

ping sparks pass between the terminals at more or less rapid intervals, exactly like the sparks of a small Leyden jar. They pierce writing-paper with minute holes. It is usually necessary to approach the terminals to a distance of 0.3 inch, when the striking-distance of 8040 cells is 0.34 inch, in order to produce this static discharge.

It has been found that an accumulated charge of a condenser of 42.8 microfarads capacity, charged with the potential of 3240 cells, produced neither an elongation nor a contraction of a metallic rod 0.2 inch when suddenly discharged through. This charge deflagrates 10.5 inches of platinum wire 0.0125 inch in diameter.

More dense sparks were obtained with one of Apps's coils for producing 6-inch sparks when the primary was connected with 1080, 2280, 3480 chloride-of-silver cells, than when it was used with a zinc-carbon bichromate-of-potash battery of 6 cells, producing a current 300 times as great, thus showing the influence of high potentials in inducing secondary currents.

These currents of high potentials have also a marked effect in inducing magnetism, when the actual current is taken into account.

The second part of the paper, which is in course of preparation, will deal with the discharge in rarefied gases, in the so-called vacuum tubes.

December 20, 1877.

Dr. ALLEN THOMSON, Vice-President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "Notes on Supersaturated Saline Solutions." By CHARLES TOMLINSON, F.R.S. Received September 14, 1877.

There is probably no subject in science that is more involved in contradiction than that of supersaturation. All the phenomena connected with it seem to behave differently in the hands of different inquirers, so that the facts affirmed by one writer are simply denied by another; and the same theory which seems to have been disproved by one is again and again brought forward by another.

Take one point by way of example, namely, the nuclear action of bodies in producing the sudden crystallization of a supersaturated saline solution. Ziz, in 1809, stated that not only air, but solids, act best as nuclei when dry: if wet, or boiled with the solution, or thrown into it

while hot and allowed to cool with it, they are inactive. Löwel (1850-57) denies that air, whether wet or dry, has any nuclear action; but he admits that solids exposed to the air become active, and that alcohol is always active. Selmi and Goskynski, in 1851, assert that dry air is nuclear, and acts by getting rid of water at the surface, and producing small crystals there which continue the action. This seems to be a revival of Gay Lussac's theory, namely, that air is absorbed at the surface of the solution and precipitates a portion of the salt in the same way that one salt may precipitate another, and this precipitate continues the crystallization. Lieben, in 1854, states that soot is a nucleus, also platinum black whether ignited or not; that pounded glass heated in sulphuric acid produces sudden crystallization, but that platinum sponge and precipitated sulphate of baryta after being heated have no action. Schröder, in 1859, remarks that it is always a matter of chance whether such or such a substance produces crystallization. "Such facts," he says, "singularly increase the difficulty of interpreting theoretically the phenomena of supersaturation." He concludes that the only general rule that can be admitted in the presence of so many opposed and contradictory results is that bodies act on supersaturated solutions only after having been exposed to the air. In 1866 Gernez and Viollette and in 1868 Schiff are satisfied that there is only one nucleus for a supersaturated solution, and that is a salt of the same kind as the one in solution or one isomeric therewith. In 1866 Jeannel opposes this theory of pancrystallography, as he calls it, on the ground that it cannot be supposed that crystals, often of rare salts, are to be found waiting in the atmosphere, ready to enter our flasks as soon as they are uncovered. Pellogio* also, in 1875, "gives proofs that the phenomena of supersaturation are not so simple as the French physicists would imply, namely, that the only nucleus is a salt of the same kind," seeing that some supersaturated solutions, such as those of hyposulphite of soda, acetate of lead, acetate of soda, &c., may be exposed to the air, in places where the air is any thing but still, for fifteen or twenty days without the formation of crystals. He states further that porous bodies are active, such as common sponge, platinum black, iron reduced by hydrogen, and carbon. For example, carbon was raised to a red heat, quenched under mercury, and introduced into a solution of 100 sodic sulphate to 102 of water: it fell to the bottom and disengaged gas for some time; crystallization then set in and spread all through the mass. Viollette, on the contrary, finds that bodies greedy of water and capable of being hydrated, such as the fused sulphates of copper and of iron, and porous bodies recently calcined, such as carbon, have no action on supersaturated saline solutions.

In the midst of all this conflict of testimony, my own results were not likely to escape censure. Gernez and others opposed me; and Professor

* *Rendiconti R. Ist. Lomb.*, 1 Luglio. References to most of the other authorities are given in previous papers.

Grenfell, some eighteen months ago, joining in the opposition, having told me of his experiments, I asked him to prepare a paper on the subject. In this *, as also in a second paper, he is at variance with all his predecessors. In his second paper he gives a summary of the three principal theories that have been propounded, and goes into minute experimental detail in support of his own objections to them and of his own views. His theory rests on absorption, which, it will be seen above, is not so novel as he supposes it to be.

There is one point upon which all observers, down to the time of Prof. Grenfell, are agreed, namely, that if a supersaturated solution, say of Glauber's salt, boiling in a flask, be tied over with bladder and left until cold, it will, if the bladder be pierced, immediately become solid. Indeed this has long been a common lecture-table experiment; and I thought it a step in advance when I showed that if this experiment be made in the open air of the country, the solution remains liquid. I also exposed solutions to the air of my garden with various results, as I have already had the honour of describing to the Society †. But Prof. Grenfell goes far beyond any thing that I, or previous observers, have done; he exposes these solutions to the air of rooms, places them on various surfaces, handles them, makes mud pies with them ‡, and yet they do not crystallize. It is probable that his minute method of dealing with drops may alter the conditions; but certainly in the air of my house and garden I cannot expose my flasks as he does his drops without producing sudden solidification.

I will give two or three out of a multitude of similar results. Last summer I made a strong solution of sodium sulphate and filtered it into two long-necked globular flasks, which were reboiled and tied over with bladder. They were left during three weeks and remained unchanged. The bladder over one flask was then pierced and the solution immediately crystallized. I took the other flask into the fields and uncovered it; it remained liquid during a long walk, although the wind blew into the neck with such force as to produce a musical note. When the still uncovered flask was brought into the house the solution soon crystallized. I have repeated this experiment with this and other salts with similar results, and it furnishes an argument against the Gernez and Viollette theory.

A strong solution of alum was tied over just after being boiled. It was repeatedly shaken during several days. While resting on the mantleshelf of my study the bladder was pierced, when immediately some amorphous powder at the bottom of the solution began to assume

* Proc. Roy. Soc. xxv. p. 124.

† Proc. Roy. Soc. xx. p. 41.

‡ In April last, during an interval between heavy showers which had thoroughly washed the mould of my garden, I poured upon it a solution of Glauber's salt (2 to 1) and it crystallized immediately.

shape and grow into fine octahedral forms, increasing from the bottom upwards, with the usual evolution of heat.

Two similar flasks in my study, containing sodium sulphate, were pricked with a pin; one crystallized immediately and the other did not. Next day I enlarged the opening into the second flask to the size of a small pea, and after two or three hours the solution became solid.

Pellogio and others claim for sponge and other absorbent bodies a nuclear action. I immersed three bits of sponge in boiling water, and took them out with forceps and put them in a dinner-plate, which together with the forceps were placed in the kitchen oven. Three flasks, containing sodium sulphate solution (3 salt to 1 water), prepared the day before, were taken into the kitchen, and the bits of sponge, now dry, hard, and warm, were taken up with the forceps and dropped severally into the flasks, which were covered with small beakers. The flasks were put upon the window-ledge in my study. One of the solutions crystallized after eight hours; the other two formed a good deal of the modified salt, in which the sponge became entangled: the second solution crystallized on the third day; the third solution had not crystallized after ten days, although it was repeatedly shaken; the beaker was then taken off, and the solution crystallized within half an hour. In all three cases the sponge soon absorbed a portion of the solution and became thoroughly soaked; the solutions were exposed to considerable cold in the window, the weather being chilly, but the sponge was quite inactive.

Such results as these are certainly in opposition to the absorption theory; but the difficulty in this, as in other parts of the subject, lies in the fact that one observer, in an apparently carefully conducted experiment, finds sponge, charcoal, and other porous bodies to be nuclear, while another equally good observer (M. Viollette, for example) declares them to be inactive. In trying the effect of vapours on these solutions some years ago, a sponge dipped in ether, chloroform, bisulphide of carbon, &c. was held over the surface: whenever the sponge touched the surface crystallization set in; or passing the sponge hastily over the surface the portion adhering to the sponge crystallized immediately, while the solution in the vessel did not.

In repeating Prof. Grenfell's experiments on drops, I find the same spirit of contradiction to prevail. The solutions which with him are so permanent are transitory with me. I found sodic sulphate solutions of various strengths, from six parts of salt to one of water, to one part of salt to one of water, to be sensitive in every room of my house and on every kind of surface on which they were deposited. My method was to boil the filtered solution in a clean flask and then to cover the neck with a small beaker. A couple of dropping-tubes (constantly kept in water, except when in actual use, so as to keep them free from nuclei) were employed for getting a supply of the solution from the flasks. A large drop, made up of 5 or 10 or more single drops, was in this way deposited

on the surface to be tried, and around this were arranged a number of single drops. My observation agrees generally with that of Prof. Grenfell, that the large drop is more ready to solidify than the smaller ones; and the reason that I should give for this is, that the larger surface is more likely to catch nuclei from the air than the smaller ones. In the early stage of this inquiry it was thought that the sudden shock produced by the inrush of air when the bladder was penetrated produced crystallization; but Prof. Turner, of University College, London, showed that if the neck of the flask containing the solution were drawn out to a capillary opening the solution could be retained in the flask without becoming solid. Löwel also showed that solutions could be kept longest in ordinary flasks in proportion as their necks were narrow; and another early observer noticed that while a flask with its neck upright led to crystallization, it was less likely to do so if the neck were placed at an angle, and still less so if placed horizontally. This evidently points to the increased difficulty of a nucleus getting into the flask under such circumstances.

I tried a large number of experiments on supersaturated solutions in Prof. Grenfell's manner, not only with those of sodic sulphate, but also of sodic acetate, magnesian sulphate, zinc sulphate, potash alum, ammonia alum, sodic carbonate, and some others. On uncovering the flasks for the purpose of filling the dropping-tube, the solutions in the flasks often crystallized suddenly*; and it has not unfrequently happened that the drop of sodic sulphate and of some other solution at the end of the dropping-tube suddenly became solid before it fell, and thus acted as a nucleus to the contents of the tube.

I repeated Prof. Grenfell's experiments with oily surfaces. A drop of oil (olive or niger) rubbed with the finger over a glass plate made the plate powerfully nuclear: drops of sodic sulphate became immediately solid, the large drop swelling up into a well-shaped conical figure with a rounded top.

Experiments with magnesian sulphate (2 to 1 solution) showed this salt in drops to be even more sensitive than sodium sulphate. Drops of zinc sulphate (2 to 1) and of sodium carbonate (3 to 1) soon became solid on glass, paper, and other surfaces, whether in the open air of my garden or in different rooms of my house.

Potash alum solution (2 to 1) in drops crystallized rapidly into an opaque white mass, or into transparent crystals in concentric circles

* In reboiling, the flask must be kept in motion over the flame so as to melt the salt gradually and not allow one part of the flask to become much hotter than another. As the solution approaches boiling, the flask, being chemically clean, is liable to violent bumpings. These may be prevented by keeping in each solution a fragment or two of charcoal (that from cocoanut-shell being most efficacious), as pointed out in my paper in the 'Proceedings' for 1869, p. 251. That the action of the charcoal is not impaired by use has been shown in these repeated reboilings as well as in the cases given by me in the *Phil. Mag.* for August 1875.

round a central pit. A similar solution of ammonia alum behaved in a similar manner: an opaque white ring formed at the circumference of the drop and gradually closing in on the centre, and drawing matter therefrom, produced a pit-like depression. The large drop of the solution often displayed suddenly beneath the surface a small well-shaped octahedron, and this increased in bulk until the whole solution was solid. The crystal would sometimes appear while the large drop was in the act of being built up; and the solutions in the alum flasks, more frequently than in the other flasks, became solid and had to be reboiled.

Sodium acetate in drops solidified both in crystalline lines and also in heaps, these varieties occurring in the same set of drops. The crystalline lines originated generally in a radiant-point, and in some cases two or three such points caused the needles to cross. Sometimes in the open air a speck of soot or a small insect formed the radiant-point. Within doors the carrying of the plate across the room caused several of the drops suddenly to crystallize. It was noticed with this and other saline solutions that the deformed drops crystallized first, while those that formed perfect lenses with sharply cut edges remained liquid for a longer time. Drawing a point across these so as to deform their shape generally led to crystallization; but when a tail thus formed sprang back again and the drop recovered its form there was no crystallization. In general when a point was drawn through a drop, crystallization set in where the point made its exit. Oil of almonds smeared over a plate and polished with the pad of the hand was eminently nuclear.

These experiments with drops were conducted during the week of fine weather commencing on the 26th of June of the present year. I had often noticed during many years past, while conducting experiments on the cohesion figures of liquids, the motions of camphor, creosote, &c. on water and on surface-tension generally, that the phenomena varied greatly from day to day. On dull moist days drops spread out into films sluggishly or not at all; camphor, creosote, &c. would be more or less inert; in short, all surface-tension phenomena would be in a depressed state. Whereas on bright sunshiny days, with a brisk dry air, although the temperature might not be high, some force seemed to be present which exalts all the phenomena of surface-tension. It may be the evaporative force or the crystalline force; but whatever it is it has a powerful influence on the phenomena of supersaturation. As already noticed, the solutions in the flasks frequently became solid, and the drop at the end of the dropping-tube could not be delivered on account of its solidifying. The drops on various surfaces formed well-shaped lenses, and any thing that tended to destroy their symmetry led to crystallization. These facts bear affirmatively on the details already laid before the Society by Prof. Van der Mensbrugghe and myself on the connexion between the surface-tension of liquids and supersaturation*. Although the con-

* Proc. Roy. Soc. xx. p. 342.

clusions of this paper have been called in question by some of the French authorities on this subject, yet I venture to submit that the foregoing details support our main conclusion, which may be so far modified as to present itself in this form, namely, that during certain states of the atmosphere, if not always, such a connexion really exists.

On the 1st of July rain set in, and the weather during some days was cloudy and unsettled. The effect of this change on some of the solutions was remarkable; but I saw reason to direct my attention specially to the behaviour of the sodic acetate solution. During these wet and unsettled days the solution exposed in drops remained permanently liquid. I found that even when crystals of the salt from a drop that had previously become solid were brought into contact with the liquid drops, they no longer produced crystallization. This led me to suspect that the salt is deliquescent as well as efflorescent in varying states of the atmosphere. Accordingly on placing a known weight (30 grains) of the dry salt on filtering-paper in the scale-pan of a balance I found that it increased in weight from day to day in wet weather, and lost weight in dry. The same experiment was tried repeatedly with the supersaturated solution, of which 30 grains placed in the glass scale-pan became 33 in the course of some hours, and went on increasing from day to day during damp weather; but when dry weather set in it lost in weight, and when the solution crystallized there was a further loss of 6 or 8 grains from evaporation consequent on the heat liberated during solidification.

Here, then, is the explanation of the fact which has excited the surprise of several observers, namely, that supersaturated solutions of sodic acetate may remain liquid for days, and even for weeks together, exposed all the while to the dust of the room. Such a fact has been cited in disproof of the theory of aerial nuclei, and was appealed to by the French chemists as a complete answer to the theory of surface-tension; and it had so powerful an effect on the mind of Professor Van der Mensbrugghe as to lead him to withdraw from all further connexion with that theory.

With a westerly wind, a bright sun, a tolerably high temperature, and a moist air, I have known the drops of sodic acetate to remain liquid during eleven or twelve days, and drops that had previously become solid liquefied again. The point of a needle drawn across the drops, which had thus absorbed water, had no action in inducing crystallization, though a considerable amount of friction was used. This was on glass; on writing-paper each drop, after remaining liquid for some days, had a wet halo round it of considerable width; but it seems that as the paper absorbed water from the drop, the drop was absorbing moisture from the air. On passing the point of a pencil through the drops on paper, so as to draw out a tail, there was no action, and a crystal of the salt placed in the drop was equally inert. In fact the solution was no longer supersaturated. Sodic carbonate remained for days together on paper without

crystallizing: the paper became wet and sodden, and all appearance of drops ceased.

All the solutions thus exposed in drops are more or less affected by the state of the atmosphere as to moisture. The drops of the solution of alum instead of becoming solid in a short time, as they do when the air is dry, remain for hours in the liquid state, and then one or more of them would display an octahedral crystal in the midst of the liquid, which was the more conspicuous in a compound drop, or one made up of five or ten single drops. In damp weather I have known the drops to remain liquid during from twelve to twenty hours; and I have no doubt that in the damp days of winter this solution, as well as that of other salts, will not be so ready to crystallize as the drops were during the months of June, July, and August, when these experiments were made.

Drops of sodic sulphate in my bedroom crystallized some of them immediately, and all after a few hours. But in a damp air, after heavy rain, the time was considerably extended. I have even found them to remain liquid much longer on paper than on glass. On the other hand I have found magnesian sulphate solidify more readily on paper than on glass, and plumbic acetate and zincic acetate more readily on glass than on paper. But the facility with which the drops solidify on various surfaces seems to vary from day to day; and hence it does not appear possible to tabulate the salts in the order of their sensitiveness, seeing that the order of one day, or even of one hour, may be quite different from that of another day or hour. Thus on two different days in August equally fine, only on one day the wind was in an easterly, and on the other in a westerly quarter, the order in which the solutions respectively crystallized was as follows:—

Wind easterly.

1. Sodic sulphate.
2. Sodic acetate.
3. Alum.
4. Magnesian sulphate.
5. Zincic sulphate.
6. Zincic acetate.
7. Plumbic acetate.
8. Sodic carbonate.

Wind westerly.

1. Alum.
2. Magnesian sulphate.
3. Zincic sulphate.
4. Sodic sulphate.
5. Zincic acetate.
6. Plumbic acetate.
7. Sodic carbonate.
8. Sodic acetate.

The following Table shows the order of crystallization during a southerly wind on a day in August, when heavy showers occurred with intervals of bright sunshine:—

1. Sodic sulphate crystallized within a few minutes on glass, and somewhat later on paper.
2. Ammonia alum: the large drop and several of the small ones were covered immediately with minute octahedra; the action was more rapid on glass than on paper.

3. Potash alum was also singularly active, but more so on glass than on paper.
4. Zinc acetate crystallized almost immediately.
5. Magnesian sulphate crystallized almost immediately.
6. Zinc sulphate crystallized after about half an hour.
7. Sodic carbonate flattened and crystallized by evaporation, first on paper and then on glass.
8. Sodic tartrate similar to 7.
9. Lead acetate became solid after about three hours.
10. Sodic acetate was liquid after forty-eight hours, when the experiment was terminated.

On the table in my study, in which this last experiment was conducted, was a glass plate on which drops of sodic acetate had been deposited fifteen days before—one compound drop surrounded by six single drops, all of which, except two single drops, had crystallized. These two had apparently retained their surface-tension, while the other drops had become more or less deformed. There was heavy rain on the 27th August, and on the morning of the 28th all the drops, which had previously crystallized, were quite liquid. Sodic arseniate solution of the same strength (2 to 1) behaves in a similar manner, absorbing moisture from the air in damp weather, and remaining liquid the while; whereas drops of sodic phosphate solution (2 to 1) become solid after a few hours even in damp weather.

An extreme case of persistence in the liquid form during damp weather is afforded by sodic acetate that has undergone the watery fusion; that is, the salt newly crystallized is melted in a test-tube over a spirit-lamp in its own water of crystallization and so boiled. Drops of this on a glass plate will remain liquid during many hours, and even days, if the wind is in a westerly or southerly quarter. Meanwhile they absorb moisture from the atmosphere, as was proved by balancing 44 grains of this solution; it gained 11 grains in the course of a couple of days, and then crystallized slowly with large crystals and interposed water, as in an evaporating-dish, and without that loss of weight which accompanies the sudden crystallization of a supersaturated saline solution. There is only one exception to this loss of weight that I am aware of, and that is in the case of a salt under the aqueous fusion. This, in suddenly solidifying, becomes very hot; but there is no loss of water from evaporation, because the whole of the water present becomes constitutional water in the salt. For example, $45\frac{1}{2}$ grains of recently fused sodic acetate were poured into a balanced watch-glass while still warm, and the solution formed a resplendent convex lens. In the course of an hour it had gained one grain in weight from the absorption of moisture. While watching it, its bright surface became suddenly clouded: from a point in the circumference crystalline lines diverged, so as quickly

to cover the surface like a fan, while an opaque appearance was seen to descend throughout the depth of the mass, very much after the behaviour of the 7-atom sodic sulphate when touched by a nucleus. The watch-glass and the metal pan on which it rested became very hot (the temperature rising, as was afterwards found with the remainder of the solution, from 60° to 129° F.)*, but still with no loss of weight, seeing that the whole of the water of the solution had again become water of crystallization. If there is any water to spare it is driven off by the heat of combination, and there is a corresponding loss in weight.

Some of the drops, when exposed to the air, form pulpy masses, such as those of sodic, zincic, and magnesic sulphates; but by continued exposure they soon develop into hard well-formed crystals. This is, I suppose, the explanation of an effect that I obtained some years ago. A spoon of white metal shaped like a deflagrating-spoon, with its wire run through a flexible porous plug, was lowered into a boiling solution of sodic sulphate: the plug was then fitted into the neck of the flask, which it closed accurately, and the solution was left to cool. The spoon was next drawn up full of the solution and left in the air of the flask. In the course of some days the liquid in the spoon evaporated and left a salt which, on being lowered into the solution, was inactive; but if the spoon containing the salt were first taken out and exposed for a few seconds to the open air and then lowered into the solution, it was powerfully nuclear. This exposure to the open air or to the air of a room seems to be necessary, in some cases, to the formation of a crystalline salt, either by evaporation or by the action of some other force.

Schröder insists on the powerful action of the air in connexion with the phenomena of supersaturated saline solutions; and Löwel asks, "What is this mysterious action of the air which endows bodies with a nuclear action?" He supposed it to be catalytic in its mode. The French theory supposes the air to act as a carrier of salts of the same kind, which, it says, are alone capable of acting as nuclei. In the course of my experiments the air has had abundant opportunities of becoming charged with salts of the same kind as those I was working with in solution. For example: during all the time that I was weighing the solutions of sodic acetate, so as to note their increase in weight by the absorption of moisture from the air, an evaporating-dish, containing sodic acetate in crystals, was within a foot and a half of my balance; but although for days together the solution caught moisture from the air, it never caught a crystal of the salt from the air. When, however, the wind got round to the north, or to the east, the solutions of this and of other salts crystallized, not only when exposed as drops, but also often in the covered flasks which supplied the drops.

When the air is from a dry quarter the results are tolerably uniform;

* In my first paper (Phil. Trans. 1868, p. 663) a case is given in which sodic acetate solution in solidifying rose from 14° to 104° F.

when from a damp one they vary, although the conditions are apparently the same. For example, one day last summer, when the wind was westerly, and there was a bright warm sun occasionally obscured by clouds, I placed twelve watch-glasses on a parapet wall in my garden, and poured into them, while still warm from the boiling, a solution of sodic sulphate (2 salt to 1 water). In the course of about twenty minutes four of the solutions crystallized in large crystals of the normal salt, with interposed liquid not supersaturated; four formed thin crystalline needles, which started from a radiant-point as from the action of a nucleus; and the remaining four, which remained liquid, were touched on the surface with a needle dipped in olive-oil, and they crystallized in the pulpy form from the point.

I think the foregoing details go some way to prove that supersaturated saline solutions behave differently among themselves and to different bodies under varying atmospheric conditions. If so, many contradictory details connected with this subject are on the road to reconciliation. A supersaturated saline solution in a closed flask in which the air is saturated with moisture is in a different condition (at least as regards surface-tension and the power of spreading the drop of oil into a film) as compared with a similar flask loosely covered and in the open air on a fine day; so that an oil &c. introduced into the closed flask, as in M. Viollette's and Mr. Liversidge's experiments, may be inactive, while under the other condition the same oil may be active. One fine day in August I placed four flasks containing sodic sulphate solution (2 to 1) in the open air, and let fall from a dropping-tube (just taken out of water and put into ether) a drop of ether on the surface of each. It was striking to see, from the cohesion-figure of the ether, long lines of crystals radiating to the sides and bottom of each flask. A fifth flask of the same solution was treated some days later in my laboratory with a drop of ether; but this was inactive, and the solution did not crystallize until the flask was inverted against my thumb. On a day in September, when the wind was S.E., I let fall into each of four flasks containing a similar solution a drop of paraffine-oil (marked "lubricating"). It formed a well-shaped lens on each surface without any separation of salt; but on gently shaking each flask, so as to break up the lens, there was a sudden crystallization, and the normal salt was produced in all four flasks. Some hours later I repeated the experiment in my study with five flasks of a similar solution. The oil spread upon all five surfaces and the solutions crystallized immediately, the normal salt being again produced. I have hundreds of such cases in my note-books which have supplied material for former papers; but in referring to them no discredit is intended to be cast upon the negative results obtained by other observers who deny that oils, ether, &c. have any nuclear action. I would suggest that in my numerous experiments, carried on during several years, it is not probable that there was always a lurking fallacy in the shape of a crystal of the same kind as that of the

solution either floating in the air and waiting to enter the flask as soon as it was uncovered, or lying concealed in the oils, ether, and other bodies which I regarded as nuclei. The varied behaviour of oils under different conditions, as pointed out in my second memoir*, has not been attended to; and because in many experiments in the hands of others oils have been inactive, it has been denied that oils have any action at all on these solutions. I confess that I was not able to reconcile the results as stated by my opponents with my own results, until within the last few months, when I set to work to repeat Professor Grenfell's experiments, during which it became plain to me, what I had not before suspected, that the hygrometric condition of the air has a powerful influence on these solutions; so that under one set of conditions bodies may act as nuclei, while under another set they may be inactive, as in the case already cited, where sodic acetate solution by exposure during damp weather becomes inert even to a crystal of the salt itself. But the exact determination of these conditions can only be settled by a further long and laborious experimental investigation upon which I am now engaged.

II. "Notes on Physical Geology.—No. III. On a new Method of finding Limits to the Duration of certain Geological Periods."

By the Rev. SAMUEL HAUGHTON, M.D. Dubl., D.C.L. Oxon., F.R.S., Professor of Geology in the University of Dublin.
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In the preceding Note I have proved that the elevation of Asia and Europe displaced the axis of maximum inertia through 69 miles†, in the direction of the meridian of the Andes, away from Greenwich. The axis of rotation being thus separated from the axis of figure by 69 miles, or 1° , commenced to revolve uniformly on a right cone round the axis of figure, and would continue to do so for ever, if not prevented by friction.

Astronomers are agreed that the motion of the pole at present is secular and very slow, all traces of wabbling caused by the elevation of Asia and Europe having disappeared.

The object of the present note is to discuss the motion of the axis of rotation, from the period of the elevation of the continent until (by reason of friction) the axis of rotation came again to coincide with the axis of figure; and, if possible, to calculate the absolute length of time that has elapsed from the epoch of elevation.

* Phil. Trans. 1871, pp. 53, 55, 66. See also Proc. Roy. Soc. 1873, p. 210.

† It is stated, in error, as 207 miles in Note II. (*suprà*, p. 57). All the numerical displacements are printed *three times* their proper values, because, through inadvertence, in integrating equation (8), Note I. page 53, I forgot to divide the numerical coefficient by 3. This error affects all the subsequent coefficients.