THURSDAY, APRIL 16, 1896.

OLD AND NEW THEORIES OF EVOLUTION

The Primary Factors of Organic Evolution. By E. D. Cope, Ph.D., Professor of Zoology and Comparative Anatomy in the University of Pennsylvania. Pp. xvi + 532. (Chicago : The Open Court Publishing Company, 1896.)

The Present Evolution of Man. By G. Archdall Reid. Pp. 370. (London : Chapman and Hall, Limited, 1896.)

PROF. COPE tells us in his preface that his work may be regarded as containing a plea on behalf of the Lamarckian view of the factors of evolution ; and he believes that evidence has now been accumulated to demonstrate the doctrine, which, he says, he has defended as a working hypothesis for twenty-five years. At p. 9 of the introduction, he states, referring to one of his own papers : " By the discovery of the palæontologic succession of modifications of the articulations of the vertebrate, and especially mammalian skeleton, I first furnished an actual demonstration of the reality of the Lamarckian factor of use, or motion, as friction, impact, and strain, as an efficient cause of evolution." Such statements as these lead the reader to expect that at last we shall have something of the nature of proof of the inheritance of acquired characters, and that the difficulties and objections of those who hold Weismann's views will be fairly met and satisfactorily answered.

The work is divided into three parts, headed respectively : "The Nature of Variation," "The Causes of Variation," and "The Inheritance of Variation." The first part deals with variation, phylogeny, parallelism, and catagenesis. Very full accounts are given of the varieties that occur in some of the animals inhabiting the United States, with special reference to climatal conditions. Aridity is said to produce a blanching of colours, while moisture produces intensity. Some groups increase in size as they spread southward, others towards the north, and this is connected with the centre of the area of distribution being in the south or the north. We have also a careful description of the progressive development of several important groups as indicated by their fossil remains, with a general outline of the phylogeny of the mammalia ; while the chapter on parallelism deals with the general correspondence between the course of development of the individual and of the class or order to which it belongs.

At the end of this part of the volume, which has been purely descriptive and has entirely avoided any reference to natural selection or to the broader features of variation, we find this extraordinary statement.

"It has been proved, as it appears to me, that the variation which has resulted in evolution has not been multifarious or promiscuous, but in definite directions. It has been shown that phylogeny exhibits a progressive advance along certain main lines, instead of having been indefinite and multifarious in direction."

Of these two statements the latter is true, and has been fairly proved by the facts which have been set forth ; while the former is absolutely untrue, and if the facts which this volume sets before the student do not show it to be untrue, it is only because they have been selected and set forth in such a way as to illustrate the theory that variations are in definite directions only. For example, although Dr. J. A. Allen is quoted largely to show the variations of birds in definite directions in accordance with changes of climate, nothing is said of his more important work on "The Mammals and Winter Birds of Florida," in which he has given detailed measurements showing that all the commoner species do exhibit "multifarious variations" which are also "multifarious and indefinite in direction." He shows that the total length, as well as the length of the wing, the tail, the beak, and the feet, all vary simultaneously but to a large extent independently. Further, he shows that each of the primary wing feathers, and each of the toes also vary simultaneously and to a large extent independently. Other writers have shown that in mammalia the skull and all its parts vary simultaneously; while what is known of the variation of the muscles, the nerves, the blood-vessels, the intestines, and other internal organs, show that these even exceed the external organs in the multifarious and indefinite character of their variations. All this is the common knowledge of every biologist ; yet we have a great authority and experienced biological teacher, first omitting all reference to these facts, and then declaring that he has *proved* that they do not exist !

Coming now to the second division of the book, we find abundant evidence as to the changes effected in individuals by the action of various external causes, by far the larger portion being devoted to a statement of the supposed mechanical origin of the peculiar forms of the teeth and bones in the vertebrata, illustrated by the various lines of evolution made known by palæontology, and always assumed to be the result of use (or disuse) and motion. Then follows a short chapter on natural selection, which is described in the most cursory manner, almost immediately diverging to sexual selection, to which more space is given. We then have a single paragraph devoted to protective modifications of colour or form ; and the author here takes the opportunity of dealing a blow at the Darwinians by first misstating their views, and then demolishing his own misstatement. He says :

"Much is to be found of interest on this attractive subject in the writings of Wallace, Poulton, Beddard, and others. The two authors first named ascribe these colour and form characters to natural selection *as a cause*. This is, however, impossible ; yet natural selection has undoubtedly been the cause of their survival."

The italics are Prof. Cope's. He then goes on :

"The first objection to the belief that natural selection is the primary cause of organic evolution has already been stated as follows : 'A selection cannot be the cause of those alternatives from which it selects. The alternatives must be presented before the selection can commence.' But the supporters of the view that natural selection is the origin of variation, allege that it produces this result by the continual survival of minute differences which are useful, thus accumulating variation. That minute advantageous differences will secure survival no one can doubt, but it must be remembered that the variations which constitute evolution have been in a vast number of cases too minute

to be useful. But the general question is not affected by the supposition that advantageous variations may be sometimes minute. Minute or great, they have to be assumed in the argument for selection; and whether minute or great, they have a definite cause."

This very ingenious argument is well calculated to impress those readers of Prof. Cope's book who have no other sources of information that natural selection is quite a subsidiary agent in causing evolution, and that—as he says in his concluding paragraph—"the stimuli of chemical and physical forces and also molar motion, or use, or its absence, are abundantly sufficient to produce variations of all kinds in organic beings."

But they can produce this effect only on the assumption that all the modifications so produced in individuals are, partially at least, transmitted to their offspring; while those very numerous cases in which essential characters could not possibly have been produced by the causes he suggests, are entirely unnoticed. Such are all those curious structures which are only used once in a lifetime; those whose only function is to alarm enemies; most of the protective forms, motions, and colours of insects, as illustrated in the stick and leaf insects, or in those which are deceptively like moss, or flowers, or the dung of birds; the poison-fangs of snakes and the stench-glands of skunks, and innumerable other examples which will occur to every naturalist. Note, too, how "*as a cause*" in the first quotation is changed immediately afterwards to "the primary cause," and the implication that we believe natural selection to be the "origin of variation" and "the cause of the alternatives from which it selects," a theory for which Prof. Cope never states his authority, and which, so far as I know, has never been even suggested, except by incompetent or careless reviewers. Strange to say we have the acknowledgment that "minute advantageous differences will secure survival," but it is followed by the proviso that "the variations which constitute evolution have been in a vast number of cases too minute to be useful." This, I suppose, means that the changes produced by external causes in the individual are too minute to be useful till transmitted and accumulated by inheritance. Whether that is so or not, no evidence whatever has been adduced; while abundant evidence exists in the works of Prof. Cope's own countrymen, and in the measurement of many hundreds of specimens of common species in this country, that normal variability is *not* minute but very large, and that this variability extends to every part and structure, and to every external and internal organ when search has been made for it. That such well-known facts as these should be entirely ignored, and the extraordinary and wholly unprovable statement made, that the variations which constitute evolution "have been in a vast number of cases too minute to be useful," seems to show that the advocates of Neo-Lamarckism feel that they have a very bad case.

In the third part, on the inheritance of variation, we expect to find some experimental facts bearing on the question at issue. But I can only find assumptions and opinions. Breeders of animals, it is said, all believe in the inheritance of the results of nutrition and exercise, and pages are given to prove such beliefs; and after describing the evolution of the American trotting-horse, Prof. Cope says:

"Viewed as phenomena, there is every appearance and indication that the changes acquired by individuals through the exercise of function have been to some degree transmitted, and have been cumulative, and that this has been one factor in the evolution of speed."

However unsatisfactory is the author's treatment of the evidence for the doctrine which forms the main subject-matter of the book, we did not expect that he would repeat the absurd argument which Lord Salisbury used at Oxford, and which has been so destructively criticised by Herbert Spencer. Yet in the chapter on "The Energy of Evolution" he gives, among the "weighty considerations" showing that natural selection cannot be the cause of the origin of new characters, the following statement:

(3) "In order that a variation of structure shall survive, it is necessary that it shall appear simultaneously in two individuals of opposite sex. But if the chance of its appearing in one individual is very small, the chance of its appearing in two individuals is very much smaller. But even this concurrence of chances would not be sufficient to secure its survival, since it would be immediately bred out by the immensely preponderant number of individuals which should not possess the variation."

Whence of course it follows, that without the Lamarckian factors to produce the right variations at the right time, natural selection is powerless, as it will have nothing to select from! It really seems incredible that after nearly forty years' discussion of evolution and natural selection such an argument as that here quoted can be set forth in a serious book by a life-long teacher and worker in the field of biology.

It is refreshing to turn to Mr. Archdall Reid's volume which, though unnecessarily diffuse, is full of original ideas and acute reasoning. The larger part of it is devoted to a discussion of the general subject of organic evolution. This is exceedingly well done, and it contains a very forcible argument against the possibility of the inheritance of acquired characters in the higher animals, derived from the facts of cell-division and specialisation in the development of the individual. This argument has not, within my knowledge, been so clearly and forcibly set forth by any other writer. There are also some very acute criticisms of the writings of Herbert Spencer and others on evolution, and great stress is laid on a rather neglected subject, the development of acquired characters during the growth of the individual, though on this point the author's views seem rather exaggerated and open to criticism. The latter portion of the book, which gives the title to the work, though original is somewhat disappointing, as it is entirely limited to evolution against disease. The author argues that this is effected solely by natural selection, and in the facts presented by the various amounts of resistance of different races to certain zymotic diseases he finds another powerful argument against the Lamarckian theory. He maintains that there is no such thing as hereditary disease, but only hereditary tendency to contract the disease. He traces most of the zymotic diseases to the unhealthy crowding that is universal in civilised communities, and he has some very strong remarks on the way in which our false civilisation is exterminating so many of the lower races. One of

these passages may be quoted as a fair example both of the author's style and of the interesting subjects he discusses.

"Are not all our efforts, whether prompted by philanthropic or religious zeal, by which we seek to protect and preserve the aboriginal races of the New World, wholly mistaken? Are they not in effect absolutely murderous? We gather them into close school-rooms and churches, where teachers and missionaries speak to them from infected lungs. We endeavour to persuade them to abandon their nomadic habits and form settled communities. We—and thereby we prove our own barbarism, the imperfection of our own civilisation—force them in climates where clothes are wholly unnecessary, and therefore a species of dirt, to wear clothes, than which a better vehicle for air and earth-borne disease cannot be well conceived. In fact we strive to bring them at one bound into that state of society which has become possible to us only at the cost of tens of millions of lives during thousands of years."

There are a few errors and perhaps some fallacies in this very interesting and well-written volume; but much may be forgiven in a book that is both original and suggestive; while in its thorough-going advocacy of the main doctrine of Weismann—the non-inheritance of acquired characters—it affords an excellent antidote to the elaborate but one-sided arguments of Prof. Cope.

ALFRED R. WALLACE.

THE ATOMIC THEORY AGAIN.

A New View of the Origin of Dalton's Atomic Theory: a Contribution to Chemical History, &c. By Henry E. Roscoe and Arthur Harden. Pp. 191. (London: Macmillan and Co., 1896.)

La Théorie Atomique et la Théorie Dualistique. Transformation des formules. Différences Essentielles entre les deux théories. Par E. Lenoble, Professeur de Chimie a l'Université libre de Lille. Pp. 94. (Paris: Gauthier-Villars.)

THE origin of the former of these two books is well explained in the following passage from the short introduction:

"It may seem remarkable that after the lapse of nearly a century since John Dalton first applied the atomic theory of matter to chemical phenomena, it should be possible to find anything new respecting the genesis of his ideas. And this is the more remarkable when we remember that the life and scientific labours of the great Manchester chemist have formed the subject of independent memoirs at the hands of two such able contemporaries as Charles Henry and Angus Smith. The explanation is to be found in the unlooked-for discovery, in the rooms of the Literary and Philosophical Society of Manchester, where the whole of Dalton's experimental work was carried out, of his laboratory and lecture note-books contained in a number of manuscript volumes. A careful study of these has led us to conclusions concerning the origin of the atomic theory of chemistry which differ widely from those which have been generally accepted. It has hitherto been supposed that it was the experimental discovery of the law of combination in multiple proportions which led Dalton, seeking for an explanation of this most remarkable fact, to the idea that chemical combination consists in the approximation of atoms of definite and characteristic weight, the atomic theory being thus adopted to explain the facts ascertained by chemical analysis. This prevailing view is found on examination to rest upon the authority of contemporary chemists, rather than on any explicit statement on the part of the author himself; for, strange as it may appear, no attempt to explain the genesis of his ideas is to be found in any of Dalton's published writings."

It now appears that Dalton was probably led to his theory by an attempt to apply the Newtonian doctrine of the atomic constitution of matter to the explanation of the physical properties of gases, and more especially to the case of the gases present in atmospheric air.

The evidence upon which this conclusion is based is derived partly from the newly-discovered manuscript notes of a course of lectures given by Dalton at the Royal Institution in London early in 1810. In the course of these he says that it was the consideration of the constitution of mixed elastic fluids which led him to contemplate the effect of differences of size in the particles, and thus "it became an object to determine the relative *sizes* and *weights*, together with the relative *numbers* of atoms in a given volume. This led the way to the combinations of gases, and to the *number* of atoms entering into such combinations. . . . Thus a train of investigation was laid for determining the *number* and *weight* of all chemical elementary principles which enter into any sort of combination one with another." This is a statement of Dalton's own recollection of the course of events after the lapse of seven or eight years from the time when he made his first attempts at estimating atomic weights. To this must be added the fact that the first part of his "New System of Chemical Philosophy," published in 1808, contains no account of any chemical analyses, and in the short chapter on chemical synthesis, at the end of this first part, the author speaks of the application of certain general rules which he lays down "to the chemical facts already well ascertained," the experiments conducted by himself being reserved for part ii., published two and a half years later.

On the other hand, Dr. Thomas Thomson, after a visit to Dalton in 1804, makes the very definite statement upon which chemists have generally relied. He says: "Mr. Dalton informed me that the atomic theory first occurred to him during his investigations of olefiant gas and carburetted hydrogen gas." If this was the impression carried away by an interested visitor at the time when Dalton was occupied by the earlier stages of his investigations, it is impossible to avoid the conclusion that there was some foundation for it. Dalton was occupied with the idea of atoms, their relative sizes, &c., from 1801. In the summer of 1804 he collected and analysed the gas from ponds ("System Chem. Phil.," p. 445). In 1805, he says (MS. Lecture 17, p. 16 in the book) the idea occurred to him that the sizes of the particles of elastic fluids *must* be different. We cannot, therefore, admit that the authors have fully made out their case, though it does appear probable that the idea of atomic structure was growing in Dalton's mind before he made any chemical analyses for himself; but whether it had taken the final definite shape in which it appears in the notes of the lectures at the Royal Institution, and in the "New System of Chemical Philosophy," appears to us to be still open to question.

The second book on our list is a production of wholly different type. This little volume explains how to trans-