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 Antartic 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. xliv. 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. xliv. 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),