

occasionally omitted; no mention is made, for example, of the ordinary methods of obtaining melting-points.

It is noteworthy also that processes relating to the purification of substances for physical study are not touched upon. Accounts of the best systems of fractionation, either by distillation or crystallisation, or of distillation under reduced pressure, &c., have, it seems to us, a better right to a place in a book of this kind than, say, the chapter on glass-blowing. Again, no particular notice is taken of methods which have to be used when only a small quantity of material is available. It frequently happens that a substance can only be obtained sufficiently pure in but small quantity, and if methods of obtaining boiling-point, density, refractive index, &c. in such cases were more widely known, physical constants would no doubt be more generally estimated in the course of ordinary chemical investigations.

It is needless to state that the book is full of useful hints both on methods and apparatus, and will be indispensable to those for whom it is specially designed. It is also worthy of special recognition as being yet another effort on the part of Prof. Ostwald to place physical chemistry on a level with other departments of experimental investigation.

J. W. RODGER.

OUR BOOK SHELF.

Handbook of British Hepaticæ. By M. C. Cooke, M.A., LL.D. 1 vol. 8vo. 310 pp. 7 plates. 200 woodcuts. (London: W. H. Allen and Co., 1894.)

PROBABLY no group in the British flora has received so little attention as the Hepaticæ. This is due partly to the ordinary botanical text-books describing merely the life history of the ubiquitous *Marchantia polymorpha*, and ignoring or passing over with but scanty reference the foliaceus group. But chiefly is it due to the want of a handbook by which beginners could identify their plants and obtain references to the literature of the subject. Sir W. J. Hooker's magnificent monograph, which appeared in 1816, contained plates with copious descriptions of all the British species then known; but it is now scarce, costly, and having all the species described under one generic name, *Jungermannia*, it becomes necessary, after identifying a plant by it, to refer to some other source to ascertain the now accepted name. Hooker's "English Flora," vol. v., in dealing with the same group, divides the frondose group into several genera, but retains the generic name of *Jungermannia* for the whole of the foliaceus group.

In 1865 Dr. M. C. Cooke published, as a supplement to *Science Gossip*, a catalogue with outline figures of all the British species. This is now out of print. Since then notes scattered through various journals have formed the whole of the British literature upon the subject, except the commencement of a monograph by the late Dr. B. Carrington.

Dr. M. C. Cooke has now filled up the gap by producing a "Handbook of the British Hepaticæ," containing full descriptions of all the species, about two hundred in number, known to inhabit the British Islands. The volume opens with an introduction of 20 pp., describing the position, structure, reproduction, and subdivisions of the group. This is followed by a detailed account of the species, each arranged upon the same plan. First come the diagnostic characters, followed by copious synonymy, then the habitat, and finally a full description. Each species is also represented by an outline figure, either in the text or in one of the seven plates at the end of the

volume. A bibliography and index complete the work. The size and clearness of the type will be appreciated by those who use the book, as it should be, in conjunction with microscopical examination of specimens. Altogether a very useful work has been produced, which ought to fill a gap already too long vacant.

C. H. W.

The Royal Natural History. Edited by Richard Lydekker. Parts 1 and 2. (London: Frederick Warne and Co., 1893.)

YET another "Natural History." There is certainly a demand for such, and without doubt there is a supply. The work is to be in six volumes, and the parts, published monthly, will complete the series in three years. The paper and typography leave nothing to be desired. The illustrations are in almost every instance, so far as our knowledge of the published parts goes, excellent; many of them are as artistic as they are accurate; and when we add that the editor of the series is an able and well-known zoologist, there can be no doubt but that the reader or purchaser will get full value for their expenditure of time or money.

In noticing a work of this nature, when the facts are as above stated, there is but little room for criticism, and despite the shock which the first blazing sound of its advent conveyed to our senses, despite the fact that "it is not compiled or translated from foreign sources," and that "the co-operation of the Bibliographic Institutes of Leipsic and Vienna" has been secured so as to obtain "all that is best and newest among the productions of the greatest natural history publishers of Europe," we yet most heartily recommend the work to all our readers, and we anticipate that most of those who take any interest in zoology will place it on their book shelves.

Of the six volumes, as was to be expected in a work of this kind, the larger number (five) is to be devoted to the backboneed animals, and but one to the boneless crew; and of the first five volumes, two and a half will relate to the mammals, one and a half to the birds, and but one to the reptiles, amphibians, and fish. It is not at all a fair division, but then the mammals are thought to be the most generally interesting class, and we are promised a lot of information about "the larger game." The first two parts are devoted to the monkeys, and we have an account of nearly all the known species, accompanied with an immense number of illustrations. One suggestion occurred to us while reading over the account of the habits of the baboons; that when plants are referred to they should, when their scientific names are used, be quoted specifically as well as generically; thus a "very remarkable kind of West African plant" is mentioned as the "welwitschia," but the editor would never think of quoting the *Anubis* baboon as the "cynocephalus." We hope it will be a long time before *Welwitschia mirabilis* will be exterminated by the baboons. From a natural history stand-point there is really no such plant as an "*ixia*," but there are several species of the genus *Ixia*, upon the bulbous stems of which it would appear these baboons feed.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. No notice is taken of anonymous communications.]

The Origin of Lake Basins.

I WELCOME the criticism of my article on the glacial origin of a certain class of lakes by an experienced geologist like Mr. Oldham, because it probably embodies the strongest argument that can be adduced on the other side—at all events as regards

the one aspect of the problem which he alone touches upon. He urges that my paper contains a fallacy and a misrepresentation. The alleged fallacy is, that because the lakes in question are found in glaciated and not in otherwise similar non-glaciated regions, "therefore the rock-basins in which the lakes lie were excavated by glaciers." But this is not my argument, and therefore not my fallacy. What I say is—"there must be *some* causal connection between glaciation and these special types of lakes. What the connection is we shall enquire later on." That there is a "causal connection" Mr. Oldham asserts as strongly as I do myself, though his is a different, and as I have endeavoured to show, an untenable one.

This brings us to the alleged misrepresentation, which is, that I have imputed to the opponents of the ice-erosion theory the view that the earth movements which, as they allege, produced the lakes, occurred in the period just before the ice-age came on. Mr. Oldham says, this is an unreasonable and unfounded limitation, since the movements in question probably occurred throughout the glacial period itself. I quite admit the validity of this criticism, and that I should have added, "or during the glacial period itself," to, "immediately before" it. I certainly had this probability in my mind, and the reason I did not express it was twofold. In the first place, all the advocates of the earth-movement theory appeared to assume, either directly or implicitly, the preglacial origin of the lakes; and secondly, this assumption gave them the strongest argument against my views, and I therefore gave them the benefit of it. Mr. Oldham appears to have overlooked this. Yet it is clear that the shorter you make the time since the formation of lake basins by earth-movements the more difficulty there is in explaining the total absence of valley-lakes from all the non-glaciated mountain regions of the world, since there is less time for them to have been all silted up. When arguing this point I said—in the passage evidently referred to by Mr. Oldham—"The only way to get over the difficulty is to suppose that earth-movements of this nature occurred only at that one period, just before the ice-age came on, and the lakes produced by them in all other regions have since been filled up." I thus gave my opponents the benefit of an extreme supposition which was all against myself; while the more reasonable view, that earth-movements are just as likely to have occurred during and since the glacial epoch as before it, renders my argument from the geographical distribution of lakes much stronger, since it is impossible to believe that, if lake basins as large and as deep as those of Geneva, Maggiore, Como, Constance, and Garda, were formed in non-glaciated regions as recently as the middle or latter part of the glacial epoch, a considerable number of them would not be still in existence.

Of course, if it can be shown that filled up lake-basins exist in tropical and subtropical regions, corresponding in number, position, size, and depth, with those of glaciated areas, the argument from geographical distribution will break down. At present I am not aware of any evidence that such is the case. But even if it were so, there remains the singular correlation between the size and depth of lake basins and the known size of the glaciers that occupied their valleys; together with the surface and bottom contours of the lakes themselves, so strongly opposed to their production by any form of valley-subsidence or earth-movements.

A friend has pointed out an unsound argument in my article on "The Erosion of Lake-basins" in the *Fortnightly*, and I therefore ask to be allowed to state what it is, and thus avoid its being possibly made the subject of discussion in the pages of NATURE. As a proof of the very great erosive power of ice I have adduced Dr. Helland's estimate of the quantity of Scandinavian *isbris* in Northern Europe. But it is evident that this only proves the great carrying power of the ice, since the rock and gravel would be mostly of sub-aerial origin. It, however, indicates a very long period during which the ice-sheet was at work, while the clayey element in it would be due to erosion. The larger part of this, however, would certainly have been carried away into the North Sea during the passage of the ice-sheet across the Baltic. The enormous quantity of boulder-clay in North America, which I have also referred to, is a better indication of the extent of true ice-erosion. ALFRED R. WALLACE.

THE question you have allowed me to raise is too important and far reaching to justify its dissipation upon personal issues. It cannot be thought unreasonable that those geologists who propound transcendental theories should justify the mechanical

postulates on which they claim to base them. This is all I have asked.

Dr. Wallace asks me to explain what will happen when sufficient pressure is applied to ice not only to crush it, but to induce regelation. I have already explained in my work, that the notion of fracture and regelation taking place in glaciers is at issue with the details of their differential motion as tested by experiment. There is no evidence that ice which on pressure being applied to it has ample room to move, will undergo regelation at all. The pressure when crushing ensues will be dissipated in the direction of least resistance, and most probably upwards. This emphasises Mr. Deeley's statement, and he wrote as a champion of Dr. Wallace, that "fracture and regelation have little to do with the question."

Dr. Wallace then returns to his charge against me that I have in some way committed myself in my work to a position inconsistent with the one I am now maintaining. I can assure him that if he has read this meaning into my words, it was not what they were meant to convey. In giving the history of the "Glacial Nightmare," I entered largely into the views of Charpentier, and in so far as he championed glaciers as against ice sheets I agree with him. I have said that his views "*are for the most part sound and unanswerable, since they finally established for the Alpine country and for Switzerland the fact that glaciers were formerly much more extensive,*" &c. Beyond this I could not go, since my work was written to prove the unscientific character of the extravagant conclusions of the later glacialists, including Charpentier himself after he became a follower of Agassiz. Apart from this, however, what your readers I am sure would welcome would be an *argumentum ad rem*, and not one *ad hominem*.

In demanding that the advocates of the glacial theory in its extravagant form should justify their premises and postulates, I must not be understood to decline to meet the geological case against the glacial excavation of lobes. I have met it at great length already in my recent work, but not so ably and not so thoroughly as Mr. Spencer met it in his elaborate and crushing examination of the critical case of the North American lakes, which I commend most heartily to the study of enthusiastic champions of omnipotent ice.

The geological question, however, is necessarily contingent upon the mechanical question, and no amount of ingenuity will in the long run enable those who invoke ice as the author of all kinds of geological work to evade the duty of proving its capacity to do that work, and notably to explain how it can travel over hundreds of miles of level country, or suddenly begin to excavate deep and extensive lake basins after it has been moving gently over its own bed of soft materials for many miles, or, indeed, how it can excavate on level ground at all. The first step is to show that ice can convey thrust in a way to compass these ends; the second one is to show whence this thrust is to be derived. Your readers who are committed to no theories unsupported by facts, will not quarrel with the reasonable demand that these first steps should be surmounted before we advance any further. Those who like to traverse cloud-land on the wings of fancy may be otherwise satisfied. To them I would only say that the result cannot be science; it must remain nothing more than poetry.

HENRY H. HOWORTH.

30 Collingham Place, Earls Court, December 30, 1893.

Hindoo Dwarfs.

IN your issue of November 9, 1893, is a notice of some photographs, by Colonel A. T. Fraser, of two dwarfs, taken in the Kurnool district of the Madras Presidency, near Bellary. From the account given of these dwarfs—the hereditary nature of the deformity, its limitation to the males of the family, the inability to walk, the normal bodily growth up to six years of age—it seems possible, if not probable, that the family is afflicted with the disease known as pseudo-hypertrophic paralysis (Duchenne's paralysis). Any physician could settle the question immediately on seeing one of the subjects in question; and very probably a study of the photographs would be sufficient. I have had cases of this disease in my wards at the General Hospital, sent from Bellary. Perhaps Colonel Fraser would kindly send me a copy of one of the photographs, or show them to another medical officer, and tell us his opinion. A. E. GRANT.

Madras, December 2, 1893.