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The Influence of Darwin upon Entomology.

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The published letters of Charles Darwin show that he had a very poor opinion of systematic work in zoology. His labour in preparing the *Monograph on the Cirripedia* showed him that a large proportion of the descriptions of species are slovenly and superficial, and he thought that this bad work was encouraged by the custom of appending the describer's name to the species. Thus he wrote to Sir Joseph Hooker (then Dr. Hooker), October 6th, 1848:—"I have lately been trying to get up an agitation . . . . against the practice of Naturalists appending for perpetuity the name of the *first* describer to species. I look on this as a direct premium to hasty work, to *naming* instead of *describing*. . . . Botany, I fancy, has not suffered so much as zoology from mere *naming*; the characters, fortunately, are more obscure. . . . Why should naturalists append their own names to new species when Mineralogists and Chemists do not do so to new substances?" (*Life and Letters*, London, 1887, vol. i., pp. 364, 365.) A little later he carried on a correspondence with Hugh Strickland on the same subject. I quote a large part of his concluding letter. He writes on February 4th, 1849, "of the evil done by the 'mihi' attached to specific names; I can see most clearly the *excessive* evil it has caused; in mineralogy I have myself found there is no rage merely to name; a person does not take up the subject without he intends to work it out, as he knows that his *only* claim to merit rests on his work being ably done, and has no relation whatever to *naming*. . . . I think a very wrong spirit runs through all Natural History, as if some merit was due to a man for merely naming and defining a species; I think scarcely any, or none is due; if he works out *minutely* and anatomically any one species, or systematically a whole group, credit is due, but I must think the mere defining a species is nothing, and that no *injustice* is done him if it be over-looked, though a great inconvenience to Natural History is thus caused. I do not think more credit is due to a man for defining a species than to a carpenter for making a box. But I am foolish and rabid against species-mongers, or, rather, against their vanity; it is useful and necessary work which must be done; but they act as if they had actually made the species, and it was their own property" (*loc. cit.*, i., 370, 371). Again writing to Sir Joseph Hooker, on April 9th, 1849, he speaks of "the miserable and degrading passion of mere species-naming" (*loc. cit.*, i., 376). Although these strong opinions and expressions were roused in Darwin by the contemplation of bad systematic work in the Crustacea, the future student of the Insecta will find his task much lightened if they are considered to have a general bearing. Systematic labour is certainly "useful and necessary work which must be done," and there are reasons of expediency why the authorship of a name must be readily available (as Darwin himself felt compelled to admit). But if this "necessary" entomological work is not to lose much of its usefulness due regard must be paid to the warning conveyed in these early letters of our great English naturalist.

A few years later Darwin had done with his systematic monograph, and soon became entirely absorbed in the work which was to culminate in 1859 in the *Origin of Species*. These enquiries led him to believe that too exclusive attention to systematic work injures the reasoning faculties and the powers of generalising. Thus, he wrote to Sir Joseph Hooker, on September 25th, 1853, shortly before the appearance of the last *Cirripede* volumes: "How few generalisers there are among systematists; I really suspect there is something absolutely opposed to each other and hostile in the two frames of mind required for systematising and reasoning on large collections of facts" (*loc. cit.*, ii., 39, 40). Again, he wrote to A. R. Wallace, on December 22nd, 1857: "I am a firm believer that without speculation there is no good and original observation. . . . So few naturalists care for anything beyond the mere description of species" (*loc. cit.*, ii., 108). In a letter to Sir Joseph Hooker on November 21st, 1859, he emphasises the value of generalisation: "It is an old and firm conviction of mine that the naturalists who accumulate facts and make partial generalisations are the *real* benefactors of science. Those who merely accumulate facts I cannot very much respect" (*loc. cit.*, ii., 225). The same ideas are conveyed in a letter to H. W. Bates on December 3rd, 1861, referring to his paper on "Mimicry" in the *Trans. Linn. Soc.*: "I can understand that your reception at the British Museum would damp you; they are a very good set of men, but not the sort to appreciate your work. In fact, I have long thought that *too much* systematic work [and] description somehow blunts the faculties. The general public appreciates a good dose of reasoning, or generalisation, with new and curious remarks on habits, final causes, &c., far more than do the regular naturalists" (*loc. cit.*, ii., 379). He wrote again on November 20th, 1862, after reading the paper on "Mimicry": "Your paper is too good to be largely appreciated by the mob of naturalists without souls, but rely on it that it will have *lasting* value, and I cordially congratulate you on your first great work" (*loc. cit.*, ii., 393). Although the earlier reflections on systematic work came out of his study of the *Cirripedes*, the later were at any rate partially due to his experience of the students of insects. He seems, indeed, to have a somewhat poor opinion of entomological work, perhaps due to his experience with his own collections made on the "Beagle." At any rate, he wrote to Sir Joseph Hooker on September 2nd, 1860: ". . . . If you get to the top of Lebanon . . . . you ought to collect any beetles under stones there; but the Entomologists are such slow coaches. I dare say no result could be made out of them. [They] have never worked the Alpines of Britain" (*loc. cit.*, ii., 337). "[They]" in the last sentence is substituted for words of mock abuse, with no doubt a basis of truth intended to be expressed beneath the jest. Darwin evidently considered that the entomologists, as a whole, would be among the most uncompromising opponents of his views on evolution and natural selection. Thus he wrote to Sir Charles Lyell on March 17th, 1863, arguing that evolution would ultimately prevail: "But this result, I begin to see, will take two or three lifetimes. The entomologists are enough to keep the subject back for half a century" (*loc. cit.*, iii., 17). Such remarks in letters are, of course, not intended to be criticised as deliberate expressions of mature opinion, and there can be little doubt that in this case much

too despondent an attitude is assumed. A study of the *Transactions of the Entomological Society of London* from 1858 onwards will reveal numerous papers by well-known adherents of the new views, such as H. W. Bates, A. R. Wallace, and Sir J. Lubbock. One paper of H. W. Bates on South American butterflies is of peculiar interest. It was written as a letter to Adam White, from Ega, on the Upper Amazon, on May 20th, 1857, over a year before the Darwin-Wallace paper on natural selection was read before the Linnean Society on July 1st, 1858. Mr. Bates' letter is published as the first paper in vol. v of series ii (1858-1861) of the *Transactions*. Speaking of the *Heliconiidae*, he says: "This family I look upon as mostly a modern creation, the species unfixed, very susceptible of change, in conjunction with the least modification of local circumstance; but these theoretical notions I suppose you do not care about." This must be one of the first, if not the very first expression of opinion in favour of evolution published by a London scientific society. Not only did the Entomological Society publish a large number of papers by these great pioneers, but again and again they filled the most important offices. Thus, although Bates was a corresponding member of the Society when he wrote the paper from which I have quoted, he was on the Council in 1864, 1866, 1867, 1872, 1877, was a Vice-President in 1870, 1873, 1876, 1879, 1880, and President in 1868, 1869, and 1878. Wallace was a member of Council in 1866, 1872, Vice-President in 1864, 1869, and President in 1870, 1871. Lubbock was a Vice-President in 1862, 1868, and 1881, and President in 1866, 1867, 1879, 1880. The majority of the senior members of the Society were undoubtedly opposed to the new views, but there was evidently no attempt to boycott those who were known as strong and convinced supporters of them.

Although Darwin had written in such depressing terms of the entomologists in 1863, only four years later he went to the opposite extreme in a letter to Professor Haeckel. Writing on May 21st, 1867, he said: "No body of men were at first so much opposed to my views as the members of the London Entomological Society, but now I am assured that, with the exception of two or three old men, all the members concur with me to a certain extent" (*loc. cit.*, iii., 69). The words "to a certain extent" are, of course, elastic, but, stretching them to the utmost, it must be conceded that this last letter is as optimistic as the former is pessimistic. The members of the Society were fair, and gave a hearing and an important position to an opponent; but he still remained an opponent. A convinced evolutionist did not feel himself in the congenial society of those who agreed with him in principle even if they differed in detail in 1867, nor, for that matter, in 1877. By 1887 an immense improvement had been effected, but Darwin's words could only be used of this date by those of a very sanguine temperament. However, the changes were well under weigh which were to make them entirely appropriate before the end of the next decade.

It is interesting to remember that the three epoch-making papers on mimicry by H. W. Bates, A. R. Wallace, and R. Trimen appeared respectively in 1862, 1866, and 1870, in the *Transactions of the Linnean Society* and not in those of the Entomological Society. This fact is no doubt partly due to the special suitability of the quarto form

of publication for these monographs and partly to the appropriate channel afforded by the Society, which first gave natural selection to the world in 1858, but it probably also indicates that the Entomological Society was not at that date exactly a congenial home for the free discussion and subsequent publication of such hypotheses. I well remember, about the year 1875, when I was an undergraduate, the gravity and, indeed, almost consternation with which Professor Westwood, when he enquired what I was studying, received my reply that I was reading the *Origin of Species*. He told me that it was a book which so young a man ought not to read except under the most careful guidance, and he seemed to think that there was some failure of duty or, at any rate, some want of caution in my being allowed to have the book all!

The great change in relation to these opinions which has gradually come over the Society and over British entomology generally is especially due to the energy, zeal, and ability of a single man. Darwin described Huxley as his "general agent"; in relation to entomology his agent was Raphael Meldola. He became a member of the Society in 1872, was elected on the Council in 1874 and 1875, becoming Secretary in 1876, an office which he retained till 1880. In 1884 he was a Vice-President, and on the Council in 1885. I do not refer to the offices he has held at a later date, because the struggle was then practically over. Throughout the whole of the period included between the above-mentioned dates, and especially during his tenure of the office of Secretary, he was unremitting in his efforts to interest the Society in evolution and natural selection as applied to the problems of insect life and structure. Darwin received many letters from Dr. Fritz Müller containing most interesting and suggestive observations. These were translated by Meldola and brought before the Society. In 1879 he brought before the Society, and published in the *Proceedings* (p. xx), a translation of Fritz Müller's paper, which had only just appeared in *Kosmos* (May, 1879, p. 100), making known his suggestion as to the reason for resemblances between protected species in the hypothesis which has since been known as Müllerian mimicry, or the hypothesis of common warning or synaposematic colours. This new suggestion he sustained even against H. W. Bates, who had himself suggested the older theory of mimicry, and later against W. L. Distant. In 1882 (*Ann. Mag. Nat. Hist.*, Dec.) he extended the suggestion to explain broader resemblances between the species of distasteful groups generally. The outcome of his energy has been that the Müllerian suggestion has produced far more effect here than in its native country, and that the natural centre for controversy for the discussion of such questions shifted from the Linnean to the Entomological Society. In 1882 Meldola published a translation of Weismann's *Studies in the Theory of Descent*, which had also been brought before his notice by Darwin, who, indeed, suggested the English edition. This work has strong personal interest to the present writer inasmuch as it was the cause of his gradual absorption in the problems of insect bionomics, and abandonment of the histological researches on the lower Mammalia upon which he had up to that time been engaged.

When we enquire as to the effect produced by these changes upon the direction and scope of entomological enquiry, the answer is both interesting and in many ways curious and unexpected. The result has been a return of the spirit which animated the older enquirers

before zoological science became locked fast in the paralysing grip of pure systematics. When we read Réaumur or De Geer, the whole point of view is entirely familiar. In describing some of the wonderful means of defence of the larva of *Cerura vinula*, De Geer merely speaks of the "caterpillar of the sallow." Our sympathies are with Lyonnet, who carefully describes the anatomy of "the caterpillar which eats the wood of the willow." These men were *naturalists*, interested in the infinitely difficult and infinitely numerous problems presented by living nature. We find the same spirit in the early Darwinian writers; it shines forth clearly not only in the bionomic monographs, but also illuminates the systematic papers of Bates, Wallace, and Trimen, and now it has become the common heritage of entomology. Systematic work is as "useful and necessary" as ever, indeed even more so, for it becomes a necessity not only as an end in itself, but as the foundation for endless other inquiries. This, then, is the great gain which British entomology owes to Darwin's influence, received first through the early Darwinian writers, and then through the energy and ability of Raphael Meldola—that we are inspired to become naturalists and observers, rather than collectors, that we describe and distinguish species as the means for knowing more about them as living animals, and that endless new lines of observation are opened up to us from the high vantage ground which we occupy as firm believers in the doctrine of evolution and the process of natural selection as its motive cause.