

value of underdrainage and other methods of reclaiming alkali lands.' The appropriation for this Division is \$5000 greater than last year.

The fund used in making investigations as to the adaptability of the South for profitable tea culture was increased from \$1000 to \$5000.

The fund provided for the Division of Publications is \$130,020, an increase of \$28,360. The amount set aside for the printing of Farmers' Bulletins is \$22,500 greater than last year. Four-fifths of the Farmers' Bulletins are to be sent out by members of Congress instead of two-thirds as formerly.

Other items of the appropriation act are as follows: Biological Survey \$30,300, an increase of \$2740; Division of Botany \$43,080, an increase of \$14,280; Division of Pomology \$18,400; Public Road Inquiry \$14,000, an increase of \$6000; Division of Statistics \$146,160; Library \$14,000; and Museum \$2260.

The item of \$200,000 for a new laboratory which was taken out of the Agricultural Bill and put with those for other public buildings in the Sundry Civil Bill failed to pass.

#### VARIATION AND SOME PHENOMENA CONNECTED WITH REPRODUCTION AND SEX.

### II.

#### EFFECT OF CHANGED CONDITIONS IN ASEXUAL REPRODUCTION.

THIS brings us to the consideration of the question reserved: Are genetic variations ever found in asexual reproduction?

If the views expressed in the earlier part of this address are correct it would seem to follow that genetic variations are variations in the actual constitution, and are inseparably connected with the act of conjugation. The act of conjugation gives us a new constitution, a new individuality, and it is the

characters of this new individual in so far as they differ from the characters of the parents which constitute what we have called genetic variations. According to this the answer to our question would be that genetic variations cannot occur in asexual reproduction, and that if any indefinite variability recalling genetic variability makes its appearance\* it must be part of the genetic variability and directly traceable to the zygote from which the asexual generations started.

But if genetic variability is not found in asexual reproduction the question still remains, can the other kinds of variations—namely, those due to the direct action of external forces upon the organism—be transmitted in asexual reproduction? Now we have already seen that the effect of external agencies acting upon the organism must be regarded under two heads, according as to whether the reproductive organs are or are not affected. If the reproductive organs are not affected, then variations caused by the impact of external forces will

\* Weismann, *On Heredity*, vol. ii, English edition, p. 161. Warren, E. 'Observation on Heredity in Parthenogenesis,' *Proc. Roy. Soc.* 65, 1899, p. 154. These are the only observations I know of on this subject. They tend to show the presence of a slight variability, but they are not entirely satisfactory. In connection with this matter I may refer to Weismann's view that *Cypris reptans*, the species upon which his observations were made, reproduces entirely by parthenogenesis, and has lost the power of sexual reproduction. This view is based on the fact that he has bred forty consecutive parthenogenetic generations and has never seen a male. As Weismann bases some important conclusions on this view, with regard to the importance of conjugation in rejuvenescence of organisms, I may point out that the fact that he has bred forty successive generations and has never seen a male cannot be regarded as conclusive evidence that males never appear. We know of many cases in which reproduction can continue for more than forty generations without the intervention of conjugation, e. g., ciliated infusoria, many plants, and of other species of crustacea in which the male is very rare and only appears after long intervals.

not be transmitted; if, on the other hand, they are affected, the next generation will show the effect. We have further seen that in the case of sexual reproduction a modification of the reproductive organs will, because of the intervention of conjugation, appear as an increase in genetic variability only. How will the matter stand in the case of asexual reproduction? First, with regard to modifications which do not affect the reproductive system—they, as in sexual reproduction, will not be transmitted. Secondly, as regards modifications which do affect the reproductive organs—they will be transmitted, *i. e.*, they will affect the next generation; and the question arises, how will they be transmitted? For here we have the opportunity wanting in the case of sexual reproduction of studying the transmission of modifications of the reproductive system without the complications introduced by the act of conjugation.

In considering this matter, it must be remembered that the reproductive organs are with regard to external influences exactly as any other organ. They can be modified either directly or indirectly, though they are in animals often less liable to direct modification by reason of their internal position.\* These modifications may, as in the case of other organs, be obvious to the eye of the observer, or they may be so slight as only to be detected by an alteration of function. Now, in the case of the reproductive organs this alteration of function will show itself in the individuals of the next generation (if not before) which proceed directly and without any complication from the affected tissue. How will these individuals be affected? Will they all be affected in the same kind of way or will they be affected in different ways? Finally, will the modification last their lives only, or

will it continue into subsequent asexually produced generations?

Let us endeavor to answer these questions:

(1) How will the offspring be affected? That will depend entirely upon how the reproductive organ was affected. Will the modification in the offspring have any adaptive relation whatever to the external cause? Now here we have a capital opportunity, an opportunity not afforded at all by sexual reproduction, of examining by experiment and observation the Lamarckian position. My own opinion is that there will be no relation of an adaptive kind between the external cause and the modification of the offspring. For instance, let us imagine, as an experiment, that a number of parthenogenetically reproducing organisms are submitted to a temperature lower than that at which they are accustomed to live. Let us suppose that the cold affects their reproductive organs and produces a modification of the offspring. Will the modification be in the direction of enabling the offspring to flourish in a lower temperature than the parent? My own opinion, as I have said, is that there will probably be no such tendency in the offspring, if all possibility of selection be excluded. But that is only an opinion. The question is unsettled, and must remain unsettled until it is tested upon asexually reproducing organisms.

(2) Will they all be affected in the same kind of way? Yes, presumably they will. I arrive at this conclusion, not by experiment, but by reasoning from analogy. In the case of other organs of the body, the same external cause produces in all individuals acted upon, roughly speaking, the same kind of effect, *e. g.*, action of sun upon skin, effect of transplanting maize, Porto Santo rabbits, etc. The question, however, cannot be settled in this way. It requires an experimental answer.

(3) Will the modification last beyond

\*How far the abnormal position of the testes of mammalia may receive its explanation in this connection is a question worthy of consideration.

the life of the individuals produced by the affected reproductive organ? I can give no answer to this question. We have no data upon which to form a judgment. We cannot say whether it is possible permanently to modify the constitution of an organism in this way, or whether, however strong the cause may be, consistently of course with the non-destruction of life, the effects will gradually die away—it may be in one, it may be in two or more generations. There are cases known which might assist in settling these questions, but I must leave to another opportunity the task of examining them. I refer to such cases as *Artemia salina*, various cases of bud variation which cannot be included under the head of growth variation.

#### SENILE DECAY AND REJUVENESCENCE OF ORGANISMS.

Another question, also of the utmost importance, confronts us at this point. As is well known, organisms are liable to wear and tear, sooner or later some part or parts essential to the maintenance of the vital functions wear out and are not renewed by the reparative processes which are supposed to be continually taking place in the organism. This constitutes what we call senile decay, and leads to the death of the organism. As a good example of the kind of cause of senile decay, we may mention the wearing out of the teeth, which in mammals at any rate are not replaced; the wearing out of the elastic tissue of the arterial wall, which is probably not replaced. There is no reason to suppose that the reparative process of any organism is sufficiently complete to prevent senile decay. There is probably always some part or parts which cannot be renewed, even in the simplest organisms. Maupas has shown that this holds for the ciliated Infusoria, and he has also shown how the renewal of life, which of course must be effected if the species is

to continue, is brought about. He has shown that it is brought about by conjugation, during which process the organism may be said to be put into the melting-pot and reconstituted. For instance, many of the parts of the conjugating individuals are renewed, including the whole nuclear apparatus, which there is every reason to believe is of the greatest importance to living matter.

On reconsidering the life of the Metazoa in light of the facts established by Maupas for the Infusoria, we see that all Metazoa are in a continual state of fission, as are the ciliated Infusoria. They are continually dividing into two unequal parts, one of which we call the parent and the other the gamete. The parent Metazoon must eventually die; it cannot be put into the melting-pot; its parts cannot be completely renovated. The gamete can be put into the melting-pot of conjugation, and give rise to an entirely reconstituted organism, with all the parts and organs brand new and able to last for a certain time, which is the length of life of the individual of the species.

Is there any other way than that of conjugation by which an organism can acquire a complete renewal of its organs? Is the renewal furnished by the development of all the parts afresh which takes place in a parthenogenetic ovum such a complete renewal? This question cannot now be certainly answered, but the balance of evidence is in favor of a negative answer. And this view of the matter is borne out by a consideration of the facts of the case. In all cases of conjugation which have been thoroughly investigated, the nuclear apparatus is completely renewed. It would appear indeed as though the real explanation of the uninuclear character of the Metazoon gamete is to be sought in the necessity of getting the nuclear apparatus into the simplest possible form for renewal. Now in the de-

velopment of a parthenogenetic ovum the ordinary process of renewal of the nucleus is often in partial abeyance. As a rule it only divides once instead of twice, and there is, of course, no reinforcement by nuclear fusion. It is, of course, possible that the reinforcement by nuclear fusion which occurs in conjugation may have a different explanation from the nuclear reconstitution which takes place in the formation of polar bodies and similar structures. On the other hand, it may all be part of the same process. We cannot tell. So that we are unable to answer the question whether for complete rejuvenescence a new formation of all parts of the organism is sufficient, or whether a reconstitution of the nuclear apparatus of the kind which takes place in the maturation of the Metazoon ovum and the division of the micro-nucleus of *Paramecium* is also required; or finally, whether in addition to the latter phenomenon a reinforcement and reconstitution by fusing with another nucleus is also necessary for that complete rejuvenescence which enables an organism to begin the life cycle again and to pass through it completely.

With regard to buds in plants there is reason to believe that they share in the growing old of the parent. That is to say, if we suppose the average life of the individual to be 100 years, a bud removed at 50 will be 50 years of age, and only be able to live on the graft for 50 more years.

#### HEREDITY.

Having now spoken at some length of the phenomenon of variation, I must proceed to consider from the same general point of view the phenomenon of heredity.

As we have seen, in asexual reproduction heredity appears, as a general rule, if not always, to be complete. The offspring do not merely present resemblances to the parent—they are identical with it. And this fact does not appear to be astonishing when

we consider the real nature of the process. Asexual reproduction consists in the separation off of a portion of the parent, which, like the parent, is endowed with the power of growth. In virtue of this property it will assume, if it does not already possess it, and if the conditions are approximately similar, the exact form of the parent. It is a portion of the parent; it is endowed with the same property of growth; the wonder would be if it assumed any other form than that of the parent. Indeed, it is doubtful if the word heredity would ever have been invented if the only form of increase of organisms was the asexual one, because there being no variation to contrast with it, it would not have struck us as a quality needing a name, any more than we have a name for that property of the number two which causes it to make four when duplicated.

The need for the word heredity only becomes apparent when we consider that other form of reproduction in which the real act of reproduction is associated with the act of conjugation. Looking at reproduction from a broad point of view, we may sum up the difference between the two kinds, the sexual and the asexual, by saying that whereas the essence of sexual reproduction is the formation of a new individuality, asexual reproduction merely consists in increasing the number of one kind of individual. From this point of view sexual reproduction is better termed the creation of a new individuality, for that, and not the increase in the number of individuals, is its real result. Inasmuch as conjugation of two organisms is the essential feature of sexual reproduction, it would appear that the number of individuals would be actually diminished as a result of it; and this does really happen, though in a masked manner, for we are not in the habit of looking upon the spermatozoon and ovum as individuals, though it is absurd not to

do so, as they contain latent all the properties of the species, and are sometimes able to manifest these properties (parthenogenetic ova) without conjugating. In some of the lower organisms the fact that conjugation does not result in an increase of the number of individuals, but only in the production of a new individuality, is quite apparent, for in them two of the ordinary individuals of the species fuse to form one (many Protozoa).

So that sexual reproduction gives us a new individuality which can spread to almost any extent by asexual reproduction. This asexual reproduction gives us a group of organisms which is quite different from a group of organisms produced by sexual reproduction. Whereas the latter groups constitute what we call species, the former group has, so far as I know, no special name, unless it be variety; but variety is not a satisfactory name, for it has been used in another sense by systematizers.

Heredity, then, is really applicable only to the appearance in a zygote of some of the properties of the gametes. A zygote has this property of one of the precedent gametes, and that property of the other, in virtue of the operation of what we call heredity; it has a third property possessed by neither of the precedent gametes in virtue of the action of variation, the nature of which we have already examined. It is impossible to say which property of a gamete will be inherited, and it is impossible to predict what odd property will result from the combination of the properties of the two gametes. Of one thing only are we certain, that they are never the same in zygotes formed by gametes produced in immediate succession from the same parent.

We may thus regard the activities of the zygote as the resultant of the dashing together of the activities of the gametes.

Conjugation, then, is a process of the utmost importance in Biology; it provides

the mechanism by which organisms are able to vary, independently of the conditions in which they live. It lies, therefore, at the very root of the evolution problem; the power of combining to form a zygote is one of the fundamental properties of living matter.

#### SPECIES.

Now let us consider one of the effects of this property upon organisms. The effect to which I refer is the division of animals into groups called species. Species are groups of organisms, the gametes of which are able to conjugate and produce normal zygotes. Now in Nature there appear to be many causes which prevent gametes from conjugating. First and most important of all is some physical incompatibility of the living matter which prevents that harmonious blending of the two gametes which is essential for the formation of a normal zygote. Very little is known as to the real nature of this incompatibility; in fact it is hardly an exaggeration to say that nothing is known. It may be that there is actual repulsion between the gametes, or it may be in some cases, at least, that the gametes are able to fuse, but not to undergo that intimate blending which is necessary for the production of a perfect zygote. In some cases we know that something like this happens; for instance, a blend can be obtained between the horse and the ass, but it is not a perfect blend, the product or zygote being imperfect in one most important particular—namely, reproductive power.

A second cause which prevents conjugation is a purely mechanical one—viz, some obstacle which prevents the two gametes from coming together. As an instance of this I may refer to those cases amongst plants in which conjugation is impossible because the pollen tube is not long enough to reach the ovule. In yet other cases conjugation is impossible because the organ-

isms are isolated from one another either geographically or in consequence of their habits. There are probably many causes which prevent conjugation, but, whatever they may be, the effect of them is to break up organisms into specific groups, the gametes of which do normally conjugate with one another.

In many cases, no doubt, the gametes of organisms which are kept apart in Nature by mechanical barriers will conjugate fully if brought together. But in the great majority of cases it is probable that no amount of proximity will bring about complete conjugation. There is physical incompatibility. Here is a fruitful opening for investigation. Observations are urgently needed as to the real nature of this incompatibility.

#### IMPORTANCE OF THE STUDY OF VARIATION.

Another and most important effect of conjugation is, as we have seen, the much-spoken-of constitutional or genetic variations. They are, as we have already insisted, of the utmost importance to the evolutionist. Evolution would have been impossible without them, for it is made up of their summation. It becomes, therefore, desirable to find out to what extent a species is capable of varying. This can only be done, as Mr. Bateson has pointed out, by recording all variations found. Mr. Bateson, in his work already referred to, has carried this out, and has shown the way to a collection of these most important data. In order to carry it further, I would suggest that the collection be made not only for structure, but also for function. This has been done largely for the nervous functions by psychologists and naturalists who pay special attention to the instincts of animals; but we want a similar collection for other functions. For instance, the variations in the phenomena of heat and menstruation, and of rut amongst mammals, and so on. To do this is really only to

apply the methods of comparative anatomy and comparative physiology to the members of a species, as they have already been applied to the different species and larger groups of the animal kingdom. Such investigations cannot fail to be of the greatest interest. Indeed, when we have learnt the normal habits and structure of a species, what more interesting study can there be than the study of the possibilities of variation contained within it? Then when we know the limits of variability of any given specific group, we proceed to try if we can by selective breeding or alteration of the conditions of life alter the variability, and perhaps call into existence a kind of variation quite different in character from that previously obtained as characteristic of the species.

#### THE EVOLUTION OF HEREDITY AND THE ORIGIN OF VARIATION.

These remarks bring me to the consideration of a point to which I am anxious to call your attention, and which is an important aspect of our subject. Has the variability of organisms ever been different from what it is now? Has or has not evolution had its influence upon the property of organisms as it is supposed to have had upon their other properties? There is only one possible answer to this question. Undoubtedly the variability of organisms must have altered with the progress of evolution. It would be absurd to suppose that organisms have remained constant in this respect while they have undergone alteration in all their other properties. If the variability of organisms has altered, it becomes necessary to inquire in what direction has it altered? Has the alteration been one of diminution, or has it been one of increase? Of course, it is possible that there has been no general alteration in extent with the course of evolution, and that the alteration, on the whole, has been one of quality only. But

passing over this third possibility, let us consider for the moment which of the two first named alternatives is likely to have occurred.

According to the Darwinian theory of evolution, one of the most important factors in determining the modification of organisms has been natural selection. Selection acts by preserving certain favorable variations, and allowing others less favorable to be killed off in the struggle for existence. It thus will come about that certain variations will be gradually eliminated. Meanwhile the variations of the selected organisms will themselves be submitted to selection, and certain of these will be in their turn eliminated. In this way a group of organisms becomes more and more closely adapted to its surroundings; and unless new variations make their appearance as the old unfavorable ones are eliminated, the variability of the species will diminish as the result of selection. Is it likely that new variations will appear in the manner suggested? To answer this question we must turn to the results obtained by human agency in the selective breeding of animals. The experience of breeders is that continued selection tends to produce a greater and greater purity of stock, characterized by small variability, so that if the selective breeding is carried too far, variation almost entirely ceases, and there is little opportunity left for the exercise of the breeder's art. When this condition has been arrived at, he is obliged, if he wants to produce any further modifications of his animals, to introduce new blood—*i. e.*, to bring in an individual which has either been bred to a different standard, or one in which the variability has not been so completely extinguished.

It would thus appear, and I think we are justified in holding this view, at any rate provisionally, that the result of continued selection will be to diminish the variability of a species; and if carried far enough, to

produce a race with so little variability, and so closely adapted to its surroundings, that the slightest alteration in the conditions of life will cause extinction.\*

If selection tends to diminish the variability of a species, then it clearly follows that as selection has been by hypothesis the most important means of modifying organisms, variation must have been much greater in past times than it is now. In fact it must have been progressively greater the farther we go back from the present time.

The argument which I have just laid before you points, if carried to its logical conclusion—and I see no reason why it should not be so carried—to the view that at the first origin of life upon the earth the variability of living matter consequent upon the act of conjugation must have been of enormous range; in other words, it points to the view that heredity was a much less important phenomenon than it is at present. Following out the same train of thought, we are inevitably driven to the conclusion that one of the most important results of the evolutionary change has been the gradual increase and perfection of heredity as a function of organisms and a gradual elimination of variability.

This view, if it can be established, is of the utmost importance to our theoretical conception of evolution, because it enables us to bring our requirements as to time within the limits granted by the physicists.

\*The expression extinction of species seems to be used in two senses, which are generally confused. Firstly, a species may become modified so that the form with which we are familiar gradually gives place to one or more forms which have been gradually produced by its modification. That is to say, a character or series of characters becomes gradually modified or lost in successive generations. This is not really extinction, but development. Secondly, a species may gradually lose its variability, and become fixed in character. If the conditions then change, it is unable to adapt itself to them, and becomes truly extinct. In this case it leaves no descendants. We have to do with death, and not with development.

If variation was markedly greater in the early periods of the existence of living matter, it is clear that it would have been possible for evolutionary change to have been effected much more rapidly than at present—especially when we remember that the world was then comparatively unoccupied by organisms, and that with the change of conditions consequent on the cooling and differentiation of the earth's surface, new places suitable for organic life were continually being formed. It will be observed that the conclusion we have now reached, viz, that variation was much greater near the dawn of life than it is now, and heredity a correspondingly less important phenomenon, is a deduction from the selection theory. It becomes, therefore, of some interest to inquire whether a suggestion obtained by a perfectly legitimate mode of reasoning receives any independent confirmation from other sources. The first source of facts to which we turn for such confirmation must obviously be paleontology. But paleontology unfortunately affords us no help. The facts of this science are too meagre to be of any use. Indeed, they are wanting altogether for the period which most immediately concerns us—namely, the period when the existing forms of life were established. This took place in the prefossiliferous period, for in the earliest fossiliferous rocks examples of almost all existing groups of animals are met with.

But although paleontology affords us no assistance, there is one class of facts which, when closely scrutinized, do lend some countenance to the view that when evolutionary change was at its greatest activity, *i. e.*, when the existing forms of life were being established, variation was considerably greater than it is at the present day.

But as this address has already exceeded all reasonable limits, and as the question

which we are now approaching is one of very great complexity and difficulty, I am reluctantly compelled to defer the full consideration and treatment of it to another occasion. I can only hope that the far-reaching importance of my subject and the interest of it may to some extent atone for the great length which this address has attained.

ADAM SEDGWICK.

CAMBRIDGE UNIVERSITY.

---

*THE PROTEIDS OF LIVING MATTER.\**

OF all the phenomena of nature vital phenomena have always appeared to the human mind the most complicated and intricate, so much so that even many scientific men have ascribed them to an inexplorable cause—the so-called vital force. This 'vitalism' is adhered to by many even to-day. In scrutinizing the various vital phenomena we observe, however, a great difference in the degree of complexity. There are on the one hand actions of an admittedly purely chemical, physical, and mechanical nature; and on the other those of organization, genetic differentiation, and of irritability on which differences of opinion still exist. The former appear of relatively simple character compared with the latter, which seem to offer difficulties of explanation insurmountable for science in its present state of development.

Protoplasm even of the simplest cells represents a highly complicated machinery. The organization corresponds to the construction of a machine, while its motive power consists in various forms of energy. Hence, two principal questions arise: (1) How is the machinery constructed? (2) What is the nature of the primary energy moving the machinery? The latter question is of a simpler kind than the former. We

\* A paper read before the joint meeting of the Biological and Chemical Societies of Washington, May 5, 1900.