

fortunately not told. In the next page we find that the molluscs or soft animals are composed of radiata, star-fishes, &c., polyyps, corals, &c., naked molluscs and testacea or shell-fish. Need we carry our investigation further?

The terms which the author supposes are used by surgeons and physicians are of the most astounding nature, and many of them, although possibly used a century or two ago, are perfectly new to us. We only confine ourselves to the letter A. Do any of our readers suffer from acatharsia, acratia, acrisy, acropathy, acropy, acroteriasm, acrothymion, adenopharyngitis, aerophobia, agalaxy, agennesia, agrypnocoma, anagogy, anopsy, antispas, antritis, apagma, apolepsy, apolysis, apotrepis, or arthropuosis? Let them have recourse to such remedial agents as acidulum, acopica, adipson, alborga (a kind of sandal wood made of mat weed), aldehydic acid (which "is a solution of oxide of silver in aldehyde"), alloxan (which our readers will be surprised to learn is "the action of nitric with uric acid"), amyllum (which we are told is "a preparation of starch"), anacathartics (which are "any medicines that operate upwards; a cough attended with expectoration;" it is of course the "medicine that operates upwards" and not the cough, that we recommend as a remedial agent), antephiatic medicine, or arrowroot (consisting of starch, albumen, volatile oil, chloride of calcium, and water), which we presume may be obtained at an antidotarium. Should surgical aid be required, an arthrembolium may prove of service. We can only account for this appalling list of medical terms and for the information regarding alloxan, arrowroot, &c., on the supposition that Dr. Nuttall's medical attendant is an incorrigible wag, and that he took a most dishonourable advantage of his position.

Our readers will, we think, by this time be satisfied that our introductory remarks upon this discreditable production were not at all too severe. It is a disgrace to English science that such books should find a respectable publisher.

Das Gesetz der vermiedenen Selbstbefruchtung bei den höheren Pflanzen. Von Dr. O. W. Thome. (Williams and Norgate.)

WE opened this little pamphlet in the hope of finding in it a new contribution to the literature of the self-fertilisation and cross-fertilisation of plants, but were disappointed to discover that it consisted of little besides a *résumé* of the labours of others in this field. The instances in which the self-fertilisation of hermaphrodite flowers is prevented by the fact that the stigma and the stamens ripen at different times, are mostly taken from Prof. Hildebrand's "Die Geschlechter-Vertheilung bei den Pflanzen," and from that botanist's contributions to the "Botanische Zeitung." On the laws of dimorphism and trimorphism we have little but the examples so elaborately worked out by Mr. Darwin in the genera *Linum*, *Primula*, and *Lythrum*. It is singular that from the time that Sprengel first called attention to the provisions which favour cross-fertilisation in plants, now more than seventy years since, so little had been done in this field until the researches of the two eminent botanists above named, and even now they have so few fellow-labourers. There is no department of physiological botany more beneath the eye of every dweller in the country, or of any one who possesses a garden, none which presents so many points of interest even to the casual observer, and so many illustrations for the advocate of the doctrine of "design," and none in which a careful series of observations would be more fertile in results of importance. If country botanists would bestow a portion of the energy which has been wasted in mere collecting, and the eradication of rare plants from their native haunts, on systematic physiological observations, the gain to genuine science would be immense.

A. W. B.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.]

The Gulf-Stream

As a note appended to Prof. Wyville Thomson's lecture "On Deep-Sea Climates," published in your number for July 28, may lead your readers to suppose the divergence of opinion between my colleague and myself upon the subject of the Gulf-Stream to be greater than it is, I shall be obliged by your placing before them a few observations which may put the question really at issue between us in a more definite form.

The term "Gulf-Stream" is continually used in two different senses. By myself it has been employed to designate the current issuing from the Gulf of Mexico through the "Narrows" between Florida and the Bahamas; and this I apprehend to be its true signification. My colleague's definition of it I give in his own words:—"The water of the North Atlantic thus consists first of a great sheet of warm water, the general northerly reflux of the equatorial current, the most marked portion of it passing through the strait of Florida, and the whole generally called the Gulf Stream." He thus distinctly recognises, with myself, the participation of a *Subsidiary Current with the Gulf-Stream proper* in the production of the two great temperature-phenomena of the North Atlantic, determined by the *Porcupine* soundings—(1) the elevation of the temperature, not of its surface-layer only, but of a stratum 800 or 900 fathoms deep, by a north-east movement of tropical water; and (2) the depression of the temperature of its deepest portions by a reflux of glacial water from the Arctic and Antarctic basins.

Now I have on no occasion, so far as I can recollect, spoken so disrespectfully of the Gulf-Stream proper as to "deny that it exercises any influence upon the temperature of the basin of the North Atlantic," nor am I aware of having expressed a "doubt whether it reaches the coast of Europe at all." That the Gulf-Stream proper, by raising the temperature of the portion of the Atlantic basin over which it can be distinctly traced, has a most important *indirect* influence upon the temperature of its north-eastern extension, I cannot doubt for a moment; and that its *direct* influence is traceable to the western coast of Europe, as far north as the Bay of Biscay, I accept on the authority of the recently-published Admiralty charts of its course and distribution. But I *have* expressed a doubt as to the extension of the Gulf-Stream proper to the channel between the North of Scotland and the Faroe Islands; and I have ventured to think it an open question whether the super-heating of the surface-water observed on a hot Midsummer day beyond the northern border of the Bay of Biscay was not as probably due to the direct influence of the sun as to the extension of the Gulf-Stream to that locality.

The main questions between my friend and myself are therefore as follows: What are the relative shares of the Gulf-Stream proper, and of the Subsidiary Current, in producing the elevation of temperature in the upper stratum of the North Atlantic to a depth of about 800 fathoms—and what is the motive power of that Subsidiary Current? These questions can only be answered, as it seems to me, by an appeal to certain general probabilities—definite data for their determination being still deficient, for, in the first place, all the calculations which have been made as to the quantity of water which issues from the Narrows, and the amount of heat which it conveys, are based (if I recollect aright, having here no access to books on the subject) upon the assumption that both its temperature and its rate of movement are the same throughout its depth as they are at its surface. Now, until reliable proof shall have been furnished as to both these particulars by our friends of the United States Coast Survey, I must claim a suspension of judgment, many probabilities leading to the suspicion that the bottom-flow may be considerably less rapid than the surface-current, and its temperature considerably lower. Secondly, even admitting in its full force the reputed "glory" of the Gulf-Stream at its exit from the Narrows, I fail to see the evidence that either its heat or its movement is directly concerned in the flow of the warm upper stratum of the north-east extension of the Atlantic towards the Hebrides, the Feroes, and Spitzbergen. For as the stream of superheated water, on its emergence into the open ocean, spreads itself out like a fan, it must necessarily become shallower as it extends instead of deeper, and this (if I remember aright) is what all observation

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 conduced 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 pinnales.

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.