classical termination, become monosyllabic, such as "thallus," "sorus," "hypha," and "ascus," just as we still speak of a "corolla," a "stigma," a "hilum," and a "raphe." But, with regard to the great majority of terms in current use in descriptive cryptogamic botany, we entertain not the smallest doubt that the change will gradually be brought about which has, within the last forty years, become established in phanerogamic botany; and we would venture to suggest to our fellow-workers in cryptogamic botany in this country and in America, whether it will not be best to accept it frankly once for all.

ALFRED W. BENNETT.

Exact Thermometry.

I AM quite in agreement with Prof. Sydney Young (NATURE, December 19, p. 152), that after the lapse of a sufficient time—let us say, an infinite time—the constant slow rise of the zero-point of a thermometer at the ordinary temperature will attain a definite limit; but I cannot accept his view that the effect of heating the thermometer to a high temperature is simply to increase the rate at which *this* final state is approached. If the results of experiment at the ordinary temperature be expressed in a mathematical formula which admits of making the time infinite, the limiting value of the rise (on that condition) will not exceed on the average 2° C., even in a thermometer of lead glass. After exposure to a high temperature, and in the same thermometer, so great an ascent as 18° C. is a possible measurement, actually realized. The two phenomena are therefore very different in their nature.

The view that, owing to the more rapid cooling of the outer parts of the bulb after it has been blown, the inner parts are in a state of tension, and that it is the gradual equalization of the tension throughout the glass that causes the contraction, has frequently been held, and will probably be for a long time the favourite hypothesis upon the subject. It breaks down, however, when we attempt to calculate what the amount of the contraction might be, on the supposition that it is well founded: only a very small portion of the contraction could be thus accounted for. I regret that I cannot now conveniently refer to Guillaume's interesting demonstration of this result.

Prof. Young has placed on record an experiment with three thermometers, which he heated to 280° C. The zero movement, however, only ranged from 1° to 1°·2,—small readings which might very possibly have been obtained, or not, on either of the thermometers at other times. It is consequently very difficult to draw any inference from this experiment. I may, however, mention that closed thermometers made of lead glass are very apt to show a rise of zero after heating to about 120° C. and upwards to some temperature in the neighbourhood of 270° C., and after that a descent of zero; the temperature of 280° C. would in that case be an unsatisfactory one for a test experiment, and the effect of plasticity might very possibly be masked. On the other hand, if the three thermometers were of hard glass, all the zero movements would in that case be greatly diminished, and the results would be in less bold relief.

I do not know any substance more curious or interesting in its properties than glass; and I should be glad if Prof. Young—into whose able hands the matter has fallen—could decisively test my suggestion that plasticity is the main cause of the zero ascent after 120° C. Probably it has little or nothing to do with the ascent at the ordinary temperature. It is, however, known that fine threads of glass are undoubtedly plastic at the ordinary temperature.

EDMUND J. MILLS.

Melrose, N.B., December 29, 1889.

THE PALÆONTOLOGICAL EVIDENCE FOR THE TRANSMISSION OF ACQUIRED CHARACTERS.[1]

MUCH of the evidence brought forward in France and Germany in support of the transmission of acquired characters, which has been so ably criticized in Weismann's recent essays, is of a very different order from that forming the main position of the so-called Neo-Lamarckians in America. It is true that most American zoologists, somewhat upon Semper's lines, have supported the theory of the direct action of environment, always assuming, however, the question of transmission. But Cope, the able if somewhat extreme advocate of these views, with Hyatt, Ryder, Brooks, Dall, and others, holding that the survival of the fittest is now amply demonstrated, submit that, in our present need of an explanation of the origin of the fittest, the principle of selection is inadequate, and have brought forward and discussed the evidence for the inherited modifications produced by reactions in the organism itself—in other words, the indirect action of environment. The supposed arguments from pathology and mutilations have not been considered at all: these would involve the immediate inheritance of characters impressed upon the organism and not springing from internal reactions, and thus differ both in the element of time and in their essential principle from the above. As the selection principle is allowed all that Darwin claimed for it in his later writings, this school stands for Lamarckism *plus*—not *versus*—Darwinism, as Lankester has recently put it. There is naturally a diversity of opinion as to how far each of these principles is operative, not that they conflict.

The following views are adopted from those held by Cope and others, so far as they conform to my own observations and apply to the class of variations which come within the range of palæontological evidence. In the life of the individual, adaptation is increased by local and general metatrophic changes, of necessity correlated, which take place most rapidly in the regions of least perfect adaptation, since here the reactions are greatest; the main trend of variation is determined by the slow transmission, not of the full increase of adaptation, but of the disposition to adaptive atrophy or hypertrophy at certain points; the variations thus transmitted are accumulated by the selection of the individuals in which they are most marked and by the extinction of inadaptive varieties or species: selection is thus of the *ensemble* of new and modified characters. Finally, there is sufficient palæontological and morphological evidence that acquired characters, in the above limited sense, are transmitted.

In the present state of discussion, everything turns upon the last proposition. While we freely admit that transmission has been generally assumed, a mass of direct evidence for this assumption has nevertheless been accumulating, chiefly in the field of palæontology. This has evidently not reached Prof. Weismann, for no one could show a fairer controversial spirit, when he states repeatedly: "Not a single fact hitherto brought forward can be accepted as proof of the assumption." It is, of course, possible for a number of writers to fall together into a false line of reasoning from certain facts; it must, however, be pointed out that we are now deciding between two alternatives only, viz. pure selection, and selection *plus* transmission.

The distinctive feature of our rich palæontological evidence is that it covers the entire pedigree of variations: we are present not only at but before birth, so to speak. Among many examples, I shall select here only a single illustration from the mammalian series—the evolution of the molar teeth associated with the peculiar evolution of the feet in the horses. The feet, starting with plantigrade bear-like forms, present a continous series of readjustments of the twenty-six original elements to digitigradism which furnish proof sufficient to the Lamarckian. But, as selectionists would explain this complex development and reduction by panmixia and the selection of favourable fortuitous correlations of elements already present, the teeth render us more direct service in this discussion, since they furnish not only the most intricate correlations and readjustments, but the complete history of the addition

[1] This article is an informal reply to the position taken by Prof. Weismann in his essays upon heredity. I have borrowed freely from the materials of Cope, Ryder, and others, without thinking it necessary to give acknowledgment in each case.

of a number of entirely new elements—*the rise of useful structures from their minute embryonic, apparently useless, condition*, the most vulnerable point in the pure selection theory. Here are opportunities we have never enjoyed before in the study of the variation problem.

The first undoubted ancestor of the horse is *Hyracotherium;* let us look back into the early history of its multicuspid upper molars, every step of which is now known. Upon the probability that mammalian teeth were developed from the reptilian type, Cope predicted in 1871 that the first accessory cusps would be found on the anterior and posterior slopes of a single cone, *i.e.* at the points of interference of an isognathous series in closing the jaws. Much later I showed that precisely this condition is filled in the unique molars of the Upper Triassic *Dromotherium.* These with the main cusp form the three elements of the tritubercular crown. Passing by several well-known stages, we reach one in which the heel of the lower molars intersects, and, by wearing, produces depressions in the transverse ridges of the upper molars. At these points are developed the intermediate tubercles which play so important a *rôle* in the history of the Ungulate molars. So, without a doubt, every one of the five main component cusps superadded to the original cones, is first prophesied by a point of extreme wear, replaced by a minute tubercle, and grows into a cusp. The most worn teeth, *i.e.* the first true molars, are those in which these processes take place most rapidly. We compare hundreds of specimens of related species; everywhere we find the same variations at the same stages, differing only in size, never in position. We extend the comparison to a widely separate phylum, and find the same pattern in a similar process of evolution. Excepting in two or three side lines the teeth of all the Mammalia have passed through closely parallel early stages of evolution, enabling us to formulate a law: *The new main elements of the crown make their appearance at the first points of contact and chief points of wear of the teeth in preceding periods.* Whatever may be true of spontaneous variations in other parts of the organism, these new cusps arise in the perfectly definite lines of growth. Now, upon the hypothesis that the modifications induced in the organism by use and disuse have no directive influence upon variations, all these instances of sequence must be considered coincidences. If there is no causal relationship, what other meaning can this sequence have? Even if useful new adjustments of elements already existing may arise independently of use, why should the origin of new elements conform to this law? Granting the possibility that the struggle for existence is so intense that a minute new cusp will be selected if it happens to arise at the right point, where are the non-selected new elements, the experimental failures of Nature? We do not find them. Palæontology has, indeed, nothing to say upon individual selection, but chapters upon unsuccessful species and genera. Here is a practical confirmation of many of the most forcible theoretical objections which have been urged against the selection theory.

Now, after observing these principles operating in the teeth, look at the question enlarged by the evolution of parallel species of the horse series in America and Europe, and add to the development of the teeth what is observed in progress in the feet. Here is the problem of correlation in a stronger form even than that presented by Spencer and Romanes. To vary the mode of statement, what must be assumed in the strict application of the selection theory? (*a*) that variations in the lower molars correlated with coincident variations of reversed patterns in the upper molars, these with metamorphoses in the premolars and pocketing of the incisor enamel; (*b*) all new elements and forms at first so minute as to be barely visible immediately selected and accumulated; (*c*) in the same individuals favourable variations in the proportions of the digits involving readjustments in the entire limbs and skeleton, all coincident with those in the teeth; (*d*) finally, all the above new variations, correlations and readjustments, not found in the hereditary germ-plasm of one period, but arising fortuitously by the union of different strains, observed to occur simultaneously and to be selected at the same rate in the species of the Rocky Mountains, the Thames Valley, and Switzerland! These assumptions, if anything, are understated. Any one of them seems to introduce the element of the inconstant, whereas in the marvellous parallelism, even to minute teeth markings and osteological characters, in all the widely distributed forms between *Hyracotherium* and *Equus*, the most striking feature is the constant. Viewed as a whole, this evolution is one of uniform and uninterrupted progression, taking place simultaneously in all the details of structure over great areas. So nearly does race adaptation seem to conform to the laws of progressive adaptation in the individual, that, endowing the teeth with the power of immediate reactive growth like that of the skeleton, we can conceive the transformation of a single individual from the Eocene five-toed bunodont into the modern horse.

The special application of the Lamarckian theory to the evolution of the teeth is not without its difficulties, some of which have been pointed out to me by Mr. E. B. Poulton. To the objection that the teeth are formed before piercing the gum, and the wear produces a loss of tissue, it may be replied that it is not the growth, but the reaction which produces it, which is supposed to be transmitted. Again, this is said to prove too much; why is the growth of these cusps not continuous? This may be met in several ways: first, in the organism itself these reactions are least in the best adapted structures, a proposition which is more readily demonstrated in the feet than in the teeth—moreover, since the resulting growth never exceeds the uses of the individual, there is a natural limit to its transmission; secondly, the growth of the molars is limited by the nutritive supply—we observe one tooth or part growing at the expense of another; third, in some phyla we do observe growth which appears to lead to inadaptation and is followed by extinction. In one instance we observe the recession of one cusp taking place *pari passu* with the development of the one opposed to it. These and many more general objections may be removed later, but they are of such force that, even granting our own premises, we cannot now claim to offer a perfectly satisfactory explanation of all the facts.

The evidence in this field for, is still much stronger than that against, this theory. To sum up, the new variations in the skeleton and teeth of the fossil series are observed to have a definite direction; in seeking an explanation of this direction, we observe that it universally conforms to the reactions produced in the individual by the laws of growth; we infer that these reactions are transmitted. If the individual is the mere pendent of a chain (Galton), or upshoot from the continuous root of ancestral plasm (Weismann), we are left at present with no explanation of this well-observed definite direction. But how can this transmission take place? If, from the evident necessity of a working theory of heredity, the *onus probandi* falls upon the Lamarckian—if it be demonstrated that this transmission does not take place—then we are driven to the necessity of postulating some as yet unknown factor in evolution to explain these purposive or directive laws in variation, for, in this field at least, the old view of the random introduction and selection of new characters must be abandoned, not only upon theoretical grounds, but upon actual observation.

Reading between the lines of Weismann's deeply interesting essays, it is evident that he himself is coming to this conclusion.

HENRY FAIRFIELD OSBORN.
Princeton College, August 23.