THURSDAY, OCTOBER 4, 1894.

ANOTHER SUBSTITUTE FOR DARWINISM.

Nature's Method in the Evolution of Life. (London: T. Fisher Unwin, 1894.)

ALMOST every educated man who can write good English, but who cannot understand Darwin's theory of Natural Selection, seems to feel compelled to explain his difficulties and to offer his own preferable theory in the form of a volume on Evolution. We are thankful that the present anonymous volume is a small one; but that is its chief, if not its only merit. The writer has not, in the first place, made any serious attempt to understand the theory he objects to as inadequate; and, in the second place, his own theory is so vague and so entirely unsupported by either fact or argument as to be altogether worthless. A few extracts from the book will serve to support both these statements.

In the first chapters discussing the Darwinian theory we have this statement:—

"Deviations, although minute, tend, it is alleged, to accumulate, and the accumulations over prolonged periods of time ultimately produce variations from the original type, sufficient to constitute new species." (p. 10.)

Of course no such "tendency" was ever alleged by Darwin. The difference in size between the Shetland pony and the dray-horse is said to be due to difference of climate and food—

"There is no reason to doubt that the size of the former is due to an unfavourable climate and insufficient quantity and quality of food, and that of the latter to comfort combined with a generous diet."

But he ignores the case of the lap-dog and Italian greyhound on the one hand, and the Dingo or Esquimaux dog on the other, where the same contrasted conditions have apparently acted in a manner precisely opposite. Again, he seems to think that the struggle for existence is only the struggle for food, and that such a struggle must cause deterioration. He supposes the case of rabbits on a small island, and says—

"The rabbits possessing the strongest vitality and able to live on the smallest quantity of food, will have proved themselves the fittest. . . . But have the rabbits of the highest type come through the struggle unscathed? Have the fittest of the survivors become fitter to continue the conflict than the rabbits that were fittest when the conflict began? If so, it would follow that scarcity of food is more favourable to animal life than abundance." (p. 28.)

Here he clearly falls into confusion through some idea of abstract "fitness"—fitness independent of the conditions of existence, as shown by his statement on the next page that the struggle for existence "is evidently inimical to beneficial variation." Again (p. 31) he asks: "Is there any ground for believing that excessive use develops beneficial variation?" showing that he entirely misunderstands the theory of the natural selection of individual variations.

This misconception is further shown by quoting the inability of the ostrich to fly as an example of "the failure of natural selection"; and as a still more glaring example of this failure he refers to the curious Chaparral Cock of California, a ground cuckoo which lives in the open woodlands, runs very quickly, but rarely flies. The alleged "failure" is supposed to exist because the mounted cowboys catch the bird with their whips, and it does not escape by flying! It never seems to have occurred to this writer that both these birds are striking examples of the success of natural selection, since they have both become well adapted to a terrestrial life, as shown by their abundance in individuals. The notion seems to be that every bird which cannot fly as well as a swallow or a falcon must be a failure. Yet on the author's own theory, which, as we shall see, is a modified form of special creation, the failure, if it existed, would be even more deplorable.

This theory, which he calls "Nature's Law of Selection," is thus defined—

"What, for want of a better term, we call the progress of species, is not evolving a new organism out of one previously existing, but by substituting another more closely adapted to the conditions." (p. 62.)

How this other one is substituted is a mystery which is but imperfectly explained further on, in a chapter on "The Method of Evolution," in which we are told that—

"Every organism is the product of a particular combination of force acting on matter according to certain fixed laws, and that the same combination of force, united with matter, has a constant and persistent individuality, which is reproductive."

And this enigmatical proposition is supposed to be made clearer by the next sentence.

"As there are elemental substances, so there may be elemental forces possessing special qualities and affinities, which may have, from time to time, as conditions became favourable, combined with each other to work out evolution." (p. 67.)

If the former statement was obscure, this latter statement, of what "may be" and "may have," renders that obscurity perceptibly greater. Then follow several pages about the Power Loom as compared with the Loom of Life, after which we have a further statement of how the different life forces have acted successively on the simple cell "embodying the first vital force," and thus developed the various organisms. (p. 71.) In order to give us a concrete example of the theory at work, we have this account of the origin of the whale, and the author may well be complimented on his courage in attacking so difficult a problem which almost brought Darwin himself to grief. But a greater than Darwin is here. Read and wonder.

"According to our theory, the life force of the whale proceeds to fashion its skeleton on the type of its terrestrial antecessor, and builds the structure to the junction of the antecedent form with the new, and somewhat beyond the first point of differentiation between them. The bones of the hind limbs begin to be formed, but forthwith the new force special to the whale, coming into play, supersedes the forces that would have completed the antecedent type, and the whale is produced."

That is how it was done! For brilliancy of invention and clearness of exposition this is only comparable with that fascinating account, by Adrianus Tollius, of the origin of stone implements by natural causes, as quoted by Mr. Tylor.

"He gives drawings of some ordinary stone axes and hammers, and tells how the naturalists say that they are

generated in the sky by a fulgureous exhalation conglobed in a cloud by the circumfixed humour, and are as it were baked hard by intense heat, and the weapon becomes pointed by the damp mixed with it flying from the dry part, and leaving the other end denser, but the exhalations press it so hard that it breaks out through the cloud, and makes thunder and lightning. But, he says, if this be really the way in which they are generated, it is odd that they are not round, and that they have holes through them, and those holes not equal through, but widest at the ends. It is hardly to be believed he thinks."[1]

Here we have an example of a brilliant and comprehensive theory—a theory able to explain everything, yet subject to petty criticism! And we fear that our anonymous author's equally brilliant theory of the orgin of the whale will be not less unfortunate. Of course we are assured that the theory explains almost everything—homology, embryology, rudimentary organs, &c., though he does, modestly, admit that it does *not* explain why hybrids are sterile. In order not to misrepresent the writer one more passage must be quoted, because he there brings his ideas more nearly into accord with that theory of discontinuous variation which has been recently put forward.

"Evolution proceeded by successive distinct gradations or stages. The differentiation of every new species resulted from forces *ab extra* superimposed on, and, to some extent, superseding or modifying the forces that produced the species or genus immediately preceding in the same line of development. The fecundated ovum of a species was, as it were, fecundated a second time with a new force, and the ovum thus bi-fecundated produced, instead of the species to which it belonged, a new species built upon a modification of its predecessor."

The theory is therefore one of special creation through the ordinary process of descent. The "new forces *ab extra*" which produced a whale from a terrestrial animal were also at work every time one species of tit, or warbler, or beetle, or snail, was modified in adaptation to a slightly different mode of life, and became a new species. Thus all is explained; except why there is any variation of these specially adapted species, why they increase at such an enormous rate necessitating such wholesale destruction, why there is any struggle for existence. All these phenomena, which are the very essence of a theory of descent with modification by natural selection, are entirely out of place in a theory of special creation, and are therefore the condemnation of any such theories. ALFRED R. WALLACE.

THE MEAN DENSITY OF THE EARTH.

The Mean Density of the Earth. An Essay to which the Adams Prize was adjudged in 1893 *in the University of Cambridge.* By J. H. Poynting, Sc.D., F.R.S. (London: C. Griffin and Co., Limited, 1894.)

THIS essay, which contains an account of Prof. Poynting's well-known investigation of the mean density of the earth, though the last Adams prize essay, is the first to which that prize has been awarded for experimental work. We hope that it is the first of a long series of essays in which the candidates will attack the questions proposed by experiment as well as by mathematical analysis. We can hardly expect, however, that the level reached by the magnificent experimental work of Prof. Poynting will always be maintained.

The essay consists of two parts, the first containing an account of previous determinations of the mean density, the second an account of Prof. Poynting's own determination by means of the ordinary balance.

The first part begins with an account of the astronomical or geodetical methods, in which the attraction of a mountain was compared with that of the earth, as in the experiments of Bouguer in Peru, of Maskelyne and Hutton on Schehallien, of James and Clark on Andrews Seat, of Carlini on Mount Cenis, and of Mendenhall on Fujiyama; or with that of the slab of matter above the surface of a mine as in Airy's Harton Pit experiments, and von Sterneck's experiments in Pribram and Freiberg The beautiful method employed by von Sterneck in his pendulum experiments ought to be more widely known in England. The object of the astronomical method has undergone a curious reversal. It was originally to deduce the mass of the earth from a supposed knowledge of the distribution of matter in the locality of the experiment, whereas now it is rather to find the distribution of matter in this locality, assuming the mass of the earth to be known.

The other methods are laboratory methods, and depend upon the measurement of the attraction between known masses. Prof. Poynting points out a very interesting under-estimate of this attraction made by Newton. In the Principia, Newton estimated that two spheres of the density of the earth, each a foot in diameter, would, if separated by quarter of an inch and left to their own attractions, take nearly a month to come into contact. Prof. Poynting shows that there is a mistake in the arithmetic, and that in reality the spheres would come into contact in between five and six minutes.

It is now very nearly a century since the first measurements of the attraction between two masses in a laboratory were published by Cavendish ("Experiments to Determine the Density of the Earth," *Phil. Trans.* 1798), who used the torsion balance. Since then this method has been used by Reich, Baily, Cornu and Baille, and Boys; while the ordinary balance has been used by von Jolly, Prof. Poynting himself, and by König, Richarz and Krigar Menzel, working in collaboration, while the method of the pendulum balance has been used by Wilsing. The labour expended over these investigations may be estimated from the fact that, to take only two modern instances, Prof. Poynting's experiments extended over twelve years, while those of Cornu and Baille were commenced in 1870, and are not yet completed. The essay contains a clear and critical account of the preceding experiments. The result of the criticism is to raise, if possible, Cavendish's fame as an experimenter. Of Baily's laborious research, Prof. Poynting says: "The critical examination it has received in later years has entirely destroyed any confidence in the result. It remains, however, as a most remarkable and useful example of the danger of substituting multiplication of observations for consistency." The contrast between the amount of work which has been published on the numerical magnitude of the attraction, with that which

[1] "Early History of Mankind," second edition, p. 227.