THURSDAY, FEBRUARY 12, 1891.

ESSEX AND THE TECHNICAL INSTRUCTION ACT.

THE distribution of the funds placed at the disposal of the various County Councils, in accordance with the Technical Instruction Act of 1889 and the Local Taxation Act of 1890, is now engaging attention throughout the country, and it is widely recognized, by those who are responsible for the proper administration of these funds, that the money cannot be better spent than in furthering the cause of technical education with special reference to the needs of their own districts. It is, in fact, an open secret that the Councils are expected to apply their funds in accordance with the Acts of Parliament, and the voices of public opinion and of the framers and supporters of those Acts have frequently been heard to this effect. The facts and figures which we have from time to time published in the columns of NATURE have helped to keep our readers alive as to the present state of affairs in the different counties. The unanimity which prevails, so far as concerns the general principle of applying the funds to the purposes of technical instruction, is certainly a most encouraging indication of the direction in which public opinion is moving. The main difficulties in the way of apportioning the grants are likely to arise, however, when the various claims come to be considered by the Councils to whom they are submitted. This particularly applies to counties like Essex, where no great manufacturing centres exist, and where the occupations of the rural population are agricultural or maritime. It may be difficult at first sight to see clearly how the grants can be applied in such cases, so as to satisfy the wants of a non-urban community, and at the same time to convey assurance to the Council that the money has been well spent in accordance with the spirit of the Acts. It may be pointed out, however, that agriculture clearly comes within the definition, and is, in fact, recognized as a branch of technical science, and no County Councillor who has the maritime interests of his district at heart would grudge the extension of a similar recognition to the claims of applied marine zoology, of navigation, boat-building, or any of the other industries carried on along our coasts.

Essex may be taken as a typical example of a county which is both maritime and agricultural, and the action taken by its Council will no doubt be eagerly watched by the Councils of other counties similarly constituted. As being one of the home counties, moreover, its case presents particular interest. The total amount at the disposal of the county is about £21,000, of which about £4000 goes to West Ham as the county borough, leaving £17,000 for the urban centres and rural districts of the remainder of the county. Numerous claims for grants have been sent in, and will receive attention in due course. Many grants for the carrying on of scientific and technical instruction in institutions already in existence in the larger towns will no doubt be justifiably made. But the means by which the ultra-urban districts can be provided for have yet to be developed, and the scheme put forward by the Essex Field Club, to which we have already briefly alluded in these columns, certainly seems to be sufficiently comprehensive to meet the wants of the case. Of the qualifications of the Club to carry on the work effectively it is not within our province to speak. It will suffice to say that the deputation from the Club, which was received by the County Council on February 2, comprised Sir Henry Roscoe, Profs. W. H. Flower, R. Meldola, and G. F. Boulger, Mr. F. W. Rudler, and others interested in the scheme. Lord Rayleigh, although unable to attend personally, had consented to allow his name to be added to the Committee.

The members of the deputation, whose scientific strength is unquestionable, expressed their approbation of the scheme, and spoke in the highest terms of the work hitherto done by the Club; if their expression of opinion is allowed that weight which it undoubtedly carries, there can be no doubt that Essex possesses in the Field Club an organization which the County Council would do well to avail themselves of. The scheme itself, which was formally submitted by the deputation, will be found in abstract in another portion of our columns. How nearly it falls in with the views of those most competent to speak authoritatively on the question will be gathered from the speech made by Sir W. Hart-Dyke at the recent Conference of the National Association for the Promotion of Technical and Secondary Education, in the course of which he said:—"To my mind the only practical way in which to carry on agricultural teaching is to have a central system. You must, I think, group together different villages and different schools, and have peripatetic teachers. If you do this you will find that the extra cost to school managers on the one hand, and to the ratepayers on the other, will be very small indeed, and yet you will be able to carry out an excellent system of agricultural education." An institution such as the Essex Field Club proposes to establish in connection with their Museum at Chelmsford would meet these views exactly.

MODERN BIOLOGY AND PSYCHOLOGY.

Animal Life and Intelligence. By C. Lloyd Morgan, F.G.S., Author of "Animal Biology," "The Springs of Conduct," &c. (London: Edward Arnold, 1890-91.)

THIS very interesting volume is nearly equally divided between the two subjects indicated by its title. In the earlier chapters we have excellent accounts of the nature of animal life and its relation to the environment; of the processes of life; of reproduction and development; of variation and natural selection; of heredity and the origin of variations; and of organic evolution. The later chapters deal with the senses and sense-organs of animals; the nature of mental processes in man, serving as a basis for our judgment as to the nature and amount of animal intelligence; the mental processes of animals are then very fully and carefully discussed in three long and very suggestive chapters; and this brings us to a final and very metaphysical chapter on mental evolution. It will be impossible here to do more than notice a very few of the interesting subjects which the author discusses with a fullness of knowledge and a judicial impartiality worthy of all praise.

In his chapter on "Variation and Natural Selection," Mr. Morgan deals with the question whether isolation, with no change in the conditions of existence, can lead to divergence of character, and thus to the formation of distinct species. He decides in the affirmative; and as he approaches the question apparently without any bias, it will be useful to see how far his arguments are well founded. He says:—

"Let us suppose that an island is divided into two equal halves by the submersion of a stretch of lowland running across it. Then the only possible cause of divergence would lie in the organisms themselves thus divided into two equal groups."

Before going further, it is well to note the utter impossibility of any such equality of conditions as is here supposed. Probably there is no island in the world but presents considerable climatal differences in its northern and southern, or in its eastern and western halves, even if its contour and geological structure were such as to admit of equal division. Neither is it conceivable that any division could separate every species upon the island into two equal portions, and without this the organic conditions would differ considerably. But, as we are considering a purely hypothetical case, we will allow the possibility of a sufficiently equal division in all respects. Mr. Morgan thus proceeds:—

"We have seen that variations may be advantageous, disadvantageous, or neutral. The neutral form a fluctuating, unfixed, indefinite body. But they afford the material with which Nature may make, through intercrossing, endless experiments in new combinations, some of which may be profitable. Such profitable variations would escape elimination, and, if not bred out by intercrossing, would be preserved. In any case the variety would tend to advance through elimination as previously indicated. But in the two equal groups we are supposing to have become geographically isolated, the chances are many to one against the same successful experiments in combination occurring in each of the two groups. Hence it follows that the progress or advance of the two groups, though analogous, would not be identical, and divergence would thus be possible under practically similar conditions of life."

Now this passage seems to me to be founded on a misconception of the true nature of the process of species production through variation and selection. The evidence we possess as to variation—and Mr. Morgan has given us some additional facts in this volume on the variation of bats—clearly shows that all the organs and parts of animals vary, in the two directions of greater or less development, about a mean value, which mean represents the typical or perfect character of the species for the time being. This typical character has been reached ages ago by all species in countries whose conditions are tolerably stable, and the remarkable fixity of character proved to exist, not only by the mummied animals of Egypt, but by those much older forms which were the contemporaries of Neolithic man, indicates that for such species, so long as conditions remain approximately unchanged, there are no "profitable variations" possible. They have long ago been brought into almost complete harmony with their environment; all possible combinations have long ago been tried; and the fact that though in every successive generation there are variations of every organ and every character in both *plus* and *minus* directions, these are not taken advantage of, but the mean assemblage of characters constituting the species remains unchanged, proves that changes in any direction without change of environment would be disadvantageous. There can therefore for such a species be no "profitable variations" so long as conditions remain, as they do *ex hypothesi*, absolutely unchanged; no "progress or advance" is possible without such change; and to suppose that there can be further divergence and specialization under the conditions assumed is to maintain that, what natural selection, acting on the innumerable variations yearly occurring in the whole species, has been unable to do in the past, will be done in the future by the same causes acting on the two halves of the species separately.

It cannot of course be denied that, however many combinations and variations have occurred in any species during the last ten thousand years, some few superior variations may occur in the next ten thousand, but there will probably be a corresponding number of cases in which the best variations of the year are below the usual standard, and there is no reason to believe that the one will do more than balance the other. The proviso which Mr. Morgan duly makes—"if not bred out by intercrossing"—would certainly require to be taken account of, since the idea that single favourable sports have had any part in the formation of new species is now rarely adopted by evolutionists. We must also always remember Darwin's maxim, generally admitted to be a sound one, that "Nature does not produce absolute perfection but only relative perfection,"—which again implies that when each species has reached an equilibrium with its environment there is for it no further perfection possible under the circumstances, no "profitable" variations tending to modify its mean specific characters of which natural selection can take account. For these various reasons it seems to me that any permanent modification of a species by mere isolation of a portion of it, and without some adequate change in the environment, is almost inconceivable.

Connected with this question is that of the existence of useless specific characters, which are not and never have been correlated with useful characters. Mr. Morgan here very properly suggests that the difficulty is as to what is to give such useless characters any fixity, and without fixity they will not be classed as specific. In a later chapter, on "Heredity and the Origin of Variations," he himself suggests a possible escape from this difficulty in the supposition that the intercrossing of individuals differing somewhat in character does not result in mere "hereditary mixture" but in "organic combination,"—meaning, I presume, that by such intercrossing new characters or the rudiments of new organs may be produced which were not present in any of the ancestral forms. He supposes that such combinations may initiate definite lines of variation, and that we may thus obviate the difficulty as to the origination of organs or structures whose first rudiments cannot be conceived to have been useful to their possessors. It seems to me probable that, however originated, there *are* such "lines of variation," and that some of the unknown laws of variation *do* lead to the initiation of the structures or organs which have been essential to the development of

the varied types of the organic world ; but I nevertheless maintain that this does not necessitate the acceptance of the doctrine of useless "specific" characters, or that of the formation of new species by isolation in an unchanged environment. For, by the assumption, these lines of variation and these nascent structures are produced by favourable combinations within the limits of a species. They appear more or less sporadically ; they are at first of no utility ; there is therefore nothing to give them fixity or to lead to their general and uniform development in all the individuals composing the species. Thus they must remain, sometimes dying out, sometimes advancing, till under some changed conditions of the environment they become of use in the struggle for existence. From that moment they become subject to the law of natural selection. All individuals not possessing these characters, or possessing them in too small a degree, are eliminated, leading at once to the steady increase of the character and its constant presence in all individuals of the species. It has now become a "specific" character, but only because it has become useful. The definite "line of variation" is now followed because it is a useful line. But, the moment it reaches a maximum of utility, elimination prevents any further development in that direction although the tendency may still exist, and variations which are now injurious may still continue to appear though they cease to be preserved. This is the view I have already expressed in regard to Prof. Geddes's theory of variation in plants ("Darwinism," p. 428), admitting the tendency of vegetative development in the various ways he suggests to be highly probable, but denying that such causes can produce definite fixed characters without the eliminating agency of natural selection.

It has been objected that this view is inconsistent with the theory that the ornamental appendages and colours of male birds and insects have been produced by exactly such a tendency to development along certain lines of variation ; but it is forgotten that in this case such development is strictly correlated with the superabundant energy and vital activity of male animals, characters which are the subject of both sexual and natural selection. They are therefore increased within the limits of hurtfulness, while their utility as recognition marks, and as indications of sexual maturity, keeps them tolerably constant.

In discussing Weismann's theory of the continuity of the germ-plasm, Mr. Morgan gives reasons for believing that it is in some respects a retrograde step, and that the earlier view, of the continuity of reproductive cells, is the more probable ; and after discussing the various opposing theories—Darwin's pangenesis, Haeckel's perigenesis, Spencer's physiological units, and some others—he arrives at the conclusion that there is a continuity of reproductive cells ; that hereditary similarity is due to the fact that parents and offspring are derived eventually from the same germinal cells ; that there is no convincing evidence that in the Metazoa special modifications of the body so influence the germ as to become hereditary ; but nevertheless, he concludes, there is no reason why such influence should not be assumed as a provisional hypothesis.

The chapter on "Organic Evolution" is a most interesting one, discussing, as it does, in the author's suggestive manner, the various problems arising out of the theory of variation and natural selection. The greater part of this discussion is clear and convincing, but a few cases must be noticed in which the essential point of the argument appears to have been overlooked or insufficiently appreciated.

In considering the agency of natural selection it is urged that it can only act where there is a direct advantage to some individuals or disadvantage to others ; that the advantage must be immediate and present ; that the advantage must be sufficient to decide the question of elimination or non-elimination ; and that we must distinguish between mere indiscriminate destruction and selective elimination. Now, throughout this discussion, and especially in the last portion of it, Mr. Morgan fails to give due weight to the enormous scale on which Nature works, both as regards the number of individuals, the space over which they are distributed, and the time during which the process is going on. If we take all these factors fully into consideration, we shall, I think, see that there is really no importance in what seems to us fortuitous destruction, and that, sooner or later, every beneficial or injurious variation, such as we know are abundantly produced every year, must produce a corresponding effect. He says :—

"A hundred are born, and but two survive. It is a mistake to say that of the hundred born the two survivors are necessarily the very best of the lot. It is quite possible that indiscriminate destruction got rid of ninety of all sorts, and left only ten subject to the action of a true elimination."

Now, this would be quite true, and a valid argument, if a species usually consisted of a few hundred individuals, and the question of modification by natural selection had to be decided within ten or fifty years. But when the hundred individuals are multiplied by perhaps a million spread over a large area, and when the operations of accidental destruction and elimination go on during thousands and even millions of generations, we feel sure that, on the average of the whole, the worst will be strictly eliminated, the best as strictly preserved. A passage is quoted from Prof. Weismann about the destruction of eggs by weasels, cats, crows, &c., of the helpless young birds by the same enemies, of others by cold and hunger in winter, and yet others by the dangers of migration, and it is said: "There is here, first, a certain amount of fortuitous destruction ; secondly, some selection applied to the eggs, &c." But surely, as regards the whole species, and on an average over long periods, "fortuitous destruction"—that is, destruction which overtakes the very best as well as the very worst—must be totally insignificant, as compared with true selective elimination. The capacity of the parents to conceal the nest and eggs, and to protect the young birds, will have been constantly increased by selection, as well as every other faculty and character that is of value to the species, till a condition is reached by which the standard population of the species can be permanently maintained. In different years different qualities ensure survival, and thus some may often survive for a few years which are not so well fitted on the whole as some that have succumbed. But in considerable periods, including years of

the severest trial, all these comparatively imperfect individuals will be destroyed, and only the very best be left to continue the race.

In discussing the origin of the beautiful forms and colours of birds, insects, and flowers, it again seems to me that there is some want of perception of the exact points at issue. Mr. Morgan argues, as I think very justly, that even the higher animals have no sense of beauty, and that "the word æsthetic should be resolutely excluded in any discussion on sexual selection." He urges, as I had myself done (NATURE, vol. xlii. p. 291), that a considerable portion of the beauty of flowers, as well as that of birds and insects, is due to symmetry, elegance of outline, surface texture, and other causes. It is, he says, the nectar, not the beauty of the flower, that attracts the bee; and in birds, "the mate selected has been that which has excited the strongest sexual appetence; his beauty has probably not, as such, been distinctly present to consciousness." All this seems to me to be excellent, but in another part of his work Mr. Morgan imputes to me opinions which seem to me erroneous, and which I am not aware of having expressed. Thus, at p. 206, speaking of flowers, he says:—

"And when we ask in this case, as we asked in the case of the beautiful colours and forms of animals, what has guided their evolution along lines which lead to such rare beauty, we are given by Mr. Wallace himself the answer: 'The preferential choice of insects.' If these insects have been able to produce, through preferential selection, all this wealth of floral beauty (not, indeed, for the sake of beauty, but incidentally in the practical business of life) there would seem to be no *a priori* reason why the same class, and birds and mammals, should not have been able to produce, through preferential selection, all the wealth of animal beauty."

I do not remember ever having used the term "preferential choice" as applied to insects and the special colours or markings of flowers. I have always held that these are merely signs of the presence of products which the insect needs and enjoys, and that there is probably no more preference for those particular colours and marks on the part of the insect than there is preference by us for a particular *number* because it indicates our friend's house, or for a particular *colour* because it is that of the seal of a favourite wine. Both number and colour may be in themselves either indifferent or even disagreeable to us, but they none the less serve their purpose of recognition marks. I see no more difficulty in the beauty of flowers and birds and insects being all incidental to the general laws of growth and development, subject always to the law of elimination, than in the beauty of landscape, of foliage, of crystals, of corals diatoms and shelly mollusks, of the exquisite forms and motions of the gazelle, the horse, or the kitten, which have all been produced without any question of preferential selection.

Again, after quoting my statement of certain facts showing that isolation is produced by the likes and dislikes of animals, Mr. Morgan says:—

"Mr. Wallace thus allows, nay, he lays no little stress upon, preferential mating, and his name is associated with the hypothesis of recognition marks. But he denies that preferential mating, acting on recognition marks, has any effect in furthering a differentiation of form or colour."

Now the passage Mr. Morgan quotes referred almost exclusively to preferential association, not to preferential mating, which I consider to be a result of the former. And this preferential association must certainly lead by elimination to a furthering of the differentiation of form or colour exactly so far as that differentiation was useful, and it even might be continued farther, as I believe it sometimes has been, till checked by absolute hurtfulness, if correlated with the extreme vigour and activity of male animals.

Mr. Morgan discusses at considerable length the question of whether the effects of use and disuse are hereditary. He admits the very imperfect character of the evidence in favour of the proposition that they are so, and he adduces, as in his opinion one of the best cases, "the instinctive avoidance" of nauseous and stinging insects by most birds. As neither the nauseous taste nor the stings are usually fatal, the avoidance of them is not of eliminating value, and cannot, therefore, have been produced by natural selection. Hence he thinks the inheritance of individual experience probable. But the "instinctive avoidance" is here assumed, whereas there is now good reason to believe that in the case of nauseous insects, and probably also of stinging insects, the avoidance is the result of individual experience or observation. Some of the most curious phenomena of mimicry can only be explained on this hypothesis.

To many readers, the latter portion of the volume, dealing with the senses and intelligence of animals, will be the most attractive. The chapter on the senses of animals is an admirable summary of the most recent observations and researches on this subject, and the explanation of the probable mode of vision of insects by means of their compound eyes is especially clear and very instructive. Then follow chapters on the mental processes of man and of animals, characterized by clear definitions and acute analysis. It is impossible to summarize these chapters, but some of the author's conclusions may be quoted. On the question of the psychology and intelligence of ants, bees, and other insects, after pointing out their widely different structure and sense-organs, he says:—

"Remember their compound eyes with mosaic vision, coarser by far than our retinal vision, and their ocelli of problematical value, and the complete absence of muscular adjustments in either one or the other. Can we conceive that, with organs so different, anything like a similar perceptual world can be elaborated in the insect mind? I for one cannot. Admitting, therefore, that their perceptions may be fairly surmised to be analogous, that their world is the result of construction, I do not see how we can for one moment suppose that the perceptual world they construct can in any accurate sense be said to resemble ours."

The following passage in like manner gives the author's conclusions as to the difference between the mental nature of man and the higher animals:—

"Furthermore, it seems to me that this capacity of analysis, isolation, and abstraction constitutes in the possessor a new mental departure, which we may describe as constituting, not merely a specific, but a generic difference from lower mental activities. I am not prepared, however, to say that there is a difference in kind between the mind of man and the mind of the dog. This

would imply a difference in origin or a difference in the essential nature of its being. There is a great and a marked difference in kind between the material processes which we call physiological, and the mental processes we call psychical. They belong to wholly different orders of being. I see no reason for believing that mental processes in man differ thus in kind from mental processes in animals. But I do think that we have, in the introduction of the analytic faculty, so definite and marked a new departure that we should emphasize it by saying that the faculty of perception, in its various specific grades, differs generically from the faculty of conception. And believing, as I do, that conception is beyond the power of my favourite and clever dog, I am forced to believe that his mind differs generically from my own."

This seems a very fair statement of the case, and one to which, so far as it goes, I have no objection to make. But in the concluding chapter, on mental evolution, we have a serious attempt to overcome the difficulty of the relation between the physiological and psychical processes here stated to belong to "two wholly different orders of being." This is supposed to be done by the adoption of the *monistic hypothesis*, which assumes that these "wholly different orders" of things are really identical—that *neurosis is psychosis*. "The neurosis is the outer or objective aspect, the psychosis is the inner or subjective aspect." Then the subject is attempted to be made clearer by the adoption of other terms—"kinesis" for physical manifestations of energy, "metakinesis" for all manifestations of the mental or conscious order; and we have the following statement:—

"According to the monistic hypothesis, *every mode of kinesis has its concomitant mode of metakinesis, and when the kinetic manifestations assume the form of the molecular processes in the human brain, the metakinetic manifestations assume the form of human consciousness.*"

If this means anything, it means, what has been stated in simpler but equally exact and more intelligible language, that all force is will-power. But it goes further, and implies that there can be no mind like that of man, or superior to it, without a brain formed of similar materials and similarly organized with the brain of man. This necessary connection, and even identity, of the two is, however, what is not proved, and not even, in my opinion, shown to be probable.

The last few pages of the volume are devoted to a discussion of the causes which have led to the development of the higher intellectual faculties in civilized man, and a difficulty is found in explaining this development, except on the ground that the increase of intellectuality acquired by use of the intellectual faculties is inherited. The objection may be made that there is no proof of any increase of average intellect in Europe during the last two or the last twenty centuries; and, on the other hand, it seems probable that, although the unintellectual may generate more rapidly, a smaller proportion of the offspring survive. It is suggested that the development of the social habit, the mutual aid and protection thus afforded, may well have left a balance of the life-energy previously employed for individual self-preservation, available for the increase of pure intellect. The exceedingly sporadic character of exceptional intellectual power favours this view, which is analogous to that which I have suggested as having led to the development of the accessory plumes of male birds. Whether the very existence of such faculties can be adequately explained by increased brain-development alone, is another matter.

The present notice, necessarily confined to a few of the more salient features of the book, gives no fair idea of the great variety of topics treated, nor of the originality and clearness which are its great characteristics. The numerous woodcuts and diagrams are all illustrative of the text, and are fully explained, and the author is particularly happy in his use of diagrams and formulæ to illustrate the more obscure or difficult conceptions. The diagram, at p. 141, to explain Weismann's theory, as illustrated by the question, "Does the egg produce the hen, or does the hen produce the egg?" is one of these, and will render the problem intelligible to many who would otherwise have a difficulty in understanding it. On the whole, the work will prove a boon to all who desire to obtain a general knowledge of the more interesting problems of modern biology and psychology by the perusal of a single compact, luminous, and very readable volume. ALFRED R. WALLACE.

THE LAKE-DWELLINGS OF EUROPE.

The Lake-Dwellings of Europe; being the Rhind Lectures in Archæology for 1888. By Robert Munro, M.A., M.D. (London: Cassell and Co., 1890.)

IN this work on the lake-dwellings of Europe, Dr. Munro has carried out on a wider field the inquiry begun in 1882 in his book on those of Scotland. He has brought to his task qualities of a high order. He had a large share in the exploration of the lake-dwellings in his own country, and has recorded the results in a business-like fashion. He has prepared himself for dealing with the lake-dwellers of the Continent by a painstaking examination of the evidence on the spot, and by visiting all the principal collections. He further has read the voluminous literature bearing on the subject scattered through various journals and periodicals, as well as that which lies ready to hand in separate books. The result of all this labour—and how great it has been only those can know who, like the writer, have gone over the ground—is this work "smelling of the oil" in every page, well illustrated with numerous cuts and with good indexes, and a systematic list of references. It is, indeed, to be looked upon as an encyclopædia of matters relating to lake-dwellings, rather than as an ordinary book. It is little less than a miracle that the vast array of facts brought together should have been compressed into the narrow limits of six lectures.

The lake-dwellings of Switzerland and of the surrounding parts of the Continent were laid before the archæological public in 1866 by Dr. Keller and his English translator and editor, the late Mr. Lee, to whom, some twelve years later, we owe a second and enlarged edition. Since that time discovery has rapidly followed discovery in various parts of Europe. These have been carefully recorded by Dr. Munro in the work before us. The first three hundred pages, comprising the first three lectures and part of the fourth, deal with the lake-dwellings in Switzerland, France, and Italy. The rest of the fourth lecture is devoted to the discoveries in North Germany made principally by Prof. Virchow, and to the curious