and perfecting these tools, the two chief means by which mankind is making progress in our day? What sort of work is it? It is hard; it is no child's play; it is the work of maturity and strong purpose. The material rewards are few; probably not many of your generation will appreciate your labors, and most of you perhaps will not be heard of after your day. But you will leave mankind a heritage of profit forever, you will hasten the day when all men will know that their chief benefactors are those who delve into the secrets of nature and reveal them to their fellows. Does that work appeal to you?

R. D. CARMICHAEL

RECOLLECTIONS OF DR. ALFRED RUSSEL WALLACE

IT is impossible for any man to discuss adequately the life work of Alfred Russel Wallace. His activities covered such a long period, and were so varied, that no one living is in a position to critically appreciate more than a part of them. We are very much interested, of course, and have our opinions; but we need not pretend to any final or complete judgment. All must agree that a great and significant career has just been closed, but its full measure will probably never be known to any single man.

On the other hand, it may be possible to gain a clear idea of the character and aims of Dr. Wallace; and for our purposes this is perhaps the more important thing, since his guiding principles may also become ours, while the work he did is his alone. I once asked him about the origin of his interest in biology, and in the course of his reply[1] he said: "As to my interest in biology, . . . I doubt if I had or have any *special* aptitude for it, but I have a natural love for *classification* and an inherent desire to *explain things;* also a great love of beauty of form and color." Again, in writing to the biology students of the University of Colorado, he said:[2]

> The wonders of nature have been the delight and solace of my life. . . . From the day when I first saw a bee-orchis in ignorant astonishment . . . nature has afforded me an ever-increasing rapture, and the attempt to solve some of her myriad problems an ever-growing sense of mystery and awe.

This is the spirit of the amateur, using that word in its best and true sense. When Wallace had been long in the Malay Archipelago, a relative wrote urging him to return, and in his reply he gave the reasons why he could not do so, and said:

> So far from being angry at being called an enthusiast (as you seem to suppose), it is my pride and glory to be worthy to be so called. Who ever did anything good or great who was not an enthusiast?

This was his attitude to the end of his life, and only those who have some measure of the same feeling can understand it. The worldly wisdom of a professional threading his way through the maze of opportunity to one of the prizes of life was wholly foreign to his nature; he was, instead, the "irresponsible enthusiast," keenly anxious to see and know, *loving* nature and man, always wishing to communicate to others some of the pleasure and knowledge he had gained. To some his frequent advocacy of unpopular causes suggested perfect indifference to public opinion, and a total disregard of ordinary prudence. Whether, in this or that matter, we believe him to have been right or wrong, we must admire a man who always had the courage of his convictions; and so far from being indifferent to the feelings and opinions of others, his sympathetic nature and longing for fellowship *caused* him to so zealously expound what he believed would be helpful to other men.

I had of course revelled in "The Malay Archipelago" when a boy, but my first personal relations with Dr. Wallace arose from a letter I wrote him after reading his "Darwinism," then (early in 1890) recently published. The book delighted me, but I found a number of little matters to criticize and discuss, and with the impetuosity of youth, proceeded to write to the author, and also send a letter on some of the points to *Nature*. I have

[1] *Popular Science Monthly*, April, 1903, p. 517.

[2] SCIENCE, March 29, 1912, p. 487.

possibly not yet reached years of discretion, but in the perspective of time I can see with confusion that what I regarded as worthy zeal might well have been characterized by others as confounded impudence. In the face of this, the tolerance and kindness of Dr. Wallace's reply is wholly characteristic:

I am very much obliged to you for your letter containing so many valuable emendations and suggestions on my "Darwinism." They will be very useful to me in preparing another edition. Living in the country with but few books, I have often been unable to obtain the *latest* information, but for the purpose of the argument, the facts of a few years back are often as good as those of to-day—which in their turn will be modified a few years hence. You refer to there being five species of *Aquilegia* in Colorado. But have they not each their station, two seldom occurring together? During a week's botanizing in July in Colorado I only saw two species, *caerulea* and *brevistyla*,—each in their own area. Though the Andrenidae are not usually gaily colored, yet they are not *inconspicuous*. The Chrysididae are, I should think, colored so brilliantly, partly, perhaps, to simulate stinging species, and partly to prevent their being taken for fruits or seeds when rolled up. They are *very* hard, and like many hard beetles, are colored as a *warning of inedibility*. In the Rocky Mountains I think there is a *real scarcity* of Monocotyledons, especially bulbous Liliaceae and Amaryllids, and Orchises. This struck me as being the case. You appear to have so much knowledge of details in so many branches of natural history, and also to have thought so much on many of the more recondite problems, that I shall be much pleased to receive any further remarks or corrections on any other portions of my book.[3]

This letter, written to a very young and quite unknown man in the wilds of Colorado, who had merely communicated a list of more or less trifling criticisms, can only be explained as an instance of Dr. Wallace's eagerness to help and encourage beginners. It did not occur to him to question the propriety of the criticisms, he did not write as a superior to an inferior; he only saw what seemed to him a spark of biological enthusiasm, which should by all means be kindled into flame. Many years later, when I was at his house, he produced with the greatest delight some letters from a young man who had gone to South America and was getting his first glimpses of the tropical forest. What discoveries he might make! What joy he must have on seeing the things described in the letters, such things as Dr. Wallace himself had seen in Brazil so long ago!

[3] Letter, February 10, 1890.

It is comparatively easy for many of us to teach, as we do in schools. No doubt we communicate the "essentials" of our subjects in a fairly competent manner; but would that we had in this country more grand old men with the will and right to bless the succeeding generations as they come.

Some letters of August and September, 1890, refer to a suggestion of mine that a collection of all the recorded facts bearing on evolution should be made.

The proposal you make of a collection of all the recorded facts bearing upon the various problems of *Darwinism* is a very good one. Such a body of facts would be most valuable to naturalists, but I question whether it would pay for its publication. I feel sure my publishers would not agree to "weight" my book with such a mass of additional matter. The only thing, therefore, would be to publish the materials separately, as Darwin did in his "Animals and Plants under Domestication." I hope *you* will do this yourself, as you have evidently a taste for this kind of work. . . . It would, however, be a tremendous task, as it would involve wading through the *whole* literature of natural history for the last twenty years.

In a second letter:

If half a dozen workers could be found to undertake the work of collection I should think the Royal Society would give funds for the publication, as the work would be really a *supplement* to *Darwin's works*, and might be suggested as a *Literary Memorial* to him.

The project was never even on the way to be carried out, owing to various circumstances. I believe it might even now be begun, and that it would be well worth while. For example, we have no good collection of data concerning the relations between specific characters and locality, or on the relative frequency of variation in different species, and a number of other

equally interesting topics. One constantly reads good papers on experimental work, which suffer from the almost total ignorance of the authors concerning the variability and different specific characters of the genera they are dealing with. Not only could much that is valuable be obtained from the literature, but the museums are full of materials which on examination would yield a rich harvest.

Dr. Wallace was greatly impressed with the waste of opportunity in our museums, and not very long ago (Sept. 30, 1909) wrote urging that something should be done.

If you can find time I wish you would write to "Nature"—or if at more length to the "Fortnightly Review"—on a matter of great importance to the philosophical study of biology. Our vast accumulations of plants at Kew, and of insects at the Natural History Museum contain a mass of most valuable geographical and statistical information, quite *lost, useless* and *unknown,* owing to the absurd system of devoting all the time and energies of the staff of curators, etc., to describing new species or small groups here and there, or publishing a few enormous and very costly works like Sharpe's Catalogue of Birds, —which, though intrinsically of great value, are lost to the *mass of workers* owing to *cost* and bulk. Thiselton-Dyar wrote me lately that he "*groans* over the masses of material which lie useless and unknown at Kew." I have urged the last and present Directors of the Natural History Museum to devote their influence to making a simple *Catalogue* of the Museum contents, beginning with the richest and most popular families or sub-orders of insects—Longicorns, Carabidae, Cicindelidae, Lamellicornes, etc., also *Diurnal Lepidoptera.* This catalogue or list, could be made by intelligent *clerks* only, by going over the cabinets or cases, in systematic order, and entering every *specific name* (or sp. nov.) and the *numbers* of the *specimens* in the Museum from *each separate locality.* The clerk or clerks would be under the general supervision of the Curator of the special department. From this manuscript list, a card-catalogue should be set up and stereotyped; there being a card for each species and named variety, and in the case of all widespread species, separate cards for each *Continent* or each considerable *Country.* By printing several *sets* of these cards, a card-catalogue for any subfamily or genus, or for any *geographical region* or *country,* could be made up at a *low price,* and would be invaluable to all private collectors, as telling them at once what *is* in the B. M., and *where from,* while the number of *specimens* would be some guide to the abundance or rarity of the species. I am immensely impressed with the value of the plan of *Card Catalogues,* so much used in America, but I suppose almost unknown here except for Libraries. I have no time or strength to go into this subject properly. . . .

Dr. Wallace had not seen some of the more recently published works, in which such information as he desired had actually been given; but it was and is true that all large museums might do much more for the advancement of biological science, were they to fully utilize the materials at their command. The greatest objection to catalogues compiled in the manner suggested is that the determinations of specimens are frequently unreliable, so that expert revision of the several groups would be necessary in the first place. This means more curators, and therefore more expense. It is however a very wasteful policy, which would wreck any private business, to keep up a large museum at enormous cost, and then cut off the funds at the point of providing an adequate staff to take care of the contents. It is as though a large department store were furnished with everything except enough clerks and salesmen to attend to the customers. Several curators of the U. S. National Museum, to whom I put the question, concurred in the opinion that 5 per cent. added to the total cost of running the museum, put into expert curators, would double the scientific output. In addition to taxonomic workers, museums ought also to have men with broad interests like those of Dr. Wallace, whose business it would be to survey and expound the facts relating to geographical distribution, variation, etc., obtainable from the collections. Thus at the British Museum, Hampson's great work on the moths of the world might be made the basis for many interesting generalizations, which would interest and instruct many who could not obtain or read the original severely taxonomic volumes.

In October, 1890, after I had returned to England, Dr. Wallace wrote that he was about

to prepare a new edition of his "Island Life," and asked me to help secure the information necessary to bring it up to date. I of course gladly agreed to do this, and was supplied with the loose sheets of the first edition, which I carried to the British Museum (Natural History) and the library of the Zoological Society, comparing the chapters with recent literature, and especially consulting different naturalists on their specialties. This not only proved extremely interesting work, but it gave me an introduction to many men I had wished to meet, and especially brought me into constant communication with Dr. Wallace himself. All who were approached courteously gave the best aid in their power; but one chapter, that on the British Islands, proved quite a bone of contention. Dr. Wallace had given lists of animals and plants peculiar to those islands, enumerating all the species and varieties which appeared not to have been recorded from elsewhere. He argued that while no doubt these lists required amendment, yet it was probably true that we possessed a considerable series of endemic forms. Almost without exception, the naturalists of that time expressed great scepticism on this point, while some freely ridiculed the whole idea. Even when furnishing data, they hastened to say that they were probably of no value. Since that time, careful collections have been made by British naturalists on the continent, and much work of various kinds has been undertaken which bears directly upon the question of an endemic element in the British fauna. The result has been to reveal an amount of divergence far in excess of Dr. Wallace's expectations; so much so, that when a few years ago I mentioned to him the recent results of mammalogists, he was not himself prepared to go so far, but said they surely must be splitting hairs.

Early in 1891 I went down to Parkstone and had the great pleasure of meeting Dr. and Mrs. Wallace. For about a week I spent a large part of each day at Dr. Wallace's house and sometimes went for walks with him. I now regret that I kept no notes of the conversations, but I recall that we discussed all the debatable biological and sociological questions of the day. More especially, we talked about the inheritance of acquired characters, and tried to postulate crucial experiments to prove the matter one way or the other. We found it extremely difficult to even imagine an experiment which should be above all possible criticism. There was also much to be said about geographical distribution; and just at that time I had published some remarks on alpine plants in *Nature,* which had called forth adverse criticism, to which I replied while at Dr. Wallace's house. I remember that he encouraged me to go forward in this matter, and not mind if people said I was out of my proper department. He believed in, and of course illustrated by his own conduct, the right of any man to study what he chose, and not be limited in his intellectual activities because his colleagues had labelled him this or that.

After my return home we continued to discuss the inheritance of acquired characters through the mails, especially since at that time Dr. Romanes and others had on foot a project for an experimental station. The following is from a letter of February 7, 1891:

> Your former letter (of Feb. 2) giving Romanes' reply to you, set me going and I immediately wrote to Galton. I enclose his reply, which please return when you are writing next. I then sat down and sketched a series of a dozen sets of experiments to test the two questions of "*heredity of acquired characters*" and the "amount of sterility in the hybrids between closely allied species," —and also a few to test the questions of instinct in nest building, and the "homing" power of dogs, cats, etc. These I am now sending to him and shall then receive his objections to them as affording tests. In the mean time will *you* try and formulate a few experiments which would serve as *crucial tests* of the question of the "heredity of individually acquired characters?" You may hit on some that will meet the objections he will probably make to mine. I do not think there will be any difficulty in getting good observers in paid servants under the supervision of a committee.

On February 13 Dr. Wallace reported the receipt of a long letter from Galton, criticizing some of the suggested experiments. The letter continues:

I suggested some experiments something like yours, and many others. I do not quite agree with you that if acquired characters *are* inherited, they might only be so very rarely. If *inherited* (to be of any use in the theory of evolution, and *that* is the whole question) they ought to be inherited as frequently as *other* characters are inherited, that is, I presume, in about *half* the offspring. If only one in 100 exhibited the character how could you possibly say it was not a normal variation in that individual? *Only* by the very frequent inheritance could you *prove* that there was any inheritance at all! I think you will see this. But it is too elaborate a question to discuss in letters.

On February 18, however, he discussed the matter at greater length:

As you are a student of *variation* I thought you would see my point without explanation. Now I will explain. The following three points I consider to be proved by overwhelming evidence, a summary of which is given in "Darwinism," Chap. III.

1. All increasing or dominant species (and it is from these that new species arise) vary considerably, in all their *parts, organs* and *faculties,* in *every generation.*

2. The amount of this variation is so large that when only 20 to 50 adults are compared it reaches from 10 to 20 per cent. of the mean value of such characters as can be accurately measured.

3. The proportion of individuals which vary considerably is large, reaching to one fourth, or one third of the whole number compared. In other words, the curve of variation is low. . . .

Hence it follows that *whatever character* is increased or diminished in individuals by the effect of the environment, a similar increase or diminution will occur by genetic variation, in each generation, and in certainly 5 or 10 per cent. of the individuals dealt with. Hence your supposition that in the check lots *no* such modification would occur as in those exposed to special conditions is almost an impossible one; and an effect produced on *one* or even on *five* or 10 per cent. by special conditions would be imperceptible, because similar effects would occur through normal variation and often to a much greater amount. Hence I said, that to be *clear* and *decisive* the effect produced by the conditions should be inherited by a *large proportion* of the offspring. You may say that the effects of conditions would be *additional* to the normal effects of variation. True. And if largely inherited they would soon show it, but if as you first supposed only one per cent., that would be entirely swamped by the irregularities of normal variation and inheritance. You must remember too that experiments on a very large scale, and with check experiments on an equally large scale, and all carried on for many years, would require a very large establishment and ample funds not likely to be obtained. Again, the whole *raison d'être* of this enquiry is to decide whether inheritance of acquired characters is of any importance in the origin of species. To be of importance it must rank in generality with variation, otherwise it is entirely superfluous, even if it exists, and variation could do perfectly well without it. Yet again, either there is a fundamental cause of such inheritance or there is not. If there is,—if such inheritance is a *law of nature,* why should it not rank with the inheritance of genetic variations?—which are, I presume, to the extent of about *one half?* If it was only one per cent., it might be a fluke! It would require innumerable experiments to prove it was anything else.

I have given this discussion partly to show that even in those days there was much talk of experimental work, and that the necessity for such work was fully appreciated. Dr. Romanes prepared a statement, which was widely circulated, urging that an experimental station should be established at Oxford or Cambridge,[4] but the funds were not forthcoming. We thought at one time that Oxford would rise to the occasion, but she failed to do so, and it was long after that Cambridge established a chair of genetics.

During the winter I unsuccessfully competed for a position in the Marine Biological Station at Plymouth, and Dr. Wallace kindly interested himself on my behalf. When, in April, I was appointed curator of the museum of the Institute of Jamaica, I had reason to believe that Dr. Wallace had a good deal to do with the matter, since he evidently knew all about it before I told him. He wrote me a charming letter of congratulation:

How you will revel in the land Molluscs, and how you will punish the poor slugs who have hitherto been unregarded by collectors! . . .

[4] "Life and Letters of George John Romanes," second ed. (1896), p. 269.

You will also be able to have a garden, and to be within easy reach of the higher ranges of mountains where hosts of new insects and molluscs remain for *you* to discover! As *you* will treat the poor niggers as "men and brothers," you will have no difficulty in getting any servants you require. . . .

In the following year Dr. Wallace himself thought of visiting Jamaica, and wrote:

Should you see any nice little cot to let in some nice place in the mountains, with plenty of rock and forest near by, let us know, and if we can let our house here for 6 months we *may* possibly come and be renovated by the glorious sun of Jamaica.

In 1893, after I had gone to New Mexico, Dr. Wallace wrote (Sept. 10):

I and wife went to the *Lakes* for a month in July and August,—our first visit there. I was delighted both with the scenery and the glacial phenomena. The mountains are very precipitous, with fine bold outlines and grand precipices, and their summits, at 3,000 feet, quite as grand examples of mountain structure and of denudation as 12,000 or 14,000 feet peaks in the Rockies!

The years passed by, bringing good and ill fortune, and it was not until June, 1904, that I again saw Dr. Wallace. He had moved from Parkstone to Broadstone, where he had built a house in an ideal spot, surrounded by a beautiful garden, and with a small greenhouse annexed. Adjacent to the garden is a sort of miniature forest; "this," he said, "we call the tulgey wood." Every morning he went out early, to see what flowers had opened, and to pick the strawberries. His enthusiasm over the flowers was unbounded; as he himself said, the passage of years had increased instead of dulling his love of natural beauty. We were shown the new hybrid roses, and especially the rockeries, where many beautiful alpines were growing to perfection. One day we all went to Corfe Castle, and Dr. Wallace, in spite of his age, was able to climb the hill on which that ruin stands, and examine every part of it.

In subsequent years my wife and I frequently heard about the garden, sometimes from Dr., sometimes from Mrs. Wallace. They sent us seeds of *Anchusa* and old-fashioned English pinks, which have done very well in our garden at Boulder; we sent *Rosa stellata* and the new red sunflower, both of which were first grown in England by the Wallaces. On June 26, 1911, soon after the publication of "The World of Life," Dr. Wallace wrote:

After the hard labor of my book, and the flood of correspondence about it, chiefly from admirers, —I am taking relaxation in a new *rock and bog garden,* which I have been making, and especially in growing as many as I can of the lovely genus *Primula,* especially the fine new species recently discovered in the mountains of *China* and the *Himalayas.* These I am growing as much as possible from seed, as their beauty is only shown in groups or masses; and I have already got altogether about 40 species (chiefly presents from Kew, Edinburgh, Dublin, etc.). I am very anxious to get *your* very remarkable and fine *Primula Rusbyi* from New Mexico, and in the hope that your university may have a botanical garden, or that some of your botanists may grow it; I shall greatly prize some *seed* gathered and posted in a letter as soon as the capsules are mature. Seed of the Californian *P. suffruticosa* and the Coloradan *P. Parryi* will also be very welcome, as well as of any other American species, if such there are.

P. rusbyi I had never obtained at any time; the allusion to my species was probably due to some recollection of the equally fine *R. ellisiae,* which it was impossible to procure. We did, however, obtain some roots of *P. parryi,* and Dr. Wallace wrote:

I have received a very nice little parcel of *fine roots* of the handsome *Primula Parryi,* which I saw growing luxuriantly near Kelso's cabin, below Gray's Peak, at 11,500 feet, and which I hope to see in flower again next spring, as I have given it a place where it can get its *roots in water,* as it did there, on the margin of the stream.[5]

In the same letter he says:

About two months back was much surprised and pleased to have a visit from Miss Eastwood, my companion in our trip to *Gray's Peak* and *Grizzly Gulch,* in July, 1887, where we saw the American Alpine flora at the snow-line in perfection.

Then again:

Answering letters, reading the papers, mags. and books, with a lot of *novels* fills up my time,

[5] Litt., December 17, 1911.

with attention to my Alpines and seedling Primulas, though I have promised to write an important article, when I feel up to it, *"On the Influence of the Environment on Morals."* We are having the dullest, dampest and dreariest winter I remember, after the hottest summer! . . . The political and foreign situation is now most interesting with us, and I am glad to have lived to see such a hopeful dawn.

The last time I saw Dr. Wallace was immediately after the Darwin Celebration at Cambridge in 1909. I was the first to give him the details concerning it, and vividly remember how interested he was, and how heartily he laughed over some of the funny incidents, which may not as yet be told in print. One of Dr. Wallace's most prominent characteristics was his keen sense of humor, and his enjoyment of a good story. At the banquet at Cambridge those present united in sending him a telegram expressing their sense of his great part in the event they were celebrating, and their regret that he could not be present. This was not delivered until the next morning, and Dr. Wallace was concerned lest it should have been thought that he delayed in sending a reply. I was able to assure him that we knew at the time that it was too late for delivery that day.

As recently as February 3, 1913, Mrs. Wallace wrote:

> Dr. Wallace is very well and busy, writing as hard as ever; he has just passed 90, and feels like 50.

Much later in the year (July 1) we heard from my brother that he was "splendidly well," and not many months after, the sad news appeared in the daily papers. In one of his letters he said that except for the infirmities natural to old age he felt quite as keen as he had ever done in his youth, and thought this a good sign for the persistence of personality after death. This keenness never waned to the end, and who shall say that this eager spirit has not still some place in the realm of being?

T. D. A. COCKERELL

SCIENTIFIC NOTES AND NEWS

THE Nobel prizes in the sciences have been awarded to Professor H. K. Onnes, of the University of Leiden, in physics; to Professor Alfred Werner, of the University of Zurich, in chemistry, and to Professor Charles Richet, of the University of Paris, in medicine.

AT the anniversary meeting of the Royal Society Sir William Crookes was elected president to succeed Sir Archibald Geikie. Other officers were elected and prizes were conferred as already announced in SCIENCE. At the annual dinner the principal toast, "The Royal Society," was proposed by Mr. Page, the American ambassador. The retiring president announced a gift of £5,000 for physical research from Sir James Caird.

DR. J. H. COMSTOCK, for thirty-nine years instructor and professor of entomology at Cornell University, will retire from the active duties of his chair at the close of the present academic year.

DR. HERMAN M. BIGGS has retired as chief medical officer of the Department of Health of the City of New York, having rendered distinguished service to the city in that office.

PROFESSOR CLEVELAND ABBE, the distinguished meteorologist of the U. S. Weather Bureau, celebrated his seventy-fifth birthday on December 4.

THE gold medal of the Apothecaries Society, London, has been awarded to Mr. J. E. Harting, in recognition of his services in preparing and editing the catalogue of the library in Apothecaries' Hall.

THE portrait of Professor Horace Lamb, F.R.S., was presented on November 27 by subscribers to the University of Manchester, where he has filled the chair of mathematics since 1885, and is now senior professor. The portrait of Professor Lamb was painted by his son, Mr. Henry Lamb. The presentation was made by Professor Tout and Professor Rutherford.

DR. CHARLES S. MINOT has been elected an honorary member of the Anatomical Society of Great Britain and Ireland.