LETTERS TO THE EDITOR.

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The Statistical Investigation of Evolution.

MR. THISELTON-DYER states that Prof. Weldon has shown that "selective destruction" takes place in early life amongst individuals which deviate from the "mean specific form." He further says that the actual statistical demonstration of the fact that "minimum destruction is in position coincident with the mean of the whole system," deserves to rank amongst the most remarkable achievements in connection with the theory of evolution. But, to judge from the paper by Prof. Weldon, printed in NATURE of March 7, he does not claim to have made this remarkable achievement. He says that, according to the results of the statistical investigation, in two dimensions of the shore crab, the frequency of deviations increased during an early period of growth, and that in *one* case the increase was followed by a decrease; in the other case it was not. Prof. Weldon states that if a certain law of growth can be shown to be true by experimental tests, *then* the result implies a selective destruction in the one case and *not* in the other. So that all we have is the possibility in the future of a statistical demonstration of selective destruction in the case of one particular dimensional character, and the rigid proof in the present that in the case of the other dimensional character selective destruction does not take place. Surely every man of science must admit that Prof. Weldon's results, on his own showing, have done more against selective destruction than for it.

Prof. Weldon says that if we know that a given deviation from the mean is associated with a greater or less percentage of death-rate, we do not require to know how the increase or decrease of death-rate is brought about, and all ideas of functional adaptation become unnecessary. This may be his own state of mind on the subject, but I venture to state that it is not Darwinism, and that he cannot shut out others from the most interesting and most important fields of biology in this way. Darwinism states that selective destruction is caused by the struggle for existence, and that a selected character confers an advantage in the competition to get food and beget offspring. If a certain deviation is shown to be associated with an increased or decreased chance of life, we want to know how it acts, and no statistical Gallio can prevent us trying to find out.

It does not require much search to find deviations which are associated with an increased death-rate. In the human subject cyanosis, due to the retention of the fœtal communication between the two sides of the heart, is a well-known abnormality or deviation in the infant; but I believe few, if any, children born in that condition reach the age of 20. Here we have no difficulty in understanding the reason: the deviation necessarily leads to death. But now, in comparison, take the case of a child born blind, or deaf and dumb. Here there is no intrinsic reason why life should be shortened; but in a severe competition, if the individual depended entirely on his own exertions, he might be, probably would be, starved or trampled to death before he had lived very long. I think it is of some interest and importance to know of any given character or deviation, whether it is intrinsically harmful or beneficial, extrinsically so (*i.e.* in the struggle for existence and reproduction), or quite indifferent.

Prof. Weldon is silent, to some extent, about the cases which tell against the idea of selective destruction. He found that deviations in Aurelia were as numerous in the adult as in the Ephyræ. He told me in conversation, and did not say it was in confidence, that he abandoned some experiments on the selection of Daphnia, because he found that the mere fact of keeping a large number in the same water caused a progressive disappearance of a certain conspicuous spine. His investigations also entirely ignore the diagnostic value of the characters he deals with. It seems to me that a more valuable result would be gained if a parallel investigation were made of two characters—one obviously diagnostic, the other obviously adaptive. Such characters could be found in a swimming crab.

But above all, what we want is a comparative investigation of the results of selection without change of conditions, and of change of conditions without selection. I began, not long ago, to try to inaugurate a society for carrying on a thorough investigation of this kind, but have not at present received enough support to carry out the scheme. The method of the investigation is fairly obvious and not difficult, but the difficulty is to get the money and the time to carry it out. I differ from Prof. Weldon in thinking that the questions raised by the Darwinian hypothesis are not purely statistical, but experimental, and I agree with Mr. Thiselton-Dyer—that to talk of experimentally checking the hypothesis by the statistical method is a contradiction in terms.

J. T. CUNNINGHAM.

Cleethorpes, March 15.

A True Spectrum Top and a Complementary One.

To make a true spectrum top—which is not copyright, so far as I know—take a disc of white paper, and one of black, of equal size. Spin the white one on a disc of cardboard mounted on a nail, and while it is spinning draw a small brush well charged with lamp-black water-colour paint, steadily and not too slowly from centre to circumference of the disc, thus describing a spiral line. Make a radial cut in each of the discs, and after interlocking them as in the well-known colour discs, place them on the top. We thus obtain a top in which the lines are spiral, and the relative sizes of black and of white areas are easily regulated by turning one disc to right or left, while the other is held still. If the lines be not too thin or too thick, and not too near together, and if the relative areas of black and white be adjusted suitably to the light, the top exhibits, when spun, broad bands of colour, each band containing all the colours of the spectrum in their natural order. The spaces between the lines should be not less than five times as broad as the lines themselves. The brightest effects are produced in my own case, by lamplight, with the areas of light and dark almost equal; by daylight, with the dark area about three times as great as the light. Other proportions, however, seem to give better results with other people.

A "complementary top," yielding colours complementary to those of the spectrum (*i.e.* the colours of mother-of-pearl) in a continuous band ranging from lemon-yellow, through puce to electric-blue-green, is made in the same way, except that the spiral line is to be drawn in white on the black disc.

In both cases the colours are somewhat dilute, but the proper regulation of the relative areas of black and white reduces this defect very considerably, and I have obtained bands on my spectrum top brighter and purer than any which I could get by painting a spectrum with colours on paper.

I communicate this description before my experiments are complete, in order to prevent anyone who may make the same discovery, from obtaining a copyright for the design of either these tops or of earlier ones which I made, in which one half the disc was black and the other white, with a white spiral on the black, or a black spiral on the white, or with both at different distances from the centre on the same top. Anyone who wishes to do so may make as many tops or lantern-discs as he chooses from the above description, provided he does not attempt to hinder anybody else from making or selling similar ones.

C. HERBERT HURST.

Owens College, Manchester, March 24.

A Foucault Pendulum at Dublin.

IT may perhaps interest some of your readers to learn that Foucault's pendulum experiment has recently been performed in Trinity College, Dublin, with complete success.

Immediately under one of the glass domes, by means of which the hall of the New Building is lit, a cast-iron bar was securely bolted, which terminates in a cylindrically-shaped piece of metal the axis of which is vertical. Into this cylinder a steel plug was inserted, which was drilled to receive the upper end of the wire supporting the bob, which was fitted with a screw. By placing the upper end of the wire in this position, Prof. FitzGerald and I secured a length of 45 feet for our pendulum; but, under the circumstances, we were unable to use the same weight as that adopted by Sir R. Ball when making the experiment, viz. 300 lbs., and were obliged to content ourselves with a bob weighing 16 lbs., which, however, answered admirably.

The experiment is made in the following manner:—About two feet behind the position of equilibrium of the bob, we place the electric lamp, and at a suitable distance in front a lens, so