

added a useful "index glossary," and a series of "test questions," largely culled from examination papers of the past. The work is by no means destitute of small incongruities and an occasional misuse of technical terms; and the most serious errors which it contains, contrary to the general rule, involve leading rather than subsidiary topics. The description of "living matter" as existing in the "colloidal condition" and (two pages further on) as "a semi-fluid granular substance . . . unable to absorb colouring matters when living"; the alleged origin of the cœlome of "all animals above the cœlenterata" by "the splitting of the mesoblast"; the assumption that the contractile vacuole of the protozoa is a respiratory organ "pumping in oxygenated water," and "furnishing oxygen to the animal by means of its rhythmical dilatations"; the confusion under the term "paraplasm" between modified portions of the cell-protoplasm and products of its living metabolism, with the correlated description of the protoplasm of the egg cell as a "vitellus, or yolk"; and the description of sclerenchyma as "stony tissue," are cases in point. We note with satisfaction the prominence given to the physiological and more purely chemical aspects of the subject, too often neglected in minor works on general biology. Conspicuous among leading dogmas formulated is the assertion that with the exception of ascidians and some infusorians the animal "does not contain cellulose," with the implication that certain animals form chlorophyll. We venture to think that the time has now arrived when the investigations of Beyerinck, Famintzin, Von Graff, and Haberlandt, Ambronn, and others, which have lately revolutionised our knowledge on these vitally important topics, should find expression in the elementary class-book. The author remarks in his preface that "it must be remembered that biology can be learnt in no other way than with the scalpel and the microscope," and that his volume is intended "simply and solely for the purpose of revising" a practical knowledge which the student has gained under the guidance of his teachers, "especially during the few weeks previous to the time when he intends to cross the threshold of the examination hall." If this line of conduct can be ensured, the work will fulfil a good purpose; but it may be doubted whether the overtaught medical student of to-day will regard the book as anything but a cram one. It has been compiled at considerable pains and with marked success; but as the dispensation which it seeks to further cannot possibly endure, we wish we could congratulate the author upon a devotion to some more permanent and desirable object.

Public Health Problems. By John F. J. Sykes. Illustrated. (London: Walter Scott)

THE author of this volume—which forms one of the Contemporary Science Series—has sought "to bring to a focus some of the essential points in evolution, environment, parasitism, prophylaxis, and sanitation, bearing upon the preservation of public health." It was impossible for him to deal fully in the space at his disposal with any particular part of so vast a subject, but he has contrived to give a very clear and interesting idea of the main lines of inquiry with which workers in the public health service are chiefly concerned. First he treats of internal and external influences affecting health, these influences being heredity, physical influences (light and heat), chemical media, and biological agents. Then he discusses the following aspects of communicable diseases—causation, parasitism, dissemination, and modifications. Afterwards there are series of chapters on defensive measures against communicable diseases, and on the urban dwelling. Mr. Sykes, as medical officer of health for St. Pancras and honorary secretary of the Incorporated Society of Medical Officers of Health, has had ample opportunity for the study of the questions on which he discourses, and his book ought to be of good

service in disseminating sound ideas as to the conditions on compliance with which the attainment of a higher standard of public health depends.

Galenic Pharmacy. By R. A. Cripps. (London: J. and A. Churchill, 1893.)

THE student of pharmacy will, no doubt, find plenty of instructive information in this book. It does not, however, call for an extended notice in this journal, as the author does not attempt a scientific treatment of the subject, but confines himself to dealing with it on the old lines. The various pharmaceutical operations of solution, infusion, &c., are fully described, but no attempt is made to arrange the facts on any than an empirical basis. The time has arrived, however, when pharmacy should be expounded in a more scientific manner, and many barbaric and obsolete processes excluded or re-modelled in the light of our present chemical and pharmacological knowledge.

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.

Mr. H. O. Forbes's Discoveries in the Chatham Islands.

IN a paper read before the Royal Geographical Society on March 12th, and again in an article on "The Chatham Islands and their Story" in the *Fortnightly Review* of this month, Mr. H. O. Forbes has described his very interesting discoveries in these islands, and has founded thereon certain conclusions as to the past history of the New Zealand group. The most startling new fact is the proof of the recent existence on the Chatham Islands of two birds whose nearest allies inhabited the distant group of the Mascarene Islands within the historical period. These are a flightless rail very closely allied to the *Aphanapteryx* of Mauritius, and a coot which is hardly different, except in its somewhat larger size, from the extinct *Fulica newtoni* of the same island.

It is on the flightless rail that Mr. Forbes mainly dwells in his deductions of past changes which it is supposed to imply, and it is on these deductions only that I wish to make a few remarks. He quotes Prof. A. Newton and his brother as stating that the solitaire of Roderiquez and the Dodo of Mauritius, being evidently of one stock, and there being analogous facts in the adjacent islands, they are compelled to believe that "there was once a time when Roderiquez, Mauritius, Bourbon, Madagascar, and the Seychelles were connected by dry land"; and he then argues that there must also have been a continuous land surface between this land and the ancient land comprising New Zealand and the surrounding islands. This connecting land he supposes to have been the Antarctic continent during a mild period and with great extensions over the southern ocean. When the Antarctic ice age came on the inhabitants of this continent had to migrate northwards, and some, "such as the genus *Aphanapteryx*, would seem to have split into parties, which, travelling by divergent roads, finally arrived in regions so far apart as Mauritius and the Chatham Islands, unaffected by the varying climates and surroundings they experienced, being of an ancient dominating type."

It is this tremendous hypothesis which appears to me to be not only quite unnecessary to explain the facts, but also to be inadequate to explain them. If one thing more than another is clear, it is that these comparatively small flightless birds were developed, as such, in or near to the islands where they are now found, since they could not possibly have arisen on any extensive land inhabited by carnivorous mammals and reptiles, and, if introduced into such a country, could not long survive. So far as I am aware, no doubt has ever been expressed on this point, the evidence for it being so clear and its explanation on the theory of evolution so complete; and I hardly think that Prof. Newton would now maintain that the affinities of the flightless birds of Mauritius, Bourbon, and Roderiquez implied the former union of these truly oceanic islands. Allied forms of ancestral flying birds may have reached the islands without such union;

and, owing to the total absence of terrestrial enemies and the abundance of food, may have developed into the allied flightless birds whose remains are found there.

But Mr. Forbes speaks of the genus *Aphanapteryx* itself, presumably therefore flightless, inhabiting the Antarctic continent, and migrating northwards by two routes of about 2000 miles each, in which case, this enormous extent of land must have been as free from all carnivorous land mammals and reptiles as New Zealand and Mauritius are now. If however, the birds in question lost their powers of flight in or near the islands where their remains are found, all difficulties of this kind disappear. The *Aphanapteryx* belongs to a family, the *Rallidæ* or rails, of world-wide distribution, while many of the component genera are also almost cosmopolitan, and are represented by closely allied species in distant regions. What difficulty, therefore, is there in the same or closely allied species of this widespread group finding their way at some remote epoch to Mauritius and the Chatham Islands, and, from similar causes in both islands, losing their power of flight while retaining their general similarity of structure? To put the matter briefly: if the common ancestors of the *Aphanapteryx* of Mauritius and the Chatham Islands were flightless, they could not have reached those islands from the Antarctic continent owing to the length of route and the presence of enemies; while if they possessed the power of flight no important change in land-distribution is required.

I have discussed this one point only, because it illustrates the very common practice of explaining each fresh anomaly of distribution by enormous changes of physical geography, when a much more satisfactory explanation can be given involving no such vast and unsupported revolutions in the earth's surface. I am aware that Mr. Forbes adduces many other facts and considerations in support of his view as to the former extension and habitability of the Antarctic continent, some of which appear to me to be valid and others the reverse. On most of these I have already expressed an opinion in my "Island Life"; and I only write now in order to point out that the very remarkable and interesting facts, whose discovery we owe to Mr. Forbes's energy and perseverance, do not add anything to the evidence already adduced for that view, but may be best explained in a far simpler manner, and without requiring any important changes in the geography of the southern hemisphere.

ALFRED R. WALLACE.

Swarms of Amphipods.

ONCE last winter on entering the laboratory here after it had been shut up for a few days, we found the floor, tables, shelves, window-ledges, and even dishes on the highest shelves, covered with great numbers of dead amphipods. These were found to be *Orchestia gammarellus* (the shore-hopper). About ten days ago an unusually high tide occurred, and the curator and others who were working in the biological station noticed that the steps leading to the beach were swarming with amphipods. On investigating further it was found that the amphipods were coming up in great numbers from high-water mark, that they jumped up the steps, and that they climbed the vertical concrete wall surrounding the station to a height of several feet. Many of them were found about twelve feet above the sea, having come nearly all the way on artificial ground (concrete steps and wall), and they were so abundant on the platform outside the laboratory door that it was impossible to put a foot down without treading on many. Specimens were kept, and Mr. A. O. Walker, who is here with me now, finds that these also are *Orchestia gammarellus*. This species lives normally at or about high-water mark, and it is abundant here under stones at that line, but Mr. Walker tells me that he has taken it on the one hand nearly at low-water mark, and on the other hand under stones on grass, along with beetles, and we have found it near here far above high-water mark at the side of the road. However, these last are probably exceptional cases, and we are both inclined to think that the two amphipod invasions noticed here have been caused by the *Orchestias* being driven from their usual haunts by exceptionally high tides. But whether a panic arises on the flooding of their homes, or they lose their way on our concrete, the fact remains that whereas the sea was only a couple of feet higher than an ordinary high tide the amphipods ascended on the one occasion to about twelve and on the other to perhaps twenty feet above their usual level.

Port Erin, April 29.

W. A. HERDMAN.

A Difficulty in Weismannism Resolved.

WEISMANN'S essay "On the Significance of Sexual Reproduction in the Theory of Natural Selection," published in 1886, enunciates the thesis that the object of sexual reproduction is "to create those individual differences which form the material out of which natural selection produces new species." This thesis was developed in the essay, "On the Number of Polar Bodies and Their Significance in Heredity" (1887), and still further in "Amphimixis," published late in 1891.

While "Amphimixis" must have been nearly ready, I wrote to NATURE (vol. xlv. p. 613), under the heading, "A Difficulty in Weismannism," pointing out *a posteriori* the complete insufficiency of sexual reproduction, by merely shuffling ancestral germ-plasms, to effect indefinite specific variation on the lines adopted by Weismann. My friend, Mr. Poulton, wrote (vol. xlv. p. 52) accepting my summary of Weismann's views "as fair statements," but criticising the deductions is not allowing for the effect of different groupings of the ancestral plasms in the germ-cells, and regretting that I had not awaited the publication of "Amphimixis," as "Prof. Weismann tells me," he wrote, "that the points raised by Prof. Hartog are considered in this treatise." Mr. Trow also wrote (vol. xlv. p. 102), urging that I had not allowed for the simultaneous action of natural selection or for the combinations of germ-plasms. In reply to my rejoinder of the same date, Mr. Trow again urged that I had not taken natural selection into account, and that I had misunderstood Weismann's position. The controversy was then closed.

However, neither the German edition of "Amphimixis," nor the authorised English translation published about six months later, contained the solution of my difficulty that was anticipated by Mr. Poulton. There runs through the book like a red thread the conception of 1886, that sexual reproduction is the creator of the variations on which natural selection acts. A reference of mine to the inadequacy of this, Weismann's Theory of Variation, contained in an article in the *Contemporary Review* for July, 1892 ("Problems of Reproduction"), passed without answer or comment, so far as I know.

In "The Germ Plasm, a Theory of Heredity" (1893), Weismann devotes chapter xiv. to the consideration of heredity. Herein I find the following theses, in which I preserve the italics of the original (English edition):—

1. "It [sc. amphimixis] is not the primary cause of hereditary variation," p. 414.

2. "The cause of hereditary variation must lie deeper than this [amphimixis]. It must be due to the direct effect of external influences on the biophors and determinants" [sc. of the germ plasms or ids], p. 415.

3. "The origin of a variation is equally independent of selection and amphimixis, and is due to the constant occurrence of slight inequalities of nutrition in the germ plasm," p. 431.

Obviously the position of 1886-91 has been abandoned as untenable. If we ask why, the answer is probably contained in the following passage and annexed note ("Germ Plasm," pp. 434-5):—

"It has recently been maintained that as a consequence of my theory I must adopt one of two alternatives, and assume either that the germ plasm of the higher animals consists of ids of the primitive protozoan ancestors, or that every id is constructed in accordance with the existing character of the species; my real view, however, is intermediate between these two." The note runs: "Compare Marcus Hartog, NATURE, vol. xlv. p. 102." The reference omits my letter of October 31, 1891. The deductions made by this author from my former views are logically correct, but are no longer justifiable, since I myself have gained further insight into the problems concerned.

It follows from the above—

1. That Weismann has withdrawn his whole theory of specific variation as created by sexual reproduction.

2. That my account of his views on the point at issue in 1891 was both full and fair.

3. That in 1891 no one else, not even Prof. Weismann, had perceived that "logically correct" deductions from his general theory of the germ plasm were fatal to his theory of variation.

4. That the Weismannism of to-day regards the action of external forces as the one essential cause of variation, so far approximates to the teachings of the older evolutionists.

As no reference is made in the preface to this matter, nor even in the index (for which Prof. Weismann is not responsible),