I may, however, be permitted to make a few remarks arising out of Mr. Grenfell's paper.

According to my view, a *nucleus* is a body that has a stronger attraction for the gas, or the vapour, or the salt, of a supersaturated solution than for the liquid that holds it in solution.

Nuclei, with certain limitations, cease to be such when made *chemically clean*.

A body is chemically clean, the surface of which is entirely free from any substance foreign to its composition.

Thus oils and fatty bodies are chemically clean, if chemically pure, and containing no substance, mixed or dissolved, that is foreign to their composition.

The limitations above referred to are two: (1) the oils, &c., when chemically clean, do not act as nuclei while in the mass, such as a lens or globule; but these oils, &c., whether clean or not, in the form of thin films, act powerfully as nuclei; (2) a liquid, at or near the boiling point, is a supersaturated solution of its own vapour, and a porous body, such as charcoal, pumice, &c., whether clean or not, is a powerful nucleus in separating vapour.

I have on several occasions taken the liberty of opposing M. Gernez's views as to the action of nuclei. He supposes (1) that supersaturated gaseous solutions (soda water, seltzer, &c.) give off their gas to nuclei by virtue of the air that these latter introduce into the solution: in other words, gas must escape into air, and the function of the nucleus is to carry down air; hence rough bodies act better as nuclei than smooth ones. I have shown (Phil. Mag., August 1867) in a series of twelve experiments, that air is not a nucleus, and that rough bodies are inactive, if *catharised* or made *chemically clean*. A rat's-tail file, for example, is a good nucleus, because it holds between its teeth not air, but that filmy kind of matter that is powerfully *nuclear*, and it is not easy to clean a body of this kind; but when clean, it is quite inactive. So, a flint stone that has been exposed to the air, or handled, acts as a powerful nucleus, but when broken, the newly-fractured surfaces are inactive, because chemically clean. And such surfaces are inactive, because the gaseous solution adheres to them as a whole; whereas, if a clean body be handled or exposed to the air, it becomes covered, more or less, with filmy matter, to which the gas adheres more strongly than the liquid does, and hence there is a separation.

There is, I think, abundant proof that air is not a nucleus, its function, if it have any, in this class of phenomena, being that of a *carrier of nuclei*. Proof also is wanting, I imagine, that when a nucleus determines the crystallisation of a supersaturated saline solution, a salt of its own kind is present. When M. Gernez so laboriously prepared his nuclei, so as to free them from salt, he did not perhaps reflect that he was making them chemically clean. Of course I fully admit that, in general, a salt of the same kind as the solution, acts as a powerful nucleus; but in order for it so to act it must adhere more strongly to the saline than to the liquid portion of the solution. It may even happen that a crystal of the same kind, and of the fully hydrated salt, has no nuclear action, because it is in a perfectly catharised condition. And here I must refer to the objection raised by Dr. De Coppet, that in one of my forms of showing this experiment, the hydrated crystals, say of magnesic sulphate, being introduced into the neck of the flask while the solution was boiling, and so left in the covered flask while the solution cooled, such crystals become so changed by the heat as no longer to represent the normal salt, so that when lowered into the solution they formed a different salt, and hence were no test of the point in question, as to whether a salt of the same kind may be rendered inactive as a nucleus. I admit the criticism to be just, but in my original account of the experiment (Phil. Trans., 1868, p. 665) I did not rely upon one form only. Highly supersaturated solutions in clean tubes, plugged with cotton wool, were put, when cold, under the receiver of the air-pump, and left for some time *in vacuo*, over sulphuric acid, the effect of which was to produce crystalline crusts of the normal salt on the surface, and these by shaking fell through the solutions without acting as nuclei; whereas on removing the cotton wool in the presence of air, the solutions crystallised immediately into a solid mass. So also by keeping supersaturated solutions during some months, water escapes through the cotton wool, and a crystalline crust of the normal salt creeps up the air-filled portion of the tube, and this has no nuclear character, because the adhesion between it and the solution is perfect.

So necessary is the action of a nucleus in determining crystallisation in these solutions, that, if care be taken to exclude nuclei, highly supersaturated saline solutions may, by reduction of temperature to 0° F., or from that to − 10° F., be made solid, and by placing the tubes in snow and water at 32° F., the solids rapidly melt into clear bright solutions, without any separation of salt. These effects may be shown any number of times; but whether the solution be solid or liquid, if the cotton wool be removed, crystallisation always sets in, in the case of the solid during the melting, while in that of the liquid the effect is immediate.

With respect to the editorial note that the solutions of hydrated salts contain the anhydrous salt, I have shown in the paper last quoted, and with still greater elaboration in the *Chemical News* for Dec. 10th, 1869, that such is the case with respect to sodic sulphate. I insist on this point, as it is one of first-rate importance in considering the theory of supersaturated saline solutions. I endeavour to prove that it is the anhydrous salt in solution, by showing that at various points of the scale a sudden lowering of temperature produces a shower of the well-known octahedral crystals of the anhydrous salt. I also explain in my original memoir, that it is necessary for these crystals to be deposited before the modified 7-atom salt can be formed, and that even when there is a copious deposit of this salt the liquor above it is not, as Löwel supposed, the mother liquor of the 7-atom salt, but it is still a solution of the anhydrous salt. And more than this, when the sudden change in the curve of solubility takes place at 33° or 34° C., and there is, according to Gay Lussac's supposition, a change in molecular condition, it is still the anhydrous salt that is in solution.

There are several other points that might be enlarged on, but that I fear to trespass further on your valuable space.

Highgate, N. CHARLES TOMLINSON

Astrology

THE belief in astrology which still prevails among the English lower classes to a much larger extent than is supposed, will derive a fresh impulse from the happy guesses which have been made by the editor of "Moore's Almanac" in his issue for the current year. The hieroglyphic with which it is illustrated is less vague than usual, and represents two eagles fighting in the air, and on the plains beneath them hosts of armed men (in decidedly foreign uniforms) engaged in a bloody struggle. Lest the point should be missed, the prophet begins the forecast of the year with the distinct assertion that there will be war between France and Prussia, and that the month of July will be especially disastrous to the Emperor Napoleon. Thus far events have coincided with the voice of the oracle, and seem to confirm the poet's view that

"The warrior's fate is blazoned in the skies."

But we have yet to see whether "in October the King of Prussia (if living) will meet with defeat, and the ex-King of Hanover recover some of his prestige, if not his throne also." M. Comte would have us deal tenderly with astrology, because it was, in his opinion, the first systematic effort to frame a philosophy of history out of the apparently capricious phenomena of human actions. In theory we may do so, but astronomical science is hardly likely, for the sake of sentiment, to treasure up the discarded swaddling clothes which for so many centuries impeded its onward progress.

Norton Canon, Weobley C. J. ROBINSON

On Volcanoes

HAVING only last night returned from Norway, I was not aware before to-day that No. 40 of NATURE (August 4) contained "an outline" of a lecture on volcanoes delivered by me in St. George's Hall, Langham Place, on the 19th June (not 9th as therein stated) last.

Although I cannot but feel highly flattered at the length of this notice, I must regret that the author of this "outline," who, strangely enough, signs himself by my name, has, as will be seen upon reference to the text of my lecture as reported in the *Geological Magazine* for July, omitted every word which could convey to the reader the remotest idea of the object of the lecture itself, or the conclusions arrived at from the evidence brought forward. Just as a man without life is but a corpse, neither can a mere string of facts be called even the "outline of a lecture," when we have only the body without the spirit.

The object of my lecture was to institute a comparison between the relative magnitudes of the operations of internal and external forces in determining the main external features of