
BIOLOGY

BY

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PROFESSOR OF ZOOLOGY
COLUMBIA UNIVERSITY

New York
THE COLUMBIA UNIVERSITY PRESS
1908

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A LECTURE DELIVERED AT

COLUMBIA UNIVERSITY IN THE SERIES ON SCIENCE, PHILOSOPHY AND ART NOVEMBER 20, 1907

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Set up, and published March, 1908.

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I must at the outset remark that among the many sciences that are occupied with the study of the living world there is no one that may properly lay exclusive claim to the name of Biology. The word does not, in fact, denote any particular science but is a generic term applied to a large group of biological sciences all of which alike are concerned with the phenomena of life. To present in a single address, even in rudimentary outline, the specific results of these sciences is obviously an impossible task, and one that I have no intention of attempting. I shall offer no more than a kind of preface or introduction to those who will speak after me on the biological sciences of physiology, botany and zoology; and I shall confine it to what seem to me the most essential and characteristic of the general problems towards which all lines of biological inquiry must sooner or later converge.

It is the general aim of the biological sciences to learn something of the order of nature in the living world. Perhaps it is not amiss to remark that the biologist may not hope to solve the ultimate problems of life any more than the chemist and physicist may hope to penetrate the final mysteries of existence in the non-living world. What he can do is to observe, compare and experiment with phenomena, to resolve more complex phenomena into simpler components, and to this extent, as he says, to "explain" them; but he knows in advance that his explanations will never be in the full sense of the word final or complete. Investigation can do no more than push [6]
forward the limits of knowledge.

The task of the biologist is a double one. His more immediate effort is to inquire into the nature of the existing organism, to ascertain in what measure the complex phenomena of life as they now appear are capable of resolution into simpler factors or components, and to determine as far as he can what is the relation of these factors to other natural phenomena. It is often practically convenient to consider the organism as presenting two different aspects—a structural or morphological one, and a functional or physiological—and biologists often call themselves accordingly morphologists or physiologists. Morphological investigation has in the past largely followed the method of observation and comparison, physiological investigation that of experiment; but it is one of the best signs of progress that in recent years the fact has come clearly into view that morphology and physiology are really inseparable, and in consequence the distinctions between them, in respect both to subject matter and to method, have largely disappeared in a greater community of aim. Morphology and physiology alike were profoundly transformed by the introduction into biological studies of the genetic or historical point of view by Darwin, who did more than any other to establish the fact, suspected by many earlier naturalists, that existing vital phenomena are the outcome of a definite process of evolution; and it was he who first fully brought home to us how defective and one-sided is our view of the organism so long as we do not consider it as a product of the past. It is the second and perhaps greater task of the biologist to study the organism from the historical point of view, considering it as the product of a continuous process of evolution that has been in operation since life began. In [7]
its widest scope this genetic inquiry involves not only the evolution of higher forms from lower ones, but also the still larger question of the primordial relation of living things to the non-living world. Here is involved the possibility so strikingly expressed many years ago by Tyndall in that eloquent passage in the Belfast address, where he declared himself driven by an intellectual necessity to cross the boundary line of the experimental evidence and to discern in non-living matter, as he said, the promise and potency of every form and quality of terrestrial life. This intellectual necessity was created by a conviction of the continuity and consistency of natural phenomena, which is almost inseparable from the scientific attitude towards nature. But Tyndall's words stood after all for a confession of faith, not for a statement of fact; and they soared far above the *terra firma* of the actual evidence. At the present day we too may find ourselves logically driven to the view that living things first arose as a product of non-living matter. We must fully recognize the extraordinary progress that has been made by the chemist in the artificial synthesis of compounds formerly known only as the direct products of living protoplasm. But it must also be admitted that we are still wholly without evidence of the origin of any living thing, at any period of the earth's history, save from some other living thing; and after more than two centuries Redi's aphorism *omne vivum e vivo* retains to-day its full force. It is my impression therefore that the time has not yet come when hypotheses regarding a different origin of life can be considered as practically useful.

If I have the temerity to ask your attention to the fundamental problem towards which all lines of biological inquiry sooner or later lead us it is not with the delusion that I can contribute anything new to the prolonged discussions and controversies to which it has given rise. I desire [8]
only to indicate in what way it affects the practical efforts of biologists to gain a better understanding of the living organism, whether regarded as a group of existing phenomena or as a product of the evolutionary process; and I shall speak of it, not in any abstract or speculative way, but from the standpoint of the working naturalist. The problem of which I speak is that of organic mechanism and its relation to that of organic adaptation. How in general are the phenomena of life related to those of the non-living world? How far can we profitably employ the hypothesis that the living body is essentially an automaton or machine, a configuration of material particles, which, like an engine or a piece of clockwork, owes its mode of operation to its physical and chemical construction? It is not open to doubt that the living body *is* a machine. It is a complex chemical engine that applies the energy of the food-stuffs to the performance of the work of life. But is it something more than a machine? If we may imagine the physico-chemical analysis of the body to be carried through to the very end, may we expect to find at last an unknown something that transcends such analysis and is neither a form of physical energy nor anything given in the physical or chemical configuration of the body? Shall we find anything corresponding to the usual popular conception—which was also along the view of physiologists—that the body is "animated" by a specific "vital principle," or "vital force," a dominating "archæus" that exists only in the realm of organic nature? If such a principle exists, then the mechanistic

hypothesis fails and the fundamental problem of biology becomes a problem *sui generis*.

In its bearing on man's place in nature this question is one of the most momentous with which natural science has to deal, and it has occupied the attention of thinking men in every age. I cannot trace its history, but it will be worth our while to place side by side the words of three of [9] the great leaders of modern scientific and philosophic thought. The saying has been attributed to Descartes, "Give me matter and I will construct the world"—meaning by this the living world as well as the non-living; but Descartes specifically excepted the human mind. I do not know whether the great French philosopher actually used these particular words, but they express the essence of the mechanistic hypothesis that he adopted. Kant utterly repudiated such a conception in the following well known passage: "It is quite certain that we cannot become adequately acquainted with organized creatures and their hidden potentialities by means of the merely mechanical principles of nature, much less can we explain them; and this is so certain that we may boldly assert that it is absurd for man even to make such an attempt or to hope that a Newton may one day arise who will make the production of a blade of grass comprehensible to us according to natural laws that have not been ordered by design. Such an insight we must absolutely deny to man." Still, in another place Kant admitted that the facts of comparative anatomy give us "a ray of hope, however faint, that something may be accomplished by the aid of the principle of the mechanism of nature, without which there can be no science in general." It is interesting to turn from this to the bold and aggressive assertion of Huxley: "Living matter differs from other matter in degree and not in kind, the microcosm repeats the macrocosm; and one chain of causation connects the nebulous origin of suns and planetary systems with the protoplasmic foundations of life and organization."

Do not expect me to decide where such learned doctors disagree; but I will at this point venture on one comment which may sound the key-note of this address. Perhaps we shall find that in the long run and in the large sense Kant was right; but it is certain that to-day we know very much [10] more about the formation of the living body, whether a blade of grass or a man, than did the naturalists of Kant's time; and for better or for worse the human mind seems to be so constituted that it will continue its efforts to explain such matters, however difficult they may seem to be. But I return to our more specific inquiry with the remark that the history of physiology in the past two hundred years has been the history of a progressive restriction of the notion of a "vital force" or "vital principle" within narrower and narrower limits, until at present it may seem to many physiologists that no room for it remains within the limits of our biological philosophy. One after another the vital activities have been shown to be in greater or less degree explicable or comprehensible considered as physico-chemical operations of various degrees of complexity. Every physiologist will maintain that we cannot name one of these activities, not even thought, that is not carried on by a physical mechanism. He will maintain further that in most cases the vital actions are not merely accompanied by physico-chemical operations but actually consist of them; and he may go so far as definitely to maintain that we have no evidence that life itself can be regarded as anything more than their sum total. He is able to bring forward cogent evidence that all modes of vital activity are carried on by means of energy that is set free in protoplasm or its products by means of definite chemical processes collectively known as metabolism. When the matter is reduced to its lowest terms, life, as thus viewed, seems to have its root in chemical change; and we can understand how an eminent German physiologist offers us a definition or characterization of life that runs: "The life-process consists in the metabolism of proteids." I ask your particular attention to this definition since I now wish to contrast with it another and very different one.

I shall introduce it to your attention by asking a very simple question. We may admit that [11] digestion, for example, is a purely chemical operation, and one that may be exactly imitated outside the living body in a glass flask. My question is, how does it come to pass that an animal has a stomach?—and, pursuing the inquiry, how does it happen that the human stomach is practically incapable of digesting cellulose, while the stomachs of some lower animals, such as the goat, readily digest this substance? The earlier naturalists, such as Linnaeus, Cuvier or Agassiz, were ready with a reply which seemed so simple, adequate and final that the plodding modern naturalist cannot repress a feeling of envy. In their view plants and animals are made as they were originally created, each according to its kind. The biologist of to-day views the matter differently; and I shall give his answer in the form in which I now and then make it to a student who may chance to ask why an insect has six legs and a spider eight, or why a yellowbird is yellow and a bluebird blue. The answer is: "For the same reason that the elephant has a trunk." I trust that a certain rugged pedagogical virtue in this reply may atone for its lack of elegance. The elephant has a trunk, as the insect has six legs, for the reason that such is the specific nature of the animal; and we may assert with a degree of probability that amounts to practical certainty that this specific nature is the outcome of a definite evolutionary process, the nature and causes of which it is our tremendous task to determine to such extent as we may be able. But this does not yet touch the most essential side of the problem. What is most significant is that the clumsy, short-necked elephant has been endowed—"by nature," as we say—with precisely such an organ, the trunk, as he needs to compensate for his lack of flexibility and agility in other respects. If we are asked *why* the elephant has a trunk, we must answer because the animal needs it. But does [12] such a reply in itself explain the fact? Evidently not. The question which science must seek to answer, is *how* came the elephant to have a trunk; and we do not properly answer it by saying that it has developed in the course of evolution. It has been well said that even the most complete knowledge of the genealogy of plants and animals would give us no more than an ancestral portrait-gallery. We must determine the causes and conditions that have cooperated to produce this particular result if our answer is to constitute a true scientific explanation. And evidently he who adopts the machine-theory as a general interpretation of vital phenomena must make clear to us how the machine was built before we can admit the validity of his theory, even in a single case. Our apparently simple question as to why the animal has a stomach has thus revealed to us the full magnitude of the task with which the mechanist is confronted; and it has brought us to that part of our problem that is concerned with the nature and origin of organic adaptations. Without tarrying to attempt a definition of adaptation I will only emphasize the fact that many of the great naturalists, from Aristotle onward, have recognized the purposeful or design-like quality of vital phenomena as their most essential and fundamental characteristic. Herbert Spencer defined life as the continuous *adjustment* of internal relations to external relations. It is one of the best that has been given, though I am not sure that Professor Brooks has not improved upon it when he says that life is "response to the order of nature." This seems a long way from the definition of Verworn, heretofore cited, as the "metabolism of proteids." To this Brooks opposes the telling epigram: "The essence of life is not protoplasm but purpose."

Without attempting adequately to illustrate the nature of organic adaptations, I will direct your attention to what seems to me one of their most striking features regarded from the mechanistic [13]
position. This is the fact that adaptations so often run counter to direct or obvious mechanical conditions. Nature is crammed with devices to protect and maintain the organism against the stress of the environment. Some of these are given in the obvious structure of the organism, such as the tendrils by means of which the climbing plant sustains itself against the action of gravity or the winds, the protective shell of the snail, the protective colors and shapes of animals, and the like. Any structural feature that is useful because of its construction is a structural adaptation; and when such adaptations are given the mechanist has for the most part a relatively easy task in his interpretation. He has a far more difficult knot to disentangle in the case of the so-called functional adaptations, where the organism modifies its activities (and often also its structure) in response to changed conditions. The nature of these phenomena may be illustrated by a few examples so chosen as to form a progressive series. If a spot on the skin be rubbed for some time the first result is a direct and obviously mechanical one; the skin is worn away. But if the rubbing be continued long enough, and is not too severe, an indirect effect is produced that is precisely the opposite of the initial direct one; the skin is replaced, becomes thicker than before, and a callus is produced that protects the spot from further injury. The healing of a wound involves a similar action. Again, remove one kidney or one lung and the remaining one will in time enlarge to assume, as far as it is able, the functions of both. If the leg of a salamander or a lobster be amputated, the wound not only heals but a new leg is regenerated in place of that which has been lost. If a flatworm be cut in two, the front piece grows out a new tail, the hind piece a new head, and two perfect worms result. Finally, it has been found in certain cases, including animals as highly organized as salamanders, that if the egg be separated into two parts [14]
at an early period of development each part develops into a perfect embryo animal of half the usual size, and a pair of twins results. In each of these cases the astonishing fact is that a mechanical injury sets up in the organism a complicated adaptive response in the form of operations which in the end counteract the initial mechanical effect. It is no doubt true that somewhat similar self-adjustments or responses may be said to take place in certain non-living mechanical systems, such as the spinning top or the gyroscope; but those that occur in the living body are of such general occurrence, of such complexity and variety, and of so design-like a quality, that they may fairly be regarded as among the most characteristic of the vital activities. It is precisely this characteristic of many vital phenomena that renders their accurate analysis so difficult and complex a task; and it is largely for this reason that the biological sciences, as a whole, still stand far behind the physical sciences, both in precision and in completeness of analysis.

What is the actual working attitude of naturalists towards the general problem that I have endeavored to outline? It would be a piece of presumption for me to speak for the body of working biologists, and I will therefore speak for only one of them. It is my own conviction that whatever be the difficulties that the mechanistic hypothesis has to face, it has established itself as the most useful working hypothesis that we can at present employ. I do not mean to assert that it is adequate, or even true. I believe only that we should make use of it as a working program, because the history of biological research proves it to have been a more effective and fruitful means of advancing knowledge than the vitalistic hypothesis. We should therefore continue to employ it for this purpose until it is clearly shown to be untenable. Whether we must [15]
in the end adopt it will depend on whether it proves the simplest hypothesis in the large sense, the one most in harmony with our knowledge of nature in general. If such is the outcome, we shall be bound by a deeply lying instinct that is almost a law of our intellectual being to accept it, as we have accepted the Copernican system rather than the Ptolemaic. I believe I am right in saying that the attitude I have indicated as a more or less personal one is also that of the body of working biologists, though there are some conspicuous exceptions.

In endeavoring to illustrate how this question actually affects research I will offer two illustrative cases, one of which may indicate the fruitfulness of the mechanistic conception in the analysis of complex and apparently mysterious phenomena, the other the nature of the difficulties that have in recent years led to attempts to re-establish the vitalistic view. The first example is given by the so-called law or principle of Mendel in heredity. The principle revealed by Mendel's wonderful discovery is not shown in all the phenomena of heredity and is probably of more or less limited application. It possesses however a profound significance because it gives almost a demonstration that a definite, and perhaps a relatively simple, mechanism must lie behind the phenomena of heredity in general. Hereditary characters that conform to this law undergo combinations, disassociations and recombinations which in certain way suggest those that take place in chemical reactions; and like the latter they conform to definite quantitative rules that are capable of arithmetical formulation. This analogy must not be pressed too far; for chemical reactions are individually definite and fixed, while those of the hereditary characters involve a fortuitous element of such a nature that the numerical result is not fixed or constant in the individual case but follows the law of probability in the aggregate of individuals. Nevertheless, it is possible, and has already become the custom, to designate the hereditary organization by [16] symbols or formulas that resemble those of the chemist in that they imply the *quantitative* results of heredity that follow the union of compounds of known composition. Quantitative prediction—not precisely accurate, but in accordance with the law of probability—has thus become possible to the biological experimenter on heredity. I will give one example of such a prediction made by Professor Cuénot in experimenting on the heredity of color in mice (see the following table). The experiment extended through three generations. Of the four grandparents three were pure white albinos, identical in outward appearance, but of different hereditary capacity, while the fourth was a pure black mouse. The first pair of grandparents consisted of an albino of gray ancestry, AG, and one of black ancestry, AB. The second pair consisted of an albino of yellow ancestry, AY, and a black mouse, CB. The result of the first union, AG x AB is to produce again pure white mice of the composition AGAB. The second union, AY x CB is to produce mice that appear pure *yellow*, and have the formula AYCB. What, now, will be the result of uniting the two forms thus produced [17] —*i.e.* AGAB × AYCB? Cuénot's prediction was that they should yield eight different kinds of mice, of which four should be white, two yellow, one black and one gray. The actual aggregate result of such unions, repeatedly performed, compared with the theoretic expectation, is shown in the foregoing table. As will be seen, the correspondence, though close, is not absolutely exact, yet is near enough to prove the validity of the principle on which the prediction was based, and we may be certain that had a much larger number of these mice been reared the correspondence would have been still closer. I have purposely selected a somewhat complicated example, and time will not admit of a full explanation of the manner in which this particular result was reached. I will however attempt to give an indication of the general Mendelian principle by means of which predictions of this kind are made. This principle appears in its simplest form in the behavior of two contrasting characters of the same general type—for instance two colors, such as gray and white in mice. If two animals, which show respectively two such characters are bred together, only one of the characters (known as the "dominant") appears in the offspring, while the other (known as the "recessive") disappears from view. In the next generation, obtained by breeding these hybrids together, both characters appear separately and in a definite ratio, there being in the long run three individuals that show the dominant character to one that shows the recessive. Thus, in the case of gray and white mice, the first cross is always gray, while the next generation includes three grays to one white. This is the fundamental Mendelian ratio for a single pair of characters; and from it may readily be deduced the more complicated combinations that appear when two or more pairs of characters are considered together. Such combinations appear in definite series, the nature of which may be worked out by a simple method of binomial expansion. [18] By the use of this principle astonishingly accurate numerical predictions may be made, even of rather complex combinations; and furthermore, new combinations may be, and have been, artificially produced, the number, character and hereditary capacity of which are known in advance. The fundamental ratio for a single pair of characters is explained by a very simple assumption. When a dominant and a recessive character are associated in a hybrid, the two must undergo in some sense a disjunction or separation in the formation of the germ-cells of the hybrid. This takes place in a quite definite way, exactly half the germ-cells in each sex receiving the potentiality of the dominant character, the other half the potentiality of the recessive. This is roughly expressed by saying that the germ-cells are no longer hybrid, like the body in which they arise, but bear one character or the other; and although in a technical sense this is probably not precisely accurate, it will sufficiently answer our purpose. If, now, it be assumed that fertilization takes place fortuitously—that is that union is equally probable between germ-cells bearing the same character and those bearing opposite characters,—the observed numerical ratio in the following generation follows according to the law of probability. Thus is explained both the fortuitous element that differentiates these cases from exact chemical combinations, and the definite numerical relations that appear in the aggregate of individuals.

Grandparents: AG (white) × AB (white); AY (white) × CB (black)

Parents: AGAB (white) × AYCB (yellow)

Offspring		Observed	Calculated
AGAY, ABAY, AGAB, ABAB	(White)	81	76
AGCY, ABCY	(Yellow)	34	38
ABCB	(Black)	20	19
AGCB	(Gray)	16	19
		151	152

Now, the point that I desire to emphasize is that one or two very simple mechanistic assumptions give a luminously clear explanation of the behavior of the hereditary characters according to Mendel's law, and at one stroke bring order out of the chaos in which facts of this kind at first sight seem to be. Not less significant is the fact that direct microscopical investigation is actually revealing in the germ-cells a physical mechanism that seems adequate to [19] explain the disjunction of characters on which Mendel's law depends; and this mechanism probably gives us also at least a key to the long standing riddle of the determination and heredity of sex. These phenomena are therefore becoming intelligible from the mechanistic point of view. From any other they appear as an insoluble enigma. When such progress as this is being made, have we not a right to believe that we are employing a useful working hypothesis?

But let us now turn to a second example that will illustrate a class of phenomena which have thus far almost wholly eluded all attempts to explain them. The one that I select is at present one of the most enigmatical cases known, namely, the regeneration of the lens of the eye in the tadpoles of salamanders. If the lens be removed from the eye of a young tadpole, the animal proceeds to manufacture a new one to take its place, and the eye becomes as perfect as before. That such a process should take place at all is remarkable enough; but from a technical point of view this is not the extraordinary feature of the case. What fills the embryologist with astonishment is the fact that the new lens is not formed in the same way or from the same material as the old one. In the normal development of the tadpole from the egg, as in all other vertebrate animals, the lens is formed from the outer skin or ectoderm of the head. In the replacement of the lens after removal it arises from the cells of the iris, which form the edge of the optic cup, and this originates in the embryo not from the outer skin but as an outgrowth from the brain. As far as we can see, neither the animal itself nor any of its ancestors can have had experience of such a process. How, then, can such a power have been acquired, and how does it inhere in the structure of the organism? If the process of repair be due to some kind of intelligent action, as some naturalists have supposed, why should not the higher animals and man possess a [20] similar useful capacity? To these questions biology can at present give no reply. In the face of such a case the mechanist must simply confess himself for the time being brought to a standstill; and there are some able naturalists who have in recent years argued that by the very nature of the case such phenomena are incapable of a rational explanation along the lines of a physico-chemical or mechanistic analysis. These writers have urged, accordingly, that we must postulate in the living organism some form of controlling or regulating agency which does not lie in its physico-chemical configuration and is not a form of physical energy—something that may be akin to a form of intelligence (conscious or unconscious), and to which the physical energies are in some fashion subject. To this supposed factor in the vital processes have been applied such terms as the "entelechy" (from Aristotle), or the "psychoid"; and some writers have even employed the word "soul" in this sense—though this technical and limited use of the word should not be confounded with the more usual and general one with which we are familiar. Views of this kind represent a return, in some measure, to earlier vitalistic conceptions, but differ from the latter in that they are an outcome of definite and exact experimental work. They are therefore often spoken of collectively as "neo-vitalism."

It is not my purpose to enter upon a detailed critique of this doctrine. To me it seems not to be science, but either a kind of metaphysics or an act of faith. I must own to complete inability to see how our scientific understanding of the matter is in any way advanced by applying such names as "entelechy" or "psychoid" to the unknown factors of the vital activities. They are words that have been written into certain spaces that are otherwise blank in our record of knowledge, and as far as I can see no more than this. It is my impression that we shall do better as [21] investigators of natural phenomena frankly to admit that they stand for matters that we do not yet understand, and continue our efforts to make them known. And have we any other way of doing this than by observation, experiment, comparison and the resolution of more complex phenomena into simpler components? I say again, with all possible emphasis, that the mechanistic hypothesis or machine-theory of living beings is not fully established, that it *may* not be adequate or even true; yet I can only believe that until every other possibility has realty been exhausted scientific biologists should hold fast to the working program that has created the sciences of biology. The vitalistic hypothesis may be held, and is held, as a matter of faith; but we cannot call it science without misuse of the word.

When we turn, finally, to the genetic or historical part of our task, we find ourselves confronted with precisely the same general problem as in case of the existing organism. Biological investigators have long since ceased to regard the fact of organic evolution as open to serious discussion. The transmutation of species is not an hypothesis or assumption, it is a fact accurately observed in our laboratories; and the theory of evolution is only questioned in the same very general way in which all the great generalizations of science are held open to modification as knowledge advances. But it is a very large question what has caused and determined evolution. Here, too, the fundamental problem is, how far the process may be mechanically explicable or comprehensible, how far it is susceptible of formulation in physico-chemical or mechanistic terms. The most essential part of this problem relates to the origin of organic adaptations, the production of the fit. With Kant, Cuvier and Linnaeus believed this problem scientifically insoluble. Lamarck attempted to find a solution in his theory of the inheritance of the effects of use, disuse and other "acquired characters"; but his theory was [22] insecurely based and also begged the question, since the power of adaptation through which use, disuse and the like produce their effects is precisely that which must be explained. Darwin believed he had found a partial solution in his theory of natural selection, and he was hailed by

Haeckel as the biological Newton who had set at naught the *obiter dictum* of Kant. But Darwin himself did not consider natural selection as an adequate explanation, since he called to its aid the subsidiary hypotheses of sexual selection and the inheritance of acquired characters. If I correctly judge, the first of these hypotheses must be considered as of limited application if it is not seriously discredited, while the second can at best receive the Scotch verdict, not proven. In any case, natural selection must fight its own battles.

Latter day biologists have come to see clearly that the inadequacy of natural selection lies in its failure to explain the origin of the fit; and Darwin himself recognized clearly enough that it is not an originative or creative principle. It is only a condition of survival, and hence a condition of progress. But whether we conceive with Darwin that selection has acted mainly upon slight individual variations, or with DeVries that it has operated with larger and more stable mutations, any adequate general theory of evolution must explain the origin of the fit. Now, under the theory of natural selection, pure and simple, adaptation or fitness has a merely casual or accidental character. In itself the fit has no more significance than the unfit. It is only one out of many possibilities of change, and evolution by natural selection resolves itself into a series of lucky accidents. For Agassiz or Cuvier the fit is that which was designed to fit. For natural selection,
pure and simple, the fit is that which happens to fit. I, for one, am unable to find a logical flaw in [23]
this conception of the fit; and perhaps we may be forced to accept it as sufficient. But I believe that naturalists do not yet rest content with it. Darwin himself was repeatedly brought to a standstill, not merely by specific difficulties in the application of his theory, but also by a certain instinctive or temperamental dissatisfaction with such a general conclusion as the one I have indicated; and many able naturalists feel the same difficulty to-day. Whether this be justified or not, it is undoubtedly the fact that few working naturalists feel convinced that the problem of organic evolution has been fully solved. One of the questions with which research is seriously engaged is whether variations or mutations are indeterminate, as Darwin on the whole believed, or whether they may be in greater or less degree determinate, proceeding along definite lines as if impelled by a *vis a tergo*. The theory of "orthogenesis," proposed by Naegeli and Eimer, makes the latter assumption; and it has found a considerable number of adherents among recent biological investigators, including some of our own colleagues, who have made important contributions to the investigation of this fundamental question. It is too soon to venture a prediction as to the ultimate result. That evolution has been orthogenetic in the case of certain groups, seems to be well established, but many difficulties stand in the way of its acceptance as a general principle of explanation. The uncertainty that still hangs over this question and that of the heredity of acquired characters bears witness to the unsettled state of opinion regarding the whole problem, and to the inadequacy of the attempts thus far made to find its consistent and adequate solution.

Here, too, accordingly, we find ourselves confronted with wide gaps in our knowledge which open the way to vitalistic or transcendental theories of development. I think we should resist the
temptation to seek such refuge. It is more than probable that there are factors of evolution still [24]
unknown. We can but seek for them. Nothing is more certain than that life and the evolution of life are natural phenomena. We must approach them, and as far as I can see must attempt to analyze them, by the same methods that are employed in the study of other natural phenomena. The student of nature can do no more than strive towards the truth. When he does not find the whole truth there is but one gospel for his salvation—still to strive towards the truth. He knows that each forward step on the highway of discovery will bring to view a new horizon of regions still unknown. It will be an ill day for science when it can find no more fields to conquer. And so, if you ask whether I look to a day when we shall know the whole truth in regard to organic mechanism and organic evolution, I answer: No! But let us go forward.

CHEMISTRY, by Charles F. Chandler, *Professor of Chemistry*.

ASTRONOMY, by Harold Jacoby, *Rutherfurd Professor of Astronomy*.

GEOLOGY, by James Furman Kemp. *Professor of Geology*.

BIOLOGY, by Edmund B. Wilson, *Professor of Zoology*.

PHYSIOLOGY, by Frederic S. Lee, *Professor of Physiology*.

BOTANY, by Herbert Maule Richards, *Professor of Botany*.

ZOOLOGY, by Henry E. Crampton, *Professor of Zoology*.

ANTHROPOLOGY, by Franz Boas. *Professor of Anthropology*.

ARCHAEOLOGY, by James Rignall Wheeler, *Professor of Greek Archaeology and Art*.

HISTORY, by James Harvey Robinson, *Professor of History*.

ECONOMICS, by Henry Rogers Seager, *Professor of Political Economy*.

POLITICS, by Charles A. Beard, *Adjunct Professor of Politics*.

JURISPRUDENCE, by Munroe Smith, *Professor of Roman Law and Comparative Jurisprudence*.

SOCIOLOGY, by Franklin Henry Giddings, *Professor of Sociology*.

PHILOSOPHY, by Nicholas Murray Butler. *President of the University*.

PSYCHOLOGY, by Robert S. Woodworth, *Adjunct Professor of Psychology*.

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