

Prof. Weldon concludes with the observation that "numerical knowledge of this kind is the only ultimate test of the theory of natural selection; or of any other theory of any natural process whatever."

It has tested natural selection, and shown that nothing of the nature of a *true variety* has been established by it. There is no evolution in the process described at all.

Does he not speak a little too confidently as to there being no other means of investigation into the procedure of evolution?

The true method of establishing this doctrine, as in all other matters of science, I take to be by *inductive evidence and experimental verification*. By these it has been proved that true varietal changes are produced by what Darwin called "the definite action of changed conditions of life," and he added that when this was the case "a new sub-variety would be produced without the aid of natural selection" ("Animals and Plants under Domestication," vol. ii. pp. 271, 272).

In support of this contention of Darwin's I shall be happy to supply Prof. Weldon with an abundance of facts collected in my book, "The Origin of Plant Structures," if he will promise to read it, entirely unbiassed by his established belief in the efficacy of natural selection.

GEORGE HENSLOW.

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THE points raised by Mr. Cunningham are numerous, and I trust that he will not think me wanting in courtesy if I make my answer to each of them as short as possible.

(1) I am glad Mr. Cunningham now believes that the fortuitous character of animal variation is in many cases indisputable, so that he no longer holds the view of chance adopted by Eimer and others (*cf.* Eimer, "Organic Evolution," translated by J. T. Cunningham. Macmillan, 1890).

(2) I cannot agree that the question, which the theory of natural selection attempts to answer, is the question "whether a given modification . . . originated accidentally, or as the result of a definite ascertainable cause." Without discussing the conception of an "accident" implied in this phrase, I fail to see that the theory of natural selection involves a theory of the origin of variation: all it asserts is that the variation which is known to occur does affect the death-rate.

(3) The well-known fact, that a change in surrounding conditions often produces a change in the character of a race by methods other than that of selective destruction, does not disprove the co-existence of selective destruction. For example, Mr. Cunningham has not shown that the adaptation of sunflowers to life in six-inch flower-pots is effected without selective destruction; he has only shown that a portion of the change, associated with life in pots is effected without such destruction. By dividing a sample of seed of known origin into two portions, sowing seeds of one portion in a market garden, seeds of the other portion singly in a series of flower-pots, Mr. Cunningham has produced two different series of sunflowers, which differ in stature and in other characters. I fully accept Mr. Cunningham's statement that the plants in the flower-pots were modified without selective destruction. But these plants were not all alike; and unless it can be shown that each of them produced an equal number of seeds, of equal germinating power, so that if life in flower-pots had been continued each plant, whatever its stature, would have contributed an equal number of equally fertile offspring to the next generation,—unless this can be shown, the action of natural selection is by no means disproved. If among the sunflowers of different stature growing in similar flower-pots, plants of one stature produced more seed than plants of different stature, the plants of that stature were better "adapted" to life in flower-pots than the others, and in a struggle to occupy a world filled with six-inch flower-pots, the offspring of the more fertile plants would very probably win; so that a process of natural selection would occur. So far as Mr. Cunningham has described his observations, they do not exclude the possibility that this and other kinds of selection operate. All I am anxious to know, in those cases of organic evolution which I try to understand, is how much of the observed change is due to a process of selective destruction, how much to other causes.

(4) I heartily agree with the view that it is not possible for selection, under fixed conditions, to modify a species in every direction. It is only possible for natural selection to act so as to produce a race with a minimum death-rate. For example, since muddy water of a certain salinity kills broad-fronted crabs more quickly than narrow-fronted crabs, it is probably im-

possible for natural selection to increase the frontal breadth of crabs which live in such water.

(5) In the second part of his letter, Mr. Cunningham attempts an explanation of the evolution observed in Plymouth crabs, which does not involve any selective destruction. For this purpose he makes two hypotheses, one about the growth of crabs, one about the temperature of the sea-shore at Plymouth. Neither of these hypotheses seems to me to fit the facts. If I understand the hypothesis about growth, it is this: that the frontal breadth of a crab depends on its age, while the length of a crab depends not only upon age, but upon temperature and other circumstances affecting it during growth. From this it is deduced that in a group of crabs of the same length, those with narrower fronts are older, those with broader fronts are younger, and I suppose that those with equal fronts are assumed to be of the same age. Therefore, when I say that under certain conditions the crabs with the broadest fronts die first, Mr. Cunningham assumes that under those conditions the youngest crabs die first. I do not know of any published account of the growth of crabs which supports this hypothesis, and the following facts seem to disprove it:—If we take a group of crabs, of the same length and the same frontal breadth, they are, on this view, nearly of an age: if we keep these crabs till they moult, they will grow at different rates during the moult; now those which increase abnormally much in length during the moult, will be younger than average crabs of their new length; those which show abnormally little increase in length, will be older than the majority of crabs of their new length. Mr. Cunningham says that in crabs of a given length, the youngest are the broadest; therefore those crabs which grew abnormally much ought to have broader fronts than their fellows of their new length, those crabs which grew abnormally little ought to be narrower than their fellows. I have worked out the relation between growth-rate and frontal breadth abnormality in more than 500 cases, and the relation which ought to hold, if Mr. Cunningham's hypothesis were true, does not hold.

A further disproof of the contention that the youngest crabs died first in my experiments is this: in most of the experiments about equal numbers of crabs of all lengths from 10 to 15 mm. were treated together; and all crabs used in an experiment were gathered on one day. It will hardly be contended that irregularity of growth goes so far as to produce in the same season crabs between 10 and 11 mm. long which are of the same age as crabs between 14 and 15 mm. long. If the younger crabs died first in my experiments, a mortality of 70 or 80 per cent. might be expected to kill all, or nearly all the shorter crabs, the survivors being derived almost entirely from the longer crabs. This was not the case. For example, in one experiment 200 crabs, between 10 and 15 mm. long, were treated with mud until only four were left alive. These four were respectively 10.67 mm., 11.67 mm., 11.43 mm., and 12.11 mm. long.

(6) Mr. Cunningham further supposes, and no doubt rightly, that crabs grow faster, within certain limits, the warmer the water in which they are; so that crabs 10 mm. long, grown in warm water, are probably younger than crabs 10 mm. long grown in colder water. From observations made on the temperature of the Channel water, he thinks it probable that the crabs measured in 1893 were on the whole younger than those measured in 1895, and those measured this year were oldest of all,—all the crabs being of the same length. The reason for this is that the water in the Channel was exceptionally hot in 1893, and for some time exceptionally cool this year. But the stony beach where these crabs were collected looks due south, and is uncovered for hours daily, when it is often exposed to the direct rays of the sun. I am most unwilling to believe that the temperature on such a beach was lower during the past summer than in 1893. A further point is that crabs gathered in January ought, if Mr. Cunningham's hypothesis were true, to be distinctly narrower than crabs of the same length gathered in August. Crabs gathered last January were narrower than crabs gathered in August 1893, but they were not narrower than crabs gathered last August. So that all Mr. Cunningham's ingenious hypotheses fail to fit the facts.

(7) Mr. Cunningham says that there is no evidence of the entrance of fine mud into the gill-chambers of crabs during life. If he will watch a crab breathing in muddy water, or if he will consult the works of Mr. Garstang and other students of the subject, he will see that he is mistaken. I thought the entrance of such particles into the gill-chamber so well known that I need

not describe experiments (of which I have made plenty) in proof of its occurrence.

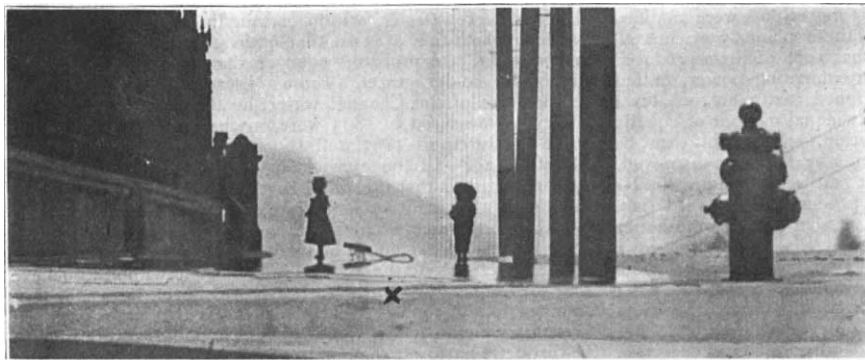
(8) I quite agree with Mr. Cunningham and Mr. Henslow, that it is my duty to describe the effect I believe fine mud to have upon the respiratory apparatus, and I am preparing such a description as quickly as I can. I hope also to be able before long to answer Mr. Cunningham's last and very pertinent question, whether crabs of given length, from the clear water outside Plymouth Sound, are broader or narrower than crabs of the same length from muddy waters within the Sound.

(9) I altogether fail to understand Mr. Henslow's letter, and I fear that my imperfect exposition has led him to misunderstand me as completely as he has misunderstood one of the clearest passages in the "Origin of Species." Mr. Henslow suggests that a variation, fit to afford material for natural selection, must be a *new* character, differing in some mysterious and undefined way from those individual differences which he refuses to call variations; and he further attributes the same view to Mr. Darwin. If Mr. Henslow will read once more the section of the fourth chapter of the "Origin of Species" headed "Illustrations of the Action of Natural Selection, &c.," he will see that Mr. Darwin does not express this opinion. The important thing to determine is not what any man, however eminent, has said about the importance of differences between individual animals, but what that importance can be shown to be. The crabs at Plymouth have not, during the past five years, exhibited any changes in the magnitude of their frontal breadth which Mr. Henslow would rank as a variation, but they have exhibited individual differences. During these five years the mean frontal breadth ratio has changed nearly 2 per cent., so that the change now going on would produce, if it were to continue at the same rate for fifty years, a change big enough to constitute a difference which most men would rank as specific. I have endeavoured to show that this change has been accompanied by a destruction which has acted selectively upon individual differences. Mr. Henslow has not seriously discussed this attempt of mine, but ridicules the idea that so small a change can be of importance in relation to evolution. If the mean stature of Englishmen were to diminish by an inch in a few years, I presume Mr. Henslow would regard such change as rapid and important; but the percentage change would be less than that which Mr. Thompson and I have demonstrated during the past five years in crabs.

W. F. R. WELDON.

Mirage on City Pavements.

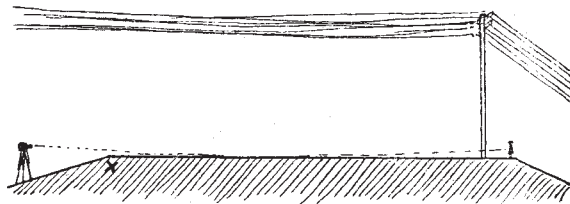
DURING my summer visits to San Francisco, I have been so frequently struck with the beautiful miniature mirages that can be seen on the flagstone sidewalks whenever the sun shines, that I determined to secure, if possible, a photograph of the phenomenon on a scale suitable for reproduction. One or two



previous attempts in past years having been partial failures owing to the smallness of the image, I secured, through the kindness of a friend, the use of a very fine tele-objective capable of giving an image six or eight times as large as an ordinary objective of 12 inches focus. The streets over some of the hills are so laid out that it is possible, on nearing the brow, to bring the eye on the level of the side-walk, and look along a perfectly level stretch of one hundred yards or more. Standing in this position it is almost impossible to resist the conviction that the

walk is flooded with a perfectly smooth sheet of water, in which the reflections of pedestrians can be seen as distinctly as in a mirror.

In order to observe the phenomenon it is necessary that a considerable stretch of level pavement be foreshortened into a very narrow strip. This is the condition in the photograph: the camera stood just below the brow of the hill, and the distance in the photograph from the X to where the children and the toy cart are standing, is an entire block (135 yards). The position of the camera and section of the hill-top are shown in the diagram. The apparent reflections, due to the



bending upward of the rays by the thin layer of heated air, come out very clearly in the picture, but the camera fails to give a correct reproduction of the extreme brilliancy of the reflecting layer of air.

On taking a few steps up the hill, decreasing the foreshortening, the glaze vanishes, and we see only the dull grey of the flagstones. Extremely hot sunshine is not necessary. I have observed the phenomenon early in the morning after a cold night, before the sun had reached the pavement, the slight warmth from the ground being sufficient. Under these conditions, however, the pavement must be more foreshortened than when in the full sunshine. The refracting layer is probably only a thin skin of warm air, which adheres as it were to the surface of the flagstones, for the mirage is unaffected by the strong winds which frequently sweep the top of the hill.

Probably these mirages can be seen on any level pavement where the eye can be brought into the proper position.

Physical Department of the University, R. W. WOOD.
Madison, Wisconsin, September 20.

Transference of Heat in Cooled Metal.

My attention has just been called to two communications to your journal, entitled "Transference of Heat in Cooled Metal." The first, by M. Henry Bourget, appears in the issue of June 30, and the second, by Mr. Albert T. Bartlett, in the issue of September 1.

About the year 1880 I had occasion to heat one end of an iron bar to a bright red heat whilst holding the cooler end in my hand. Upon plunging the heated end into a bucket of water the cooler end became suddenly so hot that I was obliged to release my hold on it.

This phenomenon interested me very much, as I could find no explanation for the apparent reflection of heat to the cooler end of the bar; and in 1888, whilst working in the physical laboratory at Johns Hopkins University, I further investigated the matter.

To one end of an iron or steel bar was soldered a thermo-electric couple, the circuit of which was closed through a very sensitive, high resistance, Rowland, reflecting, galvanometer. The bar was passed through two pasteboard screens, and was supported in a horizontal position, the screens serving to intercept any heat which might be conveyed by radiation or convection through the air from one end of the bar to the other. Under the end of the bar, remote from that to which the thermo-electric couple was soldered, was placed a compound bunsen burner, by which the end of the bar was raised to a dull red heat. The spot of light on the galvanometric scale