EVOLUTION

DARWINIAN AND SPENCERIAN

THE HERBERT SPENCER LECTURE

DELIVERED AT THE MUSEUM
8 DECEMBER 1910

BY

RAPHAEL MELDOLA, F.R.S.

OXFORD
AT THE CLARENDON PRESS
MCMX

HENRY FROWDE, M.A.
PUBLISHER TO THE UNIVERSITY OF OXFORD
LONDON, EDINBURGH, NEW YORK
TORONTO AND MELBOURNE

EVOLUTION: DARWINIAN AND SPENCERIAN

'For certain it is that God worketh nothing in Nature but by second causes; and if they would have it otherwise believed, it is mere imposture, as it were in favour towards God; and nothing else but to offer to the Author of Truth the unclean sacrifice of a lie.'

BACON, *Advancement of Learning*, Book I.

AMONG the great generalizations of the mid-Victorian era—that period which has witnessed such enormous advances in every department of natural science—the doctrine of Evolution stands out pre-eminently. With the foundation of that doctrine the names of our two great countrymen, Charles Darwin and Herbert Spencer, are indissolubly associated. The acceptance by this ancient University of a lectureship bearing the name of one of the founders of the modern doctrine is a sign of the times of the deepest significance in the intellectual development of this country. The century of Darwin's birth and the fiftieth year of the publication of the *Origin of Species* were celebrated here and at Cambridge last year; the tributes paid to his memory on those occasions are still fresh in our minds. Throughout the international chorus of admiration for the work of our great naturalist there rings out one clear note proclaiming that the method of viewing the process of organic development made known by Darwin and Wallace marked the beginning of a new epoch in human thought.

The history of evolutionary ideas in general, and of the special form of organic evolution associated with the names of Darwin and Wallace, has been fully dealt with by many able writers. It may be difficult to place the subject in any new light; nevertheless, on the present occasion, the year following the Darwinian celebrations, it seemed not inappropriate that I should endeavour to institute a comparison between the methods of the two great founders of the modern school of Evolution—to trace the impression left by each upon current thought and to attempt an analysis of the causes which have determined their respective influences.

At the outset, and in order to bring the subject within reasonable limits, let it be understood that, having no claim to have been a student of Philosophy in the special sense, I propose restricting the treatment mainly to the scientific aspect of the writings of Darwin and Spencer. It may further be useful, in view of the widespread popular belief that Evolution and Darwinism are synonymous, to insist once more upon the fact that Darwin and Wallace gave us a theory of organic development which Spencer incorporated in that general scheme of Evolution which he had independently elaborated. It is perhaps scarcely necessary to add that Evolution as a philosophical principle does not stand or fall with the proof or disproof of Natural Selection as a theory of species formation.

Both Darwin and Spencer have, fortunately for posterity, left a complete record of the development of the evolutionary idea in their own minds, so that full details of the various stages are now unnecessary. It will be sufficient for the present purpose if I select a few dates marking the more conspicuous phases. During the voyage of the *Beagle* Darwin had considered the possible mutability of species, partly as the result of his own observations and partly as the result of the discussion of the question in Lyell's *Principles of Geology*. His first systematic note-book on this subject was opened in 1837, after his return to England. In 1838—a memorable epoch for natural science—the theory of Natural Selection was conceived as the result of his perusal of Malthus *On Population*. The first rough draft of his views was prepared in 1842, and this, thanks to his son, Dr. Francis Darwin, we now possess in a permanent form. The first draft was copied and enlarged in 1844, and the larger and more comprehensive work was commenced twelve years later, viz. in 1856. The first public declaration of his views was made—as he thought prematurely—in 1858, owing to the independent discovery of the principle of Natural Selection by Alfred Russel Wallace. The history of this splendid example of scientific magnanimity is well known, but cannot be too often referred to as a pattern of chivalry and of intellectual greatness for the guidance and encouragement of the younger generations. The first edition of the *Origin of Species* was published on November 24, 1859.

During the Darwinian celebration here last year it was claimed that through Lyell, who was a pupil of Buckland's, Oxford had some influence in moulding the career of Charles Darwin, whose indebtedness to the illustrious pioneer of modern Geology is notorious. Such influence as Buckland had in forming Lyell's geological views was, however, of a negative rather than of a positive character, for the pupil's reputation was ultimately made by overthrowing the teaching of his master. In a similarly indirect way, and also through Lyell, Oxford may be said to have influenced Herbert Spencer, since he first read the *Principles of Geology* in 1840, when twenty years old, and the arguments advanced in the early editions of that work *against* Lamarck's theory of animal development led him, as he has told us in his *Autobiography*, to 'a partial acceptance of Lamarck's views'.[1] In a more direct way may Oxford claim also to have influenced Spencer, since Dean Mansel, the author of those well-known Bampton Lectures so freely quoted in the *First Principles*, was a distinguished member of this University. Whether Spencer's early acceptance of Lamarckism is responsible for his later tenacity in combating the views of that school of biologists founded by Weismann is a point which might serve for academic discussion, but whatever

[1] See also the 'Filiation of Ideas' in Duncan's *Life and Letters*, p. 536.

view may be held concerning his attitude with respect to this question, there can be no doubt that his mind was given a bias towards development as a principle at this early stage in his career. Through all his subsequent writings the underlying idea of development can be traced with increasing depth and breadth, expanding in 1850 in his *Social Statics* to a foreshadowing of the general doctrine of Evolution.[1] In 1852 his views on organic evolution had become so definite that he gave public expression to them in that well-known and powerful essay on *The Development Hypothesis*, first published in *The Leader*. In the *Principles of Psychology*, the first edition of which was published in 1855, the evolutionary principle was dominant. By 1858—the year of the announcement of Natural Selection by Darwin and Wallace—he had conceived the general scheme and had sketched out the first draft of the prospectus of the *Synthetic Philosophy*, the final and amended syllabus having been issued in 1860. The work of Darwin and Spencer from that period, although moving along independent lines, was directed towards the same end, notwithstanding the diversity of the materials which they made use of and the differences in their methods of attack; that end was the establishment of Evolution as a great natural principle or law.

This very brief epitome will make it clear that as an Evolutionist Spencer stands in an absolutely independent position. Up to the period of the publication of the *Origin of Species* and the conception of the scheme of the *Synthetic Philosophy* there had been no contact of thought between the two founders of the new doctrine beyond the presentation by Spencer of a copy of his Essays to Darwin in 1858, which present was duly acknowledged and, apparently, in such laudatory terms that Spencer has withheld the letter from publication.[2] He was absent from London on that memorable 1st of July, 1858, when the papers of

[1] 'Filiation of Ideas,' p. 541.
[2] See Darwin's *Life and Letters*, vol. ii, p. 141.

Darwin and Wallace were communicated to the Linnean Society.[1] But it is of interest to note that Spencer himself, as in the case of certain other pre-Darwinian writers, had come very near the conception of Natural Selection without grasping its full significance. In the *Westminster Review* for April, 1852, there appeared an article on a 'Theory of Population', in which occurs the following idea, afterwards transferred to the *Principles of Biology*:—

'And here, indeed, without further illustration, it will be seen that premature death, under all its forms and from all its causes, cannot fail to work in the same direction. For as those prematurely carried off must, in the average of cases be those in whom the power of self-preservation is the least, it unavoidably follows that those left behind to continue the race must be those in whom the power of self-preservation is the greatest—must be the select of their generation. So that whether the dangers of existence be of the kind produced by excess of fertility, or of any other kind, it is clear that by the ceaseless exercise of the faculties needed to contend with them, and by the death of all men who fail to contend with them successfully, there is ensured a constant progress towards a higher degree of skill, intelligence, and self-regulation—a better co-ordination of actions—a more complete life.'

The effect of the publication of the *Origin of Species* upon the mind of an Evolutionist of such pronounced views as Spencer is an interesting episode in the history of his work. With scientific candour he at once admitted the cogency of Natural Selection. Up to that time organic evolution had been for him, tacitly if not avowedly, Lamarckism—the only mechanism of development then known.[2] The Darwin-Wallace factor was thereafter given its proper function in the process of evolution—not to the exclusion of Lamarckism, but side by side therewith as an efficient cause of modification. The precise terms of his acceptance of the new view of species formation have

[1] *Autobiography*, vol. iii, p. 27.

[2] Buffon's 'direct action of environment' is included under this term.

been recorded in the *Autobiography* and elsewhere, and are of importance from the present point of view because he has himself explained his failure to realize the full significance of this factor which he came so nearly discovering in 1852. Referring to the passage quoted above from his *Westminster Review* article of that date, he says:—

'This paragraph shows how near one may be to a great generalization without seeing it. Though the process of Natural Selection is recognized, and though it is ascribed a share in the evolution of a higher type, yet the conception must not be confounded with that which Mr. Darwin has worked out with such wonderful skill, and supported by such vast stores of knowledge. In the first place, Natural Selection is here described only as furthering direct adaptation—only as aiding progress by the preservation of individuals in whom functionally produced modifications have gone on most favourably. In the second place, there is no trace of the idea that Natural Selection may, by co-operation with the cause assigned, or with other causes, produce *divergence* of structure; and of course, in the absence of this idea, there is no implication even that Natural Selection has anything to do with the origin of species. And, in the third place, the all-important factor of variation—"spontaneous" or incidental as we may otherwise call it—is wholly ignored. Though use and disuse are, I think, much more potent causes of organic modification than Mr. Darwin supposes—though, while pursuing the inquiry in detail, I have been led to believe that direct equilibration has played a more active part even than I had myself at one time thought, yet I hold Mr. Darwin to have shown beyond question that a great part of the facts—perhaps the greater part—are explicable only as resulting from the survival of individuals which have deviated in some indirectly-caused way from ancestral type. Thus the above paragraph contains merely a passing recognition of the selective process, and indicates no suspicion of the enormous range of its effects, or of the conditions under which a large part of its effects are produced.' [1]

Still more explicit in the description of his attitude towards Natural Selection are the following extracts from

[1] *Principles of Biology*, 1867, p. 500, note.

his *Autobiography* in which, with reference to the same article of 1852, he says:—

'It seems strange that, having long entertained a belief in the development of species through the operation of natural causes, I should have failed to see that the truth indicated [in the article quoted] must hold, not of mankind only, but of all animals, and must everywhere be working changes among them. If when human beings are subjected by pressure of population to a competition for the means of subsistence, it results that on the average the tendency is for the select of their generation to survive, so, little by little, producing a better adapted type, then the like must happen with every other kind of living thing similarly subjected to the "struggle for existence". And if so, this must be in all cases a cause for modification. Yet I completely overlooked this obvious corollary—was blind to the fact that here was a universally-operative factor in the development of species. There were, I think, two causes for this oversight.

'One was my espousal of the belief that the inheritance of functionally produced modifications suffices to explain the facts. Recognizing this as a sufficient cause for many orders of changes in organisms, I concluded that it was a sufficient cause for all orders of change. There are, it is true, various phenomena which did not seem reconcilable with this conclusion; but I lived in the faith that some way of accounting for them would eventually be found. Had I looked more carefully into the evidence and observed how multitudinous these inexplicable facts are—had I not slurred over the difficulties, but deliberately contemplated them, I might perhaps have seen that here was the additional factor wanted.

'A further cause was that I knew little or nothing about the phenomena of variation. Though aware that deviations of structure, in most cases scarcely appreciable but occasionally constituting monstrosities, occur among all organisms, yet I had never been led to think about them. Hence there lacked an indispensable idea. Even had I become distinctly conscious that the principle of the survival of the select must hold of all species, and tend to modify them, yet, not recognizing the universal tendency to vary in structure, I should have failed to recognize a chief reason why divergence and re-divergence must everywhere go on—why there must arise multitudinous differences of species otherwise inexplicable.'[1]

Again, referring to the state of his belief prior to the enunciation of the theory of Natural Selection, he states that before that period he was wholly Lamarckian :—

'I held that the sole cause of organic evolution is the inheritance of functionally produced modifications. The *Origin of Species* made it clear to me that I was wrong, and that the larger part of the facts cannot be due to any such cause.'[2]

He goes on to say that any annoyance he may have felt at having missed this great principle in 1852 was overwhelmed by his satisfaction at having the theory of organic evolution so completely justified, and thus giving support to that more general doctrine which he was advocating.

The effect of the Darwinian factor upon Spencer's views is amply set forth in the foregoing extracts. There is a touch of the true scientific spirit about these admissions which cannot but add lustre to the personality of the author of the *Synthetic Philosophy*, of whom Huxley said that outside the rank of biologists he (Spencer) was the only man known to him 'whose knowledge and capacity compelled respect, and who was, at the same time, a thorough-going Evolutionist' when Darwin's great work was published.[3] Whether Spencer's writings, without the impetus given to evolutionary thought by Darwin and Wallace, would have converted the scientific world is a question upon which we, who have become Evolutionists in post-Darwinian times, can hardly form a just opinion. Judging from the storm of opposition which Darwin's views at first met with among scientific men, it may,

[1] Op. cit., vol. i, p. 388 et seq.

[2] Ibid., vol. ii, p. 49. Also 'Filiation of Ideas' in Duncan's *Life and Letters*, p. 540. In a letter to Sir Edward Fry written in 1894, he again admits that in his pre-Darwinian writings he ascribed too much to the inheritance of 'functionally produced modifications'; Duncan, ibid., p. 351.

[3] Darwin's *Life and Letters*, vol. ii, p. 188.

however, be surmised that Spencer's treatment, however powerful, would not have carried the naturalists of that period much beyond the Lamarckian position. If a theory of organic evolution in which a true working mechanism had been suggested failed to carry conviction to scientific workers, still less is it likely that any effect would have been produced by a theory which assumed an inadequate mechanism. Huxley, who was on terms of personal friendship with Spencer, has told us that he remained unconvinced until the enunciation of the Darwinian theory :—

'I took my stand upon two grounds : firstly, that, up to that time, the evidence in favour of transmutation was wholly insufficient; and, secondly, that no suggestion respecting the causes of the transmutation assumed, which had been made, was in any way adequate to explain the phenomena. Looking back at the state of knowledge at that time, I really do not see that any other conclusion was justifiable.'[1]

At this stage in the history of Evolution it becomes more distinctly evident that Darwin and Spencer had approached the subject with different types of mind, and were, so to speak, addressing different audiences. Both had set out from data furnished by living organisms, his observations upon geographical distribution and geological succession having first led Darwin to question the current belief in the immutability of species. Spencer had taken for his raw material the super-organic phenomena resulting from the complex activities displayed by human aggregates. From the time of their public advocacy of Evolution as a principle the two pioneers also pursued different paths. The first volume of the Synthetic Philosophy, *First Principles*, was published in 1862, and the revised edition in 1867. The first volume of the first edition of the *Principles of Biology*, the work which might naturally be expected to bring into most intimate comparison the methods of the two founders,

[1] Darwin's *Life and Letters*, vol. ii, p. 188.

was published in 1864, and the second volume in 1867. By that time the *Origin of Species* had been before the public for eight years, and the Darwinian principle of selection had become an integral part of Spencer's mechanism of organic evolution. He was, in fact, so far as concerns the species question, an orthodox Darwinian who gave somewhat more weight to the Lamarckian factors than Darwin himself. The term 'survival of the fittest', approved by both Darwin and Wallace as an alternative for 'Natural selection', was, as is well known, introduced by Herbert Spencer.[1]

Whether for acquiescence or for disagreement there is no doubt that both public and scientific discussion concentrated itself mainly upon Darwin's writings, and that the fame of his illustrious contemporary was, so far as he handled biological questions, temporarily eclipsed by the brilliant demonstrations of 'descent with modification' which the author of the *Origin* had marshalled in his classical work. My own case is, I imagine, typical of the general attitude of the younger school of naturalists of that period. We had read the *Origin of Species* and had mastered—or thought we had mastered—the views of its author. With no very strong prepossession in favour of the orthodox view of species formation by special creation, we required but little persuasion to convert us into Evolutionists. If any feeling of astonishment arose it was that such simple and effective causes and such conclusive reasoning as was contained in Darwin's work should have given rise to any controversy at all. Herbert Spencer was to most of us a name of which we had heard vaguely, but which had, as we thought—I refer to the late sixties and early seventies—no special connexion with natural science. It must be frankly admitted that many, if not the majority, of students of that period were

[1] See 'Filiation of Ideas', p. 559. Also Wallace to Darwin, 1866, *More Letters*, vol. i, p. 268, and Darwin to Wallace in 1866. Ibid., p. 270.

introduced to Spencer through Darwin. Such of us as ventured to read the *First Principles* then learnt that the theory of organic evolution propounded in the *Origin of Species* was the application to one domain of Nature of a broader principle which Spencer had shown held good throughout every domain of Nature; that organic evolution by Natural Selection was a particular phase of the evolutionary process. Whatever might be the special mechanism which had given rise to particular lines of development among the various groups of natural phenomena, the principle that development there had been throughout the past, that development was going on at the present time, and that development would go on in the future was brought home by Spencer's treatment in a way that could not fail to leave a lasting impression upon minds still in the plastic stage of studentship. In brief, it was to Spencer that we were indebted for the expansion and consolidation of our ideas of Evolution, and speaking now, after an interval of some forty years, I see no reason for modifying this general impression, even if we have since, with the progress of knowledge, found reasons for dissenting from some of his views. The points on which many modern workers have departed from the teaching of the author of the *Synthetic Philosophy*—even if they should prove to be altogether right and Spencer altogether wrong—are, in fact, in relation to the general doctrine, but minor points affecting the special mechanism of evolution in particular classes of cases. The broad principle remains unshaken, and Spencer is unquestionably the thinker whose name will go down to posterity as *par excellence* the Philosopher of Evolution of the nineteenth century.

In stating that the writings of Darwin were of paramount influence in moulding public and scientific opinion it must not be concluded that Spencer exerted no influence or that his treatment of Evolution was considered inadequate in the early days of the doctrine. Such differences in impressing their contemporaries as are known to have existed are, from an abstract point of view, no more than an expression of the public attitude towards Science and Philosophy respectively. Spencer would have been read and his fame established even if the principle of Selection had not been discovered in his time. It must not be forgotten that this Darwinian principle has as yet been conclusively shown to hold good only in the world of life. How far it can be legitimately extended, whether it can indeed be extended at all into the domain of inorganic nature, is a matter for future decision. But we are now prepared for Evolution in every domain; perhaps it is not going too far to say that both Science and Philosophy have accepted Evolution as a faith—it is for Science to determine the *modus operandi* in each particular domain. It will be remembered that, as in the case of most great generalizations, thought had been moving in this direction for many years before Spencer definitely formulated the doctrine. Lamarck and Buffon had suggested a definite mechanism of organic development, Kant and Laplace had suggested a principle of celestial evolution, while Lyell had placed geology upon an evolutionary basis. The principle of Continuity was beginning to be recognized in physical science by those who, like Herschel, Mary Somerville—a name perpetuated by this University—and Grove, had approached the subject from the inorganic side. 'The correlation of the physical forces'—to use Grove's title—was the qualitative predecessor of that quantitative doctrine which in the hands of Mayer, Helmholtz, Joule, and William Thomson, afterwards Lord Kelvin, culminated in the great generalization now known as the Conservation of Energy. It was Spencer who brought these independent lines of thought to a focus, and who was the first to make any systematic attempt to show that the law of development expressed in its widest and most abstract form was universally followed throughout cosmical processes, inorganic, organic, and superorganic.[1]

[1] In 1860, when returning the proofs of the *First Principles*, Huxley

The distinction between Evolution and Darwinism, although constantly pointed out, still seems to me to require emphasizing—not only in justice to Spencer, but also with a view to stimulate further inquiry into the possibility of extending the Darwinian principle of Selection or Survival to departments of Evolution to which it has not yet been applied. For example, a most highly suggestive hypothesis applying the principle of the survival of stable mechanical systems of corpuscles or electrons to the evolution of the atoms of the chemical elements was advanced by Sir George Darwin in 1905.[1]

Strictly and equitably considered it must be admitted that Darwin's influence upon those fields of Evolution in which the principle of Selection does not or has not yet been proved to hold good has been of an indirect character. I venture to think that an impartial consideration of the achievements of the two great pioneers is hardly possible now, for their work has been accomplished so near our own times that we are as yet unable to obtain a correct perspective view of their relative positions. In what may be called non-selectional Evolution, Spencer must certainly be credited with as much direct influence as was produced by Darwin upon organic Evolution, although of course the possibility must always be borne in mind

wrote to Spencer: 'I rejoice that you have made a beginning, and such a beginning—for the more I think about it the more important it seems to me that somebody should think out into a connected system the loose notions that are floating about more or less distinctly in all the best minds. It seems as if all the thoughts in what you have written were my own, and yet I am conscious of the enormous difference your presentation of them makes in my intellectual state. One is thought in the state of hemp yarn, and the other in the state of rope. Work away then excellent rope-maker. . . .' *Life and Letters of Huxley*, vol. i, p. 213.

[1] *British Assoc. Rep. South Africa*, 1905, pp. 10–14. Crookes also, in his Presidential Address to Sect. B of the British Association at Birmingham in 1886, advanced arguments in favour of the evolution of the chemical elements from 'protyle', but no mechanism was suggested. Lockyer has marshalled the astro-physical evidence in his work on 'Inorganic Evolution', 1900.

that what is apparently non-selectional Evolution may hereafter be shown to be strictly Darwinian.

This last point is of such fundamental importance in relation to those border subjects where physical and biological science meet, that no excuse need be offered for inviting its fuller discussion. In the domain of chemistry, for example, I have often pondered the question whether some principle of Selection or Survival may not be applicable to the case of the synthesis of organic (i.e. carbon) compounds, many thousands of which are now known to chemists.[1] The special interest attaching to these compounds from the present point of view is of course their relationship to the lowest form of living matter. The great majority of these compounds are purely artificial, i.e. they are laboratory preparations which are unknown as products of vital activity. But great as have been the achievements of chemists in the way of producing new compounds, it is becoming more and more evident that from a scientific point of view negative results may be as important as positive results. In other words, with the progress of knowledge it is becoming apparent that the possibilities of developing new atomic groupings are subject to definite limitations.

This is expressed in modern terms by saying that certain configurations of atoms are possible and others impossible. Although tentative hypotheses connecting the stability of certain types of compounds with particular configurations have been suggested, it must be confessed that our knowledge is still in an empirical stage. We cannot deduce from known data with any degree of precision why the possibilities of synthesis are restricted in this, that, or the other direction. It may be said in a general way that the stability of any atomic system must be ultimately explicable in mechanical or dynamical terms, and that the greatest future development of our science may therefore

[1] The new edition of Richter's *Lexikon* proposes cataloguing 150,000 formulae.

be looked for from the co-operation of the chemist and physicist.

From the evolutionary standpoint may not a chemical compound be regarded as the analogue of a biological 'species'? The chemist may be looked upon as a selecting agent acting in the present state of knowledge as a more or less unconscious agent. He is as dependent for his 'species' upon intra-molecular dynamical conditions as is natural or artificial selection upon the congenital variations offered by the living organism. Moreover, the compounds which he isolates are adaptations to a particular environment—they consist of molecules capable of existence only under the particular environmental conditions imposed. Change the conditions, such for example as by raising the temperature, and a different order of chemical combination becomes possible. Out of various possible combinations of particular atoms in particular ways the chemist therefore can only produce such compounds as are capable of survival under his laboratory conditions. The 'fittest' that here survives is the product of chemical skill only in the same sense that a race is the product of artificial selection, or a species the product of natural selection. Darwin's prime conditions of organic evolution—the nature of the organism and the nature of the environment—are complied with if for 'organism' we substitute 'atomic configuration', and if by 'environment' we mean physical and chemical environment. The analogy between a chemical compound and a species is also borne out by the circumstance that in reaching the final stage intermediate unstable stages are frequently if not generally passed through—the 'unfit' with respect to the final environmental conditions. Furthermore, in some cases the internal mechanism is so evenly balanced that the final product may be said to possess a power of responsivity, being capable of existing in one or another of certain distinct modifications according to its chemical or physical environment. This is what chemists know as 'tautomerism', 'dynamical isomerism',

&c. The biological analogue of this phenomenon is the power or faculty of adaptability to a variable environment displayed by living organisms.

If this analogy be conceded—and I do not think it is overstrained—it must hold good for that primordial synthesis of organic (i.e. carbon) compounds from which were developed the simplest compounds possessing those characters associated with the term 'vitality', at this stage in its most rudimentary form. On a globe cooling down from an igneous condition many such syntheses would become possible. The conditions of survival would be of precisely the same kind, if different in degree, as those which determine laboratory syntheses of the same pyrogenic order. Most Evolutionists believe that the gap between the simpler synthetical carbon compounds and the simplest form of 'vitalized' (i. e. 'organized') carbon compound has been bridged over by natural processes.[1]

[1] On this point see Spencer's *Principles of Biology*, vol. i, chap. i and appendix; Darwin to Carus in 1866, *More Letters*, vol. i, p. 273; Huxley's Presidential Address to the British Association, Liverpool, 1870; Darwin to Mackintosh, 1882, *More Letters*, vol. ii, p. 171; Ray Lankester on 'Protoplasm', *Encycl. Brit.*; the writer's Address to the Essex Field Club on 'Darwin and Modern Evolution', in 1883; *Transactions* of the Club for that year; Karl Pearson, *The Grammar of Science*, 2nd ed., chap. ix. Also Professor Sorley's recent paper 'The Interpretation of Evolution', *Proc. Brit. Acad.*, vol. iv, 1909. The theory of 'panspermia' (see *Worlds in the Making*, by Arrhenius, chap. viii) postulates the existence of eternal and universally distributed life germs ready to develop whenever placed under favourable conditions. This hypothesis shelves the question of the possible origin of those particular chemical compounds of carbon and other elements associated with life as we know it—and no other form of life comes under consideration—and relegates these compounds to the 'Ultimates' with Energy and Matter. The history of science, however, shows how dangerous it is to brush aside mysteries, i. e. unsolved problems, and to interpose the barrier placarded 'eternal—no thoroughfare!' Not long ago the chemical atom was considered to be eternal, and attempts to arrive at a knowledge of its origin were regarded as futile. The chemical atom at that stage of scientific development was an 'Ultimate'. From later physical research we now learn that not only is the atom not eternal, but that we are likely to know more about its inner mechanism and the causes of its mortality than about the atom itself as a concrete particle. Such being the state of modern science with respect to comparatively simple particles, it may reason-

Of these processes—whether they were thermosynthetic, electrosynthetic, or photosynthetic, or the result of the interaction of any other form of energy and matter—we are at present profoundly ignorant. But it is known that for the display of 'vitality' in its simplest manifestation certain chemical combinations are essential. It is perhaps unnecessary to raise the question whether 'vitality' is simply the manifestation of the properties pertaining to particular combinations of atoms or the result of some power conferred by extraneous agency upon certain special chemical compounds. Evolution has answered this question over and over again in unequivocal terms—the invocation of extraneous 'powers' to explain processes of which we are ignorant is simply the re-introduction of long-abandoned unscientific methods.

The question now raised is the more specific one of evolutionary process. Granting the analogy between chemical compounds and species, it may be asked whether the development of the simplest living form of matter can be conceived in Darwinian terms. The survival of a chemical compound, as such, is made possible by the nature of the internal mechanism of the molecule, whether that molecule contains carbon, hydrogen, nitrogen, &c., in atomic groupings capable of manifesting 'vitality' or not. So far the principle of survival holds good. The problem that confronts the Evolutionist is the nature of the mechanism which rendered possible the persistence of a certain compound or of certain compounds possessing

ably be contended that a complex of many atoms, such as exists in the very simplest known or conceivable living germ, is still more unlikely to be possessed of immortality. If the idea of immortality is eliminated from 'panspermia' we are left in possession of the hypothesis that space may be crowded with life germs partly of terrestrial and partly of cosmic origin. Of this conception all that can be said is that it may be true, but that the question of the possible development of living from lifeless carbon compounds is left precisely where it was before. From the point of view of Evolution it is quite as reasonable to postulate the continuous and present development of these ultra-microscopic life germs here or elsewhere throughout the Universe—to erect in fact a new theory of 'Pangenesis' in a sense quite different to that used by Darwin in his celebrated 'provisional hypothesis'.

that particular constitution conferring upon them that stable instability known as life. If selection there has been, it may safely be asserted that the agency was physical, i.e. the inorganic environment. During that period—probably extending over geological ages—when lifeless carbon compounds were giving rise to living carbon compounds there can have been no struggle for life with competing organisms. The survival of the simplest types of living carbon compounds may thus be as referable to chemical constitution as the isolability of a definite synthetical compound. From this point of view the question that has to be answered is, what particular atomic configuration or configurations potentially vitalizable were capable of existence under the environmental conditions of that remote past ?[1] Given such compounds, and the subsequent course of organic evolution by the Darwinian process becomes intelligible.

The physical condition which in one direction limits the existence of such compounds is, of course, temperature. The chemical condition requires an atomic grouping of sufficient stability to exist as a definite molecule or molecular aggregate, and of sufficient instability, i.e. internal mobility, to resist destructive physical and chemical processes. In other words the compound must be possessed of the faculty of 'responsivity' with respect to its environment.[2] It would be rash to attempt to

[1] This assumes that the passage from lifeless to living matter took place only during past ages and is not taking place now. See, *per contra*, note, p. 18. The supposition that life germs may be developing now, here or elsewhere, makes no difference to the above statement of the question excepting the substitution of 'are' for 'were' and 'present' for 'remote past'.

[2] The word environment is here used in its widest possible sense as including the vital processes of assimilation and growth, in their initial stages. For growth may be regarded as the result of the addition of one carbon compound to another with which it is capable of combining by virtue of its chemical constitution, i.e. assimilation as the result of the action of one carbon compound upon another—the interaction of an organic environment and a responsive organic compound. Reproduction, as Spencer has shown, follows from growth. The term 'mobility' is used above in its chemical sense as referring to intra-

predict whether such compounds will ever be synthesized in our laboratories, but the analogy which has been made use of throughout this discussion may be of use here. It may even be permissible to go further and to suggest that the analogy which it has been attempted to establish between an organism and a chemical molecule, in the initial stages of 'vitality' passes from an analogy into a physical reality. The nearest approach to responsivity which modern chemistry can offer is among those compounds which have been referred to as tautomeric, using this term in a general sense so as to cover all cases of mobile configuration. This is the kind of instability among stable compounds which most nearly complies with the biological conditions. But such cases are at present limited both in number and in phase. In spite of the great complexity of many synthetical compounds the number of definite forms capable of being assumed in cases of tautomerism is generally limited to two, or, it may be said that the possibilities of assuming atomic configurations in response to chemical and physical stimuli are in these cases limited to two.

Now there is physical and, especially, optical evidence in support of the view that certain molecules in which the atoms are capable of assuming two or more different configurations may, under different external conditions, exist in varying proportions in two or more forms simultaneously. In such cases two or more atomic configurations may be said to be adapted to a particular environment.

molecular mobility of atomic configuration and not in the purely physical sense in which Spencer uses the term when discussing the chemical composition of organic and organized matter in Chapter I of the *Principles of Biology*. Growth in the initial stages of the development of living from lifeless organic matter would from this point of view be more analogous to inorganic growth, just in the same way that the primordial 'organized' carbon compounds were more closely related to dead carbon compounds. If it is claimed that the special and distinguishing character of living matter is its power of 'directing' without creating energy, may not the action of catalysts or 'contact' substances, which direct the course of chemical change without undergoing any change themselves be put forward as an analogue?

We have here the chemical analogue of a dimorphic or polymorphic species. Now what the biologist requires in order to bridge the gap between living and dead carbon compounds may possibly be an internal mobility or lability of the order indicated—not a restricted tautomerism, but a comparatively unlimited responsivity to varying environments ; a highly enhanced faculty of tautomerization. The survival of carbon compounds may from this point of view be the result either of extreme stability, as in ordinary pyrogenic synthesis, or of extreme internal lability conferring adaptability to variable conditions, such adaptability enabling these particular atomic groupings to resist destructive agencies. If there is anything in this suggestion, then the development of life has been just as much a process of selection as the subsequent differentiation of living organic matter into specific forms. Out of numbers of primordial synthetical products containing carbon none have survived but the stablest 'mineral' compounds, such as carbon dioxide and (possibly) hydrocarbons on the one hand, and, on the other hand, the lineal descendants of those protoplasmic corpuscles to which the most highly susceptible tautomerizable compounds gave rise. Modern physiological research, and especially the work of Bayliss and Starling, favours the view that in the lowest forms of protoplasmic life the responsivity is even now of a purely chemical character.

I am fully aware that this discussion amounts to little more than a restatement of the old problem—not of the *origin*, but of the development of living from lifeless matter, a point which Spencer has of course dealt with in a general way.[1] All that is claimed is that the case has possibly been stated in more specific terms than hitherto, and certainly in a more distinctly Darwinian sense. At certain stages of scientific development it is always useful to raise questions even if the present state of knowledge does not admit of their being answered.

[1] See particularly the *Principles of Biology*, chap. i.

But out of this treatment of the subject there arises a further question which may be worthy of some consideration. It has already been suggested that in a globe cooling down from an igneous state and containing carbon as an element, the probabilities are that many compounds of this element would be formed. Now the elements essential for living matter are carbon, hydrogen, nitrogen, and oxygen, and, without raising the at present unanswerable question as to the precise order of combination and the nature of the primordial 'organic' compounds, it is quite certain that a molecule composed of these four elements, even in its simplest form, is already a highly complex compound from a purely chemical point of view, and, as such, admits of numerous possible configurations, or, in other words, would be capable of existing in several isomeric or tautomeric forms. If only one such quaternary compound were synthesized we should therefore have several possible starting-points for future development, and if several such compounds were synthesized there would be an abundant supply of raw material for the selective action of the environment. If the principle of multiple synthesis be conceded, then the earth or the ocean in azoic times may have been as colonized by organic compounds as it subsequently became by living organic matter during the early stages of the life period. The question that may be asked is therefore whether it is likely that out of a number or possibly large numbers of dissimilar or of isomeric or tautomeric compounds one only should have possessed the necessary mobility of configuration to give rise to living matter. Of course this may have been the case, but, on the other hand, it may not. Probability would appear to be against the monogenetic development of life—so also is analogy, for it is certain that similar combinations of metallic and non-metallic atoms must have taken place in past ages at many distinct centres as shown by the occurrence of the same mineral products in widely separated parts of the earth. At any rate the notion of the possibility of there having been vital polygenesis seems worthy of being filed for future consideration. On this view the primordial protoplasmic units need not necessarily be conceived as having been all of one uniform chemical type—there would have been a sufficiently close chemical relationship to give rise to apparently similar protein compounds and yet sufficient dissimilarity of structure to produce divergence on further development. The likeness and unlikeness would be of the same order as that which obtains among the complex isomeric compounds of carbon now known to chemists. It has frequently been pointed out that even at the dawn of life, as shown by the geological record, a marked differentiation of type had taken place. This is generally explained, and no doubt correctly explained, by the obliteration of the earliest records of the life period by metamorphic changes of the primitive rocks and so forth.

But there may also be scope for the influence of primordial 'isomerism' in the sense suggested. Could we restore the very earliest records of life upon this globe we might expect to see on this view a much greater convergence towards a common type than is now shown among the fossils of the Cambrian rocks, and yet not absolute uniformity—a dissimilarity due not altogether to the struggle for life and survival of the fittest but in part to the heritage of the primordial dissimilarity of composition or of the 'isomerism' of the ancestral carbon compounds. Darwin's well-known metaphor of 'life, with its several powers, having been originally breathed by the Creator into a few forms or into one'[1] may thus have a real scientific basis.[2]

[1] *Origin of Species*, 6th ed., p. 429.

[2] It is only right to point out that the views advanced in this section are at variance with Spencer's statement concerning the inapplicability of the Selection Principle to inorganic phenomena. See 'Filiation of Ideas', p. 558. Chemical analogies have been made use of by Spencer, especially in the *Principles of Biology* (Chapter I and Appendix), but in a quite different sense to that now advocated. The nearest approach to the present treatment of the subject that I have been able to find is by Karl Pearson in the *Grammar of Science*, and especially in § 12,

Returning now to the earlier history of the modern doctrine of Evolution, it is admitted that the publication of the *Origin of Species* gave such an enormous stimulus to evolutionary thought that Spencer's writings received much greater attention than would otherwise have been accorded to them. The highly abstract reasoning and the more purely deductive treatment in the *First Principles* and its successors did not appeal to the majority of scientific workers with such force as Darwin's more concrete method of dealing with the problem of organic evolution. Spencer himself, with that candour which throughout marked his attitude toward his great contemporary, has admitted this influence in specific terms in relating the history of the revision of the first (1855) edition of his *Principles of Psychology* :—

'The tacit assumption, and towards the close of the work the avowed belief, that all organisms had arisen by evolution, and the consequent conception running throughout the whole work that the phenomena of mind were to be interpreted in conformity with that hypothesis necessarily (in 1855), roused not sympathy, but antipathy. It was only after the publication of Mr. Darwin's *Origin of Species*, some four years subsequently, and only after this work—drawing so much attention, causing so much controversy—began presently to affect deeply the beliefs of the scientific world, that the views contained in the *Principles of Psychology* came to be looked at more sympathetically.' [1]

In attempting to estimate the relative parts played by Darwin and Spencer in moulding evolutionary thought we must, as before urged, bear distinctly in mind that fundamental difference in method of attack—the expres-

p. 356, ed. 1900. To prevent possible misunderstanding it may also be advisable to emphasize that the hypothesis of multiple synthesis now suggested has no relationship to the hypothesis of the 'multiple origin' of species so ably discussed by Professor Poulton in his latest work, *Charles Darwin and the Origin of Species*, Appendix A, p. 247.

[1] Duncan's *Life and Letters*, p. 140. Darwin admits (*Origin*, 6th ed., p. 428) that the foundation on which Psychology had been based by Spencer was 'well laid'.

sion of that dissimilarity in mental constitution which has already been referred to. It would be extremely presumptuous on my part to attempt a comparative analysis of methods and results, but such comparison will assuredly have to be made by some competent philosophical critic in the future. The opinion may certainly be hazarded that the verdict of posterity will be that Spencer's mind was more of the synthetical and Darwin's of the analytical type. There is ample material in the life work of these great pioneers for measuring what may be termed the intellectual expansive force of Science and Philosophy respectively. The attitude of scientific workers toward Philosophy before the dawn of Evolution was often contemptuous—sometimes hostile, and very generally apathetic. It may be said that Spencer, more than any other writer since the time of Bacon, has succeeded in basing Philosophy upon a foundation of science. For him Philosophy was completely unified Science, a definition accepted broadly with certain qualifications and additions by that keenest of modern critics, the late Professor Henry Sidgwick :—

'No student of any special science ever acquiesces in having no idea of the relation of his part of knowledge to the rest. He may avoid Philosophy in the sense of avoiding the attempt to make his conception of the universe as clear, precise, and systematic as possible, but that only means that he will be content with a vague, obscure, and altogether inadequate conception.' [1]

'I have taken it to be the business of Philosophy—in Mr. Spencer's words—to "unify" or systematize as completely as possible our common thought, which it finds partially systematized in a number of different sciences and studies.' [2]

[1] *Philosophy, its Scope and Relations*, p. 11. The extent to which Sidgwick follows or departs from Spencer can only be adequately ascertained by reference to the whole chapter from which the above extract is taken.

[2] Ibid., p. 105. See also George Henry Lewes: 'As Science is the systematization of the various generalities reached through particulars, so Philosophy is the systematization of the generalities of generalities. In other words, Science furnishes the Knowledge and

Spencer's treatment of Evolution was philosophical in its generality—in being broader than the widest generalizations of any particular science. His generalizations were based upon a foundation of such breadth that they had to be expressed in the most general and therefore the most highly abstract terms. The function of Philosophy—if Philosophy is assigned any function in the development of human thought—is to work up available knowledge as rapidly as possible into abstract formulae or principles of sufficient generality to cover all possible applications to particular classes of cases. From this point of view Spencer's method was sound in principle, even if the very generality of his treatment led to the alienation of his generalizations in one or another of their applications from the more restricted but safer generalizations of the special sciences. This extreme breadth of treatment is no doubt one of the reasons why he failed to impress the scientific world to the same extent as Darwin. The philosophical method is a dangerous weapon in unskilled hands; scientific men knew it then and know it now. The broader the generalization and the more diverse the various classes of phenomena which it attempts to embrace, the greater the liability to error—the more unsafe does the method become. It is a method which may result in generalities too vague to have any immediate bearing upon scientific method, or which, in its haste to arrive at an abstract formula, may base conclusions upon imperfect knowledge and lead to generalizations altogether erroneous. The passage from philosophy to nonsense is a short one, as may be seen from the voluminous literature supplied by the paradoxers and faddists of all ages.

Philosophy the Doctrine.' 'Philosophy has no distinct province of knowledge: it embraces the whole world of thought; it stands in relation to the various sciences as Geography stands to Topography. All the sciences subserve its purpose, furnish its life-blood. It systematizes their results, co-ordinating their truths into a body of Doctrine.' —*Science and Speculation*, chap. i, § 4. From the Prolegomena to the 3rd ed. of the *History of Philosophy*.

But the writer of philosophy takes this risk. There was much justification for the attitude of caution on the part of scientific men towards a method which had not helped them much in the past, and which was by many, if not by most, considered to be superfluous for the future. The effectiveness of the method, however, depends altogether upon the person who uses it. On this point I shall have more to say in the concluding part of this lecture. Of Spencer's work it may be said that no more heroic, and I will add no more successful, attempt to wield single-handed such a mighty weapon as unified science has ever been made. If Science no longer looks askance at Philosophy, but recognizes therein a most powerful ally, it is mainly due in modern times to the impression produced by the author of the *Synthetic Philosophy*.

The history of the influence of his treatment of organic evolution upon the mind of his illustrious contemporary may now fittingly be set forth in juxtaposition to his reception of Natural Selection. This influence is certainly among the most instructive illustrations upon record of the effect of the abstract method upon one of the greatest contemporary wielders of the concrete scientific method.

The celebrated essay on 'The Development Hypothesis', published by Spencer in 1852, must have been read by Darwin about 1858, when, as already stated, the author sent a copy of his *Essay* containing this particular contribution to Darwin, who acknowledged it on Nov. 25 in these terms :—

'Your remarks on the general argument of the so-called development theory seem to me admirable. I am at present preparing an abstract of a larger work on the changes of species; but I treat the subject simply as a naturalist, and not from a general point of view, otherwise, in my opinion, your argument could not have been improved on, and might have been quoted by me with great advantage.' [1]

[1] *Life and Letters*, vol. ii, p. 141.

In 1866 he wrote to Hooker concerning the *Principles of Biology* :—

'I have now read the last number of Herbert Spencer. I do not know whether to think it better than the previous number, but it is wonderfully clever, and I daresay mostly true. I feel rather mean when I read him: I could bear and rather enjoy feeling that he was twice as ingenious and clever as myself, but when I feel that he is about a dozen times my superior, even in the master art of wriggling, I feel aggrieved. If he had trained himself to observe more, even if at the expense, by the law of balancement, of some loss of thinking power, he would have been a wonderful man.' [1]

In acknowledging the receipt of the complete volume in 1867, he wrote to Spencer :—

'In many parts of your *Principles of Biology* I was fairly astonished at the prodigality of your original views. Most of the chapters furnished suggestions for whole volumes of future researches.' [2]

Again, in 1870, writing to Ray Lankester with reference to the work on *Comparative Longevity* by the latter, Darwin said :—

'It has also pleased me to see how thoroughly you appreciate (and I do not think this is general with men of science) H. Spencer. I suspect that hereafter he will be looked at as by far the greatest living philosopher in England; perhaps equal to any that have lived.' [3]

Of particular significance also is the following paragraph in a letter to Wallace, written in 1872, with reference to Bastian's work on 'archebiosis' :—

'I am not convinced, partly I think owing to the deductive cast of much of his reasoning; and I know not why, but I never feel convinced by deduction, even in the case of H. Spencer's writings.' [4]

[1] Ibid., vol. iii, p. 55.

[2] *More Letters*, vol. ii, p. 442. Also the letter to Hooker, 1866, ibid., p. 235. In this letter he states, 'I have almost finished the last number of H. Spencer and am astonished at its prodigality of original thought. But the reflection constantly recurred to me that each suggestion, to be of real value to science, would require years of work.'

[3] *Life and Letters*, vol. iii, p. 120.

[4] Ibid., p. 168.

The contrast between the methods of the two founders so strikingly brought out by these extracts receives, so to speak, official sanction by Spencer's analysis of his own disposition :—

'But I must not forget another trait of nature—a relative liking for thought in contrast with a relative aversion to action. My physical constitution did not yield such overflow of energy as prompts some natures to spontaneous activity. In many directions action was entered upon rather reluctantly ; while thinking was a pleasure. Obviously this predominant tendency to contemplation has been a factor in my career.' [1]

No further evidence than that furnished by the foregoing extracts is required to prove that the influence of Darwin and Spencer respectively has been determined entirely by the difference in the method of attack adopted by the contemporary founders of modern Evolution. It is also clear that the difference in the effects of the two modes of treatment upon the scientific world involves the consideration of the fundamental question of validity of method. It is no disparagement of Darwin to say that he was working at a lower level than Spencer, because his foundation was thereby made more secure ; nor is it an exaltation of Spencer to say that he was working at a higher level than Darwin, because his foundation was thus rendered less secure. The question that posterity will have to decide is, I venture to think, not whether the scientific method is valid—because on that point there is no doubt—but how far the abstract or philosophical method is legitimate as a means for the advancement of science. We have, during the Victorian age, been provided with the unique example of two of the most powerful intellects of the time arriving at the same great generalization by different methods. Spencer arrived at Evolution as a principle from the consideration of a certain number of very diverse groups of phenomena, and based a system of philosophy upon his generalization ;

[1] 'Filiation of Ideas,' p. 536.

Darwin arrived at organic Evolution by the consideration of an immense mass of biological evidence, and out of his generalization there has also arisen a philosophy of Evolution.

So far as concerns Evolution in the abstract the two systems have now become welded. Even if with advancing knowledge it could be shown that all Evolution is Darwinian—and this is far from being the case at present—there would still be no divergence in main principle between the two founders, because Spencer was thoroughly Darwinian in his recognition of Selection, and Darwin was equally Spencerian in recognizing the evolutionary principle where Selection had not been shown to be applicable.

In view of this coalescence of results it may be permissible therefore to examine somewhat more closely into this all-important question of validity of method. And in the first place let it be understood—for there is no point on which public judgement is more apt to err—that in Science soundness of method by no means implies infallibility of results. We declare unhesitatingly that the scientific method is sound, but we also recognize that the method can deal only with facts and observations learnt by a being of finite mind with sense organs of a limited range. And so with increase of knowledge resulting from improved methods of experiment and observation—in other words, from a perfecting of our methods of gleaning information from Nature, this same scientific method enables us to amplify, check, revise, correct or modify previous results. We grope our way towards truth by laborious and tortuous paths ; we stray into many blind alleys and are led astray by many false scents and occasionally we fall headlong into a pit from which a later generation has to dig us out. It may not be paradoxical to say of Science that her main strength lies in her being cognizant of her fallibility. As Professor Macallum, quoting a saying by Duclaux, well puts it in his recent address to the physiological section of the British Association : 'The reason why Science advances is that it is never sure of anything.' The scientific worker knows full well that his methods are not infallible, but he also knows that, however imperfect the methods and results, there is an infallibility underlying them—he knows that his labours are making for the infallible truths underlying Nature's methods. Moreover, his confidence in his methods is such that he is justified in his belief that there are none other open to intellectual beings of finite intelligence. He might say of them as does Touchstone of Audrey :—'A poor . . . thing, sir, but mine own' ; nevertheless he may confidently challenge the history of the intellectual development of the human race to produce evidence of any mastery over Nature or any real knowledge of Nature's mode of working having been acquired by any other methods than those of observation, experiment, induction, and deduction.

I feel that an apology is due even for attempting the vindication of the scientific method before this audience, but in a country which is not yet pervaded by the spirit of Science there is no doubt that our cause suffers considerably from the imposition by an unscientific public of a false standard of finality. The lability—the adaptability, the unlimited receptiveness of Science for new truths by which her triumphant progress is secured is not sufficiently realized. It is the unscientific laity and not the high priests of Science who from time to time endeavour to invest her generalizations with the authority of ecclesiastical dogmas, and then, because with the progress of discovery correction or modification is found to be necessary, declare that Science as a cult is unworthy of confidence. The history of scientific progress is summed up in a few terse phrases : Method indisputably sound ; information imperfect, but constantly approximating towards greater perfection ; conclusions dependent upon information available.

Judged by these criteria the statement that Darwin's work is representative of the highest application of the scientific method is simply the assertion of a truism.

With respect to Spencer's method it may be claimed that it is philosophical inasmuch as he himself considered his whole system to be, and entitled it, a system of Philosophy. How then does Spencer's treatment differ from Darwin's, and why is it that one method should have commanded a higher degree of confidence than the other? This brings me back to the main question of the influence of the philosophical method upon scientific progress. It is with very great diffidence that I venture to handle this subject, for I have already disclaimed any pretension to have been a special student of Philosophy. But if the question be asked, what constitutes the philosophical method?—then the answer must depend very largely upon what view we take with respect to the scope of Philosophy. Here again I shelter myself under the authority of Professor Sidgwick, who states that a generation ago the predominant opinion amongst English thinkers was that the 'Science of Mind' or of 'Man' was 'Philosophy Proper'.[1] It is remarkable that a country which produced Bacon should have taken this narrow view of Philosophy—but so it was, and this no doubt explains why we, as students of Natural Science, failed to recognize any close relationship between Science and Philosophy. It was Spencer who changed the outlook for us. To his influence I have already attributed the changed attitude of Science toward Philosophy, and to his influence may also be ascribed the changed attitude of Philosophy toward Science.[2]

Now the first conclusion of importance to Science that may be drawn from Spencer's conception of Philosophy

[1] *Philosophy, its Scope and Relations*, p. 35.

[2] Far from restricting the scope of Philosophy to the 'Science of Mind' (thereby in former times meaning human mind), we are now beginning to recognize that mental science is as much the subject of legitimate scientific experimental and observational inquiry as any other branch of science: that the mode of working of the organ of mind must be understood before any true 'science of mind' can be created. In other words, 'Experimental Psychology' is taking its natural place among the sciences.

is that there is no difference in kind between his method and Darwin's, but only a difference in degree—that the philosophical method is the scientific method writ large. Within its own limits every branch of Science is striving to create a minor philosophical system out of the materials at its disposal. The generalizations of Science pass by imperceptible steps into philosophical generalizations, and no hard and fast line can be drawn between them. If the generalizations of one department of Science encroach upon another or other Departments of Science, there may be sufficient comprehensiveness to warrant the erection of a philosophical system, such, for example, as in the former use of the term Natural Philosophy to embrace unified Physical Science.[1] It is merely a question of comprehensiveness as to what shall be considered a law or principle of Science and what shall be raised to the dignity of a philosophical system.

If, therefore, it can be shown that underlying all the generalizations of Science there is a still more comprehensive principle, that principle must, from the scientific point of view, become a Natural Philosophy in the very highest conception of the term. This is just what Spencer devoted his life to proving, and we have accepted his view that Evolution—whether his precise formula is the best that can be conceived or not—is such an all-embracing principle. Not that it is thereby proved that there is no other principle at work in Nature, but that, so far as we now know, Evolution is descriptive of Nature's actual method of working, and that it is therefore amongst the broadest generalizations that unified scientific knowledge has hitherto given to Philosophy. More than this—the recognition that there is what may be termed a general mode of procedure in Nature has widened the horizon of every worker in the realm of Science. While recognizing that specialization of work is necessary in order to achieve

[1] It is of interest to note that the chief publication of the Royal Society, which embraces all 'natural knowledge', still bears the title 'Philosophical Transactions'.

results of value, the scientific investigator who has grasped the Spencerian conception in its complete significance receives the additional stimulus derived from the knowledge that his labours will result in a contribution to that universal Science which constitutes Philosophy.

The recognition that there is one universal Science is in fact tantamount to the admission that our divisions and subdivisions into special sciences are artificial and do not correspond with the reality of Nature; they are convenient but arbitrary divisions necessitated by imperfect knowledge and by the limitation of human faculty. It must, I think, be admitted that the consolidation of natural knowledge under the influence of the evolutionary idea marks a real advance in our conception of Nature as a consistent whole; it will not be denied that for that advance Science and Philosophy in the nineteenth century are mainly indebted to Herbert Spencer.

A system of Philosophy based upon Science might by virtue of its origin be expected to be capable of being used as a means for promoting further scientific progress. As in the narrower domain of each special science we are justified in using, and do in fact use, our generalizations deductively in order to test their soundness by applying them to particular cases, so the broader and more comprehensive philosophical principles should, if valid, be capable of being wielded deductively in every department of Science, since, in the Spencerian sense, a science, as now understood, is simply a subdivision of Philosophy created by human agency for purposes of expediency. The method which is valid in Science in detail cannot logically be denied to Science in its totality. The question how far Spencer's deductions are valid thus resolves itself into a question of the same order as that relating to the soundness of scientific conclusions in general. To exact a standard of infallibility from a philosophical system based upon Science is as unscientific as the imposition of finality upon the conclusions of Science. The validity of Spencer's conclusions derived from the application of the general principle of Evolution to this, that, or the other set of phenomena cannot be challenged on the ground of unscientific method, but can only be judged by the same standard as that by which we judge other scientific conclusions—the evidence submitted, or rather the weight assignable, to the evidence.

The deductive method in physical science has never been challenged: the more highly developed the science, the more freely is the deductive method employed :—

'The successful process of scientific inquiry demands continually the alternate use of both the *inductive* and *deductive* method. The path by which we rise to knowledge must be made smooth and beaten in its lower steps, and often ascended and descended, before we can scale our way to any eminence, much less climb to the summit. The achievement is too great for a single effort ; stations must be established, and communications kept open with all below.' [1]

If, therefore, the Spencerian treatment of Evolution commanded less confidence among scientific men than the more concrete method of Darwin, some other explanation must be found than the violation by the author of the *Synthetic Philosophy* of the recognized principles of the scientific method. The reasons are not difficult to produce ; how far these reasons were or are valid must be left to posterity to decide.

In the first place the man of science, by virtue of his training, is alone capable of realizing the difficulties—often enormous—of getting accurate data for induction.

[1] *Preliminary Discourse on the Study of Natural Philosophy*, 1831, p. 175. See also George Henry Lewes, *Science and Speculation*, chap. ii, § 24 : 'The distinguishing characteristic of Science is its method of graduated Verification and not, as some think, the employment of Induction in lieu of Deduction. All science is deductive, and deductive in proportion to its separation from ordinary knowledge and its co-ordination into system. The true antithesis is not between Induction and Deduction, but between verified and unverified cases of Induction and Deduction.' It is obvious that as soon as we attempt to verify an Induction we are using it deductively, and are thereby investing it with philosophic rank as being worthy of sufficient credence to consider it necessary to confront it with reality.

In other words, it is only the active worker—the original investigator—who, by personal appeal to Nature through artificially imposed conditions, i.e. experiment, or through observation, i.e. ready-made phenomena, has come to understand fully what a *fact* really means in the scientific sense; to realize how laborious is the process of wooing truth and how ambiguous are the answers often given by Nature to his cross-examinations. I have elsewhere recorded a humorous rejoinder by Darwin[1] on one of the very few occasions when it was my never-forgotten privilege to have met him; as this reply bears so closely upon the present topic I will venture to repeat it. I had been dwelling upon this very point of the difficulty of getting Nature to give a definite answer to a simple question, when, with one of those mirthful flashes that occasionally lighted up his features, he retorted: 'She will tell you a direct lie if she can.'

Judged by the standard of the scientific expert it is obvious that Spencer could not have been expected to influence the scientific world to the same extent as Darwin, for his achievements as an original investigator shrink into insignificance when compared with those of his illustrious contemporary. As Professor Bourne has well put it in last year's Herbert Spencer Lecture, it would have been practically impossible for the author of a system of philosophy based upon unified science to have become an investigator in every department of science. If an expert in any sense he was a biologist, and his position as such has been fairly stated by Professor Bourne. But although he was not constantly in direct commune with Nature, as was Darwin, it cannot fairly be said that he was not an investigator at all.

There certainly has been a tendency of late years to do injustice to Spencer in this respect. I am afraid that we of the later generations are rather too apt to minimize the work of our predecessors, upon whose shoulders we

[1] Presidential address to the Entomological Society of London, *Transactions*, 1896.

stand, forgetful of when the work was done and judging it only in the light of modern methods and appliances. The fairer test is the estimate of those who were his contemporaries as Evolutionists. Huxley, it will be remembered, speaks of him as 'outside the ranks of biologists', but this refers to the period of the publication of the Darwin-Wallace theory in 1858–9, and Spencer's work on the circulation of the sap in plants and the first edition of the *Principles of Biology* were not published until 1866–7. Happily two of his contemporaries, Hooker and Wallace, are still with us, and from both these I have received letters (see Appendix to this Lecture) showing that by his contemporaries he was regarded as an original investigator.

The difference in the impression produced upon contemporary science by Darwin and Spencer respectively cannot, however, be ascribed solely to their relative positions as original investigators; another cause must be added. Up to the time of the enunciation of the theory of Natural Selection the biological sciences were more or less in an empirical or descriptive stage. With the exception of Lamarck's famous attempt there had never been a really systematic philosophy introduced into biology. In this respect the biological sciences had lagged far behind the physical sciences. As a result of this retarded development—due largely to the mysticism attached to life—broad generalizations were strange to the minds of biologists who were, as a body, quite unaccustomed to grasp such generalizations or to use them deductively. This same influence retarded the acceptance of the Darwinian theory—still more might it be expected therefore to have retarded the recognition of any conclusions resulting from the more purely deductive treatment of Evolution by Spencer. There are no doubt many now living who can remember cases of extraordinary mental density among experts, and particularly among pure systematists—not in questioning the soundness of the theory—because that might be a legitimate subject for scientific discussion—but in their comprehension of what the theory really meant and in their failure to realize that a theory with so much *prima facie* probability might in accordance with all scientific precedent be used deductively to test its validity.[1]

It is clear that the Spencerian treatment was wanted as a co-factor with the Darwinian treatment. His work was in our hands just at the right time, and there is no doubt whatever that large numbers of science students were enormously stimulated thereby. The two prime methods of induction and deduction were for us personified by the two great founders of the doctrine of Evolution.

In pointing out that the more liberal use of the deductive method in the biological sciences was urgently needed at that period it must be understood that from the scientific point of view the scientific use of the method only comes under consideration. To accept a deduction as a scientific truth without verification is unscientific, and it must be confessed that both Darwinians and Spencerians have been too apt to accept 'what might be' or 'what ought to be' for 'what is'. But from the philosophical point of view a deduction takes another aspect. Granting that such or such a principle arrived at inductively is true, then such or such results should follow. That is the deduction for both Philosophy and Science. Now it is the business of Philosophy in the Spencerian sense, if it is in possession of the broadest of generalizations, to formulate these deductive conclusions—to tell us for guidance 'what might be' or 'what ought to be'. If that be disallowed, then there seems to be no scope for Philosophy as an instrument either of scientific progress or of human culture. If the deductive conclusions are accepted off-hand as demonstrated truths, so much the worse for Science; if they are

[1] See, for instance, Darwin's letter to D. T. Ansted in 1860, published in my address to the Entomological Society of London in 1897; *More Letters*, vol. ii, p. 175.

used legitimately as stimulants to scientific research, then they may lead to results of lasting value. It is thus a question of division of labour between two great classes of workers making for the same end.

Is the Philosopher from this point of view bound to verify his own deductions?—is it essential for him, in order to secure a hearing, to convert his plausible guesses, conjectures, suggestions, or hypotheses into positive contributions to Science? I venture to think that in principle there is no such necessity; in practice it might be expedient—if actually attempted there can be no doubt but that he would profit by the experience. But it is not the *necessary* function of the Philosopher to experiment or to observe: it is for him to work up the material supplied by Science and to elaborate therefrom doctrine for scientific verification. The question whether Spencer was or was not an original investigator thus sinks into minor significance. That he did his duty toward Science as a Philosopher is virtually admitted by Darwin in those memorable passages in his letters to Spencer and Hooker recording the impression produced upon him by the perusal of the *Principles of Biology.*

The doctrine that it is permissible to philosophize, and especially to philosophize in the Spencerian sense, without being an original investigator, requires further justification. To some it will appear a principle too dangerous for recognition; to some it may even have a savour of heresy. That it is a risky undertaking has already been admitted, but, as has also been pointed out, whether it succeeds or fails depends entirely upon the person who incurs the risk. It cannot be maintained that specialization as an investigator qualifies as a philosopher. I suppose that most of us could point to very sorry efforts at philosophizing on the part of expert researchers of the highest rank. Specialization as an investigator may actually lead to atrophy of the philosophical faculty, just in the same way that constant concentration upon abstract principles may disqualify for experimental and observational work. The two types of mind are different, and both are wanted. In Darwin alone do we meet with the unique example of the combination of the two faculties in the same individual. But Darwin, be it remembered, philosophized only within the domain of organic Evolution, while Spencer's Philosophy embraced the whole domain of Evolution.

If justification for the use of the deductive method by those who have not themselves contributed the data be looked for among the sciences, a good case can be made out. That which is sanctioned within the narrower confines of the special sciences is no less justifiable for Science as a whole. In those sciences in which the observed facts are capable of quantitative expression, such as astronomy, physics, mechanics (in the abstract sense), &c., conclusions of the greatest general importance have been arrived at deductively by men who have never carried out an experiment or made an observation. It is, of course, admitted that in these sciences the data are capable of being dealt with by the most powerful and the most perfect of all deductive weapons—the mathematical.

Now what Spencer did, virtually amounted not only to a vindication of the right of all the sciences tb stand on the same footing as regards their mode of treatment, but to the insistence on the necessity for the use of the deductive method as a means of advancement in the biological sciences in the same sense that it is a recognized method in the physical sciences. He may not state so explicitly in his writings, but there can, I think, be no doubt that this is a legitimate interpretation of his teaching. His attempt was confessedly a bold one in view of the fact that in his time the biological sciences were not amenable to quantitative treatment, and that their data had not been brought within the sphere of symbolical reasoning. It is in fact only in comparatively recent times that biological data have been dealt with quantitatively on a sufficient scale and with sufficient precision to enable them to be handled deductively. I refer, of course, to the new science of Biometrics founded by Sir Francis Galton and headed by Professor Karl Pearson—a science in the development of which this University, through the late lamented Professor Weldon, has played a most conspicuous part.

But the right to use—the necessity for using the deductive method irrespective of the consideration whether the science is amenable to quantitative treatment or not, is pre-eminently the outcome of the Spencerian Philosophy in its relations toward Science. If deduction is only to be used when the generalization from which we start has all the certainty of a mathematically demonstrated truth, we are postponing, *sine die*, the development by one legitimate method, of all those sciences in which the data are so complex as to baffle quantitative treatment now, and possibly for all time. All that can be urged therefore against Spencer's treatment is that it was premature—that, to use a homely expression, he 'rushed' the biological sciences into the deductive stage before they were ripe for such treatment. But this only amounts to the admission that he was in advance of his time. And so he was, and so has been every philosopher in every age who has ever attempted to systematize human thought.

The judgement delivered upon Herbert Spencer as the result of that comparative study of his work with that of Darwin, which I have advocated as a fitting task for some critical philosopher of a future age, cannot, I imagine, be based upon the actual contributions to Science contained in the *Synthetical Philosophy*. We have all realized that this work, in certain parts and in common with the sciences upon which it is based, suffers from 'imperfect information'—that sooner or later it will have to be recast in the light of new knowledge. Many modern specialists have dwelt upon their reasons for dissenting from Spencer's conclusions in one direction or another, but few have indicated his positive services to Science as the vitalizer of the philosophical method as an instrument in scientific progress in those departments which had failed to recognize its power. Among the later departures the most divergent is probably the Weismannian doctrine of the non-transmissibility by inheritance of what are known as 'acquired characters'—a doctrine which most of us believe to have a preponderating balance of evidence in its favour. That point was ably dealt with by Professor Bourne in last year's Herbert Spencer Lecture. To the last Spencer opposed, and we may fairly say very ably opposed, this attempt to eliminate the final trace of Lamarckism from organic evolution. It cannot be said that we are as yet in a position to write *finis* to this chapter of biological controversy.

When the time is ripe for a revised *Synthetical Philosophy* it may safely be predicted that no single individual will be able to undertake that task, but, as has well been pointed out, that a syndicate of experts will be required. And when that revision is called for and when we know more about the mechanism of development in inorganic and organic nature we shall still have a *Synthetical Philosophy* on Spencerian lines, with Evolution as the central idea. Regarded as a philosopher, the founder of that system will no more suffer in reputation by the revision of his scientific data than will the lustre of Charles Darwin's name be diminished by the revision of his scientific data in the light of scientific progress. We now smile upon Bacon's science as puerile, but he did his best with the scientific faculties within him and with the materials available in his time. His contemporary, William Gilbert of Colchester, appears to have had more of the scientific faculty, but Bacon's fame as a philosopher is not thereby diminished, nor is his rank as a philosopher determined by his contributions to science. The Spencerian philosophy as a philosophy based upon science may—nay must—undergo development, but if Evolution is true in principle—as we believe it is—that philosophy may be expected to survive throughout the ages by that most effective of all evolutionary processes, 'Descent with Modification.'

APPENDIX

EXTRACT FROM A LETTER FROM ALFRED RUSSEL WALLACE, dated June 23, 1910.

'As to Herbert Spencer, his style in his systematic work is such as to repel many readers. His terminology was often obscure and his reasoning often tremendously elaborate. But when attacking any special problem of biology or physics he was wonderfully luminous. I remember being greatly impressed by his Linnean paper, 'On Circulation and the Formation of Wood in Plants' (*Trans. Linn. Soc.*, Vol. XXV. Read March 1, 1886. Appendix C, *Principles of Biology*, Vol. II). It shows what a lot of experiments he made, how constantly he appealed to the experimental method and how admirably he reasoned on it. This paper, written in 1865, before Darwin had begun his work on the movements of plants, shows, I think, that if Spencer had been less of a thinker and more of a specializer he could have rivalled Darwin as an investigator. I have always been interested in sap motion—a problem *not yet settled*, and yet Spencer, more than forty years ago, seems to have thrown more light on it than any one else. On the whole, Spencer, I think, still ranks as the greatest all-round thinker and most illuminating reasoner of the Nineteenth Century.'

EXTRACT FROM A LETTER FROM SIR JOSEPH D. HOOKER, dated October 14, 1910.

'I have great pleasure in assuring you of the high esteem in which I held my dear late friend Herbert Spencer's scientific position. Of his ability to support his views by arguments derived from the vegetable kingdom there can be no question, but this is a very small matter in contrast to the skill with which he seized upon facts and suggestions and the patient labour with which he sought to test them by experiments, often devised and carried out by himself unaided. It was my privilege to be kept fully cognizant of these operations of his mind, his eyes and his hands, to supply him now and then with materials from Kew, and always with encouragement—but beyond these he owed me nothing.'

Oxford: Printed at the Clarendon Press by Horace Hart, M.A.