it can be made to return into its own terminal, while the other takes no part in the discharge; or, finally, the two terminals can be made to pour out independent discharges of the same name, each of which returns to its own terminal.

Having traced the relation between the two parts of the discharge, and having found means for controlling their range and influence, the authors were led to inquire whether there be any experimental evidence of the state of the tube during the occurrence of the discharge. Some experiments with two pieces of tinfoil of unequal size placed near the ends of the tube and metallically connected; and others with a strip of tinfoil placed along the tube, all gave effects showing that the discharge cannot be simultaneous throughout the tube. The phenomena appear to require for their interpretation that, in front of the pulse coming from the (positive) air-spark terminal, there is, during the interval between the pulses, a rising negative potential. This is entirely swept out by the pulse as it advances along the tube; after which the process is repeated. The condition of things behind the pulse is more difficult to determine; but an experiment with the telephone gives reason to think that parts of the tube nearer to the non-air-spark end are in a condition to demand relief, before those nearer to the air-spark terminal have ceased to require it. And on this account the discharge may, perhaps, be more nearly represented by a lazy tongs than by a bullet.

How far the results obtained from the sensitive state are applicable to ordinary discharges is a question which cannot yet be definitively answered. But the marked similarities in the phenomena, and the predisposing circumstances of striation or non-striation, as well as in the terminal peculiarities of the two kinds of discharge, point strongly to the conclusions that all vacuum discharges are disruptive; and that sensitive differ from non-sensitive discharges mainly in the scale of the discontinuity due to the disruptiveness, causing a difference between the two classes of phenomena analogous to that between impulsive and continuous forces in dynamics.

II. "On the Action of Solid Nuclei." By CHARLES TOMLINSON, F.R.S. Received April 22, 1879.

It is stated in my second paper on supersaturated saline solutions ("Phil. Trans.," 1870, p. 53), that among nuclear bodies "are permanently *porous* substances, such as charcoal, coke, pumice, meerschaum," also that "certain liquids act as nuclei by separating water instead of salt from supersaturated solutions. Absolute alcohol acts in this way."

Some eminent observers have denied the accuracy of these statements. Thus, M. Viollette concludes from his experiments that porous bodies, such as pumice, calcined baryta, lime, and gypsum, and bodies greedy of water, and capable of being hydrated, such as calcined sulphate of iron and of copper, have no influence on supersaturated solutions.

This observer, believing, as he did, that the only nucleus is a salt of the same kind as that in solution, or one isomeric therewith, and that such nuclei are generally floating in the atmosphere, so ingeniously arranged his apparatus as to exclude the action of the air, and hence his results were of a negative character.

Mr. Liversidge ("Proceedings," vol. xx), actuated by the same idea, conducted his experiments on a similar principle, namely, that of excluding the air, and he also arrived at negative results. He employed several solid dehydrating substances, such as freshly ignited quicklime, and placed them in thin glass bulbs, which, being sealed, were heated nearly to redness, and dropped into the supersaturated solutions. The flasks were plugged with cotton wool, through which a glass rod was passed, and were then boiled up and allowed to cool during some hours; when quite cold, the bulb was broken by means of the glass rod, and its contents were set free, but in no case with any result. He also found by his mode of experimenting that porous bodies and absolute alcohol are inactive.

In the "Proceedings" vol. xxvii, p. 122, some account is given by me of a series of observations, extending over several months, on the action of essential oils on a supersaturated solution of sodic sulphate. It was found that when the wind at Highgate was in a southerly or westerly quarter, the oils were inactive; but that they quickly became active when the wind was northerly or easterly.

A similar course of observations, though not extending over so long a time, has led to a similar conclusion respecting solid porous, and dehydrating bodies.

The solutions chiefly employed in the experiments that led me to this conclusion were those of sodic sulphate, sodic acetate, and alum, of various degrees of supersaturation. The solid nuclei employed were chiefly pumice and plaster of Paris; but others were also used, such as meerschaum, marble, lime from marble and chalk, coke, charcoal, and bread.

The flasks containing the solutions were sometimes carried into the open air of my garden before being uncovered. The plaster of Paris was taken out of a bottle by means of a small, clean, platinum spoon, and a fragment of pumice, about the size of a pea, was taken up with forceps, and thus dropped into the solution, the cover being immediately restored to the flask.

The active solid nuclei could be immediately rendered inactive by

exposing them for a short time to the heat of the kitchen oven or to the flame of a spirit lamp, or even to the temperature of boiling water. For this last purpose a test-tube was thrust through a flat cork, resting on the mouth of a large flask containing water, which was kept boiling. The bottom of the tube containing the plaster, &c., did not pass beyond the centre of the flask. In about ten minutes the nuclei were thus rendered inactive, and they did not recover their activity in atmospheres of carbonic acid, nitrogen, hydrogen, and nitrous oxide, after several days or even weeks. But they frequently became active in oxygen, and generally so in oxygen or air ozonised by the action of phosphorus, and always so by a short exposure to the outer air, when the wind was northerly or easterly. Inactive plaster of Paris has been exposed all night to the air of my garden during a south-west wind, without becoming active. Fragments of pumice exposed to the air of my laboratory for days together, without becoming active, have regained their activity within ten minutes on being exposed to the open air during a northerly wind. Snow was also found to be active.

The details of a few experiments may not here be out of place. The wind at Highgate, from the 28th March to the 8th April, was southerly and westerly. During all this time some plaster of Paris, rendered inactive by heat, was freely exposed on a flat glass plate to the open air of my garden, and remained inactive. On the 9th the wind passed round to the east, and remained so during some hours, gently blowing; the plaster became active on solutions of sodic sulphate and sodic acetate.

Fragments of pumice, meerschaum, and coke, rendered inactive by the heat of the oven, were put into carbonic acid on the 28th March, where they remained until the 9th April, when they were still found to be inactive on solutions of sodic sulphate. They were exposed to the air of my garden, and became active in rather more than two hours.

A similar result was obtained with inactive fragments that had been kept in nitrous oxide.

Mr. Grenfell ("Chemical News," xxxix, 16) is of opinion that the action of nuclei in bringing about the solidification of these solutions is entirely due to absorption. In one of his ingenious experiments, filtering paper, folded a number of times into a long spill, is passed down the neck of the flask so as to touch the solution, the effect being to render it solid either at once or after some time.

I repeated this experiment, with this modification, namely, that of placing the spills in the kitchen oven for about a quarter of an hour; they were then passed down the narrow necks of some flasks which they fitted with friction, and the ends were made to dip just below the surface of a solution of sodic acetate (3 to 1). One of the flasks was

immediately covered with a jar of nitrogen and the other with a jar of air. After three days the solutions were still liquid. The spills were then removed, and fresh ones, not previously heated, were introduced, when one solution immediately became solid, and the other some time after.

If the filtering paper, pumice, plaster, &c., acted by absorption, then the effect of heat would be to increase the absorptive power, and consequently their activity as nuclei; whereas the contrary result is obtained, so that absorption does not seem to be a *vera causa.*

When it is stated that the nuclei became active on being exposed to certain winds in preference to others, the locality in which the experiments were conducted must be taken into account; for, as Professor Stokes suggested at the reading of my former paper, a westerly or southerly wind might be equally active in another place. I found this to be the case at Eastbourne, where a westerly wind produced, in the course of ten minutes, a deep orange-brown stain on ozone test-paper, and made the oils singularly active after a few minutes' exposure. For example, on the 10th of June oil of lemons was inactive in the house, but on being exposed to a strong south-west wind from the sea, about a mile away from the town, it became viscid in the course of a few minutes' exposure, so that it dropped very slowly. The solutions, on contact with this oil, immediately became solid. On the 11th, a limpid paraffin oil, on being redistilled, was inactive within doors. It was taken to the end of the pier, 1,012 feet from the shore, and exposed in drops to a strong westerly wind for about three minutes, when it became powerfully active. On the 13th there was scarcely any wind, but what there was was west south-west. Freshly distilled oil of cajuput was inactive within doors, but, on being exposed to the outer air for about fifteen minutes, it became active.

In July, at Bournemouth, the weather was sultry; ozone test-paper was scarcely acted on, even after an exposure of some hours, and inactive oils did not become active under the influence of the air.

In September, at Denchworth, near Wantage, ozone was abundant, judging from its action on test-paper. Some inactive turpentine oils thickened on being exposed to the air (wind north and south-east), and the effect on the solutions was decisive.

Before concluding I must refer to the nuclear action of alcohol. In order to test this once more, a day was selected when the wind was in the east, when rain had fallen during some hours, and continued to fall all the time the experiments were being conducted in the open air of my garden. About ten flasks of sodic sulphate (3 to 1), and about the same number of potash alum (4 to 3) were prepared. In filling the belly of the flasks about half full, a small funnel with a long stem was used, and was dipped into warm water after each filling, so as not to soil the necks of the flasks; and in reboiling, each flask was

placed at an angle of about 45°, so that if any spitting occurred it would be against the belly of the flask and not into the neck. The necks were covered with small beakers, taken out of water, and the flasks were thus conveyed to the outer air and left to cool, the temperature being 40°. When cold, some absolute alcohol in a small clean beaker was taken up by means of a clean dropping tube, and a single drop or a number of drops was delivered to the surface of each solution without contact with the interior of the neck, the covering beaker, in each case, being lifted a little on one side so as not completely to uncover the mouth of the flask or admit the rain. The sodic sulphate solution in several cases became solid directly the alcohol reached it; in other cases after some minutes; in two flasks the alcohol acted by determining a copious deposit of the modified salt,* but the solution in one of them became solid on gently shaking it, and in the other equally so on adding a few more drops of alcohol. In the case of the alum solution the results were similar. As soon as the alcohol reached the surface there was a sort of trembling motion, as if the surface tension had been disturbed, and also a kind of viscous disturbance in the solution (phenomena equally applicable to the sodic sulphate solutions), when suddenly a single octohedral crystal would start into existence just below the surface; or, if a number of drops of alcohol had been added, a multitude of minute octohedra would all at once appear at the surface plane between the two liquids; or, thirdly, the action would begin from the bottom, and a large crystalline mass be formed, while heat currents rapidly ascended; or, fourthly, on gently shaking the flask, an immense assemblage of chalky-white points filled the solution and made it opaque. In no case did the alcohol fail to exert a nuclear action.

By way of contrast with the above results, the experiments were repeated on another wet day, when the wind was veering between south-east and south-west. The alcohol was now inactive so far as the solidification of the solutions was concerned, but active in throwing down a large quantity of the modified salt. The day after this the wind blew steadily from the north, and alcohol, ether, naphtha, and wood spirit were all active on solutions of sodic sulphate and potash alum, the former becoming solid immediately, and the latter within two or three minutes.

Now, if it be still asserted that whenever these solutions become

* Löwel, in his first memoir, says that if alcohol be introduced into a supersaturated solution of sodic sulphate, it immediately produces the ten-atom or normal salt with solidification of the solution; but if a large quantity of alcohol, at 36° to 40° C., be introduced, crystallization does not take place on cooling. If now the flask be left to repose, the alcohol takes water from the solution, concentrates it, and in two or three days produces the seven-atom or modified salt in fine crystals on the surface of the liquid in contact with the alcohol.

solid, a salt of the same kind is the nucleus, and that such a nucleus is present in the air, I must leave the proof of the assertion to those who make it or defend it. I cannot imagine what can be the source of the alum nucleus, for example, in the above experiments, limited as they were by such well-defined conditions. M. Gernez can only account for the existence of alum in the air on the supposition that as a good deal of that salt is used in dye-works, it must escape into the air from such a source. Until it can be proved that on a wet day and while rain is still falling there is alum in the open air, I must continue to maintain my original statement that alcohol is, under proper conditions, capable of determining the solidification of these solutions.

I have several times noticed the persistence of nuclear action on the part of a dense charcoal, such as that made from box-wood. A small piece of such charcoal was boiled in a solution of potash alum (4 to 3). When cold, a large crystalline mass was found attached to the charcoal. The flask was reboiled five times in the course of a week, and each time the same result appeared when the flask had become cold.

I have referred to bread as a nucleus, and the action is curious. New bread is not nuclear; but if stale bread, such as a second day's loaf, be cut open with a clean knife, and bits of the crumb be dug out, they act powerfully as nuclei on solutions of sodic sulphate and alum. If such bread be slowly toasted before the fire, it becomes inactive; but if it contain alum, it is active on solutions of potash or ammonia alum (about 1 to 1), whether the bread be new or stale, toasted or not. Treated by this test, I have found the bread of Highgate to contain alum; but some bread from Drury Lane gave most decided evidence of its presence.

III. "On the Results of the Magnetical Observations made by the Officers of the Arctic Expedition, 1875–76." By Staff-Commander E. W. Creak, R.N., attached to the Admiralty Compass Department. Communicated by Captain F. J. O. Evans, C.B., F.R.S., Hydrographer of the Admiralty. Received April 25, 1879.

The following narrative and results form the sequel to the "Memorandum on Terrestrial Magnetism," prepared by Professor J. C. Adams, M.A., F.R.S., and Captain F. J. Evans, R.N., F.R.S., published in the "Manual and Instructions for the Arctic Expedition, 1875," suggested by the Arctic Committee of the Royal Society.

The "Alert" and "Discovery" left Portsmouth on the 29th May, 1875, and on arrival at Godhavn, in Disko, the first magnetic observations were made. The values of the declination and inclination