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EVOLUTIONARY FAITH AND MODERN DOUBTS[1]

I VISIT Canada for the first time in delightful circumstances. After a period of dangerous isolation, intercourse between the centres of scientific development is once more beginning, and I am grateful to the American Association for this splendid opportunity of renewing friendship with my western colleagues in genetics, and of coming into even a temporary partnership in the great enterprise which they have carried through with such extraordinary success.

In all that relates to the theme which I am about to consider we have been passing through a period of amazing activity and fruitful research. Coming here after a week in close communion with the wonders of Columbia University, I may seem behind the times in asking you to devote an hour to the old topic of evolution. But though that subject is no longer in the forefront of debate, I believe it is never very far from the threshold of our minds, and it was with pleasure that I found it appearing in conspicuous places in several parts of the program of this meeting.

Standing before the American Association, it is not unfit that I should begin with a personal reminiscence. In 1883 I first came to the United States to study the development of Balanoglossus at the Johns Hopkins summer laboratory, then at Hampton, Va. This creature had lately been found there in an easily accessible place. With a magnanimity, that on looking back I realize was superb, Professor W. K. Brooks had given me permission to investigate it, thereby handing over to a young stranger one of the prizes which in this age

[1] Delivered before the American Association for the Advancement of Science on Wednesday evening, December 28, in the Convocation Hall of the University of Toronto.

of more highly developed patriotism, most teachers would keep for themselves and their own students. At that time one morphological laboratory was in purpose and aim very much like another. Morphology was studied because it was the material believed to be most favorable for the elucidation of the problems of evolution, and we all thought that in embryology the quintessence of morphological truth was most palpably presented. Therefore every aspiring zoologist was an embryologist, and the one topic of professional conversation was evolution. It had been so in our Cambridge school, and it was so at Hampton.

I wonder if there is now a single place where the academic problems of morphology which we discussed with such avidity can now arouse a moment's concern. There were of course men who saw a little further, notably Brooks himself. He was at that time writing a book on heredity, and, to me at least, the notion on which he used to expatiate, that there was a special physiology of heredity capable of independent study, came as a new idea. But no organized attack on that problem was begun, nor had any one an inkling of how to set about it. So we went on talking about evolution. That is barely 40 years ago; to-day we feel silence to be the safer course.

Systematists still discuss the limits of specific distinction in a spirit, which I fear is often rather scholastic than progressive, but in the other centers of biological research a score of concrete and immediate problems have replaced evolution.

Discussions of evolution came to an end primarily because it was obvious that no progress was being made. Morphology having been explored in its minutest corners, we turned elsewhere. Variation and heredity, the two components of the evolutionary path, were next tried. The geneticist is the successor of the morphologist. We became geneticists in the conviction that there at least must evolutionary wisdom be found. We got on fast. So soon as a critical study of variation was undertaken, evidence came in as to the way in which varieties do actually arise in descent. The unacceptable doctrine of the secular transformation of masses by the accumulation of impalpable changes became not only unlikely but gratuitous. An examination in the field of the interrelations of pairs of well characterized but closely allied "species" next proved, almost wherever such an inquiry could be instituted, that neither could both have been gradually evolved by natural selection from a common intermediate progenitor, nor either from the other by such a process. Scarcely ever where such pairs co-exist in nature, or occupy conterminous areas do we find an intermediate normal population as the theory demands. The ignorance of common facts bearing on this part of the inquiry which prevailed among evolutionists, was, as one looks back, astonishing and inexplicable. It had been decreed that when varieties of a species co-exist in nature, they must be connected by all intergradations, and it was an article of faith of almost equal validity that the intermediate form must be statistically the majority, and the extremes comparatively rare. The plant breeder might declare that he had varieties of Primula or some other plant, lately constituted, uniform in every varietal character breeding strictly true in those respects, or the entomologist might state that a polymorphic species of a beetle or of a moth fell obviously into definite types, but the evolutionary philosopher knew better. To him such statements merely showed that the reporter was a bad observer, and not improbably a destroyer of inconvenient material. Systematists had sound information but no one consulted them on such matters or cared to hear what they might have to say. The evolutionist of the eighties was perfectly certain that species were a figment of the systematist's mind, not worthy of enlightened attention.

Then came the Mendalian clue. We saw the varieties arising. Segregation maintained their identity. The discontinuity of variation was recognized in abundance. Plenty of the Mendelian combinations would in nature pass the scrutiny of even an exacting systematist and be given "specific rank." In the light of such facts the origin of species was no doubt a similar phenomenon. All was clear ahead.

But soon, though knowledge advanced at a great rate, and though whole ranges of phenomena which had seemed capricious and disorderly fell rapidly into a co-ordinated system, less and less was heard about evolution in genetical circles, and now the topic is dropped. When students of other sciences ask us what is now currently believed about the origin of species we have no clear answer to give. Faith has given place to agnosticism for reasons which on such an occasion as this we may profitably consider.

Where precisely has the difficulty arisen? Though the reasons for our reticence are many and present themselves in various forms, they are in essence one; that as we have come to know more of living things and their properties, we have become more and more impressed with the inapplicability of the evidence to these questions of origin. There is no apparatus which can be brought to bear on them which promises any immediate solution.

In the period I am thinking of it was in the characteristics and behavior of animals and plants in their more familiar phases, namely, the Zygotic phases that attention centered. Genetical research has revealed the world of gametes from which the zygotes—the products of fertilization are constructed. What has been there witnessed is of such extraordinary novelty and so entirely unexpected that in presence of the new discoveries we would fain desist from speculation for a while. We see long courses of analysis to be traveled through and for some time to come that will be a sufficient occupation. The evolutionary systems of the eighteenth and nineteenth centuries were attempts to elucidate the order seen prevailing in this world of zygotes and to explain it in simpler terms of cause and effect: we now perceive that that order rests on and is determined by another equally significant and equally in need of "explanation." But if we for the present drop evolutionary speculation it is in no spirit of despair. What has been learned about the gametes and their natural history constitutes progress upon which we shall never have to go back. The analysis has gone deeper than the most sanguine could have hoped.

We have turned still another bend in the track and behind the gametes we see the chromosomes. For the doubts—which I trust may be pardoned in one who had never seen the marvels of cytology, save as through a glass darkly—can not as regards the main thesis of the Drosophila workers, be any longer maintained. The arguments of Morgan and his colleagues, and especially the demonstrations of Bridges, must allay all scepticism as to the direct association of particular chromosomes with particular features of the zygote. The transferable characters borne by the gametes have been successfully referred to the visible details of nuclear configuration.

The traces of order in variation and heredity which so lately seemed paradoxical curiosities have led step by step to this beautiful discovery. I come at this Christmas Season to lay my respectful homage before the stars that have arisen in the west. What wonder if we hold our breath? When we knew nothing of all this the words came freely. How easy it all used to look! What glorious assumptions went without rebuke. Regardless of the obvious consideration that "modification by descent" must be a chemical process, and that of the principles governing that chemistry science had neither hint, nor surmise, nor even an empirical observation of its working, professed men of science offered very confidently positive opinions on these nebulous topics which would now scarcely pass muster in a newspaper or a sermon. It is a wholesome sign of return to sense that these debates have been suspended.

Biological science has returned to its rightful place, investigation of the structure and properties of the concrete and visible world. We cannot see how the differentiation into species came about. Variation of many kinds, often considerable, we daily witness, but no origin of species. Distinguishing what is known from what may be believed we have absolute certainty that new forms of life, new orders and new species have arisen on the earth. That is proved by the paleontological record. In a spirit of paradox even this has been questioned. It has been asked how do you *know* for instance that there were no mammals in palæozoic times? May there not have been mammals somewhere on the earth though no vestige of them has come down to us? We may feel confident there were no mammals then, but are we sure? In very ancient rocks most of the great orders of animals are represented. The absence of the others might by no great stress of imagination be ascribed to accidental circumstances.

Happily however there is one example of which we can be sure. There were no Angiosperms—that is to say "higher plants" with protected seeds—in the carboniferous epoch. Of that age we have abundant remains of a world wide and rich flora. The Angiosperms are cosmopolitan. By their means of dispersal they must immediately have become so. Their remains are very readily preserved. If they had been in existence on the earth in carboniferous times they must have been present with the carboniferous plants, and must have been preserved with them. Hence we may be sure that they did appear on the earth since those times. We are not certain, using certain in the strict sense, that the Angiosperms are the lineal descendants of the carboniferous plants, but it is very much easier to believe that they are than that they are not.

Where is the difficulty? If the Angiosperms came from the carboniferous flora why may we not believe the old comfortable theory in the old way? Well so we may if by belief we mean faith, the substance, the foundation of things hoped for, the evidence of things not seen. In dim outline evolution is evident enough. From the facts it is a conclusion which inevitably follows. But that particular and essential bit of the theory of evolution which is concerned with the origin and nature of *species* remains utterly mysterious. We no longer feel as we used to do, that the process of variation, now contemporaneously occurring, is the beginning of a work which needs merely the element of time for its completion; for even time can not complete that which has not yet begun. The conclusion in which we were brought up, that species are a product of a summation of variations ignored the chief attribute of species first pointed out by John Ray that the product of their crosses is frequently sterile in greater or less degree. Huxley, very early in the debate pointed out this grave defect in the evidence, but before breeding researches had been made on a large scale no one felt the objection to be serious. Extended work might be trusted to supply the deficiency. It has not done so, and the significance of the negative evidence can no longer be denied.

When Darwin discussed the problem of inter-specific sterility in the "Origin of Species" this aspect of the matter seems to have escaped him. He is at great pains to prove that inter-specific crosses are *not always* sterile, and he shows that crosses between forms which pass for distinct species may produce hybrids which range from complete fertility to complete sterility. The fertile hybrids he claims in support of his argument. If species arose from a common origin, clearly they should not always give sterile hybrids. So Darwin is concerned to prove that such hybrids are by no means always sterile, which to us is a commonplace of everyday experience. If species have a common origin, where did they pick up the ingredients which produce this sexual incompatibility? Almost certainly it is a variation in which something has been added. We have come to see that variations can very commonly—I do not say always—be distinguished as positive and negative. The validity of this distinction has been doubted, especially by the *Drosophila* workers. Nevertheless in application to a very large range of characters, I am satisfied that the distinction holds, and that in analysis it is a useful aid. Now we have no difficulty in finding evidence of variation by loss. Examples abound, but variation by addition are rarities, even if there are any which must be so accounted. The variations to which inter-specific sterility is due are obviously variations in which something is apparently added to the stock of ingredients. It is one of the common experiences of the breeder that when a hybrid is partially sterile, and from it any fertile offspring can be obtained, the sterility, once lost, disappears. This has been the history of many, perhaps most of our cultivated plants of hybrid origin.

The production of an indubitably sterile hybrid from completely fertile parents which have arisen under critical observation from a single common origin is the event for which we wait. Until this event is witnessed, our knowledge of evolution is incomplete in a vital respect. From time to time a record of such an observation is published, but none has yet survived criticism. Meanwhile, though our faith in evolution stands unshaken, we have no acceptable account of the origin of "species."

Curiously enough, it is at the same point that the validity of the claim of natural selection as the main directing force was most questionable. The survival of the fittest was a plausible account of evolution in broad outline, but failed in application to specific difference. The Darwinian philosophy convinced us that every species must "make good" in nature if it is to survive, but no one could tell how the differences—often very sharply fixed—which we recognize as specific, do in fact enable the species to make good. The claims of natural selection as the chief factor in the determination of species have consequently been discredited.

I pass to another part of the problem, where again, though extraordinary progress in knowledge has been made, a new and formidable difficulty has been encountered. Of variations we know a great deal more than we did. Almost all that we have seen are variations in which we recognize that elements have been lost. In addressing the British Association in 1914 I dwelt on evidence of this class. The developments of the last seven years, which are memorable as having provided in regard to one animal, the fly Drosophila, the most comprehensive mass of genetic observation yet collected, serve rather to emphasize than to weaken the considerations which I then referred. Even in Drosophila, where hundreds of genetically distinct factors have been identified, very few new dominants, that is to say positive additions, have been seen, and I am assured that none of them are of a class which could be expected to be viable under natural conditions. I understand even that none are certainly viable in the homozygous state.

If we try to trace back the origin of our domesticated animals and plants, we can scarcely ever point to a single wild species as the probable progenitor. Almost every naturalist who has dealt with these questions in recent years has had recourse to theories of multiple origin, because our modern races have positive characteristics which we cannot find in any existing species, and which combination of the existing species seem unable to provide. To produce our domesticated races it seems that ingredients must have been added. To invoke the hypothetical existence of lost species provides a poor escape from this difficulty, and we are left with the conviction that some part of the chain of reasoning is missing. The weight of this objection will be most felt by those who have most experience in practical breeding. I can not, for instance, imagine a round seed being found on a wrinkled variety of pea except by crossing. Such seeds, which look round, sometimes appear, but this is a superficial appearance, and either these seeds are seen to have the starch of wrinkled seeds or can be proved to be the produce of stray pollen. Nor can I imagine a fern-leaved Primula producing a palm-leaf, or a star-shaped flower producing the old type of *sinensis* flower. And so on through long series of forms which we have watched for twenty years.

Analysis has revealed hosts of transferable characters. Their combinations suffice to supply in abundance series of types which might pass for new species, and certainly would be so classed if they were met with in nature. Yet critically tested, we find that they are not distinct species and we have no reason to suppose that any accumulations of characters of the same order would culminate in the production of distinct species. Specific difference therefore must be regarded as probably attaching to the base upon which these transferables are implanted, of which we know absolutely nothing at all. Nothing that we have witnessed in the contemporary world can colorably be interpreted as providing the sort of evidence required.

Twenty years ago, de Vries made what looked like a promising attempt to supply this so far as *Oenothera* was concerned. In the light of modern experiments, especially those of Renner, the interest attaching to the polymorphism of *Oenothera* has greatly developed, but in application to that phenomenon the theory of mutation falls. We see novel forms appearing, but they are no new species of *Oenothera,* nor are the parents which produce them pure or homozygous forms. Renner's identification of the several complexes allocated to the male and female sides of the several types is a wonderful and significant piece of analysis introducing us to new genetical conceptions. The Oenotheras illustrate in the most striking fashion how crude and inadequate are the suppositions which we entertained before the world of gametes was revealed. The appearance of the plant tells us little or nothing of these things. In Mendelism, we learnt to appreciate the implication of the fact that the organism is a double structure, containing ingredients derived from the mother and from the father respectively. We have now to admit the further conception that between the male and female sides of the same plant these ingredients may be quite differently apportioned, and that the genetical composition of each may be so distinct that the systematist might without extravagance recognize them as distinct specifically. If then our plant may by appropriate treatment be made to give off two distinct forms, why is not that phenomenon a true instance of Darwin's origin of species? In Darwin's time it must have been acclaimed as exactly supplying all and more than he ever hoped to see. We know that that is not the true interpretation. For that which comes out is no new creation.

Only those who are keeping up with these new developments can fully appreciate their vast significance or anticipate the next step. That is the province of the geneticist. Nevertheless, I am convinced that biology would greatly gain by some cooperation among workers in the several branches. I had expected that genetics would provide at once common ground for the systematist and the laboratory worker. This hope has been disappointed. Each still keeps apart. Systematic literature grows precisely as if the genetical discoveries had never been made and the geneticists more and more withdraw each into his special "claim" —a most lamentable result. Both are to blame. If we cannot persuade the systematists to come to us, at least we can go to them. They too have built up a vast edifice of knowledge which they are willing to share with us, and which we greatly need. They too have never lost that longing for the truth about evolution which to men of my date is the salt of biology, the impulse which made us biologists. It is from them that the raw materials for our researches are to be drawn, which alone can give catholicity and breadth to our studies. We and the systematists have to devise a common language.

Both we and the systematists have everything to gain by a closer alliance. Of course we must specialize, but I suggest to educationists that in biology at least specialization begins too early. In England certainly harm is done by a system of examinations discouraging to that taste for field natural history and collecting, spontaneous in so many young people. How it may be on this side, I can not say, but with us attainments of that kind are seldom rewarded, and are too often despised as trivial in comparison with the stereotyped biology which can be learnt from text-books. Nevertheless, given the aptitude, a very wide acquaintance with nature and the diversity of living things may be acquired before the age at which more intensive study must be begun, the best preparation for research in any of the branches of biology.

The separation between the laboratory men and the systematists already imperils the work, I might almost say the sanity, of both. The systematists will feel the ground fall from beneath their feet, when they learn and realize what genetics has accomplished, and we, close students of specially chosen examples, may find our eyes dazzled and blinded when we look up from our work-tables to contemplate the brilliant vision of the natural world in its boundless complexity.

I have put before you very frankly the considerations which have made us agnostic as to the actual mode and processes of evolution. When such confessions are made the enemies of science see their chance. If we cannot declare here and now how species arose, they will obligingly offer us the solutions with which obscurantism is satisfied. Let us then proclaim in precise and unmistakable language that our faith in evolution is unshaken. Every available line of argument converges on this inevitable conclusion. The obscurantist has nothing to suggest which is worth a moment's attention. The difficulties which weigh upon the professional biologist need not trouble the layman. Our doubts are not as to the reality or truth of evolution, but as to the origin of *species,* a technical, almost domestic, problem. Any day that mystery may be solved. The discoveries of the last twenty-five years enable us for the first time to discuss these questions intelligently and on a basis of fact. That synthesis will follow on an analysis, we do not and cannot doubt.

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THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

THE MAIN FEATURES OF THE PROCEEDINGS OF THE COUNCIL AT THE TORONTO MEETING

THE Treasurer's report for 1921 was accepted and will be published in SCIENCE. It shows that the total endowment funds of the Association now amount to $121,414.77.

The Permanent Secretary's financial report for the fiscal year 1920-21 was accepted and will also be published in SCIENCE. The total income of the permanent secretary's office for the fiscal year was $56,463.20.

The council appropriated the sum of $4,000 from the treasurer's appropriable funds, to be allotted as grants for research, according to the recommendations of the Committee on Grants; and it also appropriated $500 from the same funds, to be refunded by the treasurer to the permanent secretary, on account of a $500 grant made from the permanent secretary's funds early in 1921.

The council voted (A) that the treasurer should, now and in the future, invest in securities only additions to the permanent funds, and that he should invest these additions as soon as practicable after their receipt by him; (B) that the treasurer should hold available for appropriation by the council all income from capital funds; and (C) that the balance of the income now available for grants for research after deducting the disbursements for this purpose ($4,500) authorized above, should be held by the treasurer as an emergency research fund available for appropriation by the council as grants for research. (By previous action of the council the treasurer pays annually to the permanent secretary a sum amounting to $3 for each life or sustaining member still living, on account of the journal).

The budgets for 1922 of the permanent secretary, the general secretary, and the treasurer were approved.

The action of the executive committee was approved, in the following elections to membership in the Finance Committee: A. S. Frissell, New York, N. Y.; Milton E. Ailes, Washington, D. C. The Treasurer, R. S. Woodward, is chairman of the Finance Committee.

The action of the executive committee was approved in the election of the following members to emeritus life-membership on account of the Jane M. Smith Fund: Professor B. K. Emerson (M 70, F 77), Amherst, Mass.; Professor Eugene A. Smith (M 71, F 77), University, Ala.

Forty-eight members were elected to fellowship in the association, on nominations duly approved by the section secretaries.

The council expressed by a rising vote its appreciation of the fact that Past President T. C. Mendenhall, who presided at the first Toronto meeting of the Association, in 1889, had found it possible to be present at the sec-