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EARLY DAYS OF DARWINISM.

Reading the interesting chapter contributed by Professor Huxley to that work[1] which at the present moment is in almost everybody's hands, my thoughts irresistibly reverted to the time when the now celebrated doctrine of Natural Selection first became known to me, and to the circumstances which on my part led to an immediate acceptance of it—an acceptance that I believe to have been unqualified by any scruples that then occurred to me, and an acceptance that I have never to my recollection regretted, or hesitated, when occasion required, to declare. The story I have to tell may to some appear impudently egotistical; but others may possibly be able to read it without annoyance on that score, or may even find some satisfaction in being thereby reminded of their own frame of mind when the new doctrine, or theory as it was more modestly called in those days, was first presented to their notice. There is an additional reason why, on being asked to furnish this Magazine with some remarks on the late Mr. Darwin's Life and Letters, I should throw them into the personal form just indicated. These volumes have already been the subject of so many reviews that nearly all their "plums" have been picked out by the Jack Horners of criticism, and this notwithstanding that one of the best judges of books is said to have pronounced Mr. Francis Darwin's work to be one "to read rather than to review."

It was just about thirty years ago, namely early in the year 1858, when a friend of mine, whom I had formerly joined in investigating the ornithology of Lapland, agreed with me to go to Iceland and carry on there an inquiry of a very special and limited scope. That friend was a man of an exceedingly philosophical turn of mind, and though he had never been called to the bar or graduated as a physician, he had gone through the legal and medical training which would have qualified him to practise either of those professions. He was cut off by an insidious disease before he had the opportunity of establishing a reputation that would have placed him, I believe, among the first naturalists of the age; and a short memoir[2] very imperfectly sets forth the powers of which he was possessed. Of our inquiry in Iceland I need not say more here than that it was into the supposed recent extinction of a species of bird, and into the causes which had brought about that result.[3] The prosecution of the inquiry, how-

[1] The Life and Letters of Charles Darwin. Edited by his son, Francis Darwin. 3 vols. London, 1887.

[2] Memoir of the late John Wolley (Ibis, 1860, p. 172).

[3] Abstract of Mr. J. Wolley's Researches in Iceland respecting the Gare-fowl or Great Auk (Ibis, 1861, p. 374).

ever, required our stay for nearly two months in a fishing-village, which, notwithstanding the kindness we met, was to neither of us a very agreeable place of residence. The country about us was barren even for Iceland, the scenery tame, and, above all, the weather was generally wretched. Life, both animal and vegetable, was scarce, and we had few or no books.

The upshot of this was that, when not actually engaged on our inquiry, we were thrown almost entirely on our own resources to pass the time; and discussions on all manner of subjects arose, whether in our contracted and uncomfortable quarters, or as we were riding or walking over the very desolate "heaths" and lava-streams of the neighbourhood. Both of us taking a keen interest in Natural History, it was but reasonable that a question, which in those days was always coming up wherever two or more naturalists were gathered together, should be continually recurring. That question was, "What is a species?" and connected therewith was the other question, "How did a species begin?"—the last a question all the more naturally arising from the fact that our particular business was to find out how a species had come to an end, or at least was thought to have done so. Now we were of course fairly well acquainted with what had been published on these subjects. We knew the views that had been expressed by Lamarck, and by the then unknown though not unsuspected author of the Vestiges of Creation. We knew also how strenuously Sir Charles Lyell and our own Professor Sedgwick had argued against them, and had shown them to be hypotheses with little or nothing to rest upon. In addition to that we had read—at least I certainly had—the interesting but inconclusive little work on the Variation of Species, which Mr. Vernon Wollaston (a friend of my friend's) had not long before published; and there was Mr. Darwin's famous Journal of Researches, telling of what seemed to be the extraordinary and completely unaccountable creational activity of which he had found indications in the Galapagos Archipelago, where each island appeared to have its own peculiar species, not found in any of its neighbours. Moreover, in the preceding year, I had visited North America, and while there had been frequently impressed, by hearing of them from the scientific men I met, with the opinions of the late Professor Louis Agassiz, which I had found to be accepted almost everywhere in that country, though, if I am not mistaken, they had few upholders among British botanists or zoologists. Expressed briefly, these opinions were not that each species had had its one Centre of Creation, but that many—perhaps most—species must have been created in several places, at sundry times, and possibly in vast numbers, though not a single act of creation had ever knowingly been witnessed by a human eye. Beyond all this was the uncertainty that beset the definition of a species, which, in the case of Ornithology (the branch of Natural History with which my friend and I more particularly concerned ourselves), had become a thing of almost pressing need, having reference chiefly to the labours of certain continental writers, and especially of the late notorious Dr. C. L. Brehm, who had been at the pains of raising the number of species of European birds from below five hundred, at which most authorities were inclined to reckon it, to one thousand or more, for indeed in each successive publication of his the number had risen higher and higher. It would be useless to indicate the line, even if I could be sure that I remember it, which these frequent discussions took. In a general way I think we used to exhaust ourselves in wonder over some particular cases—the prevalence of blue Foxes in Iceland, the relations between the Red Grouse and the Willow-Grouse, and so forth. Of course we never arrived at anything like a solution of any of these problems, general or special, but we felt very strongly that a solution ought to be found, and that quickly, if the study of Botany and Zoology was to make any great advance.

Arrived in England, I, on my way home, stopped to visit another friend (then rector of Castle Eden, and now a canon of Durham), who had but lately returned from the first of those journeys of exploration whereby so much light has been thrown on the Natural History of the Holy Land. Before making his pilgrimage thither, Canon Tristram, to give him his present title, had passed two winters and springs in Algeria or Tunis, and had diligently collected specimens in those countries. The consequence was that he had amassed such a series as had never before been seen. Among those that most interested me were the so-called Desert-Forms of various animals, especially reptiles, birds, and mammals. In several groups of each of these classes examples were to be seen of individuals from the desert which differed chiefly or only in coloration from those inhabiting the surrounding country, or the oases which the desert itself surrounded; but then this difference was constant. The most striking examples were presented by the birds, and among the birds by the Larks and the Chats—the last being birds allied to our Wheatear. Generally the inhabiters of the desert took a dull drab, but occasionally a warm or sand-coloured hue, while those which did not dwell in the desert wore a suit of much more decided and variegated tint. Strange to say, moreover, there were a few cases in which the desert-form put on a sooty appearance, though not the deep glossy black seen in birds otherwise similar that frequented the fertile districts. In regard to the drab and sand-coloured birds I was at once reminded of what, in a less degree, I had been shown and told the year before at Washington by the late Professor Baird, who pointed out to me the variations exhibited by examples of the same species of several groups of North-American birds, according as they came from woodland, prairie, or elevated plain-country, of which there was a very considerable series in the Museum of the Smithsonian Institution.

Among all these there were indications of a similar general law. The woodland examples were the most highly coloured. Those from the prairies were less deeply tinted; while those from the high plains—districts which, from what I heard, seemed to approach in some degree the condition of a desert such as is found in the Old World (Mauritania or Palestine)—exhibited a fainter coloration. Here then was a sign that like causes produced like effects even at the enormous distance which separated the several localities. The effects were plainly visible to the eye; what were the causes? The only explanation offered to me by Professor Baird, so far as I remembered, was that the chemical action of light, uninterrupted by any kind of shade, produced the effect that was patent. With this explanation, though it hardly seemed satisfactory, one was fain to be content.[1]

Another exceedingly curious series of specimens, which I had seen partly in Washington and partly in the Museum of the Academy of Natural Sciences of Philadelphia, could not be brought under the same ruling. This series began with examples of the common Flicker or golden-winged Woodpecker of Canada and the northern states of the Union.[2] In the southern parts of the United States, and in Mexico, a very similar and clearly allied species of Woodpecker,[3] easily recognised by the brilliant red of some of its parts, had long been known to exist. Now a large series of specimens collected from many localities about the head-waters of the Missouri River showed almost every intermediate

[1] Mr. Gould had already made some remarks to this effect (Proc. Zool. Society, 1855, p. 78). Dr. Gloger's views, long before published, were probably familiar to Professor Baird, but I was wholly ignorant of them.

[2] *Colaptes auratus* of authors.

[3] *Colaptes mexicanus* or *rubricatus*.

stage between the gold-spangled examples of the north and the ruby-tinted of the south. Moreover it was evident that the specimens from almost each valley bore a family likeness, resembling one another more closely than they did either those of any other valley or the normal northern or southern form. The late Mr. Cassin of Philadelphia, a most expert ornithologist, following the theory of Professor Louis Agassiz, was inclined to believe that every one of those valleys had its own peculiar species. Professor Baird, on the contrary, was disposed to hold that these intermediate examples were the result of hybridism between the northern and southern forms, the range of which there inosculated. But neither of these great ornithological authorities felt himself at all at liberty to pass a decided opinion on the point, and of course it was not for me to step in where they feared to tread.

To return however for an instant to the Larks. I ought to say that Mr. Tristram's series showed that, coloration apart, there was much structural variation to be observed; and as regards bill and feet, a complete series of forms could be plainly traced, which, beginning with birds having those features of moderate proportions, ended with those in which they were enormously exaggerated.[1] If one had then thought of looking at the structure of the wings the same thing might have been noticed, but I cannot say that it had then occurred to me to do so.

Not many days after my return home there reached me the part of the Journal of the Linnean Society which bears on its cover the date, 20th August, 1858, and contains the papers by Mr. Darwin and Mr. Wallace which were communicated to that Society at its special meeting on the first of July preceding, by Sir Charles Lyell and Dr. (now Sir Joseph) Hooker. I think I had been away from home the day this publication arrived, and I found it when I came back in the evening. At all events I know that I sat up late that night to read it; and never shall I forget the impression it made upon me. Herein was contained a perfectly simple solution of all the difficulties which had been troubling me for months past. I hardly know whether I at first felt more vexed at the solution not having occurred to me, than pleased that it had been found at all. However, after reading these papers more than once, I went to bed satisfied that a solution had been found. All personal feeling apart, it came to me like the direct revelation of a higher power; and I awoke next morning with the consciousness that there was an end of all the mystery in the simple phrase, "Natural Selection." I am free to confess that in my joy I did not then perceive, and I cannot say when I did begin to perceive, that though my especial puzzles were thus explained, dozens, scores, nay, hundreds of other difficulties lay in the path, which would require an amount of knowledge, to be derived from experiment, observation, and close reasoning, of which I could form no notion, before this key to "the mystery of mysteries" could be said to be perfected; but I was convinced a *vera causa* had been found, and that by its aid one of the greatest secrets of creation was going to be unlocked. I lost no time in drawing the attention of some of my friends, with whom I happened to be at the time in correspondence, to the discovery of Mr. Darwin and Mr. Wallace; and I must acknowledge that I was somewhat disappointed to find that they did not so readily as I had hoped approve of the new theory. In some quarters I failed to attract notice: in others my efforts received only a qualified approval. But I am sure I was not discouraged in consequence; and I never doubted for one moment, then nor since, that here we had one of the grandest

[1] See article "Lark," in Encyclopædia Britannica, ed. 9, vol. xiv.

discoveries of the age—a discovery all the more grand because it was so simple.[1]

First of all, here was an answer, at any rate plausible, to the question, "What is a species?" A species was an assemblage of animals—for, not being a botanist, I may leave plants alone—which were sufficiently alike to be capable of being described in a set formula of words such as is technically called a diagnosis, without reference to their ancestors, to the way in which they had come into existence, or to what sort of appearance their progeny might assume. If this diagnosis were carefully drawn up, it would follow that animals which were so constituted that the diagnosis did not hold good as regards them would have to be considered different species. So far, indeed, this was no great advance on the creed of most of the older naturalists; but it was a real relief to feel that the need of considering other qualities, some of a more or less occult kind or of a kind not easily perceptible, was swept away. A species would be merely that which could be described, or, to use a more learned word, differentiated as a species, and nothing more. Here was an enormous gain to the ordinary working zoologist, who, if he accepted the new theory, need not further trouble himself with recondite ideas of what a species was capable.

Next, to apply the theory to some of the particular cases about which our brains had been so much perplexed of late. The theory explained why the Red Grouse in the British Islands did not in winter assume the white plumage which was invariably at that season put on by its congener, the Willow-Grouse, throughout the whole of its range from Norway and Sweden, across the north of Russia and Siberia, to the coast of the Pacific, and again on the other side of that ocean, from Alaska through Canada to Newfoundland. In all that immense tract of land a Grouse that did not become white in winter would be an object so conspicuous on the six or eight months' snow that it could not possibly maintain its existence against its enemies, any more than a Grouse, if it did turn white, could survive in those parts of the British Islands where the snow does not lie so long on the ground. Again, with the Foxes of Iceland. Owing to the climatic conditions of that island, and chiefly to its discontinuous snow in winter, a blue Fox would not be at the same disadvantage in approaching its prey that one of similar colour would be in Greenland, Lapland, or Siberia, and consequently one could understand why the proportion of Foxes with a coloured pelt was so much larger in Iceland than in those other countries.[2]

Just in the same way the necessity, one may say, of the Desert-Forms of animals, and especially of birds, was at once perceptible. The Lark or Wheatear with the ordinary plumage of its kind would be far too conspicuous an object on the sandy soil, and it could only make good its existence by adopting a coloration suited to its concealment. But more than this, for indeed the purpose of this protective coloration in all these cases had long before been surmised, the way in which it had been brought about was made known by the new theory. The way was by the gradual elimination of those individuals which conformed the least to the conditions in which they found themselves; while so successfully had conformity been carried on by those which now peopled the deserts that it had led, as I afterwards learned, to the almost total disappearance of every bird-of-prey. All this seemed to be clear on the principle of Natural Selection as regards the drab and sand-coloured Desert-Forms. The presence of the black Desert-Forms was not explained to me until some time

[1] I should add that at this time I had no acquaintance personally or by correspondence with either of the discoverers.

[2] Of course I refer to the Arctic Fox (*Canis lagopus*). The ordinary Red Fox does not occur in Iceland, nor, so far as I know, does it anywhere assume a white pelt.

later, when Canon Tristram suggested, with what seems to me great plausibility, that they escape the observation and therefore the attack of their enemies by resembling the dark spots in the inequalities of the surface. In "that fierce light which beats upon" the ground and "blackens every blot," the sooty-hued Lark or Wheatear, crouching close at the sight of the passing Hawk, would to its enemy be indistinguishable from "the shadow of a rock in a weary land."

Then, too, the American Woodpeckers. If the theory were true, there must have been a time when all existing species were more generalized. Might not that time for these Woodpeckers be the present? At any rate these variable intermediate forms, occurring on the confines of the range of the two specialized forms — the golden-winged and the ruby-winged — were just what one might expect to find here and there in the animal kingdom where already differentiated forms meet. This case was the more important, for to me it always seemed to answer an objection so commonly raised in those days: "Where," it used to be said, "can you point out a case of variation in course of progress?" [1]

But it may be said that, after all, such difficulties as I had now found so easily solved were of a kind almost contemptible and beneath the notice of any but a "species-monger." The new theory of Natural Selection might serve perfectly well to explain how one variety or even race could pass into another: it might even serve to establish a Transmutation of Species, on a low view of species; but was it capable of doing more than this? And especially could the process of almost invisible steps, asserted by Mr. Darwin and Mr. Wallace to be thus continuously going on, be attended by such momentous results and end in producing effects so stupendous as those which we now-a-days express by the word Evolution?

That the doubt thus implied was occasionally staggering I do not deny; but I always found that, even if for a time I reeled under it, I could by further reflection recover my balance and resume my position. The consideration which thus enabled me to keep, on the whole, a steady attitude was one furnished by a very small amount of mathematics acquired in earlier days and fortunately yet borne in mind. One has not to go far in the study of algebra before one meets with a theorem in which one finds that certain properties can be proved for certain definite numbers in succession. If an indefinite number be taken, the same property can be proved to exist for the number next to it. Hence mathematicians (those most sceptical of men) conclude that this theorem is universally true. Now, to apply this. The existence of variation, however slight that variation might be, once accepted (and a very moderate amount of experience showed that variation did exist) who could doubt that variation might in certain circumstances go on indefinitely? Whether it would do so or not was another matter; but what naturalist had ever with good reason attempted to set a limit to variation? Until such limitation, or cause for limitation, was shown, I felt I was justified in concluding that variation might go on indefinitely— that variation might extend, as indeed there was some positive evidence of its doing, from coloration to minor points of structure, and from minor to major points. Thus it seemed to me that, if mathematicians were right in admitting the truth of Euler's proof of the Binomial Theorem, I could not be very wrong in accepting the truth of Evolution by means of Natural Selection. When afterwards I came to read Mr. Darwin's Animals and Plants under Domestication, the aptness of my application of the mathematical reasoning seemed to be more

[1] To say nothing of other animals, it is now well known that a similar state of things obtains in many groups of birds, as in the genera *Parus* (Titmouse), *Phasianus* (Pheasant), and *Coracias* (Roller).

and more perfect. In those domesticated animals and plants of which the origin was perfectly certain, we had the definite quantities required for the illustration: in the domesticated animals and plants of which the origin was not so certain, we had the indefinite quantities: in the wild animals and plants the unknown quantities. We could prove by experiment that such and such results followed from any next step with regard to our known quantities, and by experiment could prove that similar results followed from the next step with regard to our indefinite quantities. Were we not justified then in concluding that the like results would follow from our unknown quantities? [1]

A thought not very dissimilar occurred to me when I came to read the latest of his works, The Formation of Vegetable Mould through the Action of Worms, wherein he so admirably exemplified the well-known words:

> "What great events from little causes spring!"

But to return to those earlier days. For more than a year after I had read the Natural-Selection papers in the Linnean Society's Journal, I lived in great comfort of mind. My immediate difficulties had been wholly, I think I may say, solved; and though undoubtedly from time to time others occurred to me, my faith was strong that they would be successfully dissipated on the appearance of Mr. Darwin's promised book, in which the whole subject of Natural Selection was to be fully treated. In due time, November, 1859, this book, the ever-celebrated Origin of Species by Means of Natural Selection, was published. Its contents I devoured and felt happier than ever, for now I began to see that Natural History possessed an interest far beyond that which it had entered into my mind to perceive. The palæontological portion alone, brief as it was, was pregnant with meaning for those who could look backward. The generalized forms—parents of generation after generation successively becoming more and more specialized—here dimly outlined, possessed a fascination that was almost overpowering, the more so since the intricacy of the problems therein involved was, even if not answered, by no means shirked, but boldly faced, and the many proofs of the "imperfection of the Geological Record" were delightful; for to me, ignorant as I was (and am) of Geology, the strongest objection to the theory of "Descent with Modification" had seemed to be that which could be drawn from Palæontology, and it was pleasant to see how the force of this objection was reduced when fairly stated. I should be wrong if I said that it then wholly disappeared. Its disappearance was due to discoveries more recent—that of *Archæopteryx* being the first and most notable, while the affiliation of the birds to the Dinosaurs, and the "crowning mercy" of the discovery of the Horse's pedigree, are events of the last few years only. The Darwinian of the present day, instead of looking upon Geology

[1] I had often wondered that this obvious illustration had not occurred to Mr. Darwin, in none of whose works have I noticed any allusion to it; but the cause of the omission I did not suspect until I read his Autobiography. It was probably due to the fact of his not having made sufficient progress in mathematics to become aware of this simple theorem and its proof. He has told us (vol. i. p. 46): "I attempted mathematics, and even went during the summer of 1828 with a private tutor (a very dull man) to Barmouth, but I got on very slowly. The work was repugnant to me, chiefly from my not being able to see any meaning in the early steps in algebra. This impatience was very foolish, and in after years I have deeply regretted that I did not proceed far enough at least to understand something of the leading principles of mathematics." He goes on to declare that he did not believe he "should ever have succeeded beyond a very low grade." To this belief we may perhaps demur. Under good tuition there seems no reason why he should not have derived as much satisfaction from algebra as he tells us a few pages before (i. p. 33) he did from geometry, and as much delight as when the principle of the vernier was explained to him.

with suspicion, finds in her one of his firmest allies.

I may mention here that the objection from the supposed sterility of hybrids never seemed to me, as I know it did to some of my friends, very strong. I had fortunately been able some time before to establish the fact, from the experience of one of my brothers and myself, that in one case the first offspring of perfectly distinct species, or (according to some systematists) genera, were *inter se* perfectly fertile,[1] and I could not look on this case as exceptional. Moreover I was perfectly aware, from the same experience, of the difficulty occasionally encountered in inducing the tame-bred pure offspring of a species to propagate in confinement; so that I was quite inclined to believe (as I still do believe) that much of the asserted sterility of hybrids is due to some other cause than the mere fact of their being hybrids, and I have long regretted my inability to make further experiments in this direction, or to induce others more favourably situated to make them.

The various reviews of Mr. Darwin's book which I read (nearly all of them, as is well known, unfavourable to his views) produced little or no effect on me, except to lower my estimate of the general run of critics. The ideas expressed by some were fatuous, by others distinctly false. The most violent were those who knew least of the subject; and there was one notable case in which a distinguished man was found who could not even make sense of the "brief" with which he had been furnished by a learned authority who ought to have known better. This was the more remarkable because, a few days before the review appeared n print, not only its substance but much of its phraseology had been heard by me and others to issue from the eloquent lips of the late Bishop Wilberforce in the ever-memorable discussion at the meeting of the British Association at Oxford. It is fortunate for the reputation of some of the speakers that no accurate report of that discussion seems to exist. I do not profess to remember the words used by Professor Huxley in his reply to the taunting but nonsensical question of the bishop, but I well remember its withering effect; and from that moment there was no doubt which side would eventually win its way in public favour—not of course that such a consideration would for a moment weigh with a reasonable man. The scene of the conflict was very impressive—the passive features of the learned gentleman from New York, Dr. Draper, whose "paper" (a long-winded and dull essay, read from a ponderous volume of manuscript resting on a massive desk) was the nominal cause of the discussion, but whose remarks were scarcely referred to by any speaker in the course of it: the comic attempts of the President of the Section, Professor Henslow, to see justice done upon, as well as to, his old pupil and friend: the pathetic earnestness, unsupported of course by a single argument, with which Admiral Fitz-Roy, Darwin's former captain and shipmate, deprecated any share in the flagitious opinions lately promulgated by the whilom naturalist of the Beagle: the ardour which, equally to the surprise as to the delight of the crowded audience, showed that scientific men like the Dr. Hooker and the "young Mr. Lubbock" of those days could be ready in debate. Only one of those who had a place on the platform seemed to be dissatisfied with the part he was playing; and I was not alone in thinking that this might chiefly be because the solution of the mystery which his writings show him to have been long seeking to penetrate had not fallen to him. One of the egregious announcements which he then had the temerity to make or repeat must have caused him regret some months afterwards when its fallacy was exposed by his rival; but of that I need say nothing more

[1] Proceedings of the Zoological Society, 1860, p. 338.

here. On the whole it seemed to be a drawn battle, for both sides stuck to their guns.[1] It was very different two years after when the hostile forces were again arrayed at Cambridge. Then the Anti-Darwinians were smitten along the whole line, and their rout was evident to all.

Thus passed on the time. One by one I found most of my naturalist friends gradually coming to regard Darwinism as a true creed. Some few remain still without the pale. I honour their adherence to the ancient form of faith, for in nearly all cases I know it to be sincere; but I am at a loss to understand their position now that so much new light has been thrown on the most obscure questions by recent discoveries, and especially those which are the result of the much-extended study of Embryology and the shooting up of an almost new branch of science. I have watched the rise and progress of Morphology with the same kind of interest that may be excited in the mind of a lame man who watches a skating-party or a cricket-match, even though he can take no active share in the amusement; for I am too old to go to school again even under the tuition of my most brilliant pupils, and the new biological learning must be begun at the beginning.

Whether this presumptuously personal narrative be worth a recapitulation I hardly know; but it will be seen that my ready and unfaltering adherence to Darwinism arose from my finding it to supply an explanation of all the difficulties which I had encountered in an honest attempt to understand the causes of a limited number of observed facts—facts that, taken alone, were exceedingly trivial, and yet incapable, as I then believed and have ever since found, of explanation on any other hypothesis. Moreover, infinitesimally small as were these observed facts when compared with the majestic grandeur of Nature, they led me, fortunately aided by an equally small portion of mathematical knowledge, to a conception and interpretation of that grandeur which I believe that I otherwise could not have reached. If a moral be wanting it is that hardly any observation of the processes of Nature should be despised, however humble it may seem; but that such observation, to be useful and intelligible, must be accompanied by reflection, which can only be ensured by study of a very different kind.

ALFRED NEWTON.

[1] The fact, as I believe it to be, is not mentioned in Mr. Darwin's Life; but the principal discussion, which took place on Saturday, June 30th, 1860, was adjourned until the following Monday. In the time which intervened some arrangement was, I suppose, made by the leading men of the Association to let drop the matter, which had excited such strong feelings. At all events the discussion was not renewed; a wise termination, no doubt, but disappointing to a good many besides myself.