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HEREDITY.

Heredity. By Prof. J. Arthur Thomson. Pp. xvi+605. (London: John Murray, 1908.) Price 9s. net.

WE all know books on science which we ought to read with pleasure, but to which we turn with shrinking. Full, perhaps, of new facts and ideas, they are so expressed as to bore consumedly. "Heredity" belongs to another category. He who runs may read, even if he be a beginner, and he who reads will probably not cease to run until he has traversed the last page. It contains nothing very new, but most of the facts on which we found our notions of heredity are set out lucidly and in orderly array, as are almost all the theories ever based on them. It is dedicated to "Francis Galton and August Weismann, whose magistral studies on heredity have made us all their debtors." Prof. Thomson is very loyal to the masters.

The outstanding feature of the book is its great, perhaps its excessive, kindliness and toleration. Probably few biologists agree so thoroughly with so many of their fellows as the author. Practically the only hard things he has to say are about "hereditary tendencies" and "principles of heredity," which seem to him "in part the old story of explaining the working of the clock by 'principle of horology' and in part a pedantic way of saying 'we don't know.'" But for many years no serious student of science has used the word principle except as a synonym for that brief and comprehensive summary of facts which is otherwise termed a "law." We speak of the "principles of psychology," the "principles of geology," and so forth; and surely the germ of a mouse develops into a mouse, and not into a beetle, because it has, among other things, a tendency, a "predisposition" to do so. "Predisposition" is, oddly enough, approved by Prof. Thomson, who pronounces predispositions to be "mysterious" but not "mystical." No doubt they are quite as mysterious but not more mystical than eyes and noses.

The book has many merits, not the least of which are its comprehensiveness and literary charm. Its principal defect is lack of that "rigorous deductive inference of consequences" by means of which we link together and test hypotheses and so ascertain whether they are in harmony with one another and "with the conceived system of reality." It is not sufficiently critical. Theories, even when incompatible, are set out with an appreciation that is equally cordial. The law of ancestral inheritance, the theory of the continuity of the germ-plasm, and the theory of recapitulation are conspicuous examples. A statistical inquiry led Galton to the conclusion that, on the average, progeny resemble progenitors in certain degrees, whence he drew the deduction that the heritage of descendants is compounded in the stated proportions of ancestral contributions. I have ever been doubtful of the precise meaning of the term "contribution," but apparently it implies more than

mere resemblance, or it would not be used. Readers of "Heredity" will be sure to conceive a contribution as an actual something contributed to the germ-plasm by the progenitor.

Passages like the following abound:—"We know . . . that the parental heritages include ancestral contributions which may be expressed in development or lie latent." But this notion will be hard to reconcile with the idea that "the parent is rather the trustee of the germ-plasm than a producer of the child." If, however, readers elect to regard "contribution" as synonymous with Weismann's "ancestral plasm" (a collection of determinants similar to that which controlled the development of the ancestor), they will be puzzled to reconcile the statement that "an individual inheritance is a mosaic of parental and ancestral plasms" with the theory that "the individual development, especially in the stage of organ-forming, is in some measure a recapitulation of the racial history," a theory which represents ancestral contributions, not in the form of a mosaic, but in that of a series. Must we assume, then, that the remote ancestors, in whom the organs were evolved, contributed to a series, but more recent ancestors to a mosaic; or is it the right view that, since the characters of the organism vary independently, retrogression in some characters, combined with progression in others, produces, in part at least, the *appearance* of a mosaic?

Every generation follows more or less closely in the developmental footsteps of the preceding generation, and every progressive variation prolongs development by adding itself to the sum of those already made and preserved during phylogeny. Of course, therefore, development is, with large reservations, a recapitulation of the life-history. Preservation (*i.e.* reproduction) implies recapitulation. Presumably the germ-plasm can lose as well as gain; that is, failure to complete the recapitulation of the parental development in any particular may be due to true variation, not merely result from injury received during development as Prof. Thomson seems to imply. Suppose an individual varied in such a way as to lapse the progressive variations of many ancestors; then, as regards the character in question, he would be in the position of a more or less remote ancestor. Is this reversion? If not, why not? According to Prof. Thomson only the reappearance of a latent character constitutes reversion. He would find it hard to justify his position.

He insists, as others have done, that "filial regression has nothing to do with reversion," because "there is a levelling up as well as a levelling down." But the fact, admitted by him, that cessation of selection tends to be followed by retrogression, indicates that on the whole there is a greater tendency to level down than to level up. In other words, retrogressive variations tend to predominate over progressive variations. If, then, development is modified recapitulation, it is plain that filial regression has something to do with reversion. At any rate, the attempt to link the two together is not necessarily due to misunderstanding. Doubtless regression is not always a

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"biological term," but when we speak of filial regression it is.

He gives full weight to the question of the transmission of acquirements, but declares that "some subtle minds have found satisfaction in maintaining that the distinction between an acquired modification and an inborn variation is a distinction without a difference." He is mistaken. It has been maintained merely that the erroneous terms "innate" and "acquired" obscure rather than reveal the very real and immensely important difference between the two classes of characters.

He discusses the Mendelian experiments which demonstrate that in certain cases descendants tend to reproduce the unlike characteristics of ancestors in the well-known proportion, and alludes to the "increased subtlety of Mendelian interpretation." The facts are compatible with a theory of segregation or with one of latency of the recessive in the pure dominant, and *vice versa*. He does not mention, however, the crucial instance of the reappearance of latent ancestral characters in *pure-bred* varieties in which can have occurred no re-union of previously separated colour (or other) factors.

One of the principal topics discussed is the question of the causation of variations. The evidence is that some are due to the direct action of the environment (nutriment, toxins and the like) on the germ-plasm, while others are spontaneous in the sense that they result from a tendency to vary as much inherent in the germ-plasm as its tendency to grow and divide. But what is the origin of the great mass of variations, those on which racial change is founded? If variations are *usually* caused by direct action, then a human race, constantly exposed to a virulent toxin (e.g. that of malaria) or to such a complex of ill-conditions as that found in the slums of great cities, should deteriorate steadily. Natural selection could have no scope, for every generation would be inferior to its predecessor. The race would drift helplessly. If, on the contrary, variations are, with rare exceptions, spontaneous, and occur all round the specific mean, natural selection has scope, and every race, or section of a race, tends to become resistant to the ill-conditions to which it is exposed. Prof. Thomson holds the first opinion, and draws his arguments mainly from medical sources. From time immemorial doctors have attributed all sorts of filial and racial degeneracy to all sorts of parental mishap. Lately, however, a rapid change of opinion has occurred, as may be seen by examining the report (just published) of the Royal Commission on the Care and Control of the Feeble-minded. The Commission follows Sir E. Ray Lankester, who declares that "no facts are known which support these imaginative teachings." Alluding to the rather widely known fact that every race is resistant to every ill-condition precisely in proportion to the length and severity of its past experience of it, it declares that "It is not to be conceived that a race which deteriorates in every generation can emerge from the struggle not weakened, but strengthened." In truth, the hypothesis that variations are usually due to direct action is wholly incompatible with the theory

of natural selection, which, nevertheless, Prof. Thomson upholds. If additional evidence be needed it is furnished by plants, which, when propagated asexually and taken to all climates of the world, hardly vary until the first seminal generation, and then not more apparently than if no such long and diverse exposure of the germ-plasm had occurred. Obviously variations occur normally precisely when they are useful—at the genesis of a new individual when they furnish materials for natural selection. It seems reasonable to conclude, therefore, that they are under the control of natural selection, a superior or inferior tendency to vary being in itself a variation liable to selection. This hypothesis is strongly confirmed by the fact that retrogressive variations tend to predominate over progressive variations—an immensely useful tendency, for, while useful variations and structures are preserved by natural selection, useless variations and structures are planed away without elimination of individuals.

When cultivated in non-living media, the parasitic microbes of disease gradually lose their virulence, which is nothing other than the means by which they protect themselves from the cells of the body. Non-virulent saprophytic micro-organisms, introduced under fit conditions into the living body, gradually acquire virulence. In the one case, apparently, retrogression follows cessation of selection, in the other progression follows selection. The widely accepted hypothesis that microbes "acquire" and transmit virulence in the Lamarckian sense is demonstrably untenable. How could the direct action of the environment on the bodies of the microbes cause them to "acquire" the mechanism necessary for the production of such adaptive and elaborate chemical compounds as toxins? Presumably all parasitic microbes have evolved from saprophytic types. Men have made the microbes of human diseases virulent, and each human disease has made the race exposed to it resistant to itself. While races (e.g. British and Negro) which have evolved in conjunction with their familiar diseases (e.g. tuberculosis and malaria) are able to persist when exposed to them, other races (e.g. Polynesians and Red Indians) tend to perish. Disease supplies the only instance in nature in which we are able to see natural selection actually at work, and the study of diseases reveals a multitude of very beautifully adjusted and unmistakable adaptations. The facts are not disputed; the inferences, I believe, are indisputable. Prof. Thomson thinks, however,

"It would be a subtler and more convincing line of argument to say that, throughout the ages, man has been selecting the microbes, lessening the virulence, in a sense taming them—sometimes to death—as his phagocytes were strengthened by more suitable food, or as his 'opsonic' index improved, again also in relation to food."

He means that man has somehow selected the weaker, the less protected, of his persecutors for survival, that his present food would have been more suited to his ancestors than that which evolution fitted them to consume, and that negroes are more resistant than Englishmen to malaria because they are better

fed, Englishmen more resistant than negroes to tuberculosis for the same reason, and Polynesians less resistant than both races to a multitude of diseases because they are worse fed.

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EGYPT AND BABYLONIA.

Egypt and Western Asia in the Light of Recent Discoveries. By L. W. King and H. R. Hall. Pp. viii+480; illustrated. (London: Society for Promoting Christian Knowledge, 1907.) Price 10s.

THIS handsome volume from the pens of Messrs. King and Hall, of the British Museum, is intended as a supplement, or, as the authors modestly express it, "an appendix or addendum," to include all the most recent results of discoveries in Egypt and Western Asia, and thereby bring up to date the three volumes of Prof. Maspero on "The Ancient History of the Peoples of the Classic Orient," which the Society for Promoting Christian Knowledge issued between 1894 and 1896.

The period since the last volume of Prof. Maspero's history appeared has been one very rich in discovery, and archaeologists have been busy with the spade in the Greek islands and mainland, in Asia Minor, the Euphrates and Tigris valleys, as well as in Egypt and Nubia, with most startling results. In 1894 we were almost in the dark as to Egyptian history prior to the time of Seneferu, the last king of Manetho's Third Dynasty, and prehistoric Egypt was practically unknown. The so-called Minoan civilisation of Crete was undreamt of, and hardly anything was known about the early peoples of Syria and Asia Minor. Now, thanks mainly to the work of M. de Morgan, Prof. Flinders Petrie, Dr. Arthur Evans, and Prof. Winckler, we can extend our vista far beyond the horizon of 1896.

It is with the discovery of prehistoric Egypt that the volume before us opens, and here the authors bring together the latest results of the explorer in the field of prehistoric antiquities in the Nile Valley. They finally dispose of the old theory maintained by Petrie and Blankenhorn that the desert plateaus on both sides of the valley were in Palæolithic days clothed with forest, and they bring forward the more reasonable one promulgated by Beadnell that the torrents which are sometimes experienced in the desert at the present day would have been enough to have cut out the deep ravines or *wadis* in the limestone rock such as we see at Thebes in the famous ravine called the Valley of the Tombs of the Kings. Whether Palæolithic man in Egypt—where he is represented by thousands of flint tools from the desert plateaus—was contemporary with the Cave man of Europe we do not know; nor are there any data whereby even a rough estimate can be made as to when the Palæolithic period was succeeded by the Neolithic. For a considerable time anterior to the First Dynasty, copper as well as stone weapons were in use, so that even before the beginning of the historical age the Egyptians were living in the "Chalcolithic" period. The beginning of the Dynastic age is placed by Messrs. King and Hall at about 4500 B.C. (p. 13), but this

does not at all agree with the latest researches into the vexed question of Egyptian chronology, which tend rather to diminish than to lengthen out the hitherto accepted chronology. A most important monograph on this subject was written by Prof. Eduard Meyer in 1904, and is printed in the *Abhandlungen* of the Königl. Preuss. Akademie der Wissenschaften (with a *Nachträge*, 1908); but this the authors do not seem to know, nor do they refer to Prof. Breasted's concise summary of the facts relating to Egyptian chronology in the first volume of his "Ancient Records." A perusal of Meyer's or Breasted's works will show that there is very good reason for placing the beginning of the First Dynasty at not earlier than about the year 3500 B.C.

Regarding the question as to the origin of the Egyptians, Messrs. King and Hall point out that in the early dynastic period two races lived in Egypt which differed considerably in type and also in burial customs. The Dynastic people, they believe, came originally to the Nile Valley from the shores of the Red Sea by way of the Wady Hammamat, to Koptos and Kûs.

"From many indications," they say, "it would seem probable that these conquerors were of Babylonian origin, or that the culture they brought with them (possibly from Arabia) was ultimately of Babylonian origin."

The Lower Egyptians, who were conquered by the Dynastic race, were possibly of Mediterranean stock, akin to the primitive inhabitants of Palestine, Greece, Italy, and Spain.

The second chapter deals with Abydos and the first three Egyptian dynasties, but the authors do not appear to have any very clear idea as to the real history of this early period. On p. 73 it is said that the "King Sma" is "possibly identical with Aha or Narmer, more probably the latter." There is, in fact, no evidence whatever that Sma is the name of a king or even of a person at all, while, on the other hand, it is a well-known title meaning "consort," and was often assumed by queens. On pp. 61-62 it is said that Narmer is not represented at Abydos, yet at least half-a-dozen monuments bearing his name have come from there. There has been much discussion as to the validity of Dr. Borchardt's identification of King Aha with Menes, the traditional founder of the monarchy; Messrs. King and Hall dismiss the subject by saying (p. 76): "Whether Aha was called Men or not it seems evident that he and Narmer were jointly the originals of the legendary Mena." The nomen of Khasekhem, we note, is given as "Besh," but this is very doubtful; the name of the last king of the First Dynasty is transliterated everywhere as Qa, whereas, surely, the right reading is Qa-a, "the high of hand."

In the third chapter the authors discuss recent discoveries relating to Memphis and the Pyramids, and advance the theory that the city of Memphis was built by Merbapa, the Miebis of Manetho's list, and not "by the legendary and confused Mena." In support of this it may be noted that Merbapa heads the list of kings of the Sakkara Canon.

The fourth, fifth, and sixth chapters are devoted to