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THE HISTORY OF EVOLUTIONARY THOUGHT[1]

AS RECORDED IN MEETINGS OF THE BRITISH ASSOCIATION

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SIR WILLIAM THOMSON, in his address at Edinburgh in 1871, said that "the real origin of the British Association" was given in the words of a letter written by David Brewster to John Phillips on February 23, 1831, a few months before the first meeting: "The principal object of the Society would be to make the cultivators of science acquainted with each other, to stimulate one another to new exertions, and to bring the objects of science more before the public eye, and to take measures for advancing its interests and accelerating its progress." That the time was fully ripe for the birth of the association is made very clear by the words written by John Keble to a friend, referring to the D.C.L. degrees conferred, at the Oxford meeting in 1832, on David Brewster, Robert Brown, John Dalton and Michael Faraday: "The Oxford Doctors have truckled sadly to the spirit of the times in receiving the hodge-podge of philosophers as they did"—an opinion on which Lord Salisbury commented at the Oxford meeting in 1894: "It is amusing at this distance of time, to note the names of the hodge-podge of philosophers whose academical distinctions so sorely vexed Mr. Keble's gentle spirit." It is not only amusing but pathetic that such words should have been used by a revered member of a university which had done splendid service for science, as has been so well shown in Dr. R. T. Gunther's volumes.[2]

Faced by the serious duty of preparing this address, I felt that the best hope of interesting you would be to choose a subject which has received special attention at our meetings. I have selected the progress of thought on organic evolution as it may be followed in addresses,

[1] Address of the president of the British Association for the Advancement of Science, given at Nottingham, September 1, 1937.

[2] "Early Science in Oxford," vols. i–xi.

papers and discussions, mainly restricting myself to the series of meetings which began with the jubilee at York in 1881, the first of many that I have had the pleasure of attending.

The British Association provides a very favorable field for the discussion of many-sided subjects such as evolution—subjects which attract members from very different as well as from closely related sections. Hence a wide range of varied experience is open to one who can look back over more than half a century; and I do not propose to exclude some of the humorous sayings and incidents which, from time to time, have enlivened our meetings and contributed to their success. Some of them certainly deserve to be rescued from oblivion, although to perform this pious duty I must risk the enmity of the Goddess of Folly, who, as Erasmus tells us, proclaimed: "I hate a man who remembers what he hears."

The fiftieth anniversary at York was a memorable meeting, with Sir John Lubbock (Lord Avebury) as president, and the chair of every section except economics, under Grant Duff, taken by a past-president of the association.

I then enjoyed to the full one of the chief benefits conferred by our association upon its younger members —the opportunity of meeting older men, up to that time only known to them by the fame of their discoveries. Professor O. C. Marsh had come over from Yale, his main object being to buy for his university museum the second and more perfect fossil of the wonderful ancestral bird Archeopteryx with teeth and a long, lizard-like tail—clear evidence of reptilian origin. The earlier example had been bought for the British Museum at a price which was said to have provided the dowry for a professor's daughter, and Marsh soon realized, as he told me, that the second was not for sale on any terms. "We let the other go and I believe they would kill me if this were sold" was the reply given to him by the authority in Munich. He was, however, able to study the fossil, and his description and drawings of the teeth, in the Geological Section, followed the only attack on evolution itself, as distinct from its causes, which I have ever witnessed at any of our meetings. It was the exhibition by H. G. Seeley of his reconstruction of Archeopteryx from this fossil, which aroused the fury of the paleontologist, old Dr. Thomas Wright of Cheltenham: "Archeopteryx hasn't got a head, how can it possibly have teeth?" he growled, knowing nothing of the latest find or of the fact that a detached head and scattered teeth had been detected on the slab in which the older specimen was embedded. In spite of Professor Newton's positive statement and the form of the teeth, drawn by Professor Marsh at the request of the chairman, Dr. Wright, quite unconvinced, continued muttering "Archeopteryx is a very good bird," its virtue in his opinion entirely uncontaminated by any taint of reptilian affinity.

Professor Marsh also read a paper in the Zoological Section on his own wonderful discoveries of toothed birds from the rocks of the western United States. Richard Owen, president of the section, was in the chair and, with the memory of old and embittered controversies in his mind, the author told me that he had felt rather anxious in bringing this communication forward. But in that friendly atmosphere there was no reason for alarm. Owen welcomed the paper warmly and in confirmation told us, in the most charming manner, of the traces of teeth found in an embryo parrot.

The event which stands out most clearly in my memories of the jubilee meeting is Huxley's evening lecture on "The Rise and Progress of Paleontology"—the science which provides an essential part of the foundation on which geographical, geological and biological evolutionary history has been built. The insuperable difficulty felt by the older naturalists was to believe that the land had been for the most part deposited under the sea, and to account for the presence of fossils, or as they were called, "formed stones." The true solution, Huxley explained, was found and published in 1669 by Nicholas Steno, a Danish professor of anatomy at Florence, who carefully studied certain fossils, known as "glossopetre," which abounded in the Tuscan rocks and were believed to be fossil fig-leaves. Steno, who was not satisfied with this interpretation, dissected a shark's head and showed that the "glossopetre" exactly correspond in every particular with the teeth—"that in fact they were shark's teeth." The emphasis with which Huxley made this statement comes back to me after the lapse of nearly sixty years. From this Steno was led to conclude that they were the teeth of shark-like fishes living in the Tuscan sea and later embedded, with other remains, in the strata which had there accumulated.

I have not noticed the fanciful suggestion of "fossil fig-leaves" in any published version or account of Huxley's lecture that I have seen, but he certainly told us of it and it is an interesting example of the attempts made by the naturalists of the day to explain the fossils embedded in rocks then believed to be of terrestrial origin. I can not resist the temptation of quoting Plot's[3] more ingenious and amusing effort to account for the well-known layer of oyster-shells (*Ostrea bellovacina*) found ". . . at some places here in *England,* particularly at *Cats-grove* [now Katesgrove] near Reading . . .; which how they should come here with-

[3] "The Natural History of Oxfordshire, being an Essay toward the Natural History of England," by Robert Plot, D.D. Printed at the Theater in Oxford. 1677. Dedicated To the most Sacred Majesty of Charles the Second, King of Great Britain, France and Ireland, Defender of the Faith, etc. (pp. 118–122).

out a *Deluge,* seems a difficulty to most men not easily avoided."

Plot was, however, helped "to a *salvo*" for his own objection by remembering that Reading was

> a *Town* of very great *action* during the *Invasions* of the *Danes,* who cutting a deep trench cross between the *Kennet* and *Thames,* and inclosing themselves as it were in an *Island,* held it against King *Ethelred,* and *Alfred* his Brother a considerable time; from whence in all probability, the *Saxons* having removed their *Cattle* and other provisions before the *Danes* arrival, 'tis likely that they might be supplyed from their *Navy* with *Oysters,* which during the time of the aboad of the *Army* on Land, might be a very suitable employment for it: Which conjecture, if allowed, there is nothing more required to make out the possibility of the bed of *Oysters* coming thither without a *Deluge,* but that *Cats-grove* was the place appointed for the *Armies* repast.

The probability of this suggestion may be inferred from the age of the "Woolwich and Reading" beds in which the oysters are found—estimated by my friends Professor Watts and Professor Hawkins at about fifty to sixty million years.

Dr. Plot's explanation of fossils in general as well as of flowers was of a very different kind. To account for their existence he appealed to "the wisdom and goodness of the *Supreme Nature,* by the *School-men* called *Naturans,* that governs and directs the *Natura naturata* here below, to beautifie the World with these varieties; which I take to be the end of such productions as well as of most *Flowers,* such as *Tulips, Anemones,* &c. of which we know as little use as of formed stones."

The modest and withal amusing paragraph which follows I venture to quote in full as an example to be followed in scientific controversy:

> And thus I have given the grounds of my present *opinion,* which has not been taken up out of *humor* or *contradiction,* with intent only to affront other worthy Authors modest conjectures, but rather friendly to *excite them,* or any *others,* to endeavor collections of *shell-fish,* and parts of other *Animals,* that may answer such *formed stones* as are here already, or may hereafter be produced: Which when ever I find done, and the reasons alleged *solidly* answered, I shall be ready with acknowledgment to retract my *opinion,* which I am not so in love with, but for the sake of *Truth* I can chearfully cast off without the least reluctancy.

One chief object which, as I believe, Huxley had before him was to bring forward a calm, clear statement of the evidence on which alone it was possible to achieve that "reconstruction of an extinct animal from a tooth or bone," which had made so deep an impression on the imagination. The reconstruction was in fact a simple inference based on anatomical experience such as that gained by Steno when he dissected the shark and concluded that the "glossopetrae" were the teeth of shark-like fishes. But this reasoning—that a fossil tooth or bone on the surface of a rock can not by itself enable the geologist to predict that a skeleton of a certain type lies hidden beneath—seeming to diminish the glory of Cuvier's splendid work, was resented by Owen, who had replied with the bitter taunt that a tooth can tell us a great deal—a donkey can kick his master but he can not eat him. This may have been the encounter referred to by Huxley when he wrote of a friendly meeting with Owen at the Zoological Section of the association in Leeds (1858): "so that the people who had come in hopes of a row were (as I intended they should be) disappointed."[4] In the same spirit, I think, Huxley was glad to speak of the "glossopetrae" at the jubilee meeting, where Owen was president of a section, and calmly and simply, to reaffirm conclusions which are unassailable.

Huxley then passed on to Steno's further study of fossils and his proof of their relationship to terrestrial freshwater and marine organisms, and to his application of this evidence to the past condition of Tuscany—all discussed "in a manner worthy of a modern geologist" and later extended by Buffon to all parts of the world then known to be fossiliferous. These conclusions, "which almost constitute the framework of paleontology," only required one addition, made towards the end of the eighteenth century by William Smith, who showed that geological strata contained characteristic fossils so that rocks of the same age could be identified in all parts of the world, while the biologist could follow the changes in the living population of the globe—a record of constant extinction and continual generation of new species. We were then led to three general conclusions: (1) the vast length of time during which life has existed on the earth—"certainly for millions of years"; (2) the continual changes which living forms have undergone during this period; (3) the successive changes in the best-known fossil groups are such as we should expect if each series "had been produced by the gradual modification of the earliest form. . . ." This last conclusion meant evolution which so completely accorded with recent discoveries that "if it had not existed, the paleontologist would have had to invent it."

I can never forget the words spoken to me after the lecture by a dear friend of my youth, the late Viriamu Jones, principal of University College, Cardiff: "At every sentence I felt myself bowing to Huxley and saying 'you are the greatest man here; no one else could have said that as you have said it.' "

As Huxley's lecture continued in a calm spirit an embittered controversy, so his thoughts on the immen-

[4] "Life and Letters," vol. i, p. 157.

sity of past geological and biological time lead naturally to another controversy on the age of the earth conducted intermittently at our meetings between 1892 and 1921. It is, I think, a good example of the invaluable help which the British Association brings to discussion when there appears to be a difficulty in reconciling the conclusions reached by the followers of different sciences. Lord Kelvin's estimate of a hundred million years as the period during which the earth had been cool enough to permit the existence of life upon its surface—a period reduced by Professor Tait to ten million—was a great difficulty to geologists and biologists who believed that an immensely longer time was required for the history of the fossiliferous rocks and the evolution of animals and plants. Thus, to quote only one instance, Darwin, writing to Wallace in 1871 and referring to "missing links," said, "I should rely much on pre-Silurian time; but then comes Sir William Thomson, like an odious spectre." The geologists resisted more firmly. Thus Sir Archibald Geikie, in his presidential address at Edinburgh in 1892, concluded his discussion of the subject with these words: "The geological record furnishes a mass of evidence which no arguments drawn from other departments of nature can explain away, and which, it seems to me, can not be satisfactorily interpreted save with an allowance of time much beyond the narrow limits which recent physical speculation would concede." At the Leeds meeting in 1890 I had many opportunities of meeting Professor John Perry, and when we were walking together on the Sunday afternoon I asked him to tell me something of the Kelvin-Tait conclusions and how far they must be accepted. He had been a demonstrator under Kelvin and spoke of the intense interest with which he had followed his lectures at Glasgow, and he gave me no hope of escape. His change of opinion, throwing a most interesting light upon the influence of the British Association, was the result of the presidential address at Oxford in 1894, when Lord Salisbury chaffed the believers in natural selection, telling them that he did not wonder that they required many hundred million years for so slow a process, but that "if the mathematicians are right, the biologists can not have what they demand. . . . The jelly-fish would have been dissipated in steam long before he had had a chance of displaying the advantageous variation which was to make him the ancestor of the human race." When Perry read this pronouncement, sweeping aside the firm convictions of biologists and geologists, he was led to reexamine the evidence and soon found a flaw. The heat of the earth had been calculated on the assumption of a conductivity uniform through the whole mass, but Perry showed that with a conductivity becoming higher with increasing depth the Kelvin-Tait estimate of the time required for cooling to the existing temperature—on which the age of the habitable earth had been based—must be immensely lengthened. Perry told me of this destructive criticism and very kindly helped me to make use of it in the address to Section D at Liverpool in which I replied to Lord Salisbury's amusing attack on the evolutionists.

Lord Lister was our president at Liverpool in 1896, and I can not resist the temptation to digress for a moment and recall the address in which one of the greatest benefactors of mankind told us, with the utmost simplicity and modesty, the story of his life's work and the success which, in spite of all opposition, had been achieved. To hear him was an enduring inspiration.

The year 1896 was also the jubilee of Lord Kelvin's wonderful half-century of achievement in research and teaching, and I could not help feeling some regret that any criticism of his work should appear at this particular time. But in the kindly spirit of our association such doubts were quite unnecessary. I well remember how he came one day to our sectional committee-room to bring me some volumes of his works, and how, as I have recorded before, in the following year as we were traveling across Canada after the Toronto meeting and the chance of collecting insects for a few minutes at each station could not be resisted, Lord Kelvin said to his wife, "My dear, I think we must forgive Poulton for thinking that the earth is so very old when he works so hard in one day out of all the endless millions of years in which he believes!"[5]

The one line of evidence which left some anxiety in 1896 was suggested by Helmholtz, who allowed the sun only eighteen million years to have been giving out radiant heat at the present rate—a period Lord Kelvin was willing to extend to 500 million—and this estimated maximum was also accepted by Sir George Darwin, who, in his address[6] at Cape Town in 1905, spoke of the new evidence obtained by M. and Mme. Curie in their proof that radium gives out heat, and quoting in confirmation the work of R. J. Strutt, W. E. Wilson and G. H. Darwin, finally concluded that "the physical argument is not susceptible of a greater degree of certainty than that of the geologists, and the scale of geological time remains in great measure unknown." The light thrown by radium upon the Helmholtz estimate was also referred to in the presidential address of Ray Lankester at York in 1906, of J. J. Thomson, quoting the work of Strutt, Joly and Rutherford, at Winnipeg in 1909, and became a predominant subject in the joint discussion on the age of the earth, between Sections A, C, D and K, at Edinburgh in 1921.[7] Lord Rayleigh in opening this discussion concluded "that

[5] Report, British Association, Centenary Meeting, p. 78, 1931.

[6] Report, British Association, pp. 514–518, 1905.

[7] Report, British Association, pp. 413–415, 1921.

radioactive methods of estimation indicate a moderate multiple of 1,000 million years as the possible and probable duration of the earth's crust as suitable for the habitation of living beings. . . ."

Even in the present year Sir Ambrose Fleming, in his address to the Victoria Institute, is reported in *The Times* of January 12 to have maintained that "We were not in possession of any generally agreed scientific modes of geological time measurement, but only with estimates which were based for the most part on personal predilection or guesses at truth." It is to be regretted that the conclusions of scientific colleagues should be attributed to "personal predilection," and as for "guesses at truth"—what are these but hypotheses; and surely the discoverer whose imaginative effort led to the thermionic valve and did so much to endow the world with the infinite possibilities of wireless—surely he has little cause to choose for the serious efforts of others the word which in this connection carries a suggestion of shallow irresponsibility.

Geologists and biologists do not profess to know the age of the earth as the abode of life, but they are sure that, in the words used by Sir William Turner at Bradford in 1900, its birth "must have been in the far-distant past, at a period so remote from the present that the mind fails to grasp the duration of the interval."

I fear that too much of our time has been occupied by the attempt to show that the field is clear for the discussion of organic evolution, but until this could be done any such discussion appeared to be well-nigh useless.

It is, I think, a mistake to emphasize too strongly the very natural shock received by many who read the "Origin" or heard of its teachings for the first time and without any preparation; and I believe an even greater mistake to criticize the clergy for the time that elapsed before their acceptance of the new teaching. I shall never forget the reception of Aubrey Moore's paper, "Recent Advances in Natural Science in their Relation to the Christian Faith," by the Church Congress at Reading in 1883.[8] No speaker could have carried his audience with him more thoroughly: there was not a single protest or indication of dissent—nothing but enthusiastic applause. The Bishop of Oxford, Dr. Mackarness, was in the chair when the paper received this unanimous welcome—only twenty-three years after the Oxford meeting at which another Bishop of Oxford put his rude and foolish question to Huxley. It is pleasant to know that their celebrated encounter left no bitterness, for Huxley wrote in 1891 to Francis Darwin—"In justice to the Bishop, I am bound to say that he bore no malice, but was always courtesy itself when we occasionally met in after years."

I remember as a youth receiving a gentle parental warning against committing myself too entirely to a belief in evolution—a very different experience from that of our president at Hull in 1922, my friend Sir Charles Sherrington, who in 1873 was persuaded by his mother to take the "Origin" with him on his summer holiday, with the inspiring words—"It sets the door of the Universe ajar!"

I have already recalled Dr. Wright's indignation at York in 1881 as my only experience of opposition to a belief in organic evolution at any of our meetings, and the published Proceedings confirm this impression of unanimity. Thus, R. H. Traquair, addressing the biologists at Bradford in 1900, said, "I hardly think that we should now find a single scientific worker who continues to hold on to the old special creation idea"; and Lord Salisbury at Oxford in 1894, referring to Darwin, said, "He has, as a matter of fact, disposed of the doctrine of the immutability of species. It has been mainly associated in recent days with the honored name of Agassiz, but with him has disappeared the last defender of it who could claim the attention of the world." The mention of this great American naturalist recalls Tyndall's fine address at Belfast in 1874 and his memories of Agassiz's words, "I was not prepared to see this theory received as it has been by the best intellects of our time. Its success is greater than I could have thought possible."

Huxley, who had seconded the vote of thanks to Lord Salisbury, wrote to Hooker a few days later: "It was very queer to sit there and hear the doctrines you and I were damned for advocating thirty-four years ago at Oxford, enunciated as matters of course—disputed by no reasonable man!—in the Sheldonian Theater by the Chancellor. . . ."[9]

A letter written two days earlier to Boyd Dawkins records Huxley's opinion of another part of the address. "Lord Salisbury gave himself away wonderfully, but he was so good about Darwin himself that I shut my eyes to all the nonsense he talked about Natural Selection."[10]

Leaving now the subject of organic evolution itself, as generally accepted, I wish to speak on the difficult question of its motive causes, which for many years have formed the subject of addresses, discussions and papers at our meetings. The great division into two opposed theories of causation became clear in 1887 when Weismann attended the meeting at Manchester, and a discussion on "The Hereditary Transmission of Acquired Characters" was held in Section D. From

[8] "Science and the Faith," London, pp. 222–235, 1889.

[9] "Life and Letters," vol. ii, p. 379, 1900.

[10] From a letter of August 10, 1894, printed in the *Jesus College (Oxford) Magazine*, for Lent Term, 1928; and reprinted in *Hope Reports*, xvi: no. 3, p. 6, 1929. (Privately circulated to many scientific libraries.) Huxley's letter of August 18, 1894, to Lewis Campbell ("Life and Letters," vol. ii, p. 379) refers to the same subject.

that time evolutionists attending our meetings have been either "Lamarckians," following Erasmus Darwin, Lamarck, Buffon and Herbert Spencer, or "Darwinians" who followed Darwin and Wallace. Darwin himself, however, included the Lamarckian conception of "use-inheritance" as a motive cause, although believing it to be far less important than natural selection. The term "Neo-Darwinian" has therefore been applied to those who, accepting Weismann's teaching, reject "use-inheritance" altogether.

It must always be remembered that, apart from any theory of causes, the world owes its belief in organic evolution to all the great men whose researches and teaching have founded the two schools, and perhaps chiefly, at any rate among the English-speaking nations, to Herbert Spencer. I was first led to realize the extent of his transatlantic popularity when I learned from an American story greatly enjoyed in those far-off undergraduate days that his books were keenly appreciated by a bashful hero, who was so far from sharing the sublime confidence of their author, that he was only led to perform the most fateful action in life by the pressing advice of a very young nephew who assured him, in the presence of the lady, that if he was fond of her, the proper thing to do was to kiss her. Herbert Spencer's infallibility certainly lent itself to such stories as that of his supposed reply to an argument—"That can't be true, for otherwise "First Principles" would have to be re-written—and the edition is stereotyped"; or how Darwin said that to read Spencer always made him feel like a worm, but that he retained the worm's privilege of wriggling, and at another time "wonderfully clever, and I dare say mostly true." But, allowing for a style which provoked these and other amusing comments, we must never forget that believers in the doctrine of organic evolution owe an immeasureable debt to Herbert Spencer.

James Russell Lowell's amusing lines in the "Bigelow Papers"[11] appear to prove that Lamarckism was prevalent in America many years before the "Origin":

> Some flossifers think thet a fakkilty's granted
> The minnit its proved to be thoroughly wanted,
> . . . . . .
> Ez, fer instance, thet rubber-trees fust begun bearin'
> When p'litikkle conshunces come into wearin',—
> Thet the fears of a monkey, whose holt chanced to fail,
> Drawed the vertibry out to a prehensile tail.

The year of the Manchester meeting, 1887, was the fiftieth anniversary, and we are now celebrating the centenary, of the entry in Darwin's pocket-book: "In July opened first note-book on Transmutation of Species. Had been greatly struck from about the month of previous March on character of South American fossils, and species on Galapagos Archipelago. These facts (especially latter), origin of all my views."

It is especially interesting to recall that these views, as Professor Newton told us in his address to D, the Biological Section, did not include natural selection, which only came into Darwin's mind when he read Malthus, "On Population," in October, 1838. Newton, who had read the proof-sheets of the great "Life of Darwin," published later in 1887, then spoke of Wallace's independent discovery, made twenty years after Darwin's, a discovery suggested to him also by reflecting on Malthus, and of the friendship between the two great men to whom this fruitful conception had come, referring the cynic who would "point the finger of scorn at the petty quarrels in which naturalists unfortunately at times engage" to this "greatest of all cases, where scientific rivalry not only did not interfere with, but even strengthened, the good-feeling which existed between two of the most original investigators." And here I can not resist the desire to quote a part of the speech made by Wallace at the most thrilling scientific gathering I have ever attended—the fiftieth anniversary of the Darwin-Wallace Essay read before the Linnean Society on July 1, 1858, only twelve days after the arrival of Wallace's letter and manuscript from the Moluccas. Wallace then said, on July 1, 1908:

> The idea came to me, as it had come to Darwin, in a sudden flash of insight: it was thought out in a few hours . . . and sent off to Darwin—all within one week. *I* was then (as often since) the "young man in a hurry": *he*, the painstaking and patient student, seeking ever the full demonstration of the truth that he had discovered, rather than to achieve immediate personal fame. . . . If the persuasion of his friends had prevailed with him, and he had published his theory, after ten years'—fifteen years'—or even eighteen years' elaboration of it—*I* should have had no part in it whatever, and *he* would have been at once recognised, and should be ever recognised, as the sole and undisputed discoverer and patient investigator of the great law of "Natural Selection," in all its far-reaching consequences.[12]

Amusing evidence of the difficulty with which this "great law" was understood is afforded by a verse written by Lord Neaves and dated May, 1861:

> A deer with a neck that was longer by half,
> Than the rest of its family (try not to laugh),
> By stretching and stretching, became a Giraffe,
> Which nobody can deny.[13]

Yet Wallace, referring to Lamarck's hypothesis and "that now advanced," had written in his section of the joint essay:

[11] The lines are quoted from the First Part, published 1846–48.

[12] "Darwin-Wallace Celebration of the Linnean Society of London, pp. 6, 7, 1908.

[13] "The Origin of Species. A new song." In "Songs and verses, social and scientific," by an old contributor to *Maga*. Edinburgh, 2 Ed., 1868.

Neither did the giraffe acquire its long neck by desiring to reach the foliage of the more lofty shrubs, and constantly stretching its neck for the purpose, but because any varieties which occurred among its antitypes with a longer neck than usual *at once secured a fresh range of pasture over the same ground as their shorter-necked companions, and on the first scarcity of food were thereby enabled to outlive them.*

There were fortunately others who did not launch such ill-aimed criticism. Thus Professor Newton, reminding the section that the new teachings had been at once accepted by Canon Tristram[14] before the appearance of the "Origin of Species" (on November 24, 1859), expressed, with all the enthusiasm of one who was devoted to the same delightful branch of natural history, "the hope that the study of ornithology may be said to have been lifted above its fellows." It was indeed very fortunate that the Darwin-Wallace Essay should have been read so soon after its appearance by a naturalist who looked on the species question as did Tristram—a great traveler and observer who studied indefatigably the birds he loved, as living creatures and in as many countries as he could visit.

At the last meeting of the British Association in Nottingham (1893) Canon Tristram was president of Section D and, in his address, gave an account of the observations referred to by Newton at Manchester. The historic interest of this early acceptance of natural selection is such that I have prepared a brief abstract of his chief conclusions.

During a visit of many months to the Algerian Sahara in 1857–58, he "noticed the remarkable variations in different groups, according to elevation from the sea, and the difference of soil and vegetation." On his return he read the Darwin-Wallace Essay and wrote, "It is hardly possible, I should think, to illustrate this theory better than by the larks and chats of North Africa." He then explained how the colors arose by selective destruction of birds which harmonized less well than others with the surface of the desert. And similarly with other larks having "differences, not only of colour, but of structure," chiefly "marked in the form of the bill." He took as instances a very long-billed lark (*Galerita arenicola*), resorting exclusively to the deep, loose, sandy tracts, and a very short-billed allied species (*G. isabellina*), haunting the hard and rocky districts. He then pointed out that there is individual variation in the bills of larks and that the shorter-billed birds would be at a disadvantage in obtaining food from sandy areas but at an advantage among the rocks where strength is required. He concluded, "Here are only two causes enumerated which might serve to create, as it were, a new species from an old one. Yet they are perfectly natural causes, and such as I think must have occurred, and are possibly occurring still. We know so very little of the causes which in the majority of cases, makes species rare or common that there may be hundreds of others at work, some even more powerful than these, which go to perpetuate and eliminate certain forms "according to natural means of selection.' "

[14] *Ibis*, pp. 429–433, October, 1859.

The temptation to record an amusing incident which happened at one of the meetings of Section D at Manchester can not be resisted. Work was proceeding smoothly under the genial guidance of Professor Newton when, suddenly, Dr. Samuel Haughton, of Dublin, entered and from the back of the room announced in arresting tones that he had an important communication to make about the animals preserved from the flood. He believed that Mrs. Noah strongly objected to her husband's intention to take the elephants on board, fearing that their weight woud cause a dangerous displacement of the ark's metacenter. How this domestic difference was composed we had no opportunity of learning, for as the chairman, whose expression combined sympathetic amusement with mild deprecation, was rising and about to protest, Dr. Haughton, anticipating the result, had already turned towards the door, telling us over his shoulder that he was on his way to make a fuller communication on the subject to the Anthropological Section.

After this brief description of an event, which I hope you will agree ought not to be forgotten, we must return to organic evolution and to one of the most important subjects debated at any time before a meeting of the British Association—the question, "Are Acquired Characters Hereditary?"—brought before the world by Professor August Weismann, who was present at Manchester and spoke in the discussion (unfortunately not reported), introduced by Ray Lankester, in which Dr. Hubrecht, Patrick Geddes, Marcus Hartog and the present speaker took part. Weismann's conclusion that "acquired characters" are not inherited, was held by Professor Goodrich, in his address to Section D at Edinburgh in 1921, to be "the most important contribution to the science of evolution since the publication of Darwin's 'Origin of Species,' " an opinion with which the great majority of biologists will agree, although the terms employed for the two classes, the inherited and the non-inherited, together with the ideas underlying them, were shown by Adam Sedgwick, at Dover, in 1899, Archdall Reid, and others, as well as by Goodrich himself, to be incorrect. Nevertheless it will probably be impossible to abondon the word "acquired," employed by Erasmus Darwin (1794), Lamarck (1809), and Prichard (1813) as well as by later authorities. Whenever environmental conditions are followed by characteristic changes, absent when these conditions are absent; or when such changes

follow the use or disuse of the parts of an organism, or the education it has received, then we have before us the "acquired" characters maintained by Weismann to be incapable of hereditary transmission. This vital conclusion, accepted, as I believe it is, by nearly all biologists, is not appreciated as it ought to be by the general public. A brief statement of a single piece of evidence may convince some who are doubtful about a conclusion with which human life is very deeply concerned.

My old friend, the late A. A. Macdonell, professor of Sanskrit at Oxford, spoke two languages, English and German, as they are spoken by native Englishmen and Germans. I asked him whether he thought it was possible for any mature person to learn a foreign language so perfectly that he would be mistaken for a native. He replied that he was sure it could not be done and that his own ability to speak the two languages as he did had been only made possible because as a small child he had been continually taken backwards and forwards between the two countries. Yet any human being transported as a baby from his own country to another and brought up there among the natives will learn to speak as they speak. All the past generations, however many, during which his ancestors spoke the language of his birthplace, will count for nothing, will not retard his acquisition of another tongue or modify it in any way.

An interesting and amusing example is provided by the futile striving of an Englishman to pronounce the Welsh double-l, generally attempted by the substitution of "th." And even the advice given by a Welsh clergyman to the English Bishop of his diocese is unlikely to bring success: "You must put the tip of your Right Reverend tongue against the roof of your Right Reverend mouth, and hiss like a goose."

The result of education as an "acquired" character in the Weismannian sense is of such special importance that I think it is well to quote the conclusions stated by Ray Lankester in his address to the seventy-fifth meeting of the association at York. He then maintained that the "power of building up appropriate cerebral mechanism in response to individual experience, or what may be called 'educability,' is the quality which characterises the larger cerebrum, and is that which has led to its selection, survival, and further increase in volume . . . 'Educability' can be transmitted; it is a congenital character. But the *results* of education can *not* be transmitted. In each generation they have to be acquired afresh. . . . On the other hand, the nerve-mechanisms of instinct are transmitted, and owe their inferiority as compared with the results of education to the very fact that they are *not* acquired by the individual in relation to his particular needs, but have arisen by selection of congenital variation in a long series of preceding generations."[15]

Lankester was led by these conclusions to reject altogether the theory of G. H. Lewes, G. Romanes and others, "that instincts are due to lapsed intelligence," a theory also disproved by Lloyd Morgan's observations on young birds described by him at the Ipswich meeting in 1895.[16] Another very important subject brought forward by Lankester was the evidence, originally published by him in 1894,[17] that Lamarck's first and second laws of heredity "are contradictory the one of the other, and therefore may be dismissed." His statement may be briefly summarized as follows:

The first law assumes that in spite of thousands of generations during which a normal environment has

> moulded the individuals of a given species of organism, and determined as each individual developed and grew "responsive" quantities in its parts (characters); yet, as Lamarck tells us, and as we know, there is in every individual born a potentiality which has *not* been extinguished. Change the normal conditions . . . and (as Lamarck bids us observe), in spite of all the long-continued response to the earlier normal specific conditions, the innate congenital potentiality shows itself. The individual . . . shows *new* responsive quantities in those parts of its structure concerned, new or acquired characters. . . .
>
> So far, so good. What Lamarck next asks us to accept, as his "second law," seems not only to lack the support of experimental proof, but to be inconsistent with what had just preceded it. The new character which is *ex hypothesi*, as was the old character . . . which it has replaced—a response to environment . . . is, according to Lamarck, all of a sudden raised to extraordinary powers. The new or freshly acquired character is declared . . . to be capable of transmission by generation; that is to say, it alters the potential character of the species. It is no longer a merely responsive or reactive character, determined quantitatively by quantitative conditions of the environment, but becomes fixed and incorporated in the potential of the race, so as to persist when other quantitative external conditions are substituted for those which originally determined it.

The effect of Lamarck's laws on the hereditary transmission of acquired characters would be this: "a past of indefinite duration is powerless to control the present, while the brief history of the present can readily control the future."

After hearing a very condensed statement of conclusions so essentially bound up with the progress of organic evolution, I feel sure that you will wish to be reminded of Professor Ewing's words which followed

[15] Report, British Association, pp. 26–27, 1906. The conclusions here quoted had been communicated to Société de Biologie of Paris, in 1899 (Jubilee Volume) and were reprinted in *Nature*, lxi: pp. 624–625, 1900.

[16] Report, British Association, p. 734.

[17] *Nature*, li: p. 127, 1894; Report, British Association, pp. 29, 30, 1906.

the address at York: "Now is the winter of our discontent made glorious summer by this Ray of Lankester."

Returning to the unreported discussion on the inheritance of acquired characters at Manchester, I venture to bring forward certain observations opposed to a belief in Lamarckian evolution by means of inherited experience—observations which I then described and have not known to be answered. In the relationship between enemy and prey there is very commonly no opportunity for the latter to learn by experience. The wonderfully elaborate adaptations by which sedentary insects are hidden from enemies have been evolved, not by experience of enemies but by avoidance of enemies. In these examples, and they are numberless, we are driven to accept Weismann's conclusion and with him to invoke "the all-sufficiency of Natural Selection." When one of the twig-like caterpillars, of which there are so many in this country, is detected by an insectivorous bird it can do nothing and is devoured at once. Its one defence is the astonishingly perfect resemblance to a twig of the bush or tree on which it lives. It is firmly fixed and its weight also supported by an almost invisible thread so that it can not escape as many caterpillars do by dropping to the ground and sheltering in the grass or among dead leaves. Its one chance of survival is to gain so perfect a disguise that it will not be seen, and to gain this end the adaptive devices are most elaborate and wonderful: its twig-like shape and colors with the power of gradually adjusting these so as to resemble the bark of the bush or tree on which the parent moth laid the egg from which it came, even the power to reproduce exactly the appearance of lichen, the rigid stick-like attitude maintained during the hours of daylight. Finally there is the evidence, recently obtained by Robert Carrick,[18] that the disguise *does* protect; for examples of one of these caterpillars, resting on a branch of its food-plant fixed over a wren's nest containing young, were unnoticed by the parent bird which used the same branch as a convenient perch; yet seen and at once taken when placed on a white surface below.

One of the best examples of a prophetic instinct is to be found in the larva of an African Tabanid fly (*T. biguttatus*). This maggot lives and feeds in soft mud which, during the dry season when the chrysalis stage has been reached, will be traversed in all directions by wide and deep cracks in which insectivorous animals can search for prey. But the maggot, while the mud is still soft, prepares for this danger. By tunnelling spirally up and down it makes a line of weakness which will cause a pillar to separate from the mass when the mud hardens and contracts. It then tunnels into the still soft pillar and becomes a chrysalis in the center of its deeper end. However wide the cracks which appear in the mud, the maggot has arranged beforehand that they will not invade its cylinder. Dr. W. A. Lamborn, who made this most interesting discovery, observed that the summits of the pillars, forming circular discs of about the size of a penny, scattered here and there over the surface, were never thus traversed, but that an empty shell was protruding from the center of each when the fly had emerged.[19] My friend the late Professor J. M. Baldwin, the distinguished American psychologist, well remembered at many of our meetings, wrote when he heard of this discovery: "it seems *complete*—one of those rare cases of a single experience being sufficient to establish both a fact and a reason for the fact! It is beautiful."

I would ask any believer in Lamarckian evolution, or in Hering's and Samuel Butler's theory of unconscious memory residing in the germ-cells, how it would be possible to explain these prophetic instincts, adapted not to meet but to avoid future experience, except by the operation of natural selection.

The appeal of orthogenesis, or internal developmental force, as the motive cause of evolutionary progress has often been made—generally by paleontologists rather than by the observers of living forms. Any such belief in the potency of an internal tendency is, I think, open to the criticism made by Thistleton Dyer in his address to Section D at Bath in 1888:

> This appears to me much as if we explained the movement of a train from London to Bath by attributing to it a tendency to locomotion. Mr. Darwin lifted the whole matter out of the field of mere transcendental speculation by the theory of natural selection, a perfectly intelligible mechanism by which the result might be brought about. Science will always prefer a material *modus operandi* to anything so vague as the action of a tendency.

It is not necessary for me to speak on the rediscovery of Mendel's great work and all that it has meant to our biological sections in the early decades of the present century. The recent developments, following the work of Haldane, R. A. Fisher and others, and the vitally important relationship between Mendelism and natural selection were brought before us last year in Julian Huxley's illuminating address to Section D. The older belief that only large variations, or mutations as they then began to be called, were subject to Mendelian inheritance, and that small variations were not inherited at all, disappeared when further researches proved that extremely minute differences were "heritable in the normal Mendelian manner,"[20] and, with this, the foundation of Darwinian evolution became immensely strengthened. It

[18] *Trans. Roy. Ent. Soc., Lond.*, 85: part 4, p. 131, 3 pls., May, 1936.

[19] *Proc. Roy. Soc.*, B, 106: p. 83, pl. v, 1930; *Proc. Ent. Soc., Lond.*, v: p. 14, 1930.

[20] Report, British Association, p. 77 and references quoted, 1931.

is also right to remember that Bateson, the leader of Mendelian research in this country, always believed in natural selection, regarding it indeed as self-evident and not very interesting. Also that Ray Lankester, as long ago as his 1906 address at York, maintained that however far Mendelism was advanced it "would not be subversive of Mr. Darwin's generalisations, but probably tend to the more ready application of them to the explanation of many difficult cases of the structure and distribution of organisms."

The relationship between the germinal foundation of Mendelian and Weismannian heredity was considered in a paper by L. Doncaster read before Section D at the South African meeting in 1905. He then maintained that Weismann's "hypothesis that the material bearer of hereditary qualities is the chromatin of the nucleus" of the germ-cells had been confirmed by recent work on their maturation which "has shown that they contain a mechanism which seems precisely adapted to bring about that segregation of characters which forms the most fundamental part of the Mendelian theory, and it seems hardly possible that the two things are unconnected." MacBride also in his address to the same section at Newcastle in 1906 spoke of the "great epoch-making discovery of experimental embryology, *viz.*, the existence of *specific organ-forming substances.*" These fundamental discoveries bring to mind a conversation with Weismann when he had been finally driven to frame and elaborate this hypothesis, and was so appalled by the number and minuteness of the material bearers of hereditary qualities contained in a single-germ-cell that, as he told me, he could not believe that the physicists and chemists were correct in their conclusions about the size of the atom. He admitted that diverse lines of evidence led to the same result, but even so, he believed the future would prove that physicists were mistaken and that the atom was far smaller.

It is impossible to say more than a few words about the very interesting and important discussion on "the present state of the theory of natural selection" held at the Royal Society on May 14 last year. The subject was approached from many points of view by both zoologists and botanists, and their conclusions were very welcome to Darwinians who remembered the earlier opinions expressed when Mendel's great work was rediscovered. I think, however, that Professor D. M. S. Watson, in the opening address, was inclined to underestimate the value of the existing evidence for a "selective death rate," although every one will agree that "any new evidence . . . or indeed any suggestion of cases which might be capable of investigation" would be most desirable.

I may briefly mention a few experiments brought before Section D at the Bristol meeting in 1898 beginning with the work of Weldon and Thompson on the common shore crab, showing that the effect of china clay and other impurities in the sea at Plymouth was selective and promoted changes of shape which ensured that the water flowing over the respiratory surface was more efficiently filtered.

Then, on the subject of chance, the heroic help rendered by Mrs. Weldon, who four times recorded the result of 4,096 throws of dice, showing that the faces with more than three points were on the average uppermost slightly more often than was to be expected. It comes back to me very clearly because of the interesting explanation—that the points on dice are marked by little holes scooped out of the faces, and that points 6, 5 and 4, respectively opposite 1, 2 and 3 are somewhat lighter, more of the ivory having been removed; also because of Francis Galton's delight and his humorously expressed wonder whether the facts had been realized by those who had an interest other than scientific in the throwing of dice.

Experimental evidence was also submitted by Miss Cora B. Sanders (Mrs. C. B. S. Hodson) and myself, proving that when the rough angular pupa of the small tortoiseshell butterfly "is suspended from a surface against which it stands out conspicuously, it is in far greater danger than when it is fixed to one upon which it is concealed."

To the observer of living creatures, however, the most convincing evidence is provided by animals themselves. When a wild bird is seen to capture some conspicuous butterfly or moth and then immediately to reject it the association between inedibility and a warning color is more convincingly suggested than when insects are offered to animals in confinement, although such experiments are of great value and often provide the only available evidence. There are, however, instances in which abundant data for statistical investigation are furnished by the wild animals themselves. Thus the long-eared bat has the convenient habit of eating moths —its regular food—while it hangs suspended from a surface to which it returns after each capture; and as the wings are rejected, these may be collected in large numbers, yielding valuable information on the significance of concealing and warning patterns.

In the attempt to determine the motive causes of organic evolution, the work of the naturalist, the student of living nature, is essential. His task is to do what Lyell did for geology by directing attention to the forces now in operation and seeking with their help to interpret the past, and in this work it is especially valuable to study adaptations which have been developed in recent times and can, in certain instances, be proved to undergo changes even now. Thus the interesting observations of H. Lyster Jameson showed that a pale local race of the common mouse had been formed, although incompletely, in from 100 to 125

years, by the selective attacks of owls and hawks on sandhills near Dublin.[21] I therefore believe that the color of animals provide one of the most fruitful fields in which to pursue these investigations, and I regret that this work has been recently attacked by an American zoologist who, referring to the recent revival of natural selection, continues—"if the doctrine can emerge minus its sexual selection, its warning colors, its mimicry and its signal colors, the reaction over the end of the century will have been a distinct advantage."[22] It is of course impossible to discuss, on the present occasion, this confident attempt to depreciate the value of work associated with the names of Bates, Wallace, Trimen and Fritz Müller. I will only point out that their conclusions on warning colors and mimicry have been immensely strengthened and confirmed by the later observations of Guy Marshall, W. A. Lamborn, St. Aubyn Rogers, Hale Carpenter, V. G. L. van Someren and others in Africa; by the experiments conducted by some of these naturalists, and also by H. B. Cott and R. Carrack, and in the United States by Morton Jones.

It is interesting to remember that a paper by two American entomologists[23] was among the first to accept and support by fresh observations the conclusions brought forward by H. W. Bates in his great memoir on the mimetic butterflies of the Amazon Valley,[24] and that one of the authors treated the same subject more completely in a later paper[25] much appreciated by Darwin.[26]

It is also important to remember that the above-mentioned conclusions have been reached by the study of marine animals no less than terrestrial, as was shown by Herdman in his address to Section D at Glasgow in 1901, and by his experiments communicated to the same section at Ipswich in 1895; also that Garstang, with his very long and intimate experience of marine life, adopts the same interpretation of color and form with the associated attitudes and movements.

If time permitted it would be possible to speak of numerous papers on mimicry and the related subjects which have been brought before our meetings. It is impossible to attempt this now, but many will feel with me that the name of the late Dr. F. A. Dixey should not be forgotten—one who attended so regularly, so often read papers at our meetings, presided over Section D at Bournemouth in 1919, lectured at Leicester in 1907, always giving the results yielded by the study of his favorite insects, and their interpretation by the theory of natural selection; also one who delighted in the social gatherings of his section, where his rendering of "Widdicombe Fair" will be long remembered.

In my concluding remarks I am anxious to refer to a very interesting and encouraging subject—the feeling for animals and the care for their welfare to-day, as contrasted with the treatment they received a hundred years ago and even in the youth of many among us. Only last autumn *The Times* of October 12 reported that 1,000 swallows had arrived at Venice "sent there by bird-lovers from Vienna and Munich in order to save them from the effects of the cold weather. Soon after their arrival they were set free and flew south along the Adriatic coast." And a little earlier the writer of the amusing "Fourth Leader" referred to a meeting of the Society for the Preservation of the Fauna of the Empire at which the care of the opossum was discussed, comparing this with the report of happenings a hundred years earlier when there was a "humorous debate" at the Zoological Society "about puffing cigar-smoke into the cages of the monkeys," to their evident discomfort. The writer, yielding too far, we hope, to the depression of the present day, concludes: "The world, it may be, is 'man-sick' and yearning to be rid of a bad mistake. But the creature cannot be wholly vile when instead of torturing monkeys it takes thought for the opossum." It would not be right to quote from a century-old report without speaking of all that is done and has been done during many years for the care and health of animals by the great London society, and in doing this, for the education and happiness of our people. But the change of which I have spoken is most deeply impressed on those who remember, as many of us do, the misdirected hours in youth when birds were shot in our gardens and brick traps made to catch them. I feel sure that those who did these things are not essentially different from their children and grandchildren who have grown up in a kinder atmosphere. I must not occupy more time on a subject which to some may seem inappropriate, but it is bound up with education in its true sense—a leading out—and if, as Ray Lankester said at York, and we are all coming to believe, the hidden powers within *are* inherited while the results of their development are *not*, then there is no easing of the burden with the passage of time, but each generation afresh must bear the heavy responsibility of conducting this development in the best way so that its successor may be able to meet the changing and, at this time, the increasing needs. The relationship between the powers within and their development was suggested in arresting words by the late Professor Scott Holland: "To say that a man cannot be made good by Act of Parliament is such an obvious truth that people forget what an outrageous lie it is!

[21] *Jour. Linn. Soc.* (Zool.), 26: p. 465, pl. 30, 1898.
[22] "Evolution." A. Franklin Shull. New York, 1936.
[23] Walsh and Riley, "The American Entomologist," St. Louis, Mo., vol. i, p. 189, 1869.
[24] *Trans. Linn. Soc., Lond.*, xxiii: p. 495, 1862.
[25] Riley, "Third Annual Report on the Noxious . . . Insects of . . . Missouri," p. 142, 1871.
[26] "Charles Darwin and the Theory of Natural Selection" (Poulton, 1896), p. 202.

Thoughts on the development of these hidden powers by the educating influence of social environment suggest the greatest of the problems by which we are faced—the end of international war. Michael Foster, in his address at Dover in 1899, after speaking of progress in the material of warfare was led to believe that, "happily, the very greatness of the modern power of destruction is already becoming a bar to its use, and bids fair—may we hope before long?—wholly to put an end to it; in the words of Tacitus, though in another sense, the very preparations for war, through the character which science gives them, make for peace." And in his concluding pages he expressed the hope that the brotherly meeting between the English and French Associations at Dover and Boulogne might be looked upon as a sign that science, by nobler means than the development of armaments, was steadily working towards the same great end. And, in a time of still greater need and perplexity, may we not, in the same hopeful spirit, look upon the recent visit by which members of the French Association have honored us, and feel strengthened in the belief that the great end will be reached.

There are, I know, very many people who look upon the great war with later wars and rumors of wars as the close of Michael Foster's dream. The words in which Sir Arthur Schuster concluded his address at Manchester in 1915, and Sir Edward Thorps at Edinburgh in 1921, indicate, I hope, that the British Association does not thus despair, and in this belief I bring before you a passage from the far earlier address which Sir Richard Owen delivered to the twenty-eighth meeting at Leeds in 1858—a passage which makes a special appeal at a time when the British and American Associations are confidently hoping to strengthen still further the bonds of sympathy and mutual appreciation by which they have been happily united for so many years.

Referring to the transatlantic telegraph Sir Richard said:

> We may confidently hope that this and other applications of pure science will tend to abolish wars over the whole earth; so that men may come to look back upon the trial of battle between misunderstanding nations, as a sign of a past state of comparative barbarism; just as we look back from our present phase of civilisation in England upon the old border warfare.

Confident words inspired by the forging of a new link between the two great English-speaking nations. Nearly eighty years have passed since they were spoken, but with all the terrible disappointments there has been great progress, and a time will surely come, and may it come quickly, a time which shall prove that the visions of the young and the dreams of the old were prophetic of a glorious reality.

OBITUARY

VERNON LYMAN KELLOGG

VERNON LYMAN KELLOGG was born on December first, 1867, at Emporia, Kansas, close both to the place and the date of birth of his intimate, life-long friend, William Allen White, two men who between them have given that little Kansas town a noteworthy place in the history of America. Graduating from the University of Kansas in 1889, he took the next four years to prepare himself for the life of a zoologist, his studies being conducted at the University of Kansas, Cornell, Leipzig and Paris. In the quarter century from 1894 to 1920 he worked in close association with David Starr Jordan at Stanford University, where he was "professor of entomology and lecturer in bionomics." During this period he wrote eight books, most of them in collaboration with Dr. Jordan, on various aspects of zoology. These gave him his taste and revealed his talent for effective writing.

The war changed completely the course of Kellogg's life. Through his acquaintance with Herbert Hoover he became active and influential in the relief work in Belgium. Through his "Headquarters Nights" (1917), "The Food Problem" (1917), "Fighting Starvation in Belgium" (1918), "Germany in the War and After" (1919), "Herbert Hoover, the Man and his Work" (1920), he sprang into prominence as one of the effective political writers of the war period, while his administration of relief in Belgium (1915–16) and in Poland and Russia (1918–21) brought him recognition from France, Belgium and Poland. He was made an officer of the Legion of Honor (France), Commander of the Order of the Crown (Belgium), Commander of the Order of Polonia Restituta (Poland), Commander of the Order of Leopold I (Belgium), and Recipient of the National Gold Medal (Poland).

Kellogg never returned to academic life. Indeed his period of greatest influence and accomplishment began in 1919, when he became permanent secretary of the National Research Council, and from then until the time of his retirement in December, 1931, through his building up of that organization, through his service as trustee and member of the executive committee of the Rockefeller Foundation, trustee of the Brookings Institution, trustee and chairman of the executive committee of Science Service, member of the National Academy of Sciences, member of the executive committee of the American Association for the Advancement of Science, etc., and his continuous series of