the waves in such vast quantities that they are collected by the Indians as an important article of food. In addition to these there are large quantities of minute crustacea resembling the *Artemia salina*.

Waters of Owen's Lake.—A sample of water taken January 3, 1866, from opposite Franklin's Ranch, had a specific gravity of 1.076, and on analysis afforded the following results:—

In an Imperial Gallon.

Chloride of sodium . . .	2942.05
Sulphuric acid . . .	589.12
Carbonic acid . . .	1206.80
Silicic acid . . .	55.34
Phosphoric acid . . .	15.43
Potash . . .	175.49
Soda . . .	2127.07
Organic matter . . .	16.94
	<hr/>
	7128.24

The above results may be expressed as follows:—

In an Imperial Gallon.

Chloride of sodium . . .	2942.05
Sulphate of soda . . .	956.80
Carbonate of soda . . .	2914.43
Sulphate of potash . . .	122.94
Phosphate of potash . . .	35.74
Silicate of potash . . .	139.34
Organic matter . . .	16.94
	<hr/>
	7128.24

In addition to the substances above enumerated minute traces of iodine were found, but in such small proportions that its presence could only be detected when large quantities of water were operated on.

As the samples employed for analysis had, before being brought to this country, been concentrated by boiling, it is evident a loss of carbonic acid would have taken place had any admixture of sesquicarbonate been present.

The incrustations, which at certain seasons of the year are found to the extent of hundreds of tons on the lake-shores, chiefly consist of carbonates of soda in which the proportion of sesquicarbonate is variable; and specimens were examined in which monocarbonate only was present. Besides the carbonates of soda, these deposits contain 3 per cent. of chloride of sodium and about 5 per cent. of sulphate of soda, together with a trace (0.22 per cent.) of silica.

Summary.

The following are some of the more important facts observed on a careful examination of the gold-regions:—

(1) The quartz veins of California almost uniformly exhibit evidences of having been formed by successive siliceous deposits parallel to the walls of the enclosing fissure; and when fragments of exfoliated slaty rock become enclosed, their planes are usually parallel to those of the quartz in which they are imbedded.

(2) All remuneratively productive gold veins contain notable quantities of iron pyrites and other metallic sulphides; and the association of these bodies is so constant and remarkable, that it is probably the result of some chemical action regulating the distribution of the precious metal.

(3) The auriferous quartz of the Pacific gold-fields invariably contains a certain quantity of potash together with a small amount of water which is not eliminated at a temperature of 212° F. Under the microscope it only exhibits well-defined fluid-cavities in specimens that are more than usually crystalline; and these, even in the same crystal, when heated, become full at by no means uniform temperatures.

(4) Mineral veins of evidently very recent date are met with in various localities on the Pacific coast; and some of these contain both gold and silver.

(5) In addition to the gold found in quartz veins, this metal occurs in certain bands of metamorphosed slates. The outcrops of these, like those of ordinary veins, are frequently coloured red or brown by the decomposition of pyrites.

(6) The old Tertiary river-beds constituting the deep diggings, and frequently overlain by a volcanic capping, contain large

quantities of fossil wood, and are often hardened into a compact conglomerate by a cement consisting either of finely crystallized iron pyrites or of silica. This iron pyrites encloses gold which is sometimes water-worn, and at others is in a crystalline or filiform state, showing that it has not been subjected to attrition. In Australia, pyrites replacing the woody constituents of the stems of trees found in similar positions has been shown by various chemists to contain large quantities of the precious metal. The siliceous cement of the ancient river-beds of California has sometimes, but rarely, been deposited in a crystalline form, and on examination such crystals have not been found to contain fluid-cavities presenting the usual vacuities.

(7) Mineral springs are exceedingly numerous, and their waters generally escape from the ground in a state of ebullition. These springs deposit silica, sulphur, iron pyrites, &c., whilst in some instances cinnabar is brought to the surface in large quantities by solfatar action; occasionally the deposited silica takes the form of an ordinary auriferous vein, and gold is stated on good authority to have been found in quartz so produced. At the surface the silica of such deposits contains a larger amount of water than that of the older quartz veins of the country, but appears gradually to lose it, and, although generally amorphous, is sometimes met with in a crystallized state.

(8) In the districts abounding in mineral springs are lakes of which the waters are highly alkaline, and which, in addition to carbonates and sulphate of soda, contain large quantities of chloride of sodium. These have no visible outlets, and act as vast evaporating-pans, in which the waters of the various streams flowing into them are being concentrated by the action of the sun's rays. The incrustations deposited on the shores of Owen's Lake contain a much larger proportion of carbonate of soda than the salts obtained by direct evaporation of the lake-waters.

In the present state of our knowledge, the foregoing facts would appear to lead to the following conclusions:—

(a) Quartz veins have been produced by the slow deposition from aqueous solutions of silica on the surfaces of the enclosing fissures*.

(b) From the general parallelism with its walls of the planes of any fragments of the enclosing rock which may have become imbedded in a vein, it is to be inferred that they were mechanically removed by the growth of the several layers to

* By "aqueous solution" it is not intended to convey the idea that silica has been dissolved in pure water, but rather in waters containing various acids, alkalis, salts, &c.

which they adhered, and that a subsequent deposition of quartz took place between them and the rock from which they had become detached. In this way were introduced the masses of rock known as "horses."

(c) The formation of quartz veins is often due to hydrothermal agencies, of which evidences are still to be found in the hot springs and recent metalliferous veins to be met with in various parts of the Pacific coast.

(d) From the presence of gas-cavities and the variable temperatures at which the vacuities in their fluid-cavities become filled, it may be inferred that they are the result of an intermittent action, and that the fissures were sometimes traversed by currents of hot water, whilst at others they gave off aqueous vapour or gaseous exhalations*.

This is precisely what is now taking place at Steamboat Springs, where the formation of a vein is in progress, and from which currents of boiling water are often poured forth, whilst at other times the fissures only give off currents of steam and heated gases.

(e) That gold may be deposited from the same solutions which give rise to the formation of the enclosing quartz, would appear evident from the presence of that metal in pyrites enclosed in siliceous incrustations near Borax Lake, in the semi-opal of the North Star vein &c., as well as from the fact of gold having been found in the interior of the stems of trees which in deep diggings are often converted into pyrites.

(f) The constant presence of iron pyrites in auriferous veins, and when so occurring its invariably containing a certain amount of gold, suggests the probability of this sulphide being in some way necessarily connected with the solvent by which the precious metal was held in solution. Without considerable addition to our present limited knowledge of the subject, it would be impossible to explain the exact process by which the solution of gold was effected. It has, however, been shown by Wurtz† (who has since suggested the probability of ferruginous salts being the means employed by nature for effecting the solution of gold‡) that finely divided gold is soluble in the sesquichloride of iron, and more sparingly in the sesquisulphate of that metal. It is also well known that iron pyrites sometimes results from the action of reducing agents on the sulphates of iron.

* Sorby, "On the Microscopical Structure of Crystals. § 1. *Crystals formed from solution in water*," Quarterly Journal of the Geological Society, vol. xiv. pp. 453-500.

† American Journal of Science, vol. xxvi. p. 51.

‡ Memoir read before the American Association for the Advancement of Science, August 1, 1866.

If, therefore, sulphate of iron, in a solution containing gold, should become transformed by the action of a reducing agent into pyrites, the gold, at the same time being reduced to the metallic state, would probably be found enclosed in the resulting crystals of that mineral*.

(g) The silica and other substances forming the cementing material of the ancient auriferous river-beds have probably been slowly deposited from comparatively cold solutions.

(h) The connexion existing between the decomposition of felspar by the agency of boiling springs, the existence of alkaline plains, and the formation of lakes containing various salts of soda and potash will be at once obvious to the geologist.

LIII. *Chemical Notices from Foreign Journals.*

By E. ATKINSON, *Ph.D., F.C.S.*

[Continued from vol. xxxv. p. 461.]

SCHRÖDER VAN DER KOLK published shortly before his death two articles in Poggenдорff's *Annalen*, vol. cxxxi., of which the following is an abstract taken from the *Zeitschrift für Chemie*.

If a body be heated from zero to a given temperature, it takes up a certain quantity of heat which is consumed in increasing its temperature and for internal and external work. If, from the heat taken up, heat which has been transformed into external work be subtracted, the remainder represents the heat contained in the body after the process; and this quantity is called the mechanical energy of the body in this condition. Hence, in a definite condition, every body possesses a certain quantity of energy; the absolute quantity of energy is always unknown.

* From a somewhat lengthy investigation of the subject, I am induced to believe that gold invariably occurs in pyrites and other sulphides in the metallic form. Tarry matter, like that found in the solfatara at Borax Lake, has occasionally been met with in the quartz veins of California; and although the protosulphate of iron, resulting from the decomposition of the sesquisulphate, would, under certain circumstances, have the effect of precipitating gold from solution, it would probably not do so in the presence of large quantities of the persalts of that metal. A discovery, made by Mr. Daintree, of the fact that a speck of gold lying in a solution of the chloride of that metal may be increased to several times its original weight by the action of a small piece of cork introduced into the solution, is recorded by Mr. Ulrich. Mr. Wilkinson's experiments further prove that, besides gold itself, iron, copper, and arsenical pyrites, galena, blende, &c. likewise form favourable nuclei, which, if immersed in weak solutions of chloride of gold, receive a solid coating of metal by the agency of organic matter, such as a chip of wood floating in the solution.—'Notes on the Physical Geography, Geology, and Mineralogy of Victoria' (*ante cit.*), p. 44.

In the following considerations two principles are used :—

I. When mechanical work is performed an equivalent quantity of heat disappears, and conversely ; the same is presupposed with regard to chemical work.

II. Heat cannot of itself pass from a warmer to a colder body.

In due connexion with this is the principle that mechanical or chemical work can of itself be spontaneously transformed into heat ; but heat can never be changed into work without the simultaneous occurrence of a compensating action—for instance, when a quantity of heat passes from a higher to a lower temperature.

Most important in these determinations is the heat of combination, the determinations of which were mostly made by Favre. He expresses the numbers in thermal units, and refers them to equivalent quantities if the equivalent of hydrogen is taken = 1 gramme. The two constituents of a compound unite at a definite temperature ; if, then, the compound formed be cooled to the original temperature of the constituents, the heat developed during the operation is the number given by Favre. This tells how much more energy is accumulated in the components in the original condition than in the combination in its final stage. Of course the constituents and the combinations must be in the same state of aggregation, or otherwise the latent heat must be taken into account. If t is the initial and final temperature, τ that at which combination takes place, the quantity of heat A is absorbed in raising the body from t to τ ; the heat of combination, W , is disengaged at τ , as well as the quantity of heat B if the product is cooled down from τ to t . Favre's number $F = W + B - A$ gives us the difference in energy ; the real heat of combination is W . The number W gives us the chemical work consumed in the combination.

The author applies these considerations to chemical actions. Chemical combinations may be divided into two classes, according as the compounds have less or more energy than the constituents. Water may be taken as a type of the first ; heat is liberated during combination and absorbed in decomposition. For the second class, of which protoxide of nitrogen is the representative, the phenomena are reversed. Moreover all bodies are decomposed at a sufficiently high temperature.

The compound gases may be formed as follows :—

(1) On being passed through a heated tube. Both kinds of the gases as above distinguished may thus be formed. In his considerations the author takes the following division as a basis.

A. The gases have less energy than their components, and are formed on simple heating.

HO . . .	29413	HBr . . .	9322
CO . . .	14838	PCl ³ . . .	94804
CO ² . . .	48480	PCl ⁵ . . .	100373
SO ² . . .	35520	As Cl ³ . . .	7183
ClH . . .	23783		

B. The gases have less energy than their constituents, and are not formed on simple heating.

NO ⁴	SH . . .	2741
NO ²	C ² H ⁴ . . .	22000
NH ³ . . .		22728

C. The gases have more energy than their components.

NO . . .	8724	C ² N . . .	38994
ClO . . .	7370	CS ² . . .	9659
HI . . .	3606	C ⁴ H ⁴ . . .	8000

The numbers are the heats of combination according to Favre (energy of the constituents minus energy of the combination). Only the gases A are all formed on being passed through a heated tube: of the gases B and C only CS² is formed under these circumstances. The action in the heated tube may be twofold. Either heat becomes free in the formation of the compound, in which case the action excited in portions of the mass will be propagated through the entire mass, and if this takes place rapidly explosion will be the result; or the action is accompanied by an absorption of heat; the action is then restricted to the place where it is commenced, and the combination is then more complete. In decomposition, of course the reverse takes place. When, as in the case of water and carbonic acid, the compound explodes, the decomposition must be incomplete; the gases C, on the contrary, such as nitrous oxide and hypochlorous acid, must be completely decomposed, sometimes explosively, as in the case of hypochlorous acid. The latter depends on the temperature chosen, and on the velocity with which the action is propagated in the gases.

(2) Compound gases may be formed by catalytic action.

As the exciting body here remains unchanged, it can develop no energy, and can only produce changes in which no heat is absorbed. The gases C can never be formed by catalytic action; it is only possible in the case of the gases A and B.

(3) Compound gases can be formed by the action of the electrical spark.

The action of the electrical spark is essentially thermal. By sparks of adequate strength all combinations can be decomposed. If the sparks are less powerful, phenomena of combination may occur. At the same time the spark effects a change in the affi-

nity of the components. The gases A are formed by a single spark. If only a few atoms of the components are united by the electric spark, the heat which thereby becomes free is sufficient to propagate the action throughout the entire mass, and no explosion can in this case occur. The gases B can only be formed gradually by repeated passage of the spark, and only along its path. It here acts especially by alterations of affinity; the heat which becomes free in the action cannot form the compound. The gases C can also only be formed in the path of the spark; for here, besides the affinity, this must also furnish the necessary heat. Of course, in the decomposition of the gases by the electrical spark the inverse phenomena are observed. The gases which the spark can form with explosion are slowly decomposed by the electrical spark, and conversely.

(4) Compound gases can be formed by combustion.

Combustion is a chemical action which takes place with the development of heat; and this action itself develops an amount of heat sufficient to keep it up when once started. The gases C can therefore never be formed by combustion; and as, moreover, the gases B cannot be formed by simple heating, they can never result from combustion. The gases A alone can be formed by combustion, and even here those only in which the heat developed is adequate for further combustion. The same phenomena occur in the combustion of compound gases. These also can only burn with other gases, provided the heat developed is sufficient for the further combustion. The following gases containing hydrogen develop the corresponding quantities of heat for 1 equiv. $H = 1$ grm., where heat absorbed is designated as negative. The second series refers to one equivalent of oxygen, or 8 grms:—

OH . . .	29413	HO . . .	29413
ClH . . .	23783	$\frac{1}{2}(\text{CO}^2)$. . .	24240
BrH . . .	9322	$\frac{1}{2}(\text{SO}^2)$. . .	17760
$\frac{1}{3}(\text{NH}^3)$. . .	7573	CO . . .	14838
$\frac{1}{4}(\text{C}^2\text{H}^4)$. . .	5500	ClO . . .	-7370
SH . . .	2741	NO . . .	-8724
$\frac{1}{4}(\text{C}^4\text{H}^4)$. . .	-2000		
IH . . .	-3606		

Each succeeding gas cannot possibly be formed by combustion from a preceding one. Of the preceding gases, only the gases A can be formed by combustion; that is, only HO, CO, CO^2 , SO^2 , ClH, BrH: water is formed by the action of O on all the above hydrogen compounds, HCl by heating Cl with the following hydrogen compounds, &c. In like manner HO is formed by heating H with all the above-named oxygen compounds; CO^2 results from heating C with SO^2 , CO, NO, &c. In this connexion some interesting cases occur: thus the reaction $2\text{CO} + \text{S} = \text{SO}^2 + \text{C}$

must be possible; the action $2\text{CO} + \text{C} = \text{CO}^2 + \text{C}$ has already been observed by Deville, who obtained carbonic acid when he passed carbonic oxide over red-hot charcoal.

The author obtains the following numbers for the oxides of nitrogen, which express in thermal units the excess of the energy of the compound over that of its constituents:—

NO		— 8724
NO ²		< 22968
NO ³		< 16354
NO ⁴		< 35468
NO ⁵		< 43623

Here NO³ and NO⁵ are assumed to be in solution. It follows from these numbers that the aqueous NO⁵ is the most stable—moreover, that NO⁴ is far more permanent than NO². Anhydrous NO⁵ has a different deportment; it dissolves in water with considerable disengagement of heat, and therefore has more energy than aqueous NO³. Hence also NO² could never form NO⁵ with oxygen except in presence of water; NO² and O can only form one product of less energy. Such a case is the NO⁴ here formed.

The sudden ignition of many bodies when heated, for instance chromic oxide, Gadolinite, Samarskite, &c., the author explains on the supposition of a sudden change in the energy. Many bodies occur in two conditions with different energy. Yellow iodide of mercury passes into red with disengagement of heat. If a body has been transformed from one state into another by heat, it stops in it, and on cooling does not again absorb heat.

Many phenomena which have hitherto been ascribed to greater or less chemical affinity, the author thinks he can explain by his theorem of mechanical energy. Thus sulphuric acid is not a stronger acid than hydrochloric because it forms a precipitate in chloride of barium and expels hydrochloric acid. A solid body is here formed under disengagement of heat. In nature a tendency prevails to transform chemical work into heat; and that may be the cause of the formation of a solid body.

M. Becquerel has published* a series of researches on the chemical effects produced in capillary spaces. The following account of them has been taken, with occasional abridgment, from a paper by Professor De la Rive in the *Bibliothèque Universelle de Genève*.

M. Becquerel's first series of experiments were executed by means of a slightly cracked tube. The cracks are made by tracing one or two longitudinal lines with a diamond on the surface of the tube, and then applying in any part of these lines the

* *Comptes Rendus*, May 13, June 17, and November 4, 1867; and January 13 and April 20, 1868.

point of a small tube heated to redness. The crack must not be allowed to become too large; it is merely necessary that it shall allow the solutions, or at any rate one of them, to enter by capillary attraction.

One of the most remarkable effects is obtained by filling such a cracked tube with a concentrated solution of nitrate of copper, then introducing this tube into a test-glass containing a solution of monosulphide of sodium which marks 12° to 15° of the areometer, the two liquids being at the same level, so that the pressure is the same on the two sides. By simply mixing the two solutions, black sulphide of copper and nitrate of soda should be formed. This, however, is by no means the case; a short time after the experiment had been arranged, a very brilliant deposit of metallic copper with a crystalline texture began to be perceived. Little by little the deposit increased, the crack enlarged, and the tube ultimately broke. Small cylinders of copper 2 millims. in diameter could then be withdrawn.

With several other solutions of copper the effects are the same, but less rapid. Similar results are also obtained with concentrated solutions of silver, lead, gold, tin, cobalt, and nickel.

It is very difficult to obtain reduction effects with those of iron and zinc.

The author used several kinds of capillary spaces besides that above mentioned—such, for instance, as the interval between two glass or quartz plates pressed more or less closely together, the porosity of the parchment-paper used for dialysis, the capillary intervals between grains of glass or of quartz, unbaked porcelain or sandstone, tubes drawn out at one end. The first method gave good results; by its means the reduction could be effected of gold, copper, lead, nickel, cobalt, and that of platinum, which could not be accomplished by using the cracked tube.

M. Becquerel in his first memoir, after having investigated the influence of the various kinds of capillary spaces, and described a great number of cases of reduction effected by electro-capillary, or rather chemico-capillary actions, cites instances of the formation of insoluble compounds by the effect of these same actions. In conclusion, he observes that there is now a whole series of facts to be added to those he has observed, such as the phenomena due to the properties of spongy platinum and of porous bodies, to the effects of diaphragms, and those of dyeing, all of which may be classed as an important part of chemistry under the name of capillary chemistry.

In his second and third memoirs M. Becquerel endeavours, while increasing the number of facts, to analyze the physical and chemical causes which come into play in the phenomena of

metallic reductions which he has described. Before examining the explanations he has given and the experimental proofs on which he rests them, it will be convenient to dwell on some facts of detail which he develops in various parts of his investigation.

Among these may first be noticed the effects produced by different kinds of diaphragms, such as bruised quartz and glass in thin layers, fine sand, and hardened mortar, the interstices of which are capillary spaces. To examine the effect of fine sand, he introduces a certain quantity into tubes closed with a piece of sackcloth bound by means of a wire on the outside, thus forming diaphragms of 4 to 5 centims. in height; he then places successively various metallic solutions in the tube, and the solution of monosulphide of sodium in the test-glass, taking care that the sand is fine enough and the height of the column sufficient to allow the liquids only to mix slowly. Most metals are thus reduced from their solutions; it is thus with copper, silver, gold, cobalt, nickel, &c. A solution of equal parts of nitrate of copper and of silver first gives the silver in dendrites or in plates; then comes the copper, but after a considerable interval. With hardened mortar the reduction of platinum, cobalt, &c. is effected. M. Becquerel has also endeavoured to replace, in his old experiments on simple electrochemical apparatus, the kaolin which he used as a diaphragm by cracks in tubes. Thus in the couple which is called the oxygen one, he substituted for the tube filled with kaolin, which separated the solution of caustic potash from nitric acid, a slit tube closed by the lamp at one end, and he obtained the same effects. The same result was obtained in other analogous cases, in which he used simple electrochemical apparatus for obtaining simple and double sulphides and iodides.

M. Becquerel thinks the reduction of metals in capillary spaces cannot be explained, as has been attempted, simply by reference to molecular attraction, but that it is due to the combined actions of affinity, molecular attraction, and electricity.

In order to show the influence of electricity in the production of the phenomenon, M. Becquerel uses the apparatus consisting of a cracked glass tube filled with a solution of nitrate of copper, which is immersed in a test-glass containing a solution of monosulphide of sodium; he then connects these two solutions by a platinum or copper wire, which by completing the circuit forms a simple electrochemical couple, which gives rise to a current which is indicated by the galvanometer. In a short time the solution of nitrate of copper is decomposed, and the end of the wire which is immersed in this solution is covered with metallic copper. During these reactions the crack only acts by making contact between the two solutions; no trace of metallic copper

is observed in the interior of the crack on the inner surface of the tube ; but if the copper wire be removed, which suppresses any metallic contact, the fissure fills with small crystals of metallic copper, as well as the inside of the tube. In this case, according to M. Becquerel, the liquid lodged in the fissure acts as the copper wire which has been suppressed ; and there are two currents, which add on to each other, for they are in the same direction—one due to the reaction of the liquids, the other arising from the oxidation of the reduced metal. When the fissure is filled with metal, it may happen that there is no direct contact between the two liquids, and the couple then ceases to act. The same results are obtained with other metallic solutions, such as nitrate of silver. It may be remarked that, capillary spaces of the same section being far more imperfect conductors of electricity than the metallic wires whose functions they perform, the electrochemical effects should also be feebler.

M. Becquerel assumes that liquids enclosed in capillary spaces play the part of solid conductors. This he finds supported by the circumstance that, from M. E. Becquerel's experiments, the conductivity of liquids in capillary tubes is greater than is due to the section of the liquid column. This important fact would tend to prove that the increase in density of the liquid layer retained on the surface of the glass by molecular attraction comes into play to form a voltaic circuit consisting solely of liquids.

I shall not dwell on the various improvements successively made by M. Becquerel in the construction of these apparatus, nor on the numerous experiments made to replace monosulphide of sodium by other solutions. Before concluding I will restrict myself to remarking upon a few points which appear to me more important.

I will first call attention to a series of facts which would seem to prove the production of electric currents in the fissures of cracked glass vessels without the apparent intervention of chemical actions. In all the phenomena previously described, the fissures of cracked vessels, though very narrow, nevertheless enable the two liquids which moisten the sides to react chemically upon each other in the capillary spaces and to give rise to electrochemical effects. Now observe what takes place if the magnitude of the fissures be varied. If the size is relatively great, the solutions mix more or less rapidly, giving rise to the ordinary precipitates which result from a double decomposition. With a smaller size the electro-capillary phenomena of metallic reduction which we have described are obtained. At a lower degree there is an electrical current without any apparent chemical action—a current which has the same direction as if the reaction took place. The cracks may finally be so narrow that

there is neither metallic reduction nor electrical current. In this case evidently the liquids no longer communicate.

To obtain an electric current without apparent chemical action, fissures having the shape of a star are made in a small cup by touching it with a slightly moistened glass after having heated it. The cracked beaker is then filled up to the star with a solution of nitrate of baryta, and is placed in another vessel containing weak sulphuric acid. Two platinum plates, each immersed in one of the liquids, are connected by the wire of a galvanometer; and an electric current is obtained without any appearance of chemical action; that is to say, there is no deposit of sulphate of baryta. Associating in the same manner a solution of chromate of potash with one of nitrate of lead, or a solution of ferrocyanide of potassium with a solution of protosulphate of iron, no precipitate is obtained, which proves that there is no apparent chemical action, although an electric current is produced. On the other hand, working with solutions of nitrate of copper and of monosulphide of potassium and the same vessel which served for former experiments, a strong current is obtained, and some time after there is a deposit of metallic copper in the fissure. It is true that the electromotive force which takes place in the contact of these two latter solutions is far more considerable than that of the other solutions which had been submitted to experiment. M. Becquerel remarks that it is possible that with these latter solutions, the chemical reactions being extremely feeble, an electric current is produced, though the effects resulting from the chemical action which has produced it are not perceived for a long time.

In this part of his researches M. Becquerel dwells on what he calls the polar properties of the sides of the fissures of cracked vessels, acting like electrodes when these fissures furnish the passage of a current. This is also an interesting point, on which I will delay for a moment, inasmuch as it seems to me connected with a more general law discovered by Faraday. The following is M. Becquerel's experiment.

When we decompose a solution of nitrate or sulphate of copper partly contained in a cracked vessel and partly in a sound one in which the former is immersed, and if by means of a platinum electrode placed in each of these vessels the current from a battery composed of several couples be passed, a decomposition is obtained in which the negative electrode becomes covered with metallic copper; the current, however, has but small intensity, owing to the thin layer of liquid found in the crack, which in this case plays no part. It is not the same if the cracked vessel contains a solution of caustic potash and the negative electrode while the other vessel contains the metallic solution and the positive electrode. In this case water alone is

decomposed, and not the metallic solution, so long at least as the two solutions are not mixed.

If, now, two cracked vessels be taken containing acidulated water and immersed in a solution of nitrate of copper, the sides of the fissures act in that case like intermediate conductors; the outside of the positive vessel becomes covered with metallic copper, while the acid is given off on the negative electrode. The part of the narrow fissures is here that of preventing the mixture of solutions; they act then like intermediate conductors; the two plates of platinum are in conducting non-metallic solutions, which do not mix with the metallic one, though they are in contact with it.

The phenomenon we have described is quite analogous to an experiment of Faraday, in which that illustrious physicist passes an electrical current through a concentrated solution of sulphate of magnesia in contact with a certain quantity of pure water placed above, and arranged so that the two liquids cannot mix. Putting the positive electrode in the sulphate of magnesia and the negative one in water, the magnesia, instead of being deposited on the surface of the negative platinum electrode, stops in the water at the boundary of it and the sulphate. This fact, which is easily explained on Grothüss's theory, is owing to the fact that the molecule reaching the water no longer finds a molecule of acid with which it can combine, which would not be the case if the water and the sulphate could have become mixed even in small proportion. It is probably a phenomenon of the same kind which occurs in Becquerel's experiment, and that the fact he has observed is due to the circumstance that, the solution of nitrate of copper only being connected with the two acid solutions by the intervention of the capillary fissures, there can be no mixture between them. But when there is no mixture in more or less considerable proportion between two adjacent solutions, there can be no ordinary electrolysis.

In his fourth memoir, M. Becquerel describes a very ingenious method of M. E. Becquerel of measuring capillary spaces, and especially the magnitude of fissures, and even the dimensions of the pores of vegetable or animal membranes. The method in question consists in comparing the conducting-power of a liquid enclosed in the capillary space to be measured with the conducting-power of the same liquid placed in a capillary tube, the diameter and length of which are exactly known. This method is extremely delicate, and by its means intervals of a few thousandths of a millimetre may be measured; yet it requires in its application some precautions and certain preliminary determinations.

Measurements made by this method have shown that when

the interval between the plates is only a few hundredths of a millimetre, most of the metals are reduced; while when it is only a few thousandths of a millimetre, silver and gold are easily reduced, but the others only slowly and with difficulty.

Using as a capillary space the interval between two plane glass plates kept pressed against each other by a ligature, so that there is only a very small capillary interval, the effects obtained are much more regular than with slit tubes, which have not everywhere the same width. The liquid, which is a solution of a metal, is introduced between the plates by means of a small reservoir at the top, and the supply is so arranged that the exit of the liquid by the edges is inappreciable, which is easily effected. The system of two plates between which is the liquid is then immersed in an outer vessel containing the other solution, that of the monosulphide for instance.

It sometimes happens that the metallic deposits formed between glass plates merely separated by an interval of a few hundredths of a millimetre, between which is a band of filtering-paper, and kept firmly bound, exert such an expansive force that even when the bands are not broken one of the plates is. From this it is readily seen how rocks through which various solutions infiltrate, and in which are produced capillary effects resembling those we have described, may crack or even burst.

We shall not follow M. Becquerel in the applications which he makes of his new researches to explain various natural phenomena, either in inorganic or in organic nature. It is, in fact, readily understood that, as soon as we are concerned with cases in which capillarity plays an important part, this application is a natural consequence. In inorganic nature the infiltrations which take place in the slits of rocks ought to produce effects resembling those of which we have spoken, and, especially under favourable circumstances, produce a reduction of metals. In organic nature it is easy to understand the important part which the porous tissues and narrow vessels of vegetables and animals necessarily play. M. Becquerel has endeavoured to establish relations between endosmose, exosmose, and dialysis and electro-capillarity, of which, in his opinion, the former phenomena are only particular cases.

In conclusion M. De la Rive sums up the results of these researches as follows:—

The fundamental point is the influence exerted on the phenomena which occur in the contact of the two liquids by the fact that this contact takes place through a very narrow space, in other words, a capillary one. Is this influence merely negative? that is, does it simply arise from the fact that the two liquids being separated by a narrow space cannot mix, being all the while in contact?

We think that this is sometimes the case, as, for example, in the electrolysis of two liquids placed end to end in the path of the current, so as only to communicate by the intervention of capillary spaces, and therefore without being able to mix. But, further, the phenomena of exosmose, of endosmose, and dialysis show that there may be a positive influence arising either from the fact that the liquids are only in contact by very small surfaces, or from the nature of the solid which serves as envelope for the capillary spaces.

The phenomena which M. Becquerel has observed seem also to indicate that the relative nature of the solution and of the porous solid is not without influence, and in general he attributes the effects he has perceived to the production of electricity. According to him, when the two solutions are not united by a solid conductor, a case in which obviously a couple is formed, the small layer or the liquid column contained in the capillary space acts as this solid conductor; or at all events the sides of the insulating material (generally glass) of the capillary space perform the function when they are moistened by a liquid which is itself a conductor. In this manner we should explain the reduction against the sides of those metals whose solutions are in contact with them. M. Becquerel, among other proofs in support of this explanation, mentions that, according to M. E. Becquerel's researches, the liquid columns contained in capillary spaces conduct better than corresponds to the law of diameters.

We must confess to some hesitation in admitting the preceding explanation. We shall first ask whether, in the case in question (that is, when there is a chemical action between two solutions separated from each other, either by diaphragms or by capillary spaces which enable them to be in contact without mixing), electricity does really come into play. That electricity really does intervene when the two solutions thus arranged are connected by an external metallic conductor cannot be doubted; for then a couple is formed, and the ends of the conductor are true electrodes, which receive the deposits arising from the electrolytic decomposition. But when the two solutions are merely in contact without mixing, being separated by a porous diaphragm or by a capillary space, electromotive force is undoubtedly developed in the two liquid surfaces in contact, a development followed by a chemical action, and by the production of an electrical current; but the current is in this case molecular or local, and can only be perceived when the two liquids are united by a solid conductor; it would seem to us to be rather the result than the cause of the chemical action which takes place. Instead, then, of assuming that the liquid enclosed in the capillary space acts as a solid conductor, it would seem more natural

to suppose that the effect observed is only the result of the chemical action, more or less modified by the fact that the space is capillary—and in particular that the action can only take place on a small number of molecules at a time and successively, instead of acting on the whole of the two solutions at the same time. This is evidently an important point to investigate. It is very probable that the chemical action is modified by the so-called molecular attraction to which capillarity is attributed. It is also possible that this so-called molecular attraction is only one form of affinity—or rather that the two forces are only one, so that in the capillary spaces the liquid is not in the same conditions, physical or chemical, which it presents when it is in mass; thus the action of another liquid on the capillary layer would be quite different from that which it exerts in its ordinary state. It would be an action of the same kind as those of contact, the so-called capillary ones.

The greater the advance in the study of chemistry the more insufficient do we find the explanations of chemical phenomena that are merely based on the exercise of the force called affinity as hitherto understood. As I have already said, a host of circumstances demonstrate this inadequacy. One of the most remarkable of the phenomena is the modification which the presence in a liquid or a gas of an apparently inert solid brings about in the chemical relations of this gas with others. And, in fact, the limiting surface of a liquid or a gas should have different properties from the rest of the layer. Now the characteristic of liquids placed in capillary spaces is that of being exclusively formed of limiting surfaces; and therefore it is not very surprising to find that in these conditions their properties become quite different from what they are in their natural state. Considering Clausius's theoretical ideas on the constitution of liquids and gases, those of Deville on dissociation, of Graham on dialysis, and those of Bunsen on the combination of certain gases, without taking into account the older ones of Berzelius on catalysis, of Dutrochet and so many more on endosmose and exosmose, we come to the conclusion that we have an entirely new force of chemistry to deal with. It seems therefore premature to endeavour to give a rational explanation of the phenomena recently discovered by M. Becquerel by ascribing them to an electro-capillary effect. They are probably only one of the numerous forms of chemico-molecular actions in which the physical constitution of the body plays as important a part as its chemical constitution. And even if we rejected all M. Becquerel's theoretical ideas on this subject, this illustrious physicist would none the less have rendered a great service to science by directing the attention of men of science to these phenomena.

LIV. *On the Relation of Mechanical Strain of Iron to Magneto-Electric Induction.* By G. GORE, Esq., F.R.S.*

FOR making experiments on this subject I have constructed the following apparatus:—A A' is a strong base or plank of oak about 120 centims. long, 18 centims. wide, and 7·5 centims. thick (the thinner part of which, marked A, may with advantage be left of the same thickness as that of A'). B is an iron lever about 72 centims. long, and 1·5 centim. thick at its lower end, capable of moving in a vertical plane upon a strong horizontal rod or axis of steel C, about 2 centims. diameter, driven tightly into a hole in the wooden base; the lever has a piece of iron D (shown in section in fig. 2) welded upon its upper sur-

Fig. 2.

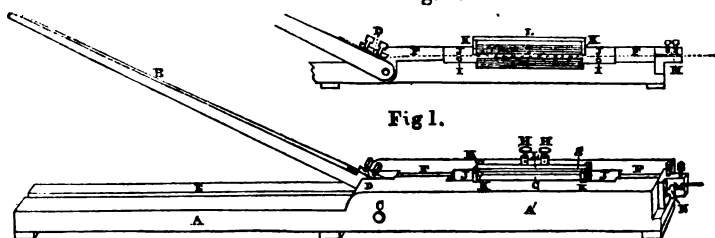


Fig 1.

face, with a hole 6 millims. in diameter drilled through it for the reception of one end of the iron wire. E is a slot cut in the wood for the reception of the lever. The wire to be strained cannot be shown in fig. 1, because it lies in a narrow groove F F', about 8 millims. wide and 3 centims. deep, cut in the base-board, the wire being at a distance of about 1·5 centim. below the surface of the board; its position is shown by the horizontal dotted line in fig. 2. G is a coil of insulated copper wire; it is about 18 centims. long and 4·3 centims. in diameter, with a hollow axis of glass tubing of about 8 millims. internal diameter; it contains about 3295 turns of wire 0·54 millim. thick (= No. 26 Birmingham brass wire-gauge), the two ends of the wire being attached to the binding-screws H, H. The coil lies tightly fixed by wedges of wood in a rectangular space in the wood, with its axis coincident with that of the iron wire. The iron wire is kept in the same horizontal plane by passing over two brass rollers (without axes) I, I, shown in section in fig. 2; these rollers are about 2·5 centims. long and 1·5 centim. in diameter, and lie upon the bottom of the hollow rectangular spaces in the wood shown at J, J. K, K are two moveable rectangular pieces of oak which slide vertically and tightly in narrow grooves formed

* Communicated by the Author.

in the sides of the rectangular space in the base-board; they have fixed to them two small vertical plates or armatures of soft iron, the lower edge of each of which has a small notch filed in it which fits and *presses* upon the iron wire; and when the apparatus is going to be used, a permanent bar-magnet L is placed with its poles resting upon the upper ends of these armatures. The iron wire employed is usually about 4 millims. in diameter, and the apparatus is sufficiently powerful to readily elongate iron wire of that size. M is a rectangular or T-piece of wrought iron of the form and dimensions indicated by the figures; it is moveable, and may be readily taken off the end of the base-board by removing the screws N (of which there is one on each side). The ends of the wires require to be held very securely during the strain; this is perfectly effected by having in D and in M two steel screws each 6 millims. diameter. The level of the top surface of the small brass rollers is slightly above that of the ends of the iron wire.

By means of this apparatus I have repeatedly verified the fact that a magnetized soft iron wire, during the act of being stretched (either with temporary or permanent elongation), *increases* in magnetism, and produces a current of electricity in the coil of wire G in a contrary direction to that of the hands of a watch, *i. e.* in

the Direction of , when we are looking at its south pole, cor-

responding to that of the north pole of the magnet above it. With a galvanometer of moderate sensitiveness, the amount of deflection of the needles was generally about 14 degrees on one side by a single stretch of the wire, or 17 degrees on each side by repeated synchronous stretches. No deflections were obtained by stretching thick wires of zinc or copper. The apparatus was placed in a direction of east and west during the experiments.

The results obtained with iron agree with the fact discovered by Mr. Joule, that straining an iron wire lowers its temperature, and also with the well-known fact that lowering the temperature of a piece of iron increases its magnetic capacity.

LV. *On the Bearing of Recent Observations upon Solar Physics.*
By G. JOHNSTONE STONEY, M.A., F.R.S., &c.*

97. **T**HE main difficulty in dealing with solar and stellar physics arises out of the scantiness and fragmentary character of our data. This makes the inquiry a very treacherous one—so much so, that some of the most eminent men have gone

* Communicated by the Author.

astray in attempting it, and no one that has ventured upon the enterprise can deem himself secure. For, on the one hand, that which at any stage of information appears the most obvious account of a phenomenon is often not the true one; and the meagre knowledge we have of solar phenomena seldom suggests much beyond the obvious account of them; and, on the other hand, if we are led astray on any point, we have little to set us right, as we cannot select our test; and all we can do is to lay down correct principles of interpretation, to bring to bear from every quarter such shreds of knowledge or information as can be laid under contribution, and to review all, from time to time, in the most cautious and painstaking manner.

We may therefore well hail with satisfaction the additional news of the sun which the late eclipse has brought us. These fresh items of intelligence are chiefly two:—that the light of the corona, so far as it was examined (that is, in its upper regions), is much of it polarized in planes passing through the centre of the sun; and that the spectrum of the prominences which were examined contained bright lines. It is with these particulars that I wish at present to endeavour to deal.

98. The protuberance upon which all the observers*, except Captain Haig, appear to have bent their chief attention was an unusually brilliant and lofty one on the eastern limb of the sun, nearly opposite to a solar spot†. It emitted five bright lines, and apparently four fainter ones not seen by most of the observers. Major Tennant satisfied himself that the hydrogen line C was one of the bright lines, the sodium line D another, and the magnesium line *b* a third‡. He had not time to measure more; but his estimate of the positions of the two other lines which he saw suggests their being the two other principal hydrogen lines, viz. F and the hydrogen line close to G. Lieutenant Herschel's measures identify one line with D, and place another near F§. Mr. Lockyer, examining another prominence since the eclipse, has satisfied himself that he saw as bright lines C, F, and a line near D||. M. Rayet, who records nine lines, supposed the brightest lines to be B, D, E, F, and G, "judging

* There is some uncertainty whether this was the prominence examined by Major Tennant, since he speaks of the prominence he examined as being on the preceding, *i. e.* the west, side of the sun. But his description of its aspect does not seem to accord with the appearance of any other than the great flame seen on the eastern limb, and he refers to its being conspicuous immediately before the sun reappeared as evidence of its great height, which also seems to imply that it was on the eastern side.

† Proceedings of the Royal Society, vol. xvii. p. 79.

‡ Monthly Notices of the Astronomical Society, vol. xxviii. p. 245.

§ See 'The Engineer' of November 6, 1868, p. 346.

|| Proceedings of the Royal Society, vol. xvii. p. 91.

from their arrangement in the field of view, the intervals between them, their colour, and general aspect”*. To bring this into harmony with the preceding determinations in which measures were made, we must suppose that M. Rayet’s B and E were in reality C and *b*. It is to be observed that the intervals between B and C, and between E and *b*, are the two smallest of the intervals between lines lettered by Fraunhofer, and therefore that these rays are the most liable to be confounded in such eye-estimates as M. Rayet made. The details of M. Janssen’s observations have not reached Europe; but he describes hydrogen as detected in the principal protuberance; and therefore his report also inclines in favour of C. On the whole, then, we may with most safety conclude that the lines seen were probably C, D, *b*, F, the hydrogen line near G, and four other very faint lines. Those which seem to be identified are the principal lines of hydrogen, sodium, and magnesium—the three gases in the solar atmosphere which, as I have elsewhere shown†, project far beyond all other known constituents.

99. The most probable account to be given of this great prominence would appear to be that which I ventured to advance in a paper which was presented a year and a half ago to the Royal Society (see Proceedings of the Royal Society, vol. xvii.). [For convenience of reference the paragraphs of this paper are numbered in succession after those of the paper in the Proceedings of the Royal Society.] There is a low-lying shell of attenuated clouds outside the photosphere, at a distance from it of somewhat more than one semidiameter of the earth. Now, in the memoir already referred to, I have shown (§ 68) that if a disturbance in the lower parts of the outer atmosphere heave a part of this shell of cloud above its natural level into a cooler region, it will, from its continuing to be exposed to the unmitigated glare of the photosphere, import an enormous amount of heat into its new neighbourhood, which will have the effect of so heating the air through which the mist is dispersed, that the entire mass of cloudy particles and interposed gases will commence a violent ascent, and thus both produce a cyclone over the photosphere which may result in a spot, and give rise above to such great columnar flames as the one which has been found to give out these bright lines. When writing my paper I did not overlook the circumstance that the intermingled gases would emit bright lines; but I was under the impression that all such lines would be extinguished by the absorption of the surrounding colder parts of the atmosphere through which they should make their way

* *Comptes Rendus*, October 12, 1868, p. 758.

† See memoir on the “Physical Constitution of the Sun and Stars,” *passim*, Proceedings of the Royal Society, vol. xvii.

Phil. Mag. S. 4. Vol. 36. No. 245. Dec. 1868.

2 G

before we could be spectators of them. It now appears that some, at least, of them escape—or, to speak more accurately, the marginal parts of some of them; for it will probably be found, if they can be submitted to examination with a sufficiently fine slit, that there is a central stripe of each blotted out by absorption, and appearing as a narrow dark line in the middle of each bright line. We must also bear in mind that there may be other lines emitted so narrow that they cannot cross the surrounding atmosphere.

100. Which lines then reached the earth? Plainly those which, as they were emitted by the incandescent gas, were broader than the corresponding dark lines of the solar spectrum, or at least than that part of the width of the latter which is due to the absorption of those strata of the sun's atmosphere through which we have to look in viewing the prominence. And as even lines of the same gas differ from one another in this tendency to spread out when heated, we see at once how one hydrogen line, C, could seem to Mr. Lockyer to extend further downwards in the solar atmosphere than another, F*. The lower part of F was no doubt effaced by the greater absorption of a hotter part of the atmosphere telling with more effect upon F than upon C. We must remember, too, that the ascending column may carry with it gases from lower regions of the solar atmosphere to heights not usually reached by them. Any rays which they would then emit while in the gaseous condition would be unobstructed in passing out through the surrounding atmosphere; and such may be some of the faint lines seen by M. Rayet. However, such constituents would probably, for the most part, soon cease to be gaseous, and augment the cloud by being chilled into a mist, or by hanging on the limits between their precipitated and gaseous conditions in the way explained in § 71. In any of these ways their spectral lines may be either enfeebled or suppressed.

101. Solar clouds may be of various kinds (see §§ 69 and 71). But the light from a sufficiently attenuated mist of particles giving a continuous spectrum would have the property of being so weakened in the spectroscope by being spread over the whole length of the spectrum, that it might be easily overlooked in such hurried observations as were made last August†. It appears probable, therefore, that the protuberance which was most

* *Comptes Rendus*, October 26, 1868, p. 838.

† Thus if a salt of sodium be burned in a candle-flame, though the sodium adds but little to the aggregate light of the flame, yet in a spectroscope with a sufficiently narrow slit the flame will give a background of almost insensible faintness upon which the two sodium lines are brilliantly rejected.

examined consisted of such a mist giving a diffuse and inconspicuous spectrum, and of the intensely heated air entangled in it giving rise to a spectrum of bright lines, nine at least of which partially escaped absorption and reached the earth.

102. The particles of cloud in this case act as the principal carriers of heat, receiving it by radiation from the photosphere, and communicating it by contact to the interspersed air; but they are not without aid in discharging this office. In fact wherever a bright spectral line is broader than the corresponding absorption line of the ordinary solar spectrum, we may be sure that the gas emitting it is exposed to direct radiations from the photosphere of those wave-lengths which correspond to the projecting portions.

103. Hitherto we have considered only the immense columnar flame to the east of the sun, with a great spot close to its base* which was probably caused by it (§ 72). But other spectroscopic observations of much interest were made. Captain Haig† saw a red line and a yellow line (probably C and D) in the spectra of two broad protuberances on the western limb. Fortunately the slit of his spectroscope was parallel to the sun's edge, the objects under examination happened to be opposite the point where the sun reappeared, and Captain Haig looked also at light coming from a situation close to the disk. Just before the sun's emergence he saw "a brilliant wide spectrum," in which the lines of the prominences were lost and two other bright lines presented themselves. This appears to be the only spectroscopic observation recorded of light taken from a stratum low enough in the sun's atmosphere to give a hope of seeing bright lines in the corona. But from other observations it appears likely that a rich harvest remains for other observers in this direction. MM. Stephan and Tisserand‡ saw an arch of light bordering the moon immediately after the beginning of totality, so bright that the moment when the total eclipse began could not be decided. Governor Hennessy§ saw the lower shell of clouds before the close of the total phase as an arch running round one-fourth of the circle: about twelve seconds after this he saw "a rough edge of brilliant white light," probably glancing through valleys on the moon, and in another second the sun had burst forth.

104. From these observations it appears certain that the brilliant wide spectrum which superseded the two narrow spectra of protuberances in Captain Haig's observations was the spectrum of the "shell of clouds" (§ 66) and the region between it

* Proceedings of the Royal Society, vol. xvii. p. 79. See also p. 82.

† Ibid. p. 74.

‡ See 'The Engineer' of November 13, 1868, p. 371.

§ Proceedings of the Royal Society, vol. xvii. p. 86.

and the photosphere. There can be little doubt now that this shell of clouds, which I erroneously supposed lay almost in contact with the hot stratum over the photosphere, is in reality no other than the long-sought boundary of the atmospheres of iron and of the other gases near it in vapour-density, and that the spectrum seen by Captain Haig will, when examined through a sufficiently fine slit, prove to be that spectrum of bright lines coincident with the fainter of the dark lines of ordinary sunlight, to which I last year took the liberty of asking the attention of the observers*. It is a matter equally of interest and importance to ascertain whether this bright spectrum may not be observed in the absence of an eclipse, in the way in which the spectra of protuberances have been made out by Messrs. Lockyer and Janssen †.

105. The whole of the light, however, which is seen with the telescope in this bright region cannot emanate from these spectral lines, although most of what is seen with the spectroscope probably does. For light from this class of spectral lines would maintain its brightness up to the edge of the solar disk, whereas the observations of 1860 point to the existence also of a *shell* of luminous matter (see latter part of § 66). Now, when we bear in mind what is said in § 64, I think we may presume that iron and several of the metals grouped about it in vapour-density (viz. the least volatile in an atmosphere of hydrogen of the metals calcium, chromium, manganese, nickel, cobalt, copper, zinc, and no doubt also barium, §§ 49 & 82) behave in the way indicated in that paragraph:—that their natural level in the sun's atmosphere is higher than the level they actually attain; that they are constantly making their way upwards, and as constantly precipitated by the increasing cold in a mist of liquid or solid particles which constitutes a shell of cloud conspicuous through the telescope and in photographs, but which will present with the spectroscope the appearance of a faint groundwork upon which bright lines from the subjacent gases will be seen] projected. The hot space within is filled with a mixture of all the constituents‡ of the sun's atmosphere; but throughout the enormous

* See a note published in the Monthly Notices of the Astronomical Society for December 13, 1867, and in Phil. Mag. S. 4. vol. xxxiv. p. 502.

† Proceedings of the Royal Society, vol. xvii. p. 91; and *Comptes Rendus* of October 26, 1868, pp. 836 & 838.

‡ The narrowness of the sodium and hydrogen lines in the solar spectrum (see footnote to § 39) would almost tempt one to conjecture that the stratum of cloud is the lower limit of the free hydrogen and sodium, if such a thing be possible, as well as the upper limit of the iron group of metals, and that the hydrogen and sodium, according as they diffuse downwards, either enter into some kind of union with metals or in some other way become so modified that they do not emit their usual spectra. Silver at a

range of cool atmosphere which extends outwards, in which the protuberances manifest themselves, and which are visible by borrowed light, three only of the known constituents of the sun (hydrogen, sodium, and magnesium) appear to be able to maintain a permanent footing in sensible quantities, although traces of the more volatile of the others* must no doubt also spread to nearly their full natural height, which is perhaps not far outside the gauze-like envelope of cloud.

106. If, now, a tempest sweeping through the sun's atmosphere hurry a portion of the vapours bounded by the shell of clouds into a much higher and cooler situation, they will there act the part described in § 68, and the cyclone beneath and the flaming column above will come into existence.

107. The discovery of bright lines in the spectra of solar flames is valuable, too, from its suggesting to us a possible, and, indeed, what is now the probable, cause of the four bright hydrogen lines in γ Cassiopeiæ, and the several other stars of which it is a type. If there be in these stars a shell of attenuated

high temperature absorbs oxygen, which is disengaged again as the temperature falls. Can it be that the hydrogen and sodium which are constantly streaming downwards unite in some such way with other metallic vapours, and that the compounds as they diffuse upwards are decomposed in equal quantities—the other metals to be precipitated as a mist of solid or liquid particles, the hydrogen and sodium to be set free as gases?

This hypothetical account of the limited breadth of the sodium and hydrogen lines is perhaps too much of a guess to have much prospect of being true; but I mention it here chiefly because it appears in some degree susceptible of verification. For if the atmospheres of hydrogen and sodium come thus to an abrupt end in a situation at which the temperature suddenly increases, we should expect to find them emit visible bright lines where they are heated by contact with the metallic atmospheres, though possibly much fainter lines than those of protuberances. Captain Haig's observation, that the lines of the prominences were lost when the spectrum widened out, is, so far as it goes, an answer in the negative; so are also the observations of spots in which no bright lines have yet been seen; but these answers do not seem decisive, and it would perhaps be worth endeavouring to ascertain at the eclipse of next year whether faint lines may not be seen in a slit of light from the shell of clouds kept separate from the light of regions further down.

* Such as copper and zinc. Dr. Miller in his 'Chemistry' records a curious instance of the volatility of copper at low temperatures. A beam which for many years was suspended over a furnace in a copper-smelting house in Norway contained minute beads of metallic copper studded through its texture: the copper must have been raised in vapour and so deposited within its fibres. Gold has been found similarly studding the beams of refineries. (Miller's 'Chemistry,' third edition, § 523). May we not further presume that those metals which, when rubbed, emit a smell are in some slight degree volatile in cold hydrogen? Rubbing the metal seems to remove a film of oxide and allow the moisture of the air to oxidize the fresh surface. The hydrogen which is then set free is probably what carries the taint of metal with it.

cloud permanently established at a sufficient height in their atmospheres, the cloud must act as a carrier of heat and raise the layer of hydrogen in which it floats to incandescence, while the adjoining strata on both sides remain cool. The cloud might be of any of the kinds enumerated in §§ 69 & 71 of my paper "On the Constitution of the Sun." And if the stellar atmosphere extend far outside the stratum of cloud, we may expect to find a dark absorption-band in the middle of each of the four bright hydrogen lines of these stars.

Appendix.

I would wish to take this opportunity of asking the reader to correct two errors in my previous papers. In a paper "On the Internal Motions of Gases compared with the Motions of Waves of Light," *Phil. Mag.* for August 1868, in the last line of § 2, for 16^2 or upwards of 250 times read $\sqrt{16}$ times. And in the paper "On the Physical Constitution of the Sun and Stars" cancel the footnote to § 28 (*Proceedings of the Royal Society*, vol. xvii. pp. 20 & 21).

From a review of the additional iron lines seen by Ångström with a continuous electric current, his account of which* I have succeeded in procuring within the last month, it is manifest that the cause of their not being seen in the spark from Ruhmkorff's machine is in most cases not what I conjectured in the footnote, but a purely physiological phenomenon. The spark is so evanescent (lasting only one three-thousandth, or thereabouts, of the time required to make the maximum impression on the retina) that no lines which are not of the exalted intensity that will compensate for this can be seen. It is evident, therefore, that a vastly feebler continuous current may reveal multitudes of lines not visible to our eyes in the spark.

LVI. *On the Action of Sodium on Valerianic Ether.* By J. ALFRED WANKLYN, *Professor of Chemistry in the London Institution*†.

THE ethers of the fatty acids were among the very first organic compounds to be submitted to the action of potassium and sodium. Löwig and Weidmann's paper describing researches on the action of the alkali-metals on these ethers was published in the year 1840. The results arrived at, although

* On the Fraunhofer Lines, together with a diagram of the violet part of the Spectrum, by A. J. Ångström and R. Thalen. Published by W. Schultz of Upsala.

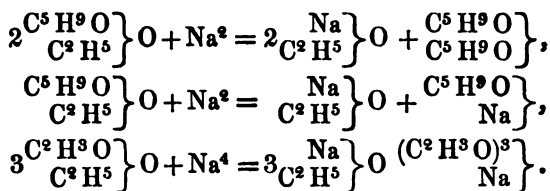
† Communicated by the Author.

for the most part correct as far as they went, were little appreciated at the time; and the subject was neglected until a few years ago, when it was taken up by Geuther, Greiner, Brandes, Frankland, Duppa, and myself.

One result has come out with the greatest clearness, and about it there has been absolute accord among all of us from the very first, viz. there is no elimination of the alcohol-radical. Neither acetate nor formiate nor valerianate of ethyl yields ethyl by the action of sodium upon it. Chloride, bromide, and iodide of ethyl, on the other hand, yield the radical ethyl (or hydride of ethyl and olefiant gas, which together represent ethyl) by treatment with potassium and sodium. In view of this fundamental difference between these two classes of ethers, and bearing in mind that in mineral chemistry no such distinction between the behaviour of the different classes of salts of a metal is to be found, I proposed in 1864 to regard the ethers of the fatty acids as salts in which not the alcohol-forming, but the acid-forming radical played the part of the metal. Acetate of ethyl I proposed to regard as ethylate of acetyl. This view, which has as yet not found much favour with chemists, is, as was shown in my former paper, in accordance with most of the known chemical history of these ethers (as, for instance, with the direction taken by the action of ammonia on them), and is in itself rational. *A priori*, we should be inclined to pronounce it more comprehensible that C^2H^3O , an already oxidized substance, should cling with less pertinacity to oxygen than C^2H^5 should do. In other words, we should expect acetic ether to split up during reaction into acetyl and peroxide of ethyl rather than into peroxide of acetyl and ethyl.

The researches of the chemists just mentioned, although agreeing with mine as to non-evolution of ethyl, differ from mine in many fundamental particulars. Their various papers, which are scattered through the chemical journals of the last five years, show that the action of the alkali-metals was regarded as being of a most complex order. Referring to Frankland and Duppa's memoir in the Philosophical Transactions, it will be seen that they regard acetic ether as polymerizing itself in presence of sodium, and then being attacked by sodium, and giving off hydrogen and alcohol, one equivalent of hydrogen for every equivalent of sodium that takes part in the reaction. And at the same time that this is going on, Frankland and Duppa consider that another portion of acetic ether deports itself quite otherwise, not polymerizing itself, but suffering a direct replacement of more or less of its hydrogen by the alkali-metal.

According to me, on the other hand, the reactions of sodium on these ethers are always simple, at any rate in the first stage:—



These are the equations by which the action of sodium on ethers of the fatty acids has been explained by me. All of them represent change of the same kind, viz. displacement of the acid-forming radical by sodium. The difference between them relates to whether the acid-forming radical is simply isolated, or enters into combination with more or less sodium.

The first of these equations was given in 1864*. The second was announced last year at the Meeting of the British Association in Dundee, and is to be found in 'The Laboratory,' p. 409. The third was also given in a modified form in Dundee, but distinctly brought forward and insisted upon at the last Meeting of the British Association at Norwich. A report of the paper read at Norwich occurs in the 'Chemical News,' 11 Sept. 1868, from which it may be learnt that one of my objections to the theory of Geuther and Frankland and Duppa is, that the equivalent of free hydrogen which they describe as complementary to every equivalent of sodium used up is mythical.

On the present occasion I have to describe particularly the research on valerianic ether wherein sodium-valeryl was obtained.

From the circumstance that so many chemists have worked at this subject with such discordant results, some idea may be formed of the difficulties of the investigation. It is essential to start with pure ethers. The aid derivable from quantitative determinations of the yield of product is required in order to make a correct interpretation of the reaction. In the instance of valerianic ether, and, indeed, in every case except that of formic, acetic, and propionic ether, there is a further source of perplexity and complication in the circumstance that there are isomers of the fatty acids, and consequently of their ethers.

Passing on to the description of my experiments:—

The valerianic ether was titrated, using alcoholic solution of potash, as has been described by me on another occasion. 100 parts of the ether neutralized 30·15 parts of potassium. The theory for pure valerianic ether requires 30·08. The valerianic ether was neutral to test-paper; and its purity may be looked upon as sufficiently guaranteed by this titration.

* Journal of the Chemical Society, vol. ii. p. 376.

In one experiment I employed—

Valerianic ether . . .	30.4 grms.
Sodium	5.8 „
Pure common ether . .	70 cub. centims

The sodium dissolved completely, without evolution of gas. To the product of the action water was afterwards added; there was again no evolution of gas. Two layers of liquid resulted on adding water, viz. an oily layer, containing, of course, much ether, and an aqueous layer, which was powerfully alkaline.

The aqueous layer was examined. The amount of free alkali ascertained by the use of test-acid was equivalent to 5.13 grms. of sodium. Furthermore, by careful distillation of the aqueous layer, there was extracted from it 4.5 grms. of absolute alcohol.

The oily layer, after being deprived of ether and dried, weighed 19 grms. A sojourn of forty hours *in vacuo* over sulphuric acid brought about a loss of 0.9 gm. A combustion made of the oil yielded the following numbers—C=66.31 per cent., H=11.7 per cent.; from which it is perfectly clear that it was not the same product as had been obtained by me in 1864 by prolonging the action, and by the employment of a higher temperature during the reaction between the sodium and the ether. The numbers obtained from an oil prepared under different conditions in 1864 were C=71.48 per cent., and H=11.67 per cent.

A further quantity of the oil having been prepared, a fractionation was undertaken. At first the liquid began to distil at about 150° C., and distilled almost to dryness at about 200° C.; but by repeated fractionation it was at length separated into two sharply defined fractions about equal in quantity.

The low fraction boiled between 130° and 140°, and was shown by titration and “limited oxidation” to be valerianic ether.

Titrated.—100 parts saturated 29.36 parts of potassium. The theory for pure valerianic ether demands that 100 parts shall saturate 30.08 parts of potassium.

“Limited oxidation.”—Treated by the well-known method of Chapman, Thorp, and Smith, it yielded only acetic and valerianic acids in equivalent proportion. Percentage of barium in the mixed baryta-salt = 46.11. The theory for an equivalent mixture of acetate and valerianate of baryta is Ba=46.13 per cent.

The upper fraction boiled at about 215° C.; only traces of liquid of still higher boiling-point were present, and these were removed. Combustions of it gave results in accordance with the formula $C^5H^{10}O$, and the liquid is a polymer of hydride of valeryl (valeral). It will be remarked that it is isomeric with valerianate of amyl. It differs from that ether by having a

higher boiling-point, and by not being attackable by alcoholic solution of potash, which very readily attacks valerianate of amyl. (I have titrated valerianate of amyl on other occasions, and found no difficulty in effecting the decomposition by means of alcoholic potash.)

The result, therefore, of the fractionation of the oily product was, that it consisted of about equal parts of unaltered valerianic ether and new product. Taking advantage of the unalterability of the new product by alcoholic potash and the easy decomposition of valerianic ether by this reagent, I made a titration of the original oily product in order to ascertain the relative proportions of valerianic ether and new product. 100 parts of the oil saturated 14·8 parts of potassium. The theory for pure valerianate of ethyl being 30·08, it follows that about half of the oil consisted of that ether—a result which confirms the conclusion drawn from the fractionation.

Collecting the experimental results we have :—

Valerianic ether employed	=	30·4	grms.
Sodium used up . . .	=	5·8	
Sodium found caustic . .	=	5·13	

The quantities required by theory in order to give one molecule of valerianic ether and one equivalent of sodium are—

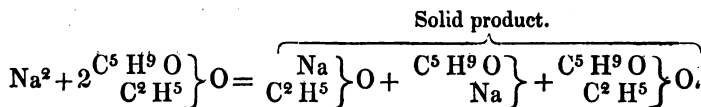
Valerianic ether	30·4	grms.
Sodium used up	5·38	
Sodium caustic	5·38	

The experiment therefore accords sufficiently with this theory. As many molecules of valerianic ether are employed as equivalents of sodium, and all the sodium is ultimately caustic.

19 grms. of oil, which on prolonged exposure *in vacuo* became 18·1 grms., were yielded by these materials. This oil consisted of 9 grms. of polyvaleral ($nC^5H^{10}O$), the rest being unchanged valerianic ether. Of course there must have been much loss of valerianic ether by the evaporation off of the 70 cubic centims. of common ether; so the unaltered valerianic ether experimentally found must have been considerably less than that actually present.

Furthermore, about half of the ethyl which was taken in the form of valerianic ether was afterwards found in the form of alcohol associated with the caustic soda.

These experimental data prove the following equation to be a correct expression of the reaction between sodium and valerianic ether at a gentle heat, and in presence of much common ether.



On the addition of water to the solid product we should have caustic soda, alcohol, polyvaleral, and valerianic ether, in the proportions which the above-described experiments have indicated.

Polyvaleral . . . $n \text{C}^5 \text{H}^{10} \text{O}$.

The properties of this very interesting compound will be more fully described on a future occasion. It is an oily liquid of a very pale yellow colour (whether this colour is essential or not I cannot tell). Boiling-point about 215°C . It has a very powerful and very peculiar smell. "Limited oxidation" appears to give only valerianic acid. Its spec. grav. in the liquid state is about 0.90 at ordinary temperatures.

London Institution,
November 1868.

LVII. *On Successive Involutes to Circles.—Second Note*.*

By J. J. SYLVESTER†.

[With a Plate.]

SINCE the appearance of the former Note on this subject, I have enjoyed the inestimable advantage of securing the cooperation of my all-accomplished and omni-capable friend Mr. Spottiswoode, to whose kindness and skill my readers are indebted for the beautiful figures given in the annexed Plate, which I shall proceed briefly to describe, and which, as far as I can learn, offer the first examples of the actual visible representation of any derived involutes of the circle beyond those of the first order. I propose, for want of a better word, provisionally to give the name of *Cyclodes* (suggested by Professor Cayley) to these spirals. They may be considered a genus of a more general class of spirals which I propose to name *algebraical spirals*, defined by the condition that the perpendicular on the tangent from a certain fixed point (which may be termed its pole) is a rational algebraical function of the angle of contingence; so that a cy-

* The thought foreshadowed in the concluding paragraph of the former note leads to the following theorem.

Let f, ϕ, ψ be quantics in α, β ; F the unicursal function obtained by elimination of α, β between

$$x=f, \quad y=\phi, \quad z=\psi;$$

Δ_x . F the discriminant of F regarded as a quantic in x and 1; $J(\phi, \psi)$ the Jacobian of ϕ, ψ ; R the result of eliminating ϕ, ψ between

$$y=\phi, \quad z=\psi, \quad J(\phi, \psi)=0;$$

Q the product of all the homogeneous linear functions of y, z which vanish at the double points of F ; then I say (and the proof is all but self-evident) $\Delta_x \cdot F = R \cdot Q^2$.

† Communicated by the Author.

clode may be said to be an *integral* algebraical spiral, *i. e.* one in which the perpendicular on the tangent becomes a rational integral function of the angle of contingency.

I find in a certain question, presently to be alluded to, the theory of the class so indisputably bound up with that of the genus, as to persuade me of the importance of the theory of the former being gone into by some one who has leisure for the investigation, and of the desirableness of an organic description being discovered or devised for the rational fractional case. The peculiar feature of the cyclode class is the absence of points of inflection, real or imaginary. The cusps of cyclodes are strictly analogous to the asymptotes in algebraical curves, like them entering and disappearing in pairs, creating partial interruptions of continuity, and thus separating the curve into distinct branches*. In the same way as the order of an algebraical curve is determined by the number of its intersections with any right line, so that of any such spiral may be characterized by half the number of its intersections with any circle having its centre at the pole. When the rational fraction which expresses the value of the perpendicular is of the degree m in the numerator and n in the denominator, the order will thus become the *dominant* of the two quantities $m + n$, $2n$.

Plate III. figs. 1, 2, 4, 5, exhibit examples of cyclodes of the first, second, and third orders, distinguished respectively, where required, by the number of accompanying dotted lines†. Let us consider more closely those of the second order, which separate themselves into two classes, the cusped and uncusped. The cusped class are the analogues of the hyperbola, the uncusped class of the ellipse, and the very remarkable secondary cyclode whose tail (to use the late Dr. Whewell's expression) is zero, and which may be termed the natural one of the order, is the analogue of the parabola. In the former Note I spoke of the point where it meets the cusp of the parent curve as an *abortive loop* or a co-incident pair of cusps; the words marked in italics are calculated to convey a false impression, and are to be considered cancelled. So, too, the passage (p. 301) commencing with the words "at such points," and ending with the words "points of retrocession," and containing the misprint "will change its *curve*" for "change its course," is erroneous and is also withdrawn. Instead of points of retrocession, I propose to call these "points of radiation" or "radiant-points;" the intervention of the cusps prevents the happening of the supposed "retrocession" at such points. This error illustrates the danger of, so to say, fighting in the dark, *i. e.*

* Parallelism for cyclodes bears some analogy to projection for algebraic curves, and operates in the way of addition or diminution upon the cusps as the latter process does upon the asymptotes.

† In figs. 5 and 3, which refer to cyclodes of the second order exclusively, it has not been thought necessary to adjoin the dotted lines.

reasoning from general mental impressions in the absence of all suggestive visible representation of geometrical forms*.

Let us compare the cusped cycloides of the second order in figs. 2, 4, where the *tail* † is negative, with the *natural* one in fig. 1, for which this tail is zero. It is interesting and instructive to trace the passage from the one to the other by following the fortunes of the double tangents, which in the figures 2 and 4 will be seen to connect the sort of Moorish arch that constitutes the middle finite branch with each of the adjoining infinite branches.

How is either of such double tangents to be determined? At the two points where it meets the curve the angle of contingence is not the same, but has increased by 180° in passing from the one to the other. Accordingly p , the perpendicular, if represented by $F\phi$ when the double tangent is regarded as a tangent at one point, will be represented by $-F(\phi + \pi)$ when that line is regarded as a tangent at the other point of contact, and the equation for finding ϕ becomes $F\phi + F(\phi + \pi) = 0$. Thus we see incidentally that p must become zero, *i. e.* that a point of radiation must necessarily exist somewhere between the two points of contact. And here I may remark incidentally that this throws light on the notable equation, applicable to any curves whatever,

$$\frac{ds}{d\phi} = p + \frac{d^2p}{d\phi^2};$$

for $\frac{d^2p}{d\phi^2}$, by virtue of the remark made in a footnote to the former paper on this subject, is the perpendicular to a tangent to the second evolute at a point corresponding to that, in the curve itself, for which p is the perpendicular to the tangent; but at corresponding points in a curve and its second evolute, the tangents, although parallel in *direction*, are opposite in *flow*. Hence $p + \frac{d^2p}{d\phi^2}$ (and not $p - \frac{d^2p}{d\phi^2}$) is the distance between these two tangents; and it is obvious that such distance is identical with the radius of curvature corresponding to the perpendicular p ; so that, viewed in this light, the differential equation above written is reduced to a *truism*. Returning to our cycloide of the second

* I believe I am correct in saying that in like manner a mistake made by Steiner in his description of a surface viewed only by himself "in the depths of his inner consciousness," was first discovered by Professor Kummer after the construction of an actual model. So impossible is it to *prove demonstration*, and to make oneself absolutely safe against the fallacy of ignoring entities on the one hand, or unduly assuming their existence on the other.

† In general the *tail* is the distance of the cusp of the first involute from the corresponding points of the involutes successively engendered therefrom.

order, we may write its equation under the form

$$p = \frac{a}{1 \cdot 2} (\phi^2 - \gamma),$$

where a is the radius of the base-circles, ϕ is zero at the apse, *i. e.* the point which divides the curve into two equal and symmetrical branches. Hence to find the nearest double tangent* we have

$$\phi^2 - \gamma + (\phi + \pi)^2 - \gamma = 0.$$

Putting $\phi + \frac{\pi}{2} = \psi$, this equation becomes

$$\psi^2 = \gamma - \frac{\pi^2}{4}.$$

The *tail* of the secondary cycloide (say τ) is obviously the distance of the apse (in respect of the centre) from the node of the parent first cycloide; and its length is $\frac{a}{2}(\gamma - 2)$, the distance of the apse from the centre being $\frac{a}{2}\gamma$. As γ increases towards 2, ψ decreases; that is, either double tangent tends more and more towards the horizontal; the Moorish arch therefore sinks, and the adjoining haunches rise until when $\phi = 0$, *i. e.* $\gamma = \frac{\pi^2}{2}$, the two double tangents are in direct opposition and merge into a triple tangent touching the arch at its centre.

As γ continues to decrease by ϕ becoming negative, the central arc sinks below the level of the adjoining branches, and the double tangents slope more and more towards its extremities, until at length they pass through the cusps; when this takes place the tangent to the second cycloide becomes perpendicular to the parent cycloide, and consequently touches the originating circle so that $p = -a$,

$$\text{i. e. } \frac{a}{2}(\phi^2 - \gamma) = -a,$$

$$\text{i. e. } \left(\psi - \frac{\pi}{2}\right)^2 - \psi^2 \frac{\pi^2}{4} = 2,$$

$$\text{i. e. } \psi = \frac{2}{\pi}, \quad \gamma = \frac{\pi^2}{4} + \frac{4}{\pi^2}, \quad \tau = \frac{a}{2} \left(2 - \frac{2}{\pi}\right)^2.$$

As γ goes on decreasing, the double tangents quit the Moorish arch altogether and connect the two infinite branches, which turn their protuberances towards each other more and more, until finally they touch and the double tangents coincide,

This happens when $\phi = -\frac{\pi}{2}$, *i. e.* $\gamma = \frac{\pi^2}{4}$.

* For of course we may write in general

$$(\phi^2 - \gamma) + ((\phi + 2i + 1)\pi)^2 - \gamma = 0,$$

i being any integer, and ϕ will give the direction of a double tangent.

As the tail goes on still to decrease, the double tangents become imaginary, the infinite branches intersect and cut out a lune, one extremity of which, the two cusps of the cyclode under consideration, and the cusp of the parent cyclode, together form a quadrangle, which continually contracts its dimension until finally it vanishes with the tail and the central arc, and the four points merge into the remarkable *round* point indicated in fig. 1, corresponding to the parabolic or transition case between the cusped and uncusped species. This paradoxical point is a mere creature of the reason, and can by no effort be made sensible to the understanding. Observe that, in this point, the curve dips its beak, so to say, into the cusp of the parent first involute, and yet touches the original circle. Professor Cayley informs me he has met with the same kind of point in an investigation into the form of the parallels to an ellipse, and proposes to call it a triangular point, as consisting of the union of a node and two cusps. At this point, in the case before us, we have

$$p = \frac{a\phi^2}{2} - a, \quad \frac{ds}{d\phi} = p + p'' = \frac{a\phi^2}{2};$$

so that, it will be observed, $\frac{d^2s}{d\phi^2}$, as well as $\frac{ds}{d\phi}$, vanishes when ϕ is made zero.

This gives me occasion to make a remark which I do not remember having seen in the text-books, viz. that for any curve, while *in general* $\frac{ds}{d\phi}$ indicates the existence of a cusp, this law is subject to the exception that if a succession of such derivatives

$$\frac{ds}{d\phi}, \quad \frac{d^2s}{d\phi^2}, \quad \frac{d^3s}{d\phi^3} \dots$$

all vanish simultaneously, there will not be a cusp in fact unless the last of the flush is of an *odd* order.

Fig. 5 exhibits the critical cases (1) of the double tangents in opposition, (2) on the point of quitting the central branch, (3) in coincidence. Mr. Spottiswoode informs me that this figure has not been drawn with the same attention to mechanical exactitude as the other figures of the Plate.

In fig. 5 are seen examples of the uncusped species. The Norwich spiral (of which a word or two more presently) belongs to this species, but is not drawn; its apse lies midway between the centre of the circle and the cusp of the first cyclode. In fig. 2 is seen an example of a symmetrical tricuspidal cyclode of the third order; in fig. 4, of a unicuspid alcyclode of the same order, where a loop replaces the missing cusps.

To return to the Norwich spiral; its radius of curvature ρ has been shown in the preceding rule to be always equal to its radius

vector r , reckoned from the centre of the circle. Now it is easy to see that whilst $\int d\phi\rho$ represents the arc of any curve, $\int d\phi r$ will represent the corresponding arc of its first pedal; so that the spiral in question possesses the remarkable property (capable, one would think, of some practical kinematic application) that these two arcs always remain equal to each other. More generally, if $p^2 + p'^2$ *, where p is a rational integral function of ϕ , and p' its first derivative in respect to ϕ , is a perfect square, the arc of the curve and of its pedal will always remain algebraically related. Here, then, we are led to consider the possibility of satisfying this diaphantine condition for cycloides beyond the second order. At a first glance the problem might seem to be impossible. For if the condition is satisfied by $p = F\phi$, a rational integral quantic in ϕ of the order n , it obviously will be satisfied also by $F(\phi + \lambda)$, λ being an arbitrary constant; and consequently we have only $(n-1)$ and not n disposable constants (or ratios) wherewith to satisfy the n conditions involved in a function of ϕ of order $2n$ being a perfect square.

This objection, however, is only apparent, and may at once be seen so to be, at all events as regards cycloides of an even order—say, of order $2m$. For we may suppose $p = F(\phi + \lambda)(f(\phi + \lambda))^2$, a quantic of the order m in $(\phi + \lambda)^2$, then $F^2 + F'^2 = f^2 + 4(\phi + \lambda)^2 f'^2$ is a quantic of the order $2m$ in $(\phi + \lambda)^2$, and the m disposable constants in f are sufficient to make this a perfect square. Thus, then, the n conditions are not absolutely incompatible. Still the disproof of the incompatibility might *seem* to involve the necessity of F being a function of $(\phi + \lambda)^2$, i. e. of the cycloide being of the symmetrical kind. Moreover, if the problem be attacked by a direct exoscopic method for cycloides of the second, fourth, and sixth orders, it will be found that the only cycloides which possess the required property *are* of the symmetrical kind, viz., for the second order, $p = \frac{a}{2}(\phi^2 - 1)$, for the fourth, $p = \frac{a}{2}(\phi^2 - 4)^2$, and for the sixth $p = \frac{a}{2}(\phi^2 - 9)^2$ †, or $p = \frac{a}{2}(\phi^2 - 9)(\phi^2 - 36)^2$. The infer-

ence, then, might appear to be almost irresistible as to the necessity of the symmetrical form holding good. But it is *not*

* It will be remembered that $r^2 = p^2 + p'^2$. I may remark incidentally that this equation enables us to extend the well-known one, $p^2 = r^2 - a^2$, applicable to the first cycloide: the general theorem which includes this as a particular case is obviously

$$p^2 = r^2 - r'^2 + r''^2 + \dots \pm a^2,$$

p being the perpendicular on the tangent of a cycloide of any order, and $r, r', r'', \dots$ the distances of the corresponding points in the cycloide and its successive evolutes from the centre of the originating circle.

† It is very easy to see that there is always one *reducible* symmetrical

so; it is true that only cycloides of *even* orders are reducible, i. e. capable of giving r as a *rational* integral function of ϕ ; but after the sixth order, i. e. beginning with the eighth, non-symmetrical reducible cycloides come into existence, and, as the order rises, become infinitely more numerous than those of the symmetrical kind.

Calling $2m$ the order, every distinct mode of making the partitions of numbers expressed by the two simultaneous equations,

$$\begin{cases} x_1 + x_2 + \dots + x_i = m \\ y_1 + y_2 + \dots + y_i = m \end{cases},$$

where i takes all possible values, gives rise to a system of equations yielding in general many solutions; and it is only when $x_1 = y_1, x_2 = y_2, \dots, x_i = y_i$ that the solutions are of the symmetrical kind. Moreover, even in that case, in *general*, and subject only to rare cases of exception, the reducing system of equations gives two distinct groups of solutions, one corresponding to symmetrical and the other to non-symmetrical cycloides*. This

cyclode of the order $2m$ defined by the equation

$$p = \frac{a}{\pi(2m)} (\phi^2 - m^2)^m,$$

corresponding to which

$$r = \frac{a}{\pi(2m)} (\phi^2 - m^2)^{m-1} (\phi^2 + m^2).$$

Thus, when $m=2$,

$$p = \frac{a}{24} (\phi^2 - 4)^2,$$

$$r = \frac{a}{24} (\phi^4 - 16);$$

when $\phi=0$, we have

$$p = \frac{2a}{3}, \quad p' = 0, \quad p'' = -\frac{2a}{3};$$

whence we may derive the following construction:—Draw an *uncusped* secondary cyclode with a tail equal to one-third of the radius; unwind from this a ternary cyclode beginning from the apse, which will become a cusp in the cyclode so engendered; and from this last cyclode, beginning at its cusp, again unwind a new cyclode, which will possess a *triangular* point at the apse of its atavian secondary cyclode. This will be a quartic reducible cyclode, and, as regards *form* (irrespective of position and magnitude), the only one that exists. By the way, it may be noticed that a system of coordinates consisting of the vectorial angle and angle of contingence furnishes what may be termed a *form* equation, i. e. one in which actual magnitude is ignored. Thus, *ex. gr.*, $\tan \theta = k \tan \phi$ is the *form* equation to a conic.

* It is to be understood that every x and y must be an *actual* integer, *zero* being for this purpose to be regarded, not as a number, but as a negation of number. Furthermore, if the x and y numbers are not only respectively equal each to each, but have all the same value (as *ex. gr. unity*), the corresponding system of equations become *incompatible*; or, to speak more philosophically, the order of the system becomes *zero*, which here *per contra*

Phil. Mag. S. 4. Vol. 36. No. 245. Dec. 1868.

2 H

wonderful theory, this outlying and unexplored region of geometry, in which the two great continents of algebra and arithmetic trend towards and come into contact at more than one point with one another, forms the subject of a communication to be brought by the author of this Note before the Mathematical Society of London, simultaneously and under the same roof with Mr. Norman Lockyer's announcement to the Royal Society of his equally, but not more surprising and certain to be prolific discovery of the sun's unsuspected chromosphere, the analogue of the ocean of forms of which the isolated power-forms $[(\phi^2 - n^2)^n]$ correspond to the piled-up rose-coloured prominences.

Athenæum Club,
November 23, 1868.

Errata in No. 243.

P. 295, footnote †, *for finite solution read finite rotation.*

— 296, for $s_1 = a\phi^2 + b\phi$ read $s_1 = a\frac{\phi}{2} + b\phi$.

for $p^2 + \left(\frac{dp}{d\phi}\right)r^2$ read $p^2 + \left(\frac{dp}{d\phi}\right)^2 = r^2$.

— 297, footnote, for $G = s' - s''' \dots$; $G' = s'' - s'''' \dots$ read
 $G = s - s'' + \dots$; $G' = s' - s''' \dots$

— 301, *delete* clause commencing with the words “at such points the curve” and ending with the words “points of retrocession.”

— 303, in concluding line of fourth paragraph omit the words “either as an abortive loop or”.

ought to be regarded as a number rather than as a negation of number; for the order of the system of equations is always lowered, not only by every x becoming equal to every y , but also by any number of x 's or of y 's becoming equal to each other; so that the order of the system sinking to zero, in consequence of all the x 's and all the y 's becoming equal, is only an extreme instance of this general law. If we go to the wider case of algebraical spirals, where $p = \frac{f(\phi)}{F(\phi)}$, the difference between the degrees of f and

F being still an even integer $2m$, where m is positive or negative, and require $p^2 + \left(\frac{dp}{d\phi}\right)^2$ to be made a perfect square, precisely the same method

of solution is applicable as when F is of the degree zero. If we call the degrees of f and ϕ κ and k respectively, so that $\kappa - q = 2m$, we have to make

$$x_1 + x_2 + \dots + x - \xi_1 - \xi_2 \dots - \xi_\eta = m,$$

$$y_1 + y_2 + \dots + y_\lambda - \eta_1 - \eta_2 \dots - \eta_\mu = m,$$

$\epsilon + \eta = \lambda + \mu = i$, where i takes all possible values,

$$x_1 + x_2 + \dots + x + y_1 + y_2 + \dots + y_\lambda = \kappa,$$

$$\xi_1 + \xi_2 + \dots + \xi_\eta + \eta_1 + \eta_2 + \dots + \eta_\mu = q.$$

Every such system of partitions give rise to a system of equations containing solutions of the diaphantine problem in question, *i. e.* the problem of making r a rational function of ϕ . When the degree of p in ϕ , *i. e.* $\kappa - q$ (and consequently m) is zero, the order of all the equation-systems undergoes a marked depression.

LVIII. *Notices respecting New Books.*

The Elements of Plane Geometry for the use of Schools and Colleges.
By RICHARD P. WRIGHT, formerly Teacher of Geometrical Drawing
&c. in Queenwood College, Hampshire. With a Preface by T. ARCHER
HIRST, F.R.S. &c., Professor of Mathematics in University College,
London. London: Longmans and Co. 1868 (pp. 211).

IN noticing this work we shall, in the first place, speak of it simply as a Treatise on Plane Geometry, and abstain from weighing its merits against those of any other manual. Regarded from this point of view, we may mention that it consists of four books; of which the first two are elementary, the third treats of ratio and of similar figures, the fourth of areas. The first two books are in substance very nearly the same as props. 1–34 of the 1st book, props. 1–33 of the 3rd book, and a few propositions in the 4th book of Euclid's 'Elements;' the differences, which at first sight look much greater than they really are, consist mainly in arrangement and in modes of statement and proof. For instance, the theorem "Through any three points A B C, not in the same straight line, one, and only one, circumference can always be drawn," and the problem "To describe a circle which shall pass through three given non-collinear points, P, Q, R," are neither more nor less than the theorem "One circumference of a circle cannot cut another at more than two points," and the problem "To describe a circle about a given triangle." The arrangement, however, is very different; *e. g.* the first chapter of the first book is devoted to theorems equivalent to props. 13, 14, 15 of Euclid's 1st book. But perhaps the capital difference is that Mr. Wright reserves the solution of problems to the end of the second book. It might be thought that to assume a variety of constructions in proving theorems without showing that previously proved theorems give the means of making those constructions, would lead to reasoning in a vicious circle; and of course this might easily happen. But we believe that Mr. Wright has avoided the danger, and that the deduction of his various theorems is perfectly rigorous.

The first chapter of the third book is devoted to a very elaborate discussion of the doctrine of ratio and proportion. The line taken will be most easily understood by a citation of two definitions. Ratio is defined thus:—"Between every two magnitudes A and B of the same kind, there exists a relation answering to the inquiry *how often the first contains the second*, which is called the ratio of A to B." Proportionality is thus defined:—"When two associated and variable magnitudes are so related that the ratio of *any* two values, A_1, A_2 of A, is equal to the ratio of the corresponding values, B_1, B_2 of B, these magnitudes are said to be proportional." It is tolerably plain, from the definition of ratio, that the discussion has to be preceded by an introductory discussion of commensurability and incommensurability, which, again, introduces the conception of limits.

And in consequence the chapter, whatever its merits in other respects, is exceedingly abstruse. It might also be inferred from the definition that it was intended to treat questions of proportionality through the medium of numerical measures; and, in fact, throughout the third and fourth books pure geometry is as much as possible dispensed with, and a good deal of them consists of what is more commonly termed mensuration, or the application of algebra to geometry. In short, the contents of the third and fourth books are, though very differently treated, in substance nearly the same as a few of the propositions in the second book of Euclid, the sixth book, and the propositions in mensuration deducible from them, together with the mensuration of the circle.

Of the parts of the book particularly well worth reading, we may mention the chapter on proportion, already described, and that on regular polygons; but besides these there are scattered up and down the book many new or, at least, unusual demonstrations, and several new constructions for the solution of problems. Moreover at the end of each chapter are many well-chosen *exercises*, in all between three and four hundred in number. In a word, Mr. Wright has produced a work of considerable merit, which no one could study from first to last without obtaining a thorough acquaintance with the elements of plane geometry; and as it is always instructive to see the same thing from two points of view, a student who had learned geometry from Euclid's 'Elements' would doubtless learn something from perusing it.

We have hitherto spoken of the book simply with reference to its contents. It has, however, a further claim on our attention; for it is expressly designed to supersede Euclid's 'Elements' as a text-book. In fact Professor Hirst mentions in his preface that it had its origin in an experiment made by himself some years ago in the school connected with University College, London; and the experiment was that of "teaching geometry instead of teaching Euclid." Now we are perfectly certain that any class taught geometry by Professor Hirst would be extremely well taught, and that at the least the usual percentage would leave the class with a sound knowledge of geometry whatever the text-book used, or if, as seems to have been the case, no printed text-book was used. Yet we think the antithesis hardly just. The real subject of the experiment was whether Mr. Hirst could teach geometry most conveniently by means of a manuscript of his own composition, or by using Euclid's 'Elements of Geometry' as a text-book; and the question suggested by the present work is whether, on the whole, it were better to use as a text-book of plane geometry Euclid's 'Elements' or Wright's 'Elements.' The question is fairly raised in the present case, because it is unembarrassed by the totally distinct question of alleged errors in treatment of detail.

Whether, as a matter of fact, Euclid's 'Elements' will be displaced by a variety of text-books, some used in some schools, some in others, we do not know; but we must not disguise our opinion that the

change, if effected, will be for the worse, even if books as good as Mr. Wright's are adopted. We think that an advocate for the use of Euclid's 'Elements' might fairly urge—

(1) That, on the whole, the first and third books of Euclid would prove easier to a learner than Mr. Wright's first and second books.

(2) That the arrangement by which theorems are treated first and problems afterwards is calculated to weaken the learner's perception of the conditions of proof, and that it might in many cases lead to an unnoticed failure of proof. And he might urge that most learners, particularly boys, more readily understand a reason for a thing *done*, than a reason for an abstract truth of which they do not see the use.

(3) That to look at truths so elementary as Euclid 35. I., and the propositions of Book II., through the medium of numerical measure is to take unnecessarily a very artificial view of those questions, although its artificial character may be disguised by familiarity.

(4) That, as a matter of science, Euclid's treatment of proportion has the advantage of being perfectly rigorous and extremely elementary, Euclid's definition embodying perhaps the most elementary conception that can be formed of equality of ratios.

(5) That, from a teacher's point of view, it is possible to make most boys understand the sixth book, and then to lead them on successively to geometrical limits and to numerical measures, when the question of incommensurability can be faced; but that to try to master all three difficulties at once is to undertake a nearly hopeless task; and that practically few, if any, learners would get any deeper notion of ratio than that of commensurable magnitudes, at least until they looked back to their elementary work after having pursued their studies into the higher branches of mathematics.

LIX. Intelligence and Miscellaneous Articles.

ON THE CONDUCTIVITY OF SOME LIQUIDS FOR HEAT.

BY A. PAALZOW.

AS far as I am aware, there are no statements respecting the conductivity of liquids for heat besides those of Despretz*. Occupied with investigations on the conducting-power of liquids for heat, I was interested, for theoretical reasons, with the question whether in liquids also there is a connexion between the conduction of heat and the conduction of electricity. Hence I instituted the experiments I am about to describe, only with the view of acquiring a general knowledge of the phenomenon in question, and not with the view of acquiring accurate numbers; for I did not possess the necessary means of solving such a problem.

I used for the experiments a cylindrical glass vessel of 60 millims.

* *Ann. de Chim. et de Phys.* vol. lxxi. p. 216.

internal diameter. It was filled with the liquid to be investigated; and on the top, touching the liquid, was a copper or leaden vessel, into which steam was continually passed from a small steam-boiler. The condensed water was reconducted into the steam-boiler.

Four horizontal thermometers were fixed laterally in the vessel, in such a manner that their bulbs coincided with the axis of the cylindrical vessel. Owing to changes affecting the glass vessels, I was not always able to give the fourth thermometer the same distance from the third; and I shall therefore not always give its temperature. The liquids to be investigated had always to be heated about eight or ten hours before they exhibited constant temperature.

Water was first of all compared with solution of chloride of sodium. The heating-vessel was of copper; the first thermometer was 10 millims. from the side of the vessel to be heated; the others followed at distances of 60 millims.

I. Water and chloride of sodium.

	Water.	Chloride of sodium.
1st thermometer	72°·4 C.	72°·2 C.
2nd thermometer	26	23·8
3rd thermometer	20	17·9
4th thermometer	19	17·4

External temperature 13°.

Forming, now, from the first three temperatures t_0 , t_1 , t_2 the quotient $q = \frac{t_0 + t_2}{t_1}$, it becomes 3·6 for water, and 3·79 for chloride-of-sodium solution, from which it follows that water conducts better than common salt.

As the thermometers were so far apart that the temperatures very rapidly decreased, the first three thermometers were fixed 30 millims. apart. Water and concentrated solution of sulphate of copper were compared.

II. Water and sulphate of copper.

	Water.	Sulphate of copper.
1st thermometer	73°·2 C.	72°·5 C.
2nd thermometer	34·3	34·2
3rd thermometer	22·2	22·2
External temperature....	14·5	14·5

The quotients are 2·78 for water, 2·77 for sulphate of copper; both, therefore, conduct equally well.

For comparing water with sulphuric acid (spec. grav. 1·25), a leaden heating-vessel was used: the first thermometer was at a distance of 14 millims. from the heating leaden surface.

III. Water and sulphuric acid.

	Water.	Sulphuric acid.
1st thermometer	63° C.	65°·7 C.
2nd thermometer	31	32
3rd thermometer	22·75	23
External temperature..	16	16

The quotients q are 2·74 for water, 2·77 for sulphuric acid; so that water conducts a little better than sulphuric acid.

In order to compare also a more concentrated solution of sulphate of zinc with water, I put the apparatus together again, and investigated mercury at the same time.

IV. Mercury, water, sulphate of zinc.

1st thermometer	82° C.	68° C.	65° C.
2nd thermometer	65	36·2	32·5
3rd thermometer	50·8	25	20·2
External temperature..	16	14	15

The quotients (q) are 2·04 for mercury, 2·57 for water, and 2·64 for sulphate of zinc.

To calculate the ratios of the conducting-powers of different liquids, the quotients

$$q = \frac{t_0 + t_2}{t_1}, \quad q_1 = \frac{t_1 + t_3}{t_2}$$

should have been constant, which, however, is not the case, as the first Table shows. At any rate, a greater number of equally distant thermometers should have been used as a control.

I content myself, in conclusion, with arranging the liquids in series in which the best-conducting precedes.

Conducting-power for—

Electricity.	Heat.
Mercury.	Mercury.
Sulphuric acid.	Water.
Chloride of sodium.	Sulphate of copper.
Sulphate of zinc.	Sulphuric acid.
Sulphate of copper.	Sulphate of zinc.
Water.	Chloride of sodium.

Poggendorff's *Annalen*, No. 8, 1868.

MOLECULAR CHANGE PRODUCED BY MAGNETISM IN STEEL BARS.

BY M. TRÈVES.

More than twenty years ago several physicists, among others

Messrs. Wheatstone, Joule, Wertheim, and De la Rive, showed in various ways the influence of magnetization on the molecular constitution of iron and of steel.

The vibrations produced in rods and wires of steel or of soft iron by an alternate succession of magnetizations and demagnetizations, as well as the modifications which magnetization produces in the elasticity and even the dimensions of these metals, have led to the assumption that the development of magnetism is due to a common direction imparted to the particles of magnetic metals, and consequently to a change in the arrangement of the molecules—a conclusion directly verified by a great number of researches, and especially by those of M. Wertheim. The following new fact confirms in a remarkable manner this mode of view.

M. Trèves has been led to examine the change of molecular condition produced in a bar of iron when it is subjected to a magnetic action.

He takes two identical steel tuning-forks, which therefore are in unison; he provides them with mirrors by Lissajou's method, and arranges them opposite and in planes at right angles to each other. One of them is surrounded by a stout helix, through which is passed the current of a battery of eight elements. If one of them be sounded by a bow the vibrations commence; and as the tuning-forks are in tune, the figure reflected in the second mirror is a perfectly fixed luminous circle. Magnetizing suddenly the tuning-fork in the middle of the coil, the luminous circle is at once inclined, changes into an ellipse, vibrates right and left with a velocity which gives the measurement of the new vibratory motion.

The tuning-forks used gave 136 simple vibrations in a second; and it has constantly been observed that a beat was produced at intervals of six seconds, which means that in a second the tuning-fork suddenly magnetized gave $136\frac{1}{6}$ vibrations. If the current be opened (that is, the tuning-fork restored to its natural state), the figure ceases to move and again becomes a fixed circle.

Since 1847 Mr. Joule*, working with soft iron and with steel wires stretched or not stretched, had observed the changes in length and volume when they are magnetized. The preceding arrangement may also be utilized for this investigation; and M. Trèves proposes to use this optical method, as well as graphically tracing vibrations, to observe these effects in steel according to their composition and physical state.—*Bibliothèque Universelle*, September 15, 1868, *Comptes Rendus*, August 3, 1868.

* Philosophical Magazine, S. 3. vol. xxx. pp. 76, 225.

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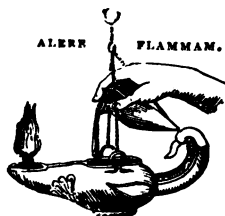
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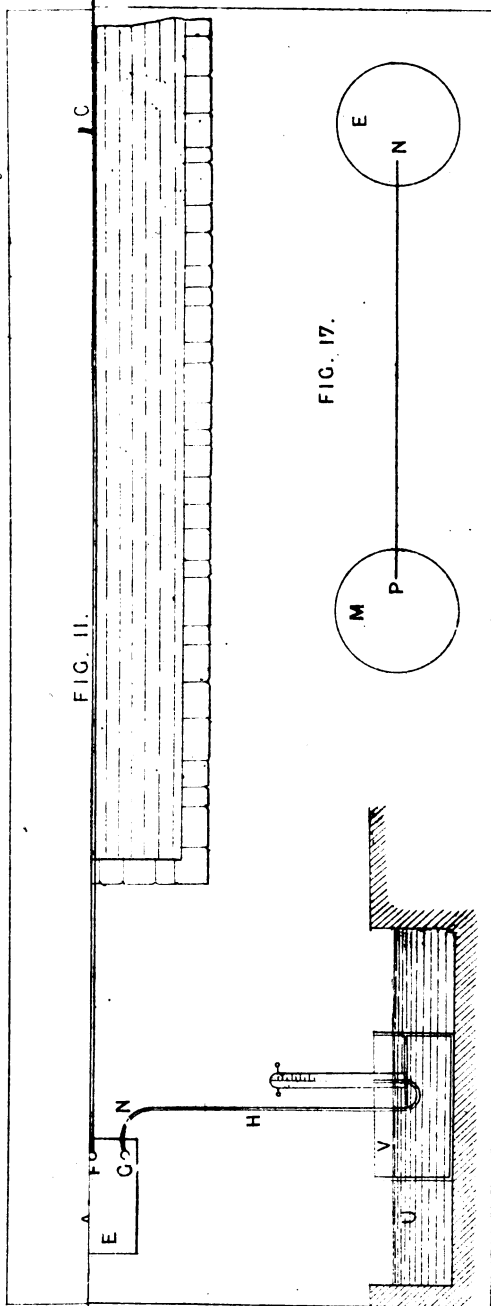
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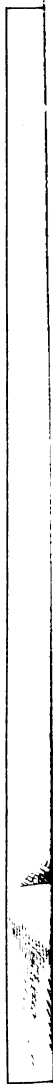


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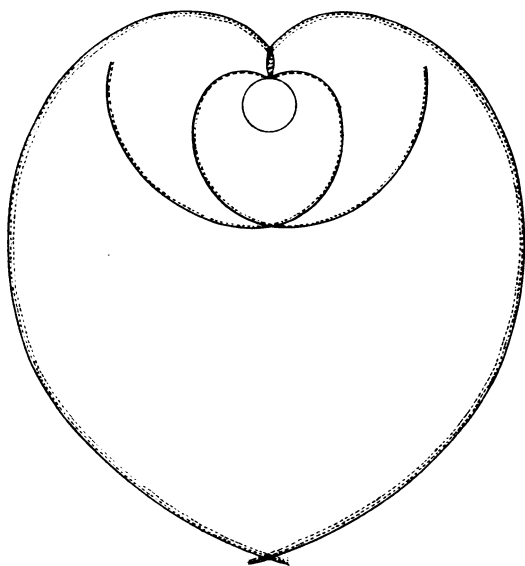
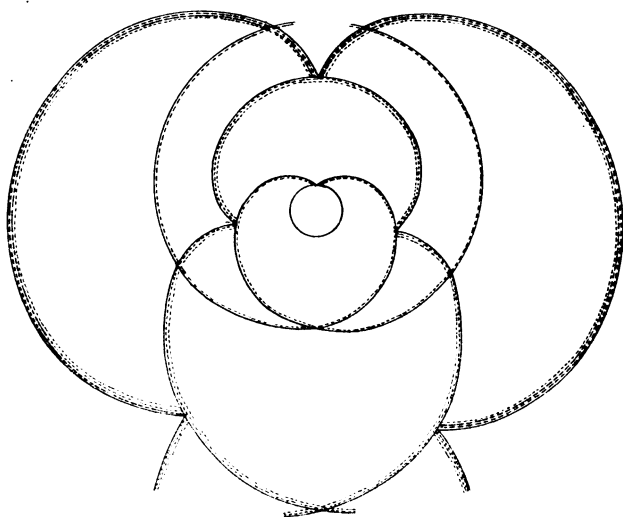


Fig. 2.



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Fig. 4.

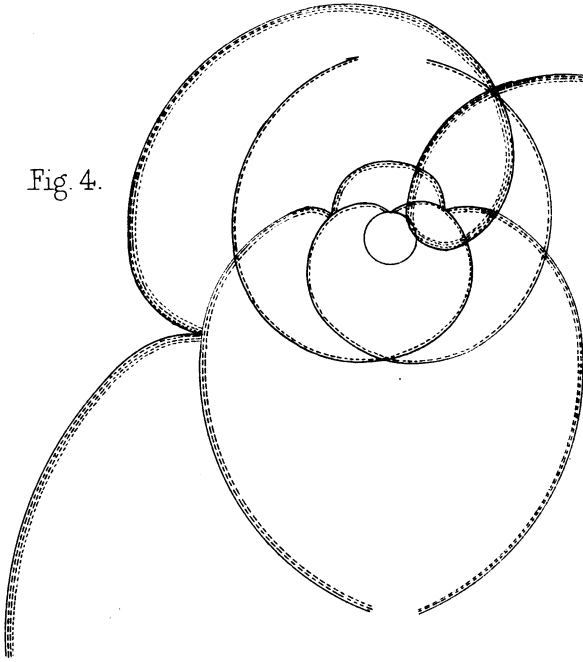
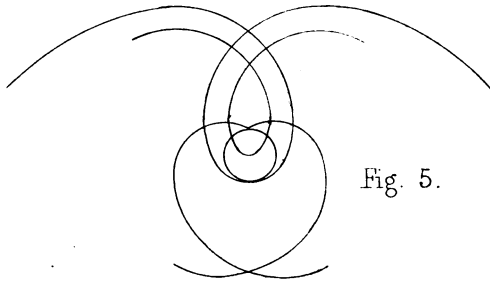


Fig. 5.



1. Triple tangent.
2. Double tangent passes through Cusp.
3. Double tangents coincide.

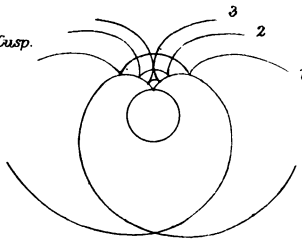


Fig. 3.

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