stated. In fact, Prof. Weldon's original interpretation has not yet been, and never may, be disproved, and, for all we know to the contrary, selection may still be acting in the direction indicated by his measurements. Mr. Bather will see, on reference to my letter, that I advanced the idea "for whatever that idea may be worth." It appears necessary to point out that the "facts" are before us in this case; it is a question of interpretation. The difficulty raised by your correspondent is one of his own creation, and not one of mine. Where have I stated, or even suggested, that the appearance of a character which formerly was of selection value, and which now appears in the later stage of individual development, is a "universal principle"? If the suggestion which I made with reference to the interpretation of Prof. Weldon's results is sound in principle, it is obvious that it refers to a (comparatively) recent cessation of selection, and so far Mr. Bather has correctly grasped my meaning. When, however, he lays it down, on his own responsibility, that the conditions then "can hardly have been very different to now," he is laying claim to a knowledge of the past and present conditions of life and to a familiarity with the time necessary to bring about the modification of species, at which I can only stand aside with envy and admiration. If Mr. Bather really must have some "known fact" after this distinct dissociation of myself from his methods, I will refer him to those cases of mimetic butterflies which have no models. There is good reason for believing that certain butterflies which are undoubted mimics, since they depart from their allies in type of pattern, have for some unknown reason survived, while the species which they imitated have, also for unknown reasons, become extinct. The mimics still retain their mimetic pattern and colouring, but the selective process which produced this type can no longer (in the absence of the model) be regarded as in active operation. Nevertheless, the mimetic disguise is retained by virtue of heredity, and appears in the last stage of the ontogeny. Your correspondent will, I hope, pardon any apparent discourtesy if I state that the correspondence on this subject is, so far as I am concerned, now closed. I can assure him that he shall hear more about the results of Prof. Weldon's measurements at no very distant future.

R. MELDOLA.

Measurements of Crabs.

MR. CUNNINGHAM, in NATURE of October 29, raises a doubt as to the trustworthiness of the results of my measurements of crabs, on the ground that the specimens of the year 1893 had been longer in spirit than those of 1895.

The value of measurements made on animals preserved in spirit naturally depends on the animal so preserved; and to any one who is acquainted with the rigid nature of the calcareous carapace of *Carcinus menas*, it seems difficult to admit the possibility of distortion from such a cause. All specimens that were not quite rigid (a condition due to the animal having moulted just before capture) were rejected as a matter of course.

The fact that a deficiency in one dimension was "compensated" by an excess in the other dimension, so far from suggesting to Mr. Cunningham a suspicion of the specimens having "undergone an artificial change of shape," would, I think, have seemed not only natural, but a feature to be expected, had he realised that every crab during its growth passes through a similar change of shape, in which the relative increase in size of the one dimension (the dentary margin) is accompanied by a corresponding relative diminution in size of the other dimension (frontal breadth). The table appended to the paper in question shows that, while the dentary margin in the youngest crabs measured increased on an average from 414 thousandths of the carapace length to 498 thousandths in the adult, the frontal breadth, on the other hand, diminished in relative size from 813 thousandths in the young to 595 thousandths in the adult; so that a compensatory relation between these two parts of the hard skeleton is a normal phenomenon in these animals.

I would venture to add that observations in recent years, on the variability of both animals and plants, tend to show that species are much more unstable than was once supposed. The changes recorded in these crabs in the space of two years are very minute, but they are persistently in one and the same direction in all the twenty-six stages of growth measured. If such a rate of change exceed what we should have expected, it may be because hitherto we have had no collections of comparative facts on which to base any reasonable expectation.

H. THOMPSON.

October 30.

I AM able to offer Mr. Cunningham direct experimental evidence that the hard carapace of a young shore-crab is not sensibly distorted by immersion in spirit for six months.

I have lately had occasion to compare the mean frontal breadth in three samples of young female crabs, gathered from the same locality in Plymouth Sound. The range of size was the same in all the samples. The samples were (1) a large number of individuals, collected in 1892 and 1893, measured after immersion in spirit during a period varying between six months and a year: the mean frontal breadth in these crabs was compared (2) with that of 569 individuals, collected during the summer of 1895, and measured fresh while still wet with seawater; and (3) with that of 595 crabs, gathered also during the summer of 1895, but preserved in spirit for about six months before measurement.

The mean difference between the frontal breadth in the spirit specimens of 1892-3 and that of the fresh specimens of 1895, was 1.47276 of Mr. Thompson's units; the difference between the crabs of 1892-3 and the spirit specimens of 1895 was 1.58992 units. The difference between the two results obtained from crabs of 1895 was therefore only 0.11716 unit—a difference so small, compared with those observed by Mr. Thompson, that it may safely be neglected, even if it be assumed to be due entirely to the spirit, and not to be within the probable error of the determination. As for the distribution of deviations from the mean in the two samples, it would take too much space to compare them in detail here; I hope to do so before very long in connection with another problem, and therefore I will only now say that the "standard deviation" (= error of mean square) of the series of fresh shells was in Mr. Thompson's units 11.7955, that of the spirit specimens being 11.9628 of the same units.

These figures seem to me sufficient to justify Mr. Thompson's conclusion; and it must therefore be held that a change, whether of oscillation or of evolution, is going on at a measurable rate among *Carcinus menas* in Plymouth Sound.

The very great importance of this result to all students of animal evolution is evident; for the careful study of cases in which change is actually going on so rapidly that it can be watched and measured in a reasonably short space of time will assuredly be found to give the best, if not the only clue to the process of evolution in general.

W. F. R. WELDON.

University College, London, October 31.

The X-Rays produced by a Wimshurst Machine.

IN my two papers of June 4 and June 18 respectively, I proved the existence of non-homogeneity in the X-rays, and gave a simple method of strengthening and maintaining the discharge of these rays from an ordinary focus tube by means of a conductor wrapped round the part of the tube level with and behind the kathode, and separated by a small sparking gap from either an earthed wire or the kathode's external wire loop, or the wire leading from the induction coil to the kathode itself. Since then my attention has been chiefly turned to the most remarkable results which can be attained with a Wimshurst machine, with which this paper is concerned. My Wimshurst has two 15-inch plates, and, not being of the latest type, has the old-fashioned metallic sectors and buttons, instead of the plain varnished glass disc, though I do not imagine that the results will be found materially different with the simpler, and probably better, form.

At first I drove the machine by hand, and hand driving is sufficient for the results mentioned in this paper—a most important fact, seeing that it renders unnecessary any other form of engine than the hand, and brings the copious production of any kind of X-rays within the reach of those who have neither battery nor dynamo—afterwards, for securing greater personal freedom and uniformity of turning force, I drove the Wimshurst by a small motor, an easy and most convenient plan, using a platinum electrode acidulated water resistance by which the rate of rotation is easily governed.

To me, accustomed only to the effects given by a $\frac{4\frac{1}{2}}{16}$ spark induction coil, the brilliancy of the shadows given by simply connecting my tube's electrodes to the brass knob terminals of the Wimshurst used without condensers was surprising, and the steadiness of the image a most grateful rest to the eyes. The bones seemed more transparent than I had ever seen them, and though at first I thought this might be only the effect of the lessening contrast brought about by increased general illumination, this does not prove to be the case. The two faults to be