Progress in Biology.¹

By W. BATESON, F.R.S.

AN address on progress in biology during the last hundred years has one element of simplicity; since, with scarcely a tinge of exaggeration, the whole subject from its inception may be held included. Though the materials studied by biologists are those which have been the objects of man's curiosity from the earliest times, yet the biological way of looking at them was new, and biology was a term deliberately selected to proclaim the consciousness of a new hope. Treviranus—Gottfried Reinhold, 1776–1837, of Bremen, elder brother of the well-known botanist Ludolf Christian (1779–1864)—was the first to use the word *Biology* (1802). He complained that the current treatments both of zoology and botany were lacking in penetration and in comprehensiveness, and that their practitioners were too often giving a divided attention, with an eye ever wandering towards medicine and other applications. The catalogues of plants and animals, and barren descriptions compiled in the name of those sciences, are a beginning, not an end. They are the materials which the science of living things is to absorb and co-ordinate. The problem before the biologist is, What is Life? and the collections have value in so far as they contribute to a solution of that problem.

Treviranus says all this, and says it very well, with a lofty but well-controlled imagination. If some one objects that he is offering old things in a new form he will not deny that, but he claims that to see them in the new form is no trifling help. Surely he has proved right. The new word connoted a new thought. Though it was not given to him to see further into the mystery than his contemporaries, nor indeed so far as many, notably Bichat and Lamarck, he was looking in the right direction, and he polarised the attention of many more.

Sir William Lawrence introduced the word into English in those lively "Lectures on Physiology" (ed. 1818) which, in spite—or perhaps in consequence—of much detraction, went through many editions and were very widely read. He too was aware that the term biology implied a certain ambition.²

We see other signs that about a century ago the study of life underwent a renaissance. Before biology was dreamed of there were plenty of good naturalists, men devoted and exact, full of curiosity, gathering truth where they could, in the right spirit of science, but it is at this critical period that we first hear those graver notes of more resolute endeavour which thenceforth constantly recur. The gain in depth and precision was of course promoted by the development of microscopy, but the aspirations of the first biologists to obtain a fuller understanding of the nature of life had been excited, and their minds were prepared before that visible revelation came in unquestionable form. I like at least to think that the questions were asked before the instrument-makers came on the field. Sage Sidrophel, who

"made an Instrument to Know
If the Moon shine at Full or no,"

¹ Address delivered on March 12 in connexion with the centenary of the Birkbeck College.

² See especially pp. 52 and 58–60, ed. 1823.

has been a great begetter of modern researches, but less fortuitous discoveries are the more honourable and command a warmer admiration.

However that may have been, as a matter of history the first great advance was the recognition of the cell; and nowadays, when we talk of biology, we mean the study of life in terms of cells—their structure, properties, and behaviour. The microscope led soon, though not immediately, to the cell theory, and cellular biology began. It is not my purpose to repeat that familiar story. I ask you to observe, however, that, little as we yet know of life, it is not a century since we began to be certain about even the proximate and easily accessible phenomena which characterise life. The rudiments of chemistry, to be sure, were still only dimly apprehended, and it may be thought that, in the absence of chemical knowledge, sound ideas about life could scarcely be expected. Perhaps; but surely the essential feature which—apart from psychical attributes—differentiates living creatures from all other systems whatever, might have been distinguished by ordinary observation, at any time. Living organisms are systems which have the power of continual and spontaneous division. In that they are unique. *How* they divide could not even be imagined, much less investigated, without the microscope; but nothing in the history of discovery is more curious than this fact, that until well into the nineteenth century, men should not have known familiarly that living things come into existence solely by a process of orderly division. The conception of the cell as a unit was necessary to give anything like accuracy to this knowledge, but it was not essential, and may not improbably be replaced hereafter.

Scarcely less remarkable is the fact that upon this process of division attention has only quite recently been concentrated. The other attributes of life, chemical and physical, have proved both more amenable to experimental study and more prolific in fine discoveries immediately applicable to medical practice; with the result that to them—at least in our English use—the once comprehensive term physiology has become exclusively attached. An overburdened study must discard something, but it is scarcely fortunate that the process of genetical division, the central phenomenon of physiology, should in its several manifestations—heredity, variation, segregation, differentiation, and the regulation of form—be unfamiliar ground to academic physiologists.

I have referred to the moment from which our survey begins as a time of renaissance. The student of to-day sometimes scarcely realises what happened then. A hundred years ago you might find an undescribed—even an unnamed—animal any day on the British coasts. Next to nothing was known of the development of any creature whatsoever, animal or plant. The student's microscope, of course, did not exist, and indeed was only coming into general use in my own student days. Neither my revered master, the professor of zoology of that time in Cambridge—a man of great learning and distinguished gifts—nor his colleague,

the professor of botany, could use a microscope to any purpose; and I doubt if either of them would have seen much in a section of anything. No doubt we were a bit behind the rest of the world just then. They were ahead of us in France, and very greatly ahead in Germany. Reputation, if not fortune, might occasionally be made by a little judicious dumping of foreign products. That state of things happily soon passed away, and an international standard of scientific public opinion in biology became universal.

Though in 1824 little was known of what we now call morphology, the unicellular organisms had not attracted serious attention at all. Such creatures, even some bacteria, had been seen, but no one had felt much concern about them. If you care to attach history to dates, let me give you two easy ones, worth remembering. 1838 is the year in which Schleiden gave formal expression to the cell theory, and being also the date of Ehrenberg's "Infusionsthierchen" with the first good pictures of bacteria and many infusoria, is doubly memorable. Ten years later we come to 1848, a date which in significance to mankind may rank with 753 B.C., or, indeed, any of the greatest anniversaries.

In written history there are only a few cardinal points—the invention of printing, the First Folio Shakespeare, Newton's "Principia," the discovery of oxygen, with some dozen more. Of these most certainly one came in 1848, when Pasteur first demonstrated to Biot his observations on the asymmetry of tartrates. From that one small bit of clean experimenting, interpreted by penetrative genius of the highest order, has grown modern surgery, a rational investigation of disease, bacteriology, much of biochemistry, hygiene, with all the consequences that those developments have had on vital statistics, the structure of populations, and contributions to almost all the arts and the multifarious activities of civilised man. 1848 was the year of the revolutions. The public was not thinking much about bacteria or biology. Mobs were in the streets and the fermentations then most regarded were metaphorical. But the world as we see it to-day is rather the product of biological discovery than of democratic institutions, and there are moments when your modern Esau would wisely barter a lot of his political rights for some honest antiseptic.

The great corpus of knowledge grows by solid increments, definite, predicable discoveries of fact. Rarely is any piece of interpretation an event of equal consequence. In that small group of fertile theories, by common consent we class the "Origin of Species." For the public of 1859 Charles Darwin's book seemed, and for most of the laity to-day it still seems, like some meteor of the heavens, to have been a revelation unheralded. That of course we know it was not. Apart from earlier adumbrations, some vague, some clear, the doctrine of evolution first took categorical form in the hands of Lamarck (1809). For fifty years the new ideas were strenuously debated, especially in France. The discussion, nevertheless, had reached England, though our contributions were largely on the non-placet side. Chambers (1844) was a timid evolutionist; Herbert Spencer, though no naturalist, a bold one (from 1852). Lyell in 1832 had argued against "evolution," adopting the word. Sir W. Lawrence had (1818) collected many illustrations of variability, but maintains

that none transgress the limits of specific difference, and he took a firm stand against the Lamarckian teaching of the transmission of acquired characters, which he declared was contrary to experience—the first, I believe, actively to denounce that illusion.

The very considerable work of Godron, "De l'Espèce" appeared in book-form in 1859,³ the year of the "Origin." Godron was a most competent botanist. His collection of evidence as to variation was the fullest then compiled. Most of it was afterwards incorporated in Darwin's "Animals and Plants under Domestication," but the book is still of value. Godron examines "la théorie de l'évolution successive de l'espèce," and rejects it. Darwin, using very similar evidence—though greater in quantity and more varied in character—with the introduction of the one wholly new consideration, natural selection, succeeded in making the doctrine of transformation acceptable. Natural selection was an undeniable *vera causa*, whereas few had felt quite comfortable about Lamarck's appeal to the effects of conditions. The presentation, moreover, was set forth in language so suave and cogent that the reader gladly resigns himself into the hands of a master. With the advances in knowledge, the additions from embryology, from palæontology, geographical distribution, and many other convergent lines, the truth of the doctrine of evolution became in broad outline finally established.

This much being admitted by all, we may inquire why had so many good naturalists, whose information was sound, resisted so firmly? With the exception of natural selection, every essential element in Darwin's case had been present to their minds before. To suppose that all these writers had suffered from theological obsessions is absurd. Lawrence, for example, flouted authority with great enjoyment. They were genuinely convinced, some of them probably against their will, that the mutability of species was contrary to observation. Remember that up to Linnæus few, if any, troubled to consider whether species were or were not mutable; but after species had been declared to be immutable, those who proposed to maintain the doctrine of mutability might be expected to prove their case.

What weighed, then, with Godron and careful men of his type was that the variability they observed did not result in the production of new species, and that in particular, as they insisted, the new forms derived from a single common origin, when interbred, do not produce offspring of impaired fertility as so many genuine species do. This critical link in the evidence is equally absent from Darwin's case. He satisfies the mind about so many other difficulties that this one is allowed to pass, and the reader, learning that many putative species give hybrids fully fertile and that between these and the totally sterile hybrids all gradations can be seen, feels that this objection must have been removed, though as a matter of fact it remains.

The progress of the last twenty years has brought us to a position from which we can at last begin to discuss these problems fruitfully. No general principles governing the incidence of interspecific sterility have been ascertained. We there find ourselves in a tangle

³ Following previous publication in *Mém. Acad. Stanislas*, Nancy 1848-9.

of empirical and as yet unrelated phenomena, specific like those of chemistry. Expectation founded on our estimates of divergence are constantly at fault. The Oxen, the Canidæ, the Finches, the Ducks, the Pheasants: who can say right off which crosses in these orders give hybrids fertile in both sexes, or in one sex only, or in neither? Which species cannot be crossed at all? Like chess openings, these things, no doubt, are governed by principle, but the principle is not obvious. For the right answers we depend largely on memory. They have been ascertained by accumulated experience and are not easily found "over the board."

We are clear that the forms of life are the products of evolution, but we are equally sure that specific distinctions are not culminating terms reached by the accumulation of small differences. The variations by which they have arisen are not yet known to us, but we are satisfied that the particular account of their origin which is the one Darwin chiefly favoured is incorrect.

Of the origin of specific distinctions we have, as I have said, no acceptable account. Appeals to adaptative value are here beset by the gravest improbability. The Darwinian principle that the fixity of a character is a measure of its value to the possessor is not tenable. The sharpest and most permanent specific differences are constantly to be found as characteristics which no one by the utmost exercise of ingenuity has been able to represent as other than trivial in adaptative significance. Whatever stress we are disposed to lay on natural selection, it does not assist us here.

We must frankly admit that modern discoveries have given little aid with the problem of the origin of

adaptation. Darwin in 1844 regarded all adaptation as the consequence of natural selection. His letters of that date speak of Lamarck with contempt. With the lapse of time, nevertheless, we find him frequently and increasingly appealing to the transmitted effects of the conditions of life, and between the two he sometimes does not distinguish so clearly as we would wish. His most urgent task was to make evolution an acceptable principle, and one argument failing he would invoke the other, until in the edition of 1876 certain passages read uncommonly like Lamarck obscured. Seizing upon one which is, to say the least of it, ambiguous, the irreverent Samuel Butler makes the flippant comment: "This comes of tinkering. We do not know whether we are on our head or our heels. We catch ourselves repeating 'important,' 'unimportant,' 'unimportant,' 'important,' like the King when addressing the jury in 'Alice in Wonderland.'" ⁴

All this matters little now. In response to Weismann's challenge that critical proof of the transmission of adaptative responses to environmental influences should be produced, none satisfactory has been forthcoming. In one respect only, I think, we have to recognise positive evidence which Weismann did not perhaps sufficiently anticipate. The germ-cells have in certain experiments been injured by special and violent treatment to which the parents have been exposed, with permanent consequences to the posterity. But there is no question here of adaptation, and such evidence does not make the origin of the adaptative mechanisms more easy to understand.

⁴ "Luck or Cunning," 1887, pp. 185-6.

(To be continued.)

Science and Philosophy.¹

By Prof. H. WILDON CARR.

II.

WHEN Auguste Comte proclaimed the passing of metaphysics and the coming of positive science, the positivity which he claimed for science depended on two presuppositions—first, that the independent existence of the physical universe is unquestionable, and second, that the authority of reason is unchallengeable. The principle of relativity has questioned the first, the theory of creative evolution has challenged the second. The two theories may seem to be quite disconnected, but when we consider the complete revolution which the fundamental concepts of science have undergone since they were formulated, they are seen to be very closely related. The first concerns the relation of philosophy to physics; the second, with which this article deals, concerns the relation of philosophy to biology and psychology.

The experience of consciousness or awareness seems to be in all simplicity a mind's contemplation of a self-revealing world. Every attempt to go behind consciousness, to treat it scientifically as an object, to analyse its nature and discover its genesis, seems to involve a vicious circle and end in a meaningless tautology. Science seems to depend on accepting consciousness at its face value, on regarding it as absolute and unquestionable in its first intention. The

reason is obvious. Consciousness is the basis of reasoning, the ultimate and final arbiter, the only authoritative court of appeal. It alone justifies the affirmation that anything is and what anything is. If consciousness be itself arraigned at its own judgment bar, it must be at once both judge and judged, and to present this double relation to thought surpasses the power of imagination or even of intellectual conception. Therefore, though we may conceive a mind inefficient to any degree, and the data presented to a mind obscure and limited to any degree, to submit consciousness itself to analysis seems to undermine the authority of the only criterion to which science can appeal. Science cannot raise the question what consciousness is, notwithstanding that it can and must inquire how and under what conditions it makes its appearance. Yet on the other hand it is impossible for science to be satisfied with such an exclusion. For biology, consciousness is a particular phenomenon. It characterises living forms, which to be understood must be studied in their integral nature, and therefore to assume its unquestionability is to spoil the symmetry of science and destroy its ideal. Moreover, it renders science vulnerable in its most vital principle.

This peculiar dilemma that science must postulate the absolute nature of consciousness for its knowing, and at the same time treat this knowing as a pheno-

¹ Continued from p. 613.