BIRD NOTES.

To the July issue of the *Quarterly Review* Dr. H. Gadow communicates an instructive article on the nature and meaning of the colours of birds. After pointing out the fallacy of the idea that the colouring of such birds as the scarlet ibis or white egret can be in any sense protective, the author discusses the diverse means by which colour is produced in birds, showing that while black and the red and yellow group are pigmentary, blues and greens are so-called structural tints, due to the reflection from the surface of the feathers of an undue proportion of short light-rays. Metallic colouring, which usually occurs in black feathers, is due, of course, to another cause. Dr. Gadow next proceeds to describe the sequence in which various colours replace one another with the advance of specialisation. As regards the cause of colour-specialisation, the author rejects both natural and sexual selection, remarking that if the latter were the inducing factor, every group and species would have its own taste, and each individual would strive to develop its yellow patches into orange and then into red. For the explanation offered in place of natural and sexual selection, we must refer our readers to the article itself.

In the August number of Witherby's *British Birds* the editor congratulates his readers on the satisfactory response which has been made this season to his appeal for assistance in marking birds. Nearly 11,000 rings were distributed, and schedules recording the marking of between 5000 and 6000 birds have been already received. The cooperation is invoked of all into whose hands ringed birds may fall. In the same issue Mr. W. Frohawk describes and illustrates the feeding habits of the razorbill, remarking that all the specimens which have come under his special notice fed on sand-launces. These fish, to the number in some cases of so many as half a dozen at a time, are held transversely in the beak, and the marvel is how the bird manages to capture and hold one after the other without losing those previously caught. Possibly each is killed when caught; but even then it is difficult to see how the catch is procured and retained.

To the August number of the *Popular Science Monthly* Prof. F. H. Herrick contributes the third and final instalment of an article on instinct and intelligence in birds. It is concluded that many of the alleged cases of intelligence are really due to habit, and that "all the intelligence which birds may on occasion exhibit seems to give way under the spell of any of the strange instincts. . . . They seldom meet emergencies by doing the intelligent act, and, in spite of the anecdotes, probably but seldom come to the effective aid of their companions in distress. On the other hand, I have more than once seen a mother bird try to pluck a hair or piece of grass from the mouth of a nestling."

Another instance of intelligence is afforded, in the author's opinion, when a gull, after feeding its young for three weeks on partially digested fish, offers them entire squids to swallow. The practice displayed by young kingfishers of arranging themselves in a row and showing a tendency to walk backwards is, however, attributed to habit formed underground; while the time it takes for hole-nesting birds to change their place of entrance when a more convenient access has been afforded is an instance of the dominance of habit over intelligence.

THE BRITISH ASSOCIATION AT SHEFFIELD.

Section D.

ZOOLOGY.

Opening Address by Prof. G. C. Bourne, M.A., D.Sc., F.R.S., President of the Section.

In choosing a subject for the address with which it is my duty, as President of this Section, to trouble you, I have found myself in no small embarrassment. As one whose business it is to lecture and give instruction in the details of comparative anatomy, and whose published work, *qualecunque sit*, has been indited on typical and, as men would now say, old-fashioned morphological lines, I seem to stand self-condemned as a morphologist. For morphology, if I read the signs of the times aright, is no longer in favour in this country, and among a section of the zoological world has almost fallen into disgrace. At all events, I have been very frankly assured that this is the case by a large proportion of the young gentlemen whom it has been my fate to examine during the past two years; and, as this seems to be the opinion of the rising generation of English zoologists, and as there are evident signs that their opinion is backed by an influential section of their elders, I have thought that it might be of some interest, and perhaps of some use, if I took this opportunity of offering an apology for animal morphology.

It is a sound rule to begin with a definition of terms, so I will first try to give a short answer to the question "What is morphology?" and, when I have given a somewhat dogmatic answer, I will try to deal in the course of this address with two further questions: What has morphology done for zoological science in the past? What remains for morphology to do in the future?

To begin with, then, what do we include under the term morphology? I must, first of all, protest against the frequent assumption that we are bound by the definitions of C. F. Wolff or Goethe, or even of Haeckel, and that we may not enlarge the limits of morphological study beyond those laid down by the fathers of this branch of our science. We are not—at all events we should not be—bound by authority, and we owe no allegiance other than what reason commends to causes and principles enunciated by our predecessors, however eminent they may have been.

The term morphology, stripped of all the theoretical conceptions that have clustered around it, means nothing more than the study of form, and it is applicable to all branches of zoology in which the relationships of animals are determined by reference to their form and structure. Morphology, therefore, extends its sway not only over the comparative anatomy of adult and recent animals, but also over palæontology, comparative embryology, systematic zoology and cytology, for all these branches of our science are occupied with the study of form. And in treating of form they have all, since the acceptance of the doctrine of descent with modification, made use of the same guiding principle—namely, that likeness of form is the index to blood-relationship. It was the introduction of this principle that revolutionised the methods of morphology fifty years ago, and stimulated that vast output of morphological work which some persons, erroneously as I think, regard as a departure from the line of progress indicated by Darwin.

We may now ask, what has morphology done for the advancement of zoological science since the publication of the "Origin of Species"? We need not stop to inquire what facts it has accumulated: it is sufficiently obvious that it has added enormously to our stock of concrete knowledge. We have rather to ask what great general principles has it established on so secure a basis that they meet with universal acceptance at the hands of competent zoologists?

It has doubtless been the object of morphology during the past half-century to illustrate and confirm the Darwinian theory. How far has it been successful? To answer this question we have to be sure of what we mean when we speak of the Darwinian theory. I think that we mean at least two things. (1) That the assemblage of animal forms as we now see them, with all their diversities of form, habit, and structure, is directly descended from a precedent and somewhat different assemblage, and these in turn from a precedent and more different assemblage, and so on down to remote periods of geological time. Further, that throughout all these periods inheritance combined with changeability of structure have been the factors operative in producing the differences between the successive assemblages. (2) That the modifications of form which this theory of evolution implies have been rejected or preserved and accumulated by the action of natural selection.

As regards the first of these propositions, I think there can be no doubt that morphology has done great service in establishing our belief on a secure basis. The transmutation of animal forms in past time cannot be proved directly; it can only be shown that, as a theory, it has a much higher degree of probability than any other that can be brought forward, and in order to establish the highest possible degree of probability, it was necessary to demonstrate that all anatomical, embryological, and palæontological facts were consistent with it. We are apt to forget, nowadays, that there is no *a priori* reason for regarding the resemblances and differences that we observe in organic forms as something different in kind from the analogous series of resemblances and differences that obtain in inanimate objects. This was clearly pointed out by Fleeming Jenkin in a very able and much-referred to article in the *North British Review* for June, 1867, and his argument from the *a priori* standpoint has as much force to-day as when it was written forty-three years ago. But it has lost almost all its force through the arguments *a posteriori* supplied by morphological science. Our belief in the transmutation of animal organisation in past time is founded very largely upon our minute and intimate knowledge of the manifold relations of structural form that obtain among adult animals; on our precise knowledge of the steps by which these adult relations are established during the development of different kinds of animals; on our constantly increasing knowledge of the succession of animal forms in past time; and, generally, on the conviction that all the diverse forms of tissues, organs, and entire animals are but the expression of an infinite number of variations of a single theme, that theme being cell-division, multiplication, and differentiation. This conviction grew but slowly in men's minds. It was opposed to the cherished beliefs of centuries, and morphology rendered a necessary service when it spent all those years which have been described as "years in the wilderness" in accumulating such a mass of circumstantial evidence in favour of an evolutionary explanation of the order of animate nature as to place the doctrine of descent with modification on a secure foundation of fact. I do not believe that this foundation could have been so securely laid in any other way, and I hold that zoologists were actuated by a sound instinct in working so largely on morphological lines for forty years after Darwin wrote. For there was a large mass of fact and theory to be remodelled and brought into harmony with the new ideas, and a still larger vein of undiscovered fact to explore. The matter was difficult and the pace could not be forced. Morphology, therefore, deserves the credit of having done well in the past: the question remains, What can it do in the future?

It is evident, I think, that it cannot do much in the way of adding new truths and general principles to zoological science, nor even much more that is useful in the verification of established principles, without enlarging its scope and methods. Hitherto—or, at any rate, until very recently—it has accepted certain guiding principles on faith, and, without inquiring too closely into their validity, has occupied itself with showing that, on the assumption that these principles are true, the phenomena of animal structure, development, and succession receive a reasonable explanation.

We have seen that the fundamental principles relied upon during the last fifty years have been inheritance and variation. In every inference drawn from the comparison of one kind of animal structure with another, the morphologist founds himself on the assumption that different degrees of similitude correspond more or less closely to degrees of blood-relationship, and to-day there are probably few persons who doubt that this assumption is valid. But we must not forget that, before the publication of the "Origin of Species," it was rejected by the most influential zoologists as an idle speculation, and that it is imperilled by Mendelian experiments showing that characters may be split up and reunited in different combinations in the course of a few generations. We do not doubt the importance of the principle of inheritance, but we are not quite so sure as we were that close resemblances are due to close kinship and remoter resemblances to remoter kinship.

The principle of variation asserts that like does not beget exactly like, but something more or less different. For a long time morphologists did not inquire too closely into the question how these differences arose. They simply accepted it as a fact that they occur, and that they are of sufficient frequency and magnitude, and that a sufficient proportion of them lead in such directions that natural selection can take advantage of them. Difficulties and objections were raised, but morphology on the whole took little heed of them. Remaining steadfast in its adherence to the principles laid down by Darwin, it contented itself with piling up circumstantial evidence, and met objection and criticism with an ingenious apologetic. In brief, its labours have consisted in bringing fresh instances, and especially such instances as seemed unconformable, under the rules, and in perfecting a system of classification in illustration of the rules. It is obvious, however, that, although this kind of study is both useful and indispensable at a certain stage of scientific progress, it does not help us to form new rules, and fails altogether if the old rules are seriously called into question.

As a matter of fact, admitting that the old rules are valid, it has become increasingly evident that they are not sufficient. Until a few years ago morphologists were open to the reproach that, while they studied form in all its variety and detail, they occupied themselves too little—if, indeed, they could be said to occupy themselves at all—with the question of how form is produced, and how, when certain forms are established, they are caused to undergo change and give rise to fresh forms. As Klebs has pointed out, the forms of animals and plants were regarded as the expression of their inscrutable inner nature, and the stages passed through in the development of the individual were represented as the outcome of purely internal and hidden laws. This defect seems to have been more distinctly realised by botanical than by zoological morphologists, for Hofmeister, as long ago as 1868, wrote that the most pressing and immediate aim of the investigator was to discover to what extent external forces acting on the organism are of importance in determining its form.

If morphology was to be anything more than a descriptive science, if it was to progress any further in the discovery of the relations of cause and effect, it was clear that it must alter its methods and follow the course indicated by Hofmeister. And I submit that an inquiry into the causes which produce alteration of form is as much the province of, and is as fitly called, morphology as, let us say, a discussion of the significance of the patterns of the molar teeth of mammals or a disputation about the origin of the cœlomic cavities of vertebrated and invertebrated animals.

There remains, therefore, a large field for morphology to explore. Exploration has begun from several sides, and in some quarters has made substantial progress. It will be of interest to consider how much progress has been made along certain lines of research—we cannot now follow all the lines—and to forecast, if possible, the direction that this pioneer work will give to the morphology of the future.

I am not aware that morphologists have, until quite recently, had any very clear concept of what may be expected to underlie form and structure. Dealing, as they have dealt, almost exclusively with things that can be seen or rendered visible by the microscope, they have acquired the habit of thinking of the organism as made up of organs, the organs of tissues, the tissues of cells, and the cells as made up—of what? Of vital units of a lower order, as several very distinguished biologists would have us believe; of physiological units, of micellæ, of determinants and biophors, or of pangenes; all of them essentially morphological conceptions; the products of imagination projected beyond the confines of the visible, yet always restrained by having only one source of experience—namely, the visible. One may give unstinted admiration to the brilliancy, and even set a high value on the usefulness, of these attempts to give formal representations of the genesis of organic structure, and yet recognise that their chief utility has been to make us realise more clearly the problems that have yet to be solved.

Stripped of all the verbiage that has accumulated about them, the simple questions that lie immediately before us are: What are the causes which produce changes in the forms of animals and plants? Are they purely internal, and, if so, are their laws discoverable? Or are they partly or wholly external, and, if so, how far can we find relations of cause and effect between ascertained chemical and physical phenomena and the structural responses of living beings?

As an attempt to answer the last of these questions, we have the recent researches of the experimental morphologists and embryologists directed towards the very aim that Hofmeister proposed. Originally founded by Roux, the school of experimental embryology has outgrown its infancy and has developed into a vigorous youth. It has produced some very remarkable results, which cannot fail to exercise a lasting influence on the course of zoological studies. We have learnt from it a number of positive facts, from which we may draw very important conclusions, subversive of some of the most cherished ideas of whilom morphologists. It has been proved by experiment that very small changes in the chemical and physical environment may and do produce specific form-changes in developing organisms, and in such experiments the consequence follows so regularly on the antecedent that we cannot doubt that we have true relations of cause and effect. It is not the least interesting outcome of these experiments that, as Loeb has remarked, it is as yet impossible to connect in a rational way the effects produced with the causes which produced them, and it is also impossible to define in a simple way the character of the change so produced. For example, there is no obvious connection between the minute quantity of sulphates present in sea-water and the number and position of the characteristic calcareous spicules in the larva of a sea-urchin. Yet Herbst has shown that if the eggs of sea-urchins are reared in sea-water deprived of the needful sulphates (normally 0·26 per cent. magnesium sulphate and 0·1 per cent. calcium sulphate), the number and relative positions of these spicules are altered, and, in addition, changes are produced in other organs, such as the gut and the ciliated bands. Again, there is no obvious connection between the presence of a small excess of magnesium chloride in sea-water and the development of the paired optic vesicles. Yet Stockard, by adding magnesium chloride to sea-water in the proportion of 6 grams of the former to 100 c.c. of the latter, has produced specific effects on the eyes of developing embryos of the minnow *Fundulus heteroclitus*: the optic vesicles, instead of being formed as a widely separated pair, were caused to approach the median line, and in about 50 per cent. of the embryos experimented upon the changes were so profound as to give rise to cyclopean monsters. Many other instances might be cited of definite effects of physical and chemical agencies on particular organs, and we are now forced to admit that inherited tendencies may be completely overcome by a minimal change in the environment. The nature of the organism, therefore, is not all important, since it yields readily to influences which at one time we should have thought inadequate to produce perceptible changes in it.

It is open to anyone to argue that, interesting as experiments of this kind may be, they throw no light on the origin of permanent—that is to say, inheritable—modifications of structure. It has for a long time been a matter of common knowledge that individual plants and animals react to their environment, but the modifications induced by these reactions are somatic; the germ-plasm is not affected, therefore the changes are not inherited, and no permanent effect is produced in the characters of the race or species. It is true that no evidence has yet been produced to show that form-changes as profound as those that I have mentioned are transmitted to the offspring. So far the experimenters have not been able to rear the modified organisms beyond the larval stages, and so there are no offspring to show whether cyclopean eyes or modified forms of spicules are inherited or not. Indeed, it is possible that the balance of organisation of animals thus modified has been upset to such an extent that they are incapable of growing into adults and reproducing their kind.

But evidence is beginning to accumulate which shows that external conditions may produce changes in the germ-cells as well as in the soma, and that such changes may be specific and of the same kind as similarly produced somatic changes. Further, there is evidence that such germinal changes are inherited—and, indeed, we should expect them to be, because they are germinal.

The evidence on this subject is as yet meagre, but it is of good quality and comes from more than one source.

There are the well-known experiments of Weismann, Standfuss, Merrifield, and E. Fischer on the modification of the colour patterns on the wings of various Lepidoptera.

In the more northern forms of the fire-butterfly, *Chrysophanus* (*Polyommatus*) *phlaeas*, the upper surfaces of the wings are of a bright red-gold or copper colour with a narrow black margin, but in southern Europe the black tends to extend over the whole surface of the wing and may nearly obliterate the red-gold colour. By exposing pupæ of caterpillars collected at Naples to a temperature of 10° C. Weismann obtained butterflies more golden than the Neapolitan, but blacker than the ordinary German race, and conversely, by exposing pupæ of the German variety to a temperature of about 38° C., butterflies were obtained blacker than the German, but not so black as the Neapolitan variety. Similar deviations from the normal standard have been obtained by like means in various species of Vanessa by Standfuss and Merrifield. Standfuss, working with the small tortoiseshell butterfly (*Vanessa urticae*), produced colour aberrations by subjecting the pupæ to cold, and found that some specimens reared under normal conditions from the eggs produced by the aberrant forms exhibited the same aberrations, but in a lesser degree. Weismann obtained similar results with the same species. E. Fischer obtained parallel results with *Arctia caja*, a brightly coloured diurnal moth of the family Bombycidæ. Pupæ of this moth were exposed to a temperature of 8° C., and some of the butterflies that emerged were very dark-coloured aberrant forms. A pair of these dark aberrants were mated, and the female produced eggs, and from these larvæ and pupæ were reared at a normal temperature. The progeny was for the most part normal, but some few individuals exhibited the dark colour of the parents, though in a less degree. The simple conclusions to be drawn from the results of these experiments is that a proportion of the germ-cells of the animals experimented upon were affected by the abnormal temperatures, and that the reaction of the germ-cells was of the same kind as the reaction of the somatic cells and produced similar results. As everybody knows, Weismann, while admitting that the germ-cells were affected, would not admit the simple explanation, but gave another complicated and, in my opinion, wholly unsupported explanation of the phenomena.

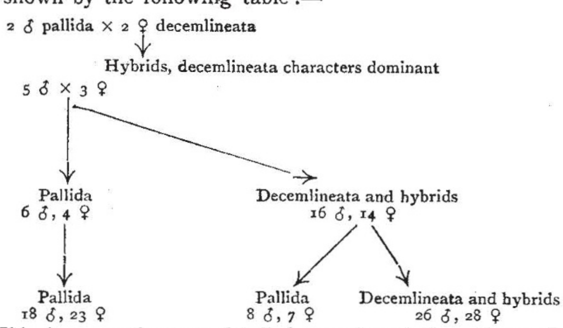
In any case this series of experiments was on too small a scale, and the separate experiments were not sufficiently carefully planned to exclude the possibility of error. But no objection of this kind can be urged against the careful and prolonged studies of Tower on the evolution of chrysomelid beetles of the genus Leptinotarsa. Leptinotarsa—better known, perhaps, by the name Doryphora—is the potato-beetle, which has spread from a centre in North Mexico southwards into the isthmus of Panama and northwards over a great part of the United States. It is divisible into a large number of species, some of which are dominant and widely ranging; others are restricted to very small localities. The specific characters relied upon are chiefly referable to the coloration and colour patterns of the epicranium, pronotum, elytra, and underside of the abdominal segments. In some species the specific markings are very constant, in others, particularly in the common and wide-ranging *L. decemlineata*, they vary to an extreme degree. As the potato-beetle is easily reared and maintained in captivity, and produces two broods every year, it is a particularly favourable subject for experimental investigation. Tower's experiments have extended over a period of eleven years, and he has made a thorough study of the geographical distribution, dispersal, habits, and natural history of the genus. The whole work appears to have been carried out with the most scrupulous regard to scientific accuracy, and the author is unusually cautious in drawing conclusions and chary of offering hypothetical explanations of his results. I have been greatly impressed by the large scale on which the experiments have been conducted, by the methods used, by the care taken to verify every result obtained, and by the great theoretical importance of Tower's conclusions. I can do no more now than allude to some of the most remarkable of them.

After showing that there are good grounds for believing that colour production in insects is dependent on the action of a group of closely related enzymes, of which chitase, the agent which produces hardening of chitin, is the most important, Tower demonstrates by a series of well-planned experiments that colours are directly modified by the action of external agencies—viz., temperature, humidity, food, altitude, and light. Food chiefly affects the subhypodermal colours of the larvæ, and does not enter much into account; the most important agents affecting the adult coloration being temperature and humidity. A *slight* increase or a *slight* decrease of temperature or humidity was found to stimulate the action of the colour-producing enzymes, giving a tendency to melanism; but a large increase or decrease of temperature or humidity was found to inhibit the action of the enzymes, producing a strong tendency to albinism.

A set of experiments was undertaken to test the question whether colouration changes induced by changed environmental conditions were inherited, increased, or dropped in successive generations. These experiments, carried on for ten lineal generations, showed that the changed conditions immediately produced their maximum effect; that they were purely somatic and were not inherited, the progeny of individuals which had been exposed to changed conditions through several generations promptly reverting when returned to normal conditions of environment. So far the results are confirmatory of the well-established proposition that induced somatic changes are not inheritable.

But it was found necessary to remove the individuals experimented upon from the influence of changed conditions during the periods of growth and maturation of the germ-cells. Potato-beetles emerge from the pupa or from hibernation with the germ-cells in an undeveloped condition, and the ova do not all undergo their development at once, but are matured in batches. The first batch matures during the first few days following emergence, then follows an interval of from four to ten days, after which the next batch of eggs is matured, and so on. This fact made it possible to test the effect of altered conditions on the maturing germ-cells by subjecting its imagos to experimental conditions during the development of some of the batches of ova and to normal conditions during the development of other batches.

In one of the experiments four male and four female individuals of *L. decemlineata* were subjected to very hot and dry conditions, accompanied by low atmospheric pressure, during the development and fertilisation of the first three batches of eggs. Such conditions had been found productive of albinic deviations in previous experiments. As soon as the eggs were laid they were removed to normal conditions, and the larvæ and pupæ reared from them were kept in normal conditions. Ninety-eight adult beetles were reared from these batches of eggs, of which eighty-two exhibited the characters of an albinic variety found in nature and described as a species under the name *pallida*; two exhibited the characters of another albinic species named *immaculothorax*, and fourteen were unmodified *decemlineatas*. This gave a clear indication that the altered conditions had produced modifications in the germ-cells which were expressed by colour changes in the adult individuals reared from them. To prove that the deviations were not inherent in the germ-plasm of the parents, the latter were kept under normal conditions during the periods of development and fertilisation of the last two batches of eggs; the larvæ and pupæ reared from these eggs were similarly subjected to normal conditions, and gave rise to sixty-one unmodified *decemlineatas*, which, when bred together, came true to type for three generations. The *decemlineata* forms produced under experimental conditions also came true to type when bred together. Of the *pallida* forms produced by experimental conditions all but two males were killed by a bacterial disease. These two were crossed with normal *decemlineata* females, and the result was a typical Mendelian segregation, as shown by the following table:—



This is a much more detailed experiment than those of Standfuss, Merrifield, and Fischer, and it shows that the changes produced by the action of altered conditions on the maturing germ-cells were definite and discontinuous, and therefore of the nature of mutations in De Vries' sense.

In another experiment Tower reared three generations of *decemlineata* to test the purity of his stock. He found that they showed no tendency to produce extreme variations under normal conditions. From this pure stock seven males and seven females were chosen, and subjected during the maturation periods of the first two batches of ova to hot and dry conditions. Four hundred and nine eggs were laid, from which sixty-nine adults were reared, constituted as follows:—

Twenty (12 ♂, 8 ♀) . .	apparently normal *decemlineata*.
Twenty-three (10 ♂, 13 ♀).	*pallida*.
Five (2 ♂, 3 ♀) . . .	*immaculotho, ax.*
Sixteen (9 ♂, 7 ♀) . .	*albida*.

These constituted lot A.

The same seven pairs of parents subjected during the second half of the reproductive period to normal conditions gave 840 eggs, from which were reared 123 adults, all *decemlineatas*. These constituted lot B. The *decemlineatas* of lot A and lot B were reared side by side under normal and exactly similar conditions. The results were striking. From lot B normal progeny were reared up to the tenth generation, and, as usual in the genus, two generations were produced in each year. The *decemlineatas* of lot A segregated into two lots in the second generation. A^1 were normal in all respects, but A^2, while retaining the normal appearance of *decemlineata*, went through five generations in a year, and this for three successive years, thus exhibiting a remarkable physiological modification, and one without parallel in nature, for no species of the genus Leptinotarsa are known which produce more than two generations in the year. This experiment is a sufficient refutation of Weismann's argument that the inheritance of induced modifications in *Vanessa urticae* is only apparent, the phenomena observed being due to the inheritance of two kinds of determinants—one from dark-coloured forms which are phyletically the oldest, and the other from more gaily coloured forms derived from the darker forms. There is no evidence whatever that there was ever a species or variety of potato-beetle that produced more than two, or at the most, and then as an exception, three broods in a year.

The modified albinic forms in this last experiment of Tower's were weakly; they were bred through two or three generations, and came true to type, but then died out. No hybridisation experiments were made with them, but in other similar experiments, which I have not time to mention in detail, modified forms produced by the action of changed conditions gave typical Mendelian characters when crossed with unmodified *decemlineatas*, thus proving that the induced characters were constant and heritable according to the regular laws.

I have thought it worth while to relate these experiments at some length, because they seem to me to be very important, and because they do not appear to have attracted the attention in this country that they deserve.

They are confirmed to a very large extent by the experiments of Prof. Klebs on plants, the results of which were published this summer in the Croonian Lecture on "Alterations of the Development and Forms of Plants as a Result of Environment." As I have only a short abstract of the Croonian Lecture to refer to, I cannot say much on this subject for fear of misrepresenting the author; but, as far as I can judge, his results are quite consistent with those of Tower. *Sempervivum funckii* and *S. acuminatum* were subjected to altered conditions of light and nutrition, with the result that striking variations, such as the transformation of sepals into petals, of petals into stamens, of stamens into petals and into carpels, were produced. Experiments were made on *Sempervivum acuminatum* with the view of answering the question whether such alterations of flowers can be transmitted. The answer was in the affirmative. The seeds of flowers artificially altered and self-fertilised gave rise to twenty-one seedlings, among which four showed surprising deviations of floral structure. In two of these seedlings all the flowers were greatly altered, and presented some of the modifications of the mother plant, especially the transformation of stamens into petals. These experiments are still in progress, and it would perhaps be premature to lay too much stress upon them if it were not for the fact that they are so completely confirmatory of the results obtained by similar methods in the animal kingdom.

I submit to you that evidence is forthcoming that external conditions may give rise to inheritable alterations of structure. Not, however, as was once supposed, by producing specific changes in the parental soma, which changes were reflected, so to speak, upon the germ-cells. The new evidence confirms the distinctions drawn by Weismann between somatic and germinal variations. It shows that the former are not inherited, while the latter are; but it indicates that the germ may be caused to vary by the action of external conditions in such a manner as to produce specific changes in the progeny resulting from it. It is no more possible at the present time to connect rationally the action of external conditions on the germ-cells with the specific results produced in the progeny than it is possible to connect cause with effect in the experiments of Herbst and Stockard; but when we compare these two kinds of experiments, we are no longer able to argue that it is inconceivable that such and such conditions acting on the germ-plasm can produce such and such effects in the next generation of adults. We must accept the evidence that things which appeared inconceivable do in fact happen, and in accepting this we remove a great obstacle from the path of our inquiries, and gain a distinct step in our attempts to discover the laws which determine the production of organic form and structure.

But such experiments as those which I have mentioned only deal with one aspect of the problem. They tell us about external conditions and the effects that they are observed to produce upon the organism. They give us no definite information about the internal changes which, taken together, constitute the response of the organism to external stimuli. As Darwin wrote, there are two factors to be taken into account—the nature of the conditions and the nature of the organism, and the latter is much the more important of the two. More important because the reactions of animals and plants are manifold; but, on the whole, the changes in the conditions are few and small in amount. Morphology has not succeeded in giving us any positive knowledge of the nature of the organism; and in this matter we must turn for guidance to the physiologists, and ask of them how far recent researches have resulted in the discovery of factors competent to account for change of structure. Perhaps the first step in this inquiry is to ask whether there is any evidence of internal chemical changes analogous in their operation to the external physical and chemical changes which we have been dealing with.

There is a great deal of evidence, but it is extremely difficult to bring it to a focus and to show its relevancy to the particular problems that perplex the zoologist. Moreover, the evidence is of so many different kinds, and each kind is so technical and complex, that it would be absurd to attempt to deal with it at the end of an address that has already been drawn out to sufficient length. But perhaps I may be allowed to allude to one or two generalisations which appear to me to be most suggestive.

We shall all agree that, at the bottom, production and change of form is due to increase or diminution of the activities of groups of cells, and we are aware that in the higher animals change of structure is not altogether a local affair, but carries with it certain consequences in the nature of correlated changes in other parts of the body. If we are to make any progress in the study of morphogeny, we ought to have as exact ideas as possible as to what we mean when we speak of the activities of cells and of correlation. On these subjects physiology supplies us with ideas much more exact than those derived from morphology.

It is, perhaps, too sweeping a generalisation to assert that the life of any given animal is the expression of the sum of the activities of the enzymes contained in it, but it seems well established that the activities of cells are, if not wholly, at all events largely, the result of the actions of the various kinds of enzymes held in combination by their living protoplasm. These enzymes are highly susceptible to the influence of physical and chemical media, and it is because of this susceptibility that the organism responds to changes in the environment, as is clearly illustrated in a particular case by Tower's experiments on the production of colour changes in potato-beetles. Bayliss and Starling have shown that in lower animals, protozoa and sponges, in which no nervous system has been developed, the response of the organism to the environment is effected by purely chemical means. In protozoa, because of their small size, the question of coadaptation of function hardly comes into question; but in sponges, many of which are of large size, the mechanism of coadaptation must also be almost exclusively chemical. Thus we learn that the simplest and, by inference, the phyletically oldest mechanism of reaction and coordination is a chemical mechanism. In higher animals the necessity for rapid reaction to external and internal stimuli has led to the development of a central and peripheral nervous system, and as we ascend the scale of organisation this assumes a greater and greater importance as a coordinating bond between the various organs and tissues of the body. But the more primitive chemical bond persists, and is scarcely diminished in importance, but only overshadowed, by the more easily recognisable reactions due to the working of the nervous system. In higher animals we may recognise special chemical means whereby chemical coadaptations are established and maintained at a normal level or in certain circumstances altered. These are the internal secretions produced by sundry organs, whether by typical secretory glands (in which case the internal secretion is something additional and different from the external secretion), or by the so-called ductless glands, such as the thyroid, the thymus, the adrenal bodies, or by organs which cannot strictly be called glands, namely, the ovaries and testes. All these produce chemical substances which, passing into the blood or lymph, are distributed through the system, and have the peculiar property of regulating or exciting the specific functions of other organs. Not, however, of all the organs, for the different internal secretions are more or less limited and local in their effects, one affecting the activity of this and another the activity of that kind of tissue or organ. Starling proposed the name hormones for the internal secretions because of their excitatory properties (ὁρμάω, to stir up, to excite).

Hormones have been studied chiefly from the point of view of their stimulating effect on the metabolism of various organs. From the morphologist's point of view, interest chiefly attaches to the possibility of their regulating and promoting the production of form. It might be expected that they should be efficient agents in regulating form, for, if changes in structure are the result of the activities of groups of cells, and the activities of cells are the results of the activities of the enzymes which they contain, and if the activities of the enzymes are regulated by the hormones, it follows that the last-named must be the ultimate agents in the production of form. It is difficult to obtain distinct evidence of this agency, but in some cases, at least, the evidence is sufficiently clear. I will confine myself to the effects of the hormones produced by the testes and ovaries. These have been proved to be intimately connected with the development of secondary sexual characters, such, for instance, as the characteristic shape and size of the horns of the bull; the comb, wattles, spurs, plumage colour, and spurs in poultry; the swelling on the index finger of the male frog; the shape and size of the abdominal segments of crabs. These are essentially morphological characters, the results of increased local activity of cell-growth and differentiation. As they are attributable to the stimulating effect of the hormone produced by the male organ in each species, they afford at least one good instance of the production of a specific change of form as the result of an internal chemical stimulus. We get here a hint as to the nature of the chemical mechanism which excites and correlates form and function in higher organisms, and, from what has just been said, we perceive that this is the most primitive of all the animal mechanisms. I submit that this is a step towards forming a clear and concrete idea of the inner nature of the organism. There is one point, and that a very important one, upon which we are by no means clear. We do not know how far the hormones themselves are liable to change, whether by the action of external conditions or by the reciprocal action of the activities of the organs to which they are related. It is at least conceivable that agencies which produce chemical disturbances in the circulating fluids may alter the chemical constitution of the hormones, and thus produce far-reaching effects. The pathology of the thyroid gland gives some ground for belief that such changes may be produced by the action of external conditions. But, however this may be, the line of reasoning that we have followed raises the expectation that a chemical bond must exist between the functionally active organs of the body and the germ-cells. For if, in the absence of a specialised nervous system, the only possible regulating and coadapting mechanism is a chemical mechanism, and if the specific activities of a cell are dependent on the enzymes which it holds in combination, the germ-cells of any given animal must be the depository of a stock of enzymes sufficient to insure the due succession of all its developmental stages as well as of its adult structure and functions. And as the number of blastomeres increases, and the need for coordination of form and function arises, before ever the rudiments of a nervous system are differentiated, it is necessary to assume that there is also a stock of appropriate hormones to supply the chemical nexus between the different parts of the embryo. The only alternative is to suppose that they are synthesised as required in the course of development. There are grave objections to this supposition. All the evidence at our disposal goes to show that the potentialities of germ-cells are determined at the close of the maturation divisions. Following the physiological line of argument, it must be allowed that in this connection "potentiality" can mean nothing else than chemical constitution. If we admit this, we admit the validity of the theory, advanced by more than one physiologist, that heritable "characters" or "tendencies" must be identified with the enzymes carried in the germ-cells. If this be a true representation of the facts, and if the most fundamental and primitive bond between one part of an organism and another is a chemical bond, it can hardly be the case that germ-cells—which, *inter alia*, are the most primitive, in the sense of being the least differentiated, cells in the body—should be the only cells which are exempt from the chemical influences which go to make up the coordinate life of the organism. It would seem, therefore, that there is some theoretical justification for the inheritance of induced modifications, provided that these are of such a kind as to react chemically on the enzymes contained in the germ-cells.

One further idea that suggests itself to me and I have done. Is it possible that different kinds of enzymes exercise an inhibiting influence on one another; that germ-cells are "undifferentiated" because they contain a large number of enzymes, none of which can show their activities in the presence of others, and that what we call "differentiation" consists in the segregation of the different kinds into separate cells, or perhaps, prior to cell-formation, into different parts of the fertilised ovum, giving rise to the phenomenon known to us as prelocalisation? The idea is purely speculative; but, if it could be shown to have any warrant, it would go far to assist us in getting an understanding of the laws of the production of form.

I have been wandering in territories outside my own province, and I shall certainly be told that I have lost my way. But my thesis has been that morphology, if it is to make useful progress, must come out of its reserves and explore new ground. To explore is to tread unknown paths, and one is likely to lose one's way in the unknown. To stay at home in the environment of familiar ideas is no doubt a safe course, but it does not make for advancement. Morphology, I believe, has as great a future before it as it has a past behind it, but it can only realise that future by leaving its old home, with all its comfortable furniture of well-worn rules and methods, and embarking on a journey, the first stages of which will certainly be uncomfortable, and the end is far to seek.

SECTION E.

GEOGRAPHY.

OPENING ADDRESS BY A. J. HERBERTSON, M.A., PH.D., PROFESSOR OF GEOGRAPHY IN THE UNIVERSITY OF OXFORD, PRESIDENT OF THE SECTION.

GEOGRAPHY AND SOME OF ITS PRESENT NEEDS.

Geographical Progress in the Last Decade.

AT the close of a reign which has practically coincided with the first decade of a new century, it is natural to look back and summarise the progress of geography during the decade. At the beginning of a new reign it is equally natural to consider the future. Our new Sovereign is one of the most travelled of men. No monarch knows the World as he knows it; no monarch has ruled over a larger Empire or seen more of his dominions. His advice has been to wake up, to consider and to act. This involves taking existing geographical conditions into account. It will be in consonance with this advice if I pay more attention to the geography of the present and future than to that of the past, and say more about its applications than about its origins. Yet I do so with some reluctance, for the last decade has been one of the most active and interesting in the history of our science.

Among the many geographical results of work in the past decade a few may be mentioned. The measurement of new and the remeasurement of old arcs will give us better data for determining the size and shape of the Earth. Surveys of all kinds, from the simple route sketches of the traveller to the elaborate cadastral surveys of some of the more populous and settled regions have so extended our knowledge of the surface features of the Earth that a map on the scale of 1 : 1,000,000 is not merely planned, but actually partly executed. Such surveys and such maps are the indispensable basis of our science.

The progress of oceanography has also been great. The soundings of our own and other Admiralties, of scientific oceanographical expeditions, and those made for the purpose of laying cables, have given us much more detailed knowledge of the irregularities of the ocean floor. An international map of oceanic contours, due to the inspiration and munificence of the Prince of Oceanographers and of Monaco, has been issued during the decade, and so much new material has accumulated that it is now being revised. A comparison of the old and new editions of Krümmel's "Ozeanographie" shows us the immense advances in this subject.

Great progress has been made on the geographical side of meteorology and climate. The importance of this knowledge for tropical agriculture and hygiene has led to an increase of meteorological stations all over the hot belt—the results of which will be of value to the geographer. Mr. Bartholomew's "Atlas of Meteorology" appeared at the beginning, and Sir John Eliot's "Meteorological Atlas of India" at the end, of the decade. Dr. Hann's "Lehrbuch" and the new edition of his "Climatology," Messrs. Hildebrandsson and Teisserenc de Bort's great work, and the recent studies of the Upper Atmosphere, are among the landmarks of progress. The record is marred only by the closing of Ben Nevis Observatory at the moment when its work would have been most necessary. To appreciate the progress of climatology it is only necessary to compare the present number and distribution of meteorological stations with those given in Bartholomew's Atlas of 1899. I have not time to recapitulate the innumerable studies of geographical value issued by many meteorological services, observatories, and observers—public and private—but I may direct attention to the improved weather maps and to the excellent pilot charts of the North Atlantic and of the Indian Ocean published monthly by our Meteorological Office.

Lake studies have also been a feature of this decade, and none are so complete or so valuable as the Scottish Lakes Survey—a work of national importance, undertaken by private enthusiasm and generosity. We have to congratulate Sir John Murray and Mr. Pullar on the completion of a great work.

In Geology, I might note that we now possess a map of Europe on a scale of 1 : 1,500,000 prepared by international cooperation, and also one of North America on a smaller scale; both invaluable to the geographer. The thanks and congratulations of all geographers are due to Prof. Suess on the conclusion of his classical work on the Face of the Earth, the first comprehensive study of the main divisions and characteristics of its skeleton. English readers are indebted to Prof. and Miss Sollas for the brilliant English translation which they have prepared.

A new movement, inspired mainly by Prof. Flahault in France, Prof. Geddes in this country, Profs. Engler, Drude, and Schimper in Germany, has arisen among botanists, and at last we have some modern botanical geography which is really valuable to the geographer. I wish we could report similar progress in zoological geography, but that, I trust, will come in the next decade.

I pass over the various expensive arbitrations and commissions to settle boundary disputes which have in many cases been due to geographical ignorance, also the important and fascinating problems of the growth of our knowledge of the distribution of economic products and powers, existing and potential, and the new geographical problems for statesmen due to the political, economic revolutions in Japan and China.

It is quite impossible to deal with the exploration of the decade. Even in the past two years we have had Peary and Shackleton, Stein and Hedin, the Duke of the Abruzzi, and a host of others returning to tell us of unknown or little known parts of the globe. We hope to hear soon from Dr. Charcot the results of the latest investigations in the Antarctic.

Further work is being undertaken by Scott and his companions, by Bruce, Amundsen, Filchner, and others in the South or North Polar ice worlds; by Longstaff, Bruce, and others in the mountains of India and Central Asia; by Goodfellow and Ryder in New Guinea; and by many other expeditions.

One word of caution may perhaps be permitted. There is a tendency on the part of the public to confuse geographical exploration and sport. The newspaper reporter naturally lays stress on the unusual in any expedition, the accidental rather than the essential, and those of us who have to examine the work of expeditions know how some have been unduly boomed because of some adventurous element, while others have not received adequate popular recognition because all went well. The fact that all went well is in itself a proof of competent organisation. There is no excuse for us in this section if we fall into the journalist's mistake, and we shall certainly be acting against the interests of both our science and our section if we do so.

The Position of Geography in the Association.

It was not my intention in this address to raise the question of what is Geography, but various circumstances make it desirable to say a few words upon it. We are all the victims of the geographical teaching of our youth, and it is easy to understand how those who have retained unchanged the conceptions of geography they gained at school many years ago cavil at the recognition of geography as a branch of science. Moreover, the geography of the schools still colours the conceptions of some geographers who have nevertheless done much to make school geography scientific and educational. Many definitions of geography are consequently too much limited by the arbitrary but traditional division of school subjects. In schools, tradition and practical convenience have, on the whole rightly, determined the scope of the different subjects. Geography in schools is best defined as the study of the Earth as the home of Man. Its limits should not be too closely scrutinised in schools, where it should be used freely as a coordinating subject.

The present division into sections of the British Association is also largely a matter of practical convenience; but we are told that the present illogical arrangement of sections distresses some minds. No doubt there are some curious anomalies. The most glaring, perhaps, is that of combining mathematics with physics—as if mathematical methods were not used in any other subject.

There is undoubtedly a universal tendency to subdivision and an ever-increasing specialisation; but there is also an ever-growing interdependence of different parts of science. The British Association is unquestionably bound to take the latter into account as well as the former. At present this is chiefly done by joint meetings of sections: a wise course, of which this section has been one of the chief promoters. It is possible that some more systematic grouping of sections might be well advised, but such a reform should be systematic, and not piecemeal. It is one which raises the whole question of the classification of knowledge. This is so vast a problem, and one on which such divergent opinions are held, that I must apologise for venturing to put forward some tentative suggestions.

It might be found desirable to take as primary divisions the Mathematical, Physical, Biological, Anthropological, and Geographical groups. Mathematical applications might also be considered in each of the sections which use mathematical notations. In the Physical Group there should be the subdivisions Physics and Chemistry. Each would devote a certain proportion of time to its applied aspects, or these might be dealt with in sub-sections, which would include Engineering and Applied Chemistry. In the Biological Group there would be Botany, Zoology, in both cases including Palæontology and Embryology, and Applied Biology, which would be dealt with in one or other of the ways I have suggested, and would include Agriculture, Fisheries, &c. (Medicine we leave out at present.) In the Anthropological Group, in addition to the present Anthropology and Economics, there should be a section on Psychology, which might or might not be attached to Physiology, and have the Education Section as a practical appendage. In the Geographical Group there would be Geography and Geology, the practical applications of Geography and Geology being considered in joint meetings with other sections or else in sub-sections—for instance, Geography and Physics for questions of Atmospheric and Oceanic Circulation, Geography and Economics for questions of Transportation, &c.

The Need for Classification and Notation in Geomorphology, &c.

So much, then, for the classification of Geography with reference to the other sciences. I should like to say a few words about the subdivisions of geography and the vexed question of terminology.

In the scheme of the Universe it is possible to consider the Earth as a unit, with its own constitution and history. It has an individuality of its own, though for the astronomer it is only one example of a particular type of heavenly bodies. As geographers, we take it as our unit individual in the same way that an anatomist takes a man. We see that it is composed of different parts, and we try to discover what these are, of what they are composed, what their function is, what has been their history.

One fundamental division is into land, water, and air. Each has its forms and its movements. The forms are more obvious and persistent in the land. They are least so in the atmosphere, though forms exist—some of which are at times made visible by clouds, and many can be clearly discerned on isobaric charts. The land is the temporarily permanent; the water and atmosphere the persistently mobile, the latter more so than the former. The stable forms of the land help to control the distribution and movements of the waters, and to a less extent those of the atmosphere. How great the influence of the distribution of land and water is on the atmosphere may be seen in the monsoon region of eastern Asia.

The study of the land, the ocean, and the atmosphere has resulted in the growth of special branches of knowledge—Geomorphology, Oceanography, and Climatology. Each is indispensable to the geographer, each forms an essential part of the geographical whole. Much research work is and will be carried on in each by geographers who find their geographical studies hampered for the lack of it. As geographical progress is to a considerable extent conditioned by progress in these subjects, it would be legitimate to examine their needs. Time, however, will admit only a note on one of the barriers to progress in geomorphology—the lack of a good classification and notation.

Geomorphology deals with the forms of the land and their shaping. Three things have to be kept clearly in view: (1) The structure, including the composition, of the more permanent substance of the form; (2) the forces which are modifying it; and (3) the phase in the cycle of forms characteristic of such structure acted on by such forms. We may say that any form is a function of structure, process, and time. The matter is even more complicated, for we have instances, *e.g.* in antecedent drainage systems, of the conditions of a previous cycle affecting a subsequent one—a kind of heredity of forms which cannot be neglected.

The geomorphologist is seeking for a genetic classification of forms, and in the works of Betrand, Davis, de la Noë and de Margerie, Penck, Richthofen, Suess, and Supan and their pupils are being accumulated the materials for a more complete and systematic classification of forms. As you all know, the question of terms for the manifold land-forms is a difficult one, and apt to engender much more controversy than the analysis of the forms themselves. I believe that we shall find it advantageous to adopt some notation analogous to that of the chemists. I have not yet had time to work such a notation out in detail, but it might take the form of using different symbols for the three factors noted above—say, letters for different kinds of structure, Arabic figures for processes and Roman figures for the stage of a cycle the form has reached.

Take a very simple set of structures and indicate each by a letter:—

			Undisturbed	Faulted
Structure	homogeneous		A	A′
	layered	horizontal	B	B′
		tilted	C	C′
		folded	D	D′
	mixed		E	E′

If pervious or impervious, a *p* or an *i* could be added—*e.g.* a tilted limestone with faults would be C′*p*.

Next, indicate the commoner erosion processes by Arabic numerals:—

Process	moving water	1
	ice	2
	wind	3
	sea	4

One process may have followed another, *e.g.* where a long period of ice erosion has been followed by water erosion we might write 2·1, where these alternate annually, say 21.

The phase of the cycle might be denoted by Roman figures. A scale of V might be adopted, and I, III, and V used for youthful, middle-aged, and old-aged, as this has been called, or early, middle, and late phases, as I prefer to term them. II and IV would denote intermediate phases.

A scarped limestone ridge in a relatively mature phase like the Cotswolds would be, if we put the process first, 1 C^1 III.; a highland like the Southern Uplands of Scotland would be denoted by the formula 1. 2·1 E^1 III.

This is the roughest suggestion, but it shows how we could label our cases of notes and pigeon-hole our types of forms—and prevent for the present undue quarrelling over terms.[1] No doubt there would be many discussions, for example, about the exact phase of the cycle, whether ice, in addition to water, has been an agent in shaping this or that form, and so on. But, after all, these discussions would be more profitable than quarrels as to which descriptive term, or place-name, or local usage should be adopted to distinguish it.

The use of such notations in geographical problems is not unknown. They were employed by Köppen in his classification of climate; and now, in the case of climatology, there is coming to be a general consensus of opinion as to what are the chief natural divisions, and the use of figures and letters to indicate them has been followed by several other authors. This should also be attempted for oceanography.

If any international agreement of symbols and colours could be come to for such things it would be a great gain, and I hope to bring this matter before the next International Geographical Congress.

[1] What I wish to make clear is that it is not necessary to invent a new term for every new variety of land form as soon as it is recognised. It will suffice at first to be able to label it. The notation will also stimulate the search for and recognition of new varieties.

The Need for Selecting Natural Geographical Units.

We have still to come to Geography proper, which considers land, water, and air, not merely separately but as associated together. What are the units smaller than the whole Earth with which our science has to deal?

When we fix our attention on parts of the Earth and ask what is a natural unit, we are hampered by preconceptions. We recognise species, or genera, families, or races as units—but they are abstract rather than concrete units. The reason for considering them as units is that they represent a historical continuity. They have not an actual physical continuity such as the component parts of an individual have. Concrete physical continuity in the present is what differentiates the geographical unit. Speaking for myself, I should say that every visible concrete natural unit on the Earth's surface consisting of more than one organic individual is a geographical unit. It is a common difficulty not to be able to see the wood for the trees; it is still more difficult to recognise that the wood consists of more than trees, that it is a complex of trees and other vegetation, fixed to a definite part of the solid earth and bathed in air. We may speak of a town or State as composed of people, but a complete conception of either must include the spacial connections which unite its parts. A town is not merely an association of individuals, nor is it simply a piece of land covered with streets and buildings; it is a combination of both.

It is true that in determining the greater geographical units, man need not be taken into account. We are too much influenced by the mobility of man, by his power to pass from one region to another, and we are apt to forget that his influence on his environment is negligible except when we are dealing with relatively small units. The geographer will not neglect man; he will merely be careful to prevent himself from being unduly influenced by the human factor in selecting his major units.

Some geographers and many geologists have suggested that land forms alone need be taken into account in determining these larger geographical units. Every different recognisable land form is undoubtedly a geographical unit. A vast lowland, such as that which lies to the east of the Rocky Mountains, is undoubtedly a geographical unit of great importance, but its geographical subdivisions are not necessarily orographical. The shores of the Gulf of Mexico could not be considered as geographically similar to those of the Arctic Ocean, even if they were morphologically homologous. The lowlands of the polar regions are very different from those at or near the tropics. The rhythm of their life is different, and this difference is revealed in the differences of vegetation.

I wish to lay great stress on the significance of vegetation to the geographer for the purposes of regional classification. I do not wish to employ a biological terminology nor to raise false analogies between the individual organism and the larger units of which it is a part, but I think we should do well to consider what may be called the life or movement going on in our units as well as their form. We must consider the seasonal changes of its atmospheric and of its water movements, as well as the parts of the Earth's crust which they move over and even slightly modify. For this purpose a study of climatic regions is as necessary as a study of morphological regions, and the best guides to the climatic regions are the vegetation ones.

By vegetation I mean not the flora, the historically related elements, but the vegetable coating, the space-related elements. Vegetation in this sense is a geographical phenomenon of fundamental importance. It indicates quality—quality of atmosphere and quality of soil. It is a visible synthesis of the climatic and edaphic elements. Hence the vast lowlands of relatively uniform land features are properly divided into regions according to vegetation—tundra, pine forest, deciduous forest, warm evergreen forest, steppe, and scrub. Such differences of vegetation are full of significance even in mountainous areas.

The search after geographical unity—after general features common to recognisable divisions of the Earth's surface, the analysis of these, their classification into types, the comparisons between different examples of the types—seem to me among the first duties of a geographer.

Two sets of studies and maps are essential—topographical and vegetational—the first dealing with the superficial topography and its surface irregularities, the latter relating to the quality of climate and soil.

Much has been said in recent years—more particularly from this Presidential chair—on the need for trustworthy topographical maps. Without such maps no others can be made. But when they are being made it would be very easy to have a general vegetational map compiled. Such maps are even more fundamental than geological maps, and they can be constructed more rapidly and cheaply. Every settled country, and more particularly every partially settled country, will find them invaluable if there is to be any intelligent and systematic utilisation of the products of the country. Possessing both sets of maps, the geographer can proceed with his task.

This task, I am assuming, is to study environments, to examine the forms and qualities of the Earth's surface, and to recognise, define, and classify the different kinds of natural units into which it can be divided. For these we have not as yet even names. It may seem absurd that there should be this want of terms in a subject which is associated in the minds of most people with a superfluity of names. I have elsewhere suggested the use of the terms major natural region, natural region, district, and locality to represent different grades of geographical units, and have also attempted to map the seventy or eighty major natural regions into which the Earth's surface is divided, and to classify them into about twenty types. These tentative divisions will necessarily become more accurate as research proceeds, and the minor natural regions into which each major natural region should be divided will be definitely recognised, described, and classified. Before this can be done, however, the study of geomorphology and of plant formations must be carried far beyond the present limits.

The value of systematic and exhaustive studies of environment such as those I suggest can hardly be exaggerated. Without them all attempts to estimate the significance of the environment must be superficial guesswork. No doubt it is possible to exaggerate the importance of the environmental factor, but it is equally possible to undervalue it. The truly scientific plan is to analyse and to evaluate it. Problems of the history of human development, as well as those of the future of human settlements, cannot be solved without this. For the biologist, the historian, the economist, the statesman, this work should be carried out as soon and as thoroughly as is possible in the present state of our knowledge.

A beginning of systematic geographical studies has also been made at the opposite end of the scale in local geographical monographs. Dr. H. R. Mill, one of the pioneers of geography in this country and one of my most distinguished predecessors in this chair, has given us in his study of south-west Sussex an admirable example of the geographical monograph proper, which takes into account the whole of the geographical factors involved. He has employed quantitative methods so far as these could be applied, and in doing so has made a great step in advance. Quantitative determinations are at least as essential in geographical research as the consideration of the time factor. At Oxford we are continuing Dr. Mill's work. We require our diploma students to select some district shown on a sheet of this map for detailed study by means of map measurements, an examination of statistics and literature which throw light on the geographical conditions, and, above all, by field work in the selected district. Every year we are accumulating more of these district monographs, which ought, in their turn, to be used for compiling regional monographs dealing with the larger natural areas. In recent years excellent examples of such regional monographs have come from France and from Germany.

The geomorphologist and the sociologist have also busied themselves with particular aspects of selected localities. Prof. W. M. Davis, of Harvard, has published geomorphological monographs which are invaluable as models of what such work should be. In a number of cases he has passed beyond mere morphology and has directed attention to the organic responses associated with each land form. Some of the monographs published under the supervision of the late Prof. Ratzel, of Leipzig, bring out very clearly the relation between organic and inorganic distributions, and some of the monographs of the Le Play school incidentally do the same.

The Double Character of Geographical Research.

To carry on geographical research, whether on the larger or the smaller units, there is at present a double need—in the first place, of collecting new information, and, in the second place, of working up the material which is continually being accumulated.

The Need for the Systematic Collection of Data.

The first task—that of collecting new information—is no small one. In many cases it must be undertaken on a scale that can be financed only by Governments. The Ordnance and Geological Surveys of our own and other countries are examples of Government departments carrying on this work. We need more of them. The presidents of the Botanical and Anthropological Sections are, I understand, directing the attention of the Association to the urgent necessity for complete Botanical and Anthropological Surveys of the kingdom. All geographers will warmly support their appeal, for the material which would be collected through such surveys is essential to our geographical investigations.

Another urgent need is a Hydrographical Department, which would cooperate with Dr. Mill's rainfall organisation. It would be one of the tasks of this department to extend and coordinate the observations on river and lake discharge, which are so important from an economic or health point of view that various public bodies have had to make such investigations for the drainage areas which they control. Such research work as that done by Dr. Strahan for the Exe and Medway would be of the greatest value to such a department, which ought to prepare a whether by government departments or by private We shall see how serious the absence of such a department is if we consider how our water supply is limited, and how much of it is not used to the best advantage. We must know its average quantity and the extreme variations of supply. We must also know what water is already assigned to the uses of persons and corporations, and what water is still available. We shall have to differentiate between water for the personal use of man and animals, and water for industrial purposes. The actualities and the potentialities can be ascertained, and should be recorded and mapped.

The Need for the Application of Geographical Methods to already Collected Data.

In the second direction of research—that of treating from the geographical standpoint the data accumulated, whether by Government departments or by private initiative—work has as yet hardly been begun.

The topographical work of the Ordnance Survey is the basis of all geographical work in our country. The Survey has issued many excellent maps, none more so than the recently published half-inch contoured and hill-shaded maps with colours "in layers." Its maps are not all above criticism; for instance, few can be obtained for the whole kingdom having precisely the same symbols. It has not undertaken some of the work that should have been done by a national cartographic service—for instance, the lake survey. Nor has it yet done what the Geological Survey has done—published descriptive accounts of the facts represented on each sheet of the map. From every point of view these are great defects; but in making these criticisms we must not forget (1) that the Treasury is not always willing to find the necessary money, and (2) that the Ordnance Survey was primarily made for military purposes, and that the latest map it has issued has been prepared for military reasons. It has been carried out by men who were soldiers first and topographers after, and did not necessarily possess geographical interests.

The ideal geographical map, with its accompanying geographical memoir, can be produced only by those who have had a geographical training. Dr. Mill, in the monograph already referred to, has shown us how to prepare systematised descriptions of the one-inch map sheets issued by the Ordnance Survey.

The preparation of such monographs would seem to fall within the province of the Ordnance Survey. If this is impossible, the American plan might be adopted. There the Geological Survey, which is also a topographical one, is glad to obtain the services of professors and lecturers who are willing to undertake work in the field during vacations. It should not be difficult to arrange similar cooperation between the universities and the Ordnance Survey in this country. At present the Schools of Geography at Oxford and at the London School of Economics are the only university departments which have paid attention to the preparation of such monographs, but other universities will probably fall into line. Both the universities and the Ordnance Survey would gain by such cooperation. The chief obstacle is the expense of publication. This might reasonably be made a charge on the Ordnance Survey, on condition that each monograph published were approved by a small committee on which both the universities and the Ordnance Survey were represented.

The Geological Survey has in recent years issued better and cheaper one-inch maps, and more attention has been given to morphological conditions in the accompanying monographs; but it is necessary to protest against the very high prices which are now being asked for the older hand-coloured maps. The new quarter-inch map is a great improvement on the old one, but we want "drift" as well as "solid" editions of all the sheets. The geographer wants even more than these a map showing the quality of the solid rock, and not merely its age. He has long been asking for a map which would indicate the distribution of clay, limestone, sandstone, &c., and when it is prepared on the quarter-inch, or better on the half-inch, scale the study of geomorphology and of geography will receive a very great stimulus and assistance.

The information which many other Government departments are accumulating would also become much more valuable if it were discussed geographically. Much excellent geographical work is done by the Admiralty and the War Office. The Meteorological Office collects statistics of the weather conditions from a limited number of stations; but its work is supplemented by private societies which are not well enough off to discuss the observations they publish with the detail which these observations deserve. The Board of Agriculture and Fisheries has detailed statistical information as to crops and live stock for the geographer to work up. From the Board of Trade he would obtain industrial and commercial data, and from the Local Government Board vital and other demographic statistics. At present most of the information of these departments is only published in statistical tables.

Statistics are all very well, but they are usually published in a tabular form, which is the least intelligible of all. Statistics should be mapped, and not merely be set out in columns of figures. Many dull Blue-books would be more interesting and more widely used if their facts were properly mapped. I say *properly* mapped, because most examples of so-called statistical maps are merely crude diagrams, and are often actually misleading. It requires a knowledge of geography in addition to an understanding of statistical methods to prepare intelligible statistical maps. If Mr. Bosse's maps of the population of England and Wales in Bartholomew's Survey Atlas are compared with the ordinary ones, the difference between a geographical map and a cartographic diagram will be easily appreciated.

The coming census, and to a certain extent the census of production, and probably the new land valuation, will give more valuable raw material for geographical treatment. If these are published merely in tabular form they will not be studied by any but a few experts. Give a geographer with a proper staff the task of mapping them in a truly geographical way, and they will be eagerly examined even by the man in the street, who cannot fail to learn from them. The representation of the true state of the country in a clear, graphic, and intelligible form is a patriotic piece of work which the Government should undertake. It would add relatively little to the cost of the census, and it would infinitely increase its value.

The Need of Reorganising the Geographical Factor in Imperial Problems.

With such quantitative information geographically treated and with a fuller analysis of the major natural regions it ought to be possible to go a step further and to attempt to map the economic value of different regions at the present day. Such maps would necessarily be only approximations at first. Out of them might grow other maps prophetic of economic possibilities. Prophecy in the scientific sense is an important outcome of geographical as well as of other scientific research. The test of geographical laws, as of others, is the pragmatic one. Prophecy is commonly but unduly derided. Mendeléeff's periodic law involved prophecies which have been splendidly verified. We no longer sneer at the weather prophet. Efficient action is based on knowledge of cause and consequence, and proves that a true forecast of the various factors has been made. Is it too much to look forward to the time when the geographical prospector, the geographer who can estimate potential geographical values, will be as common as and more trustworthy than the mining prospector?

The day will undoubtedly come when every Government will have its Geographical-Statistical Department dealing with its own and other countries—an Information Bureau for the administration corresponding to the Department of Special Inquiries at the Board of Education. At present there is no geographical staff to deal geographically with economic matters or with administrative matters. Yet the recognition of and proper estimation of the geographical factor is going to be more and more important as the uttermost ends of the Earth are bound together by visible steel lines and steel vessels or invisible impulses which require no artificial path or vessel as their vehicle.

The development of geographical research along these lines in our own country could give us an Intelligence Department of the kind, which is much needed. If this were also done by other States within the Empire, an Imperial Intelligence Department would gradually develop. Thinking in continents, to borrow an apt phrase of Mr. Mackinder's, might then become part of the necessary equipment of a statesman instead of merely an after-dinner aspiration. The country which first gives this training to its statesmen will have an immeasurable advantage in the struggle for existence.

The Need for the adequate Endowment of Geography at the Universities.

Our universities will naturally be the places where the men fit to constitute such an Intelligence Department will be trained. It is encouraging, therefore, to see that they are taking up a new attitude towards geography, and that the Civil Service Commissioners, by making it a subject for the highest Civil Service examinations, are doing much to strengthen the hands of the universities. When the British Association last met in Sheffield geography was the most despised of school subjects, and it was quite unknown in the universities. It owed its first recognition as a subject of university status to the stimulus and generous financial support of the Royal Geographical Society and the brilliant teaching of Mr. Mackinder at Oxford. Ten years ago Schools of Geography were struggling into existence at Oxford and Cambridge, under the auspices of the Royal Geographical Society. A single decade has seen the example of Oxford and Cambridge followed by nearly every university in Great Britain, the University of Sheffield among them. In Dr. Rudmose Brown it has secured a scientifically trained traveller and explorer of exceptionally wide experience, who will doubtless build up a Department of Geography worthy of this great industrial capital. The difficulty, however, in all universities is to find the funds necessary for the endowment, equipment, and working expenses of a Geographical Department of the first rank. Such a department requires expensive instruments and apparatus, and, since the geographer has to take the whole World as his subject, it must spend largely on collecting, storing, and utilising raw material of the kind I have spoken of Moreover, a professor of geography should have seen much of the World before he is appointed, and it ought to be an important part of his professional duties to travel frequently and far. I have never been able to settle to my own satisfaction the maximum income which a department of geography might usefully spend, but I have had considerable experience of working a department the income of which was not very far above the minimum. Until now the Oxford School of Geography has been obliged to content itself with three rooms and to make these suffice, not merely for lecture-rooms and laboratories, but also for housing its large and valuable collection of maps and other materials. This collection is far beyond anything which any other university in this country possesses, but it shrinks into insignificance beside that of a rich and adequately supported Geographical Department like that of the University of Berlin. This fortunate department has an income of about 6000*l.* a year, and an institute built specially for its requirements at a cost of more than 150,000*l.*, excluding the site. In Oxford we are most grateful to the generosity of Mr. Bailey, of Johannesburg, which will enable the School of Geography to add to its accommodation by renting for five years a private house, in which there will temporarily be room for our students and for our collections, especially those relating to the geography of the Empire. But even then we can never hope to do what we might if we had a building specially designed for geographical teaching and research. Again, Lord Brassey and Mr. Douglas Freshfield, a former President of this Section, have each generously offered 500*l.* towards the endowment of a professorship if other support is forthcoming. All this is matter for congratulation, but I need hardly point out that a professor with only a precarious working income for his department is a person in a far from enviable position. There is at present no permanent working income guaranteed to any Geographical Department in the country, and so long as this is the case the work of all these departments will be hampered and the training of a succession of competent men retarded. I do not think that I can conclude this brief address better than by appealing to those princes of industry who have made this great city of Sheffield what it is to provide for the Geographical Department of the University on a scale which shall make it at once a model and a stimulus to every other university in the country and to all benefactors of universities.

IONISATION OF GASES AND CHEMICAL CHANGE.[1]

THE term "catalytic" was introduced by Berzelius to describe a number of chemical actions which would only take place in the presence of a third substance, which itself was apparently unchanged throughout the reaction. The first cases of such actions were investigated by Sir Humphry Davy in 1817. He showed that many mixtures of gases were caused to unite in the presence of finely divided platinum at temperatures far below those at which union ordinarily took place. Some years afterwards Faraday investigated similar actions, and attempted to explain them by a supposed condensation of the gases on the surface of the metal.

Thirty years ago Prof. H. B. Dixon investigated the behaviour of carbon monoxide and oxygen when they were dried as completely as possible, and he discovered that in these circumstances electric sparks caused no explosion. Some years before Wanklyn had discovered that purified chlorine did not act on sodium, but he did not identify the impurity, now known to be a trace of water, which causes the vigorous action which takes place in ordinary circumstances.

In 1882 Cowper investigated the action of dried chlorine on several metals, and found that the removal of moisture in many cases inhibited the reaction.

In the following year, working in Prof. Dixon's laboratory at Balliol College, I found that purified carbon could be heated to redness in dried oxygen, and that sulphur and phosphorus could be distilled in the same gas without burning. In the investigations which followed, some thirty simple reactions have been tried by myself and others. It has been shown that hydrogen and chlorine can be exposed to light without explosion, ammonia and hydrogen chloride mixed without union, sulphur trioxide can be crystallised on lime, ammonium chloride and mercurous chloride give undissociated vapours, hydrogen and oxygen can be exposed to a red heat without explosion, and lastly, in 1907, nitrogen trioxide was obtained as an undissociated gas for the first time by carefully drying the liquid and evaporating into a dried atmosphere.

The amount of water necessary to carry on these chemical reactions is extremely small, certainly less than 1 mg. in 300,000 litres. There is no accepted explanation of its catalytic effect, and in the same way the catalytic power of platinum is still a mystery. Dr. Armstrong's theory, that only water which is capable of conducting an electric current is capable of bringing about these chemical actions, seems to be supported by the fact that water can be formed in heated tubes containing very pure hydrogen and oxygen without the explosive combination of the gases taking place. That great purity does affect the chemical activity of water was proved by an experiment shown during the lecture. Two tubes, one containing water of a very high degree of purity and the other containing ordinary distilled water, were placed side by side in the lantern. Into each was filtered some liquid sodium amalgam, and while vigorous effervescence was seen in the less pure water, the very pure specimen was apparently without action for some minutes, and even at the end of the lecture its action had not attained the same vigour as that in the other tube.

In 1893 Sir J. J. Thomson (*Phil. Mag.*, xxxvi., 321) showed that if the combination of atoms in a molecule is electrical in its nature, the presence of liquid drops of water, or drops of any liquid of high specific inductive capacity, would be sufficient to cause a loosening of the tie between the atoms, and this might result in chemical combination of the partially freed atoms to form new molecules. He showed in the same paper that drying a gas very completely stopped the passage of a current of 1200 volts. In the same year I was able in the same way to prevent the passage of discharge from an induction coil, a discharge which would traverse a spark gap of three times the distance in undried gas.

Shortly after the discovery of Röntgen rays, it was found that they would ionise a gas through which they passed. At the time it was thought that this ionisation was similar to that taking place in electrolysis. If this were so the rays would probably cause chemical union to take place even in a dried gas, and accordingly Prof. Dixon and I undertook some experiments on the subject, which were published in a joint paper (Chem. Soc. Jour., 1896). The results were negative; no chemical action could be detected. Since that time the ionisation of gases has been shown to be of quite a different nature. The negative ion has been shown to be a particle of the mass of about 1/1500th that of the hydrogen atom, and the positive ion is the residue. Since the ionisation of gases is different from that in electrolysis, the retention of this term is much to be deprecated. It is suggested that the term ionisation should be retained for electrolytic dissociation, and for the different process which takes place in gases under the action of Röntgen rays, &c., a new name, electromerism, should be adopted. The electron would thus be the negative electromer.

It is probable that electrolysis and true ionisation may take place in gases, as in the decomposition of steam by electric sparks of a particular length. An experiment recently devised seems to show that in mercury vapour, which ordinarily consists of atoms, something of the nature of ionisation without electrolysis can take place. If oxygen be admitted to the interior of a mercury lamp from which the current has just been cut off, a considerable quantity of mercuric oxide is produced, although the temperature of the lamp (about 150°) is far lower than would suffice to bring about the union of ordinary mercury vapour with oxygen.

In order to test further the question as to whether electromerism can bring about chemical change, I have investigated the action of radium bromide on very pure and dry hydrogen and oxygen. The gases were sealed up with some radium bromide contained in an open silica tube. The containing vessel was provided with a vacuum gauge, by means of which the combination of 1/5000th

[1] Discourse delivered at the Royal Institution on Friday, March 11, by Dr. H. Brereton Baker, F.R.S.