indicates. That a large proportion of it is deflected back towards the Equator is universally admitted, and that the remainder can be gathered together after its initial velocity has been expended, and forced downwards so as to displace colder water to the depth of 800 fathoms whilst still moving north-east, seems to me in the highest degree improbable.

To what then is the north-east movement of the warm upper stratum of the North Atlantic attributable? I have attempted to show that it is part of a *general interchange* between Polar and Equatorial waters, which is quite independent of any such local accidents as those that produce the Gulf-Stream proper, and which gives movement to a much larger and deeper body of water than the latter can affect. The evidence of such an interchange is twofold—that of physical theory and that of actual observation. Such a movement *must* take place, as was long since pointed out by Prof. Buff, whenever an extended body of water is heated at one part and cooled at another; it is made use of in the warming of buildings by the hot-water apparatus, and it was admirably displayed at the Royal Institution a few months since in the following experiment kindly prepared for me by Dr. Odling:—A long but very narrow trough, with plate-glass sides, having been filled with water, a tube into which a steam-jet was conveyed was introduced vertically at one end, whilst a lump of ice was wedged between the sides of the trough at its opposite extremity. Some red colouring matter mixed with gum, of such viscosity as to be carried along by any movement of the liquid mass without mingling with it, was introduced into the water at the end of the trough warmed by the steam-jet, and a like mixture of a blue colour was introduced at the end cooled by the ice. The latter very speedily sank to the bottom along the side of the ice-wedge, and then crept slowly along the floor of the trough, towards its warm end, where it rose along the side of the heated tube until it reached the surface, and then slowly flowed back towards the cold extremity. On the other hand the red liquid passed slowly along the surface in the first instance from the warm to the cold extremity, then sank (as the blue had previously done), crept along the surface of the blue layer covering the bottom of the trough, and then rose (as the blue had previously done) along the side of the heated tube to the surface. Thus a circulation was shown to be maintained in the trough by the application of heat at one of its extremities and of cold at the other, the *heated* water flowing along the *surface* from the warm to the cold end, and the *cooled* water flowing along the *bottom* from the cold to the warm end: just as it is here maintained that Equatorial water streams on the surface towards the Poles, and that Polar water returns along the bottom towards the Equator, if the movement be not interfered with by interposed obstacles, or prevented by antagonistic currents arising from local peculiarities. So far is this from being the case with the general surface-movement in the Atlantic basin, that it will concur with and supplement the motion of the Gulf-Stream proper, which may thus be regarded as a portion of the general Equatorial-polar current, deflected in the first instance by the action of the trade-winds, but subsequently rejoining the great body of water having a north-east motion of its own and imparting to its surface-layer a higher temperature than it would otherwise convey.

Now that this hypothesis is at any rate deserving of consideration, and is not to be dismissed by the *ipse dixit* even of so high an authority as Dr. Petermann, though backed by my excellent colleague, I venture to maintain on the strength of the parallel case afforded by the temperature-phenomena of the Southern Indian Ocean. For, as Capt. Maury has shown, a definite current exists in the midst of it, carrying tropical waters far into the southern temperate zone, and not attributable to any such local peculiarities as those which produce the Gulf-Stream of the Atlantic. Conversely, the Hydra soundings in the Arabian Gulf have given evidence of a northerly reflux of glacial water from the Antarctic basin along the deep-sea bed of the Indian Ocean to replace the more superficial stratum which has moved southwards. This glacial water will in its turn be raised from the depths by the heating action of the tropical sun, and then return as the southerly surface-stream to the Antarctic basin, and would there sink by surface-cooling and again flow northwards along the sea-bed.

The facts of observation, then, being in the case last cited entirely accordant with physical theory, I submit that the same theory may be fairly applied to the explanation of those temperature-phenomena of the North Atlantic, which (as it seems to me) cannot be more than very partially, if at all, accounted for by the agency of the Gulf Stream proper. And I am content to leave it

to the judgment of those who are competent to deal with the question whether the north-east movement of the vast breadth of water lying between the coast of Greenland and that of Northern Europe, usually extending to a depth of 5,000 feet, is more likely to depend upon an impetus derived from a portion of the comparatively narrow and shallow current which issues from the Narrows thousands of miles off, or to form part of a general circulation of oceanic water, the cause of which is quite independent of local accidents.

The readers of NATURE will, I am sure, join with me in deeply regretting that Prof. Wyville Thomson is prevented by illness from taking that share in the scientific exploration of the Mediterranean basin, now about to commence, which has conducted so greatly to the success of the two previous expeditions in which we have had the pleasure and advantage of being fellow-workers.

Gibraltar, Aug. 11

WILLIAM B. CARPENTER

Dr. Hooker's "Student's Flora"

ALL the reviews which I have read of the "Student's Flora of the British Islands" are, as they undoubtedly should be, appreciative, but your reviewer has done well in pointing out a few seeming discrepancies, which it would, perhaps, be well if attended to in a second edition; and, if you will permit space, I will endeavour briefly to add to your list of desiderata. In the first place I take it that a good glossary is an essential, if not an absolute necessary, adjunct to a students' manual; yet we look in vain for anything of the kind in Dr. Hooker's new "Flora." Dr. Hooker could scarcely have thought beginners in botany able to interpret many words used by him in his generic and specific descriptions. I am also sorry to see that little word "*sub*" used so extensively; for my own part I do not understand its meaning as applied in zoology and botany. I can quite understand its applied use in sub-contractor, sub-lieutenant, sub-terranean, &c., but who dares tell us that one tribe, or one family, or genus, or species is subordinate to another? I comprehend a "species" as a form of animal or vegetable life which differs slightly but materially and permanently from its nearest ally; such a form is, in my humble opinion, worthy to hold its own as a good species, not subservient or subordinate to any other form that can be so described, no matter how apparently closely related. Forms of this kind may have (I say it advisedly: we have no proof to the contrary) approached each other through natural selection, without being off-sets in a direct line from a common parent.

Again, why use the term "*sub*" at all, when a much simpler and less confusing arrangement may be employed in its stead. All naturalists are pretty well agreed that natural arrangements shall consist of orders, tribes, families, genera, species, and lastly, varieties. Beginning with "tribes" of any given order, we have—Tribe 1, consisting of certain families all agreeing in certain recognisable features or parts. Families which cannot be grouped under Tribe 1 would fall under Tribe 2, and so on numerically *ad libitum*, without one being subordinate to the other, which they really are not in nature. The same may be said of genera grouped into families, species into genera, &c.

Dr. Hooker has done well in making varieties (or, as he terms them, "sub-species") of some plants which certainly have no other specific claim. As an instance, I may cite the three almost equally common forms of *Aspidium*, all of which are included under *A. aculeatum*, Sw. This is decidedly a step in the right direction; but should not the forms have retained the name of *angulare* in preference to *aculeatum*? The latter, I believe, claims priority, but the form described under *angulare* is decidedly the most highly developed, having stalked pinnules.

Woodhay, Aug. 12

HENRY REEKS

On Supersaturated Solutions

My friend Mr. Rodwell was so good as to forward to me a copy of NATURE for the 4th inst., containing an account of some interesting experiments by Mr. Grenfell on the action of fatty bodies on supersaturated saline solutions.

During the last two years I have made a large number of experiments in order to ascertain the function of oils and fatty substances in determining the crystallisation of such solutions. The results of my inquiry are included in a paper, the abstract of which was read before the Royal Society on the evening of the 16th of June last, to which I beg leave to refer.

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 magnesian 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