theory of Weismannism *minus* natural selection. In the *second place*, we believe that Weismann means *permutations*, though he uses the term *combinations*. After a football team has been selected, the men can be arranged in 11 different ways. The arrangements would virtually constitute new teams, and newspapers would speak of them as strong and weak combinations. The combinations of the *Ahnenplasmas* can be assumed to be of a similar kind. The arrangement almost certainly counts for something. Nevertheless, Prof. Hartog's contention—that the elimination of *Ahnenplasmas* in the shuffling process would lead to ever-increasing simplicity—demands serious consideration, for duplication lessens the possible number of permutations and combinations. I would point out that we may conceive that the *Ahnenplasmas* were, in asexual unicellular organisms, either *all the same*, *all different*, or in intermediate conditions. In any one of these cases we must assume that *m*, the number of individuals, was much greater than *n*, the number of *Ahnenplasmas* present in every individual. With the evolution of sexuality (all the individuals being different) we should get combinations of, at least, *m* *Ahnenplasmas* taken *n* at a time. Different permutations of the same combination would be, of course, possible, giving rise to other combinations, using the word in the general sense. We must suppose that natural selection operated upon the variations produced by these first combinations. Natural selection had operated upon the unisexual ancestors of these sexual forms. We can at least conceive that development would follow one of two courses. Along the *first*, combinations in which more than one unit of a kind appeared would, if possible, be prevented. Such might arise, but under the operation of natural selection they would not be allowed to perpetuate themselves. Along the *second*, such combinations might arise and be perpetuated. In either case, it must be assumed that the combinations which survived were such as were best adapted to the varied combinations of external conditions. This may be made clearer by an illustration. In Rugby football, combinations of 15 in which 8 or 9 of the men—the forwards—are all the same would be strong, whereas, if all were different, they would be weak. In Association football, strong combinations could only be made up by selecting different types of players for the different places. I am inclined to believe that both cases are followed by Nature. The one which I have illustrated with reference to Rugby football cannot, however, have been generally followed. It is an adaptation for which the organism has ultimately to pay dearly, and is as dangerous to the development of the *phylum*, as we may suppose parthenogenesis to be to the *species*. Taking the case of plants, I would say that the one course may have been followed along the line of development of the main archegoniate series, the other in the development of such divergent groups as the *Ustilagineae* and *Gastronomycetes*. The argument of Prof. Hartog, therefore, while of no avail as directed against Weismannism, is of use in so far as it enables us to better understand *divergence*. I am inclined to think that it may serve also to explain the remarkable *persistence* of such forms as *Nautilus*. It suggests, too, an explanation of the disadvantage of breeding "in and-in." Finally, I would remind Prof. Hartog that neither of the disciples of Weismann apparently believes in the non-variability of the *Ahnenplasmas*. If their beliefs have a substantial foundation, it follows that the number of possible combinations becomes absolutely unthinkable.

I shall be much obliged to Prof. Hartog if he can inform me of any theory of heredity whose foundations are not "more or less mythical." There are, no doubt, many difficulties in Weismannism, before one of which, the theory, having served its time, may come to the ground. I do not think that Prof. Hartog's is one of them.

A. H. TROW.

Penarth, Cardiff, December 10.

Destruction of Immature Sea Fish.

IN your number of November 19 (p. 49) you review the Ninth Annual Report of the Scotch Fishery Board. I have not seen the Report, but assume that your reviewer's statements as to its contents are correct. My object in writing is to draw attention to the opinions attributed to Dr. T. Wemyss Fulton as to the destruction of young fish by shrimpers. I may say at once that I am one of the "very many" to whom the "results" are "surprising" as your reviewer remarks. I am an old shrimp-trawler in the Dee and along the Flintshire coast, and I have no hesitation in saying that, as regards the Dee and, I believe, the

Mersey and the Lancashire coast as far north as the Ribble, the destruction of young fish is absurdly under-estimated, whether I judge by my own experience or by that of Mr. R. L. Ascroft, of Lytham, with whom I have been in correspondence on the subject since 1889. This gentleman, however, informs me that Dr. Fulton's information was obtained from Morecambe Bay, where smaller trawls are used, and the boats drift with the tide instead of sailing. Dr. Fulton has been informed that in the Solway Firth a single boat in one year captures over 110,000 immature plaice. If the word "year" is not a mistake for "week," either the statement is immensely under-estimated or the conditions in the Solway must be very different from what they are further south. This may be judged by the following extract from a letter written by Mr. Ascroft in 1889. I may say that this gentleman (who is now, I am glad to say, a member of the Lancashire Fishery Committee) has had a long and practical experience in all kinds of sea-fishing on the Lancashire coast, and is a careful and accurate observer. He writes as follows:—"Shrimping destroys more young fish than almost any other agency. I have seen in Formby Channel 10 cwt. of young flukes destroyed, not one the size of half-a-crown, by one boat, and there were sixty boats there that day."

Now, taking the weight of a fluke the size of half-a-crown at $\frac{1}{2}$ oz., a simple calculation will show that each boat captured 35,840 young flukes (a term which includes plaice and dabs) in one day, or 215,040 in a week of six days—nearly twice as many as Dr. Fulton's figures for a year! And elsewhere Mr. Ascroft says: "You may put it as an axiom that 90 per cent. of fish that comes on a boat is destroyed, as when trawling they sail back as they have got their net, and do not commence sorting the take until the net is out again, and they do not, in shallow water, throw the rubbish" (*i.e.* everything except shrimps) "over until they turn out to haul, for fear of getting it into the net again." All of which I may say is borne out by my own experience.

The following is an extract from my diary, written July 10, 1885, when Fishery Committees were not dreamt of. The occasion was an excursion for dredging purposes of the Chester Society of Natural Science, when I took my boat and trawl to meet their steamer at the mouth of the Dee. The Green Buoy marks the bar near Prestatyn, and I let down the trawl in mid-channel (about 5 fathoms) in the hope of getting some natural history specimens:—"Began to trawl just below the Green Buoy. Got a few goodish soles, and an immense number of young soles, which always squeeze their heads through the meshes. (N.B.—Shrimp-trawling at this time of year should only be allowed within a quarter of a mile of the shore, to avoid the immense destruction of fry, which mostly lie further out.) Afterwards got a good haul of shrimps as close in (shore) as we could go." I have a perfect recollection of the occasion, and although the trawl was only down about twenty minutes I was horrified at the number of young soles which were in the net, and most of which had choked themselves. But there were very few shrimps, which mostly lie in very shallow water near the edge of a sand-bank.

As a remedy for this destruction I would suggest that the principal breeding-grounds be ascertained, and trawling on them prohibited at such times as the young fish are there. If the prohibition be evaded, then a steamer-load of very large angular stones, distributed from 100 to 200 yards apart on the selected grounds, would effectually prevent trawling, and at the same time, as they became covered with weed, afford shelter and food to the fish and shrimps. This has been done by Nature in this bay, where large boulders washed out of the drift that here forms the coast-line strew the shore at wide intervals, and render trawling for shrimps impossible, though hand nets can be and are worked.

I trust the importance of the subject will excuse the length of this letter.

ALFRED O. WALKER.

Nant y Glyn, Colwyn Bay, December 14.

The Salts in Natural Waters.

THE inquiry of your correspondent "R. B. H.," in NATURE of November 26 (p. 78), may be answered as follows. In the analysis of an ordinary water, after determining the respective amounts of lime, magnesia, (soda), carbonic acid (combined), sulphuric acid, nitric acid, and chlorides (these being the constituents met with usually in such a water), we proceed to combine the acids and bases thus: the carbonic acid is calculated to carbonate of