Darwinian evolutionism? Contributions of Alfred Russel Wallace to the theory of evolution

Evolucionismo darwinista? Contribuições de Alfred Russel Wallace à Teoria da Evolução

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Resumo

Teria sido mesmo Charles Darwin o autor da teoria do processo evolutivo? Em suas pesquisas, Darwin discute mais a origem da seleção natural do que propriamente a origem das espécies. Três anos antes da publicação do artigo de Darwin, outro naturalista, Alfred Russel Wallace, publicou um trabalho propondo que todas as espécies vivas descendiam de um único ancestral comum. Foi Wallace o primeiro a notar que cada margem dos rios amazônicos podia ser habitada por espécies diferentes de macacos. Em 1858, Wallace sintetiza a teoria da seleção natural, mas ao invés de publicar a descoberta, remete-a para Darwin que, pouco tempo depois, publica *A Origem das Espécies*. Este artigo visa discutir quais seriam as contribuições de Wallace para as teorias evolutivas.

Palavras-Chave: Teoria da Evolução; Seleção Natural; História da Biologia.

Abstract

Was Charles Darwin, the real author of the theory of the evolutionary process? In fact, Darwin discusses in his works, specifically the origin of natural selection, and not the origin of species. Three years earlier from the publication of Darwin's article, another naturalist, Alfred Russel Wallace, published a paper proposing that all living species descended from a single common ancestor. Wallace was the first to perceive that each margin of the Amazonian rivers could be inhabited by different species of monkeys. In 1858, Wallace summarized the theory of natural selection but, instead of immediately send the discovery to publication, he sent it to Darwin that, shortly after, published *The Origin of Species*. Therefore, this article aims to discuss which would the contributions of Wallace for evolutionary theories.

Keywords: Theory of Evolution; Natural Selection; History of Biology.

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2009 was marked by the commemoration of one of the most important discoveries in the history of the natural sciences. Throughout the world books, scientific and popular articles, reports, documentaries, and congresses discussed, analyzed, and commemorated the 150th anniversary of *The Origin of the Species* by Charles Robert Darwin (1809-1882). For many 2009 was the year of Darwin, because it was also the year when the bicentennial of his birth was celebrated. However, despite much having been written about the life and work of the well-off Darwin family, little importance was given to the obscure history unleashed by a letter he received in 1858. Even the day when this letter reached Darwin is now the subject of discussion.

The author of the letter was Alfred Russel Wallace (1823-1913), a young Welsh naturalist aged 35 who sent Darwin, from Ternate Island (now Pulau Ternate, in the province of North Moluccas, Indonesia), a manuscript entitled *On the tendencies of varieties to depart indefinitely from the original type*. Delirious with fever during a malaria attack, Wallace had dreamed of natural selection. Instead of publishing the discovery, he sent it to Darwin.

In a letter written by Darwin to his friend, the British geologist Charles Lyell (1797-1875), he confessed that the content of this manuscript had left him stunned:

> To Charles Lyell, 18 [June 1858]
>
> My dear Lyell
>
> Some year or so ago you recommended me to read a paper by Wallace in the *Annals* [*and Magazine of Natural History*]; where in 1855, in Vol. 16 of the second series, Wallace had published the paper "On the law which has regulated the introduction of new species," in which the postulated the monophyletism of all living species, coming from a single common ancestor, which had interested you, and, as I was writing to him, I knew this would please him much, so I told him. He has to-day sent me the enclosed, and asked me to forward it to you. It seems to me well worth reading. Your words have come true with a vengeance – that I should be forestalled. You said this, when I explained to you here very briefly my views of 'Natural Selection' depending on the struggle for existence. I never saw a more striking coincidence; if Wallace had my MS. sketch written out in 1842, he could not have made a better short abstract! Even his terms now stand as heads of my chapters.
>
> Please return me the MS., since Wallace does not say he wishes me to publish, but I shall, of course, at once write and offer to send to any journal. So all my originality, whatever it may amount to, will be smashed, though my book, if it

> will ever have any value, will not be deteriorated; as all the labour consists in the application of the theory.
>
> I hope you will approve of Wallace's sketch, that I may tell him what you say.
> My dear Lyell,| yours most truely,| *C. Darwin* (Darwin, 2000, p.274)

The letter and the manuscript *On the tendencies of varieties to depart indefinitely from the original type*, sent by Wallace, triggered a series of events which now allow us question the primacy of Charles Darwin. In this article we intend to present to the reader some important points about the works of Alfred Russel Wallace and Charles Robert Darwin, weaving in this way a comparative reading of the work of both, and assessing their respective contributions to the theory of the evolution of the species.

Wallace in the Amazon

> In the Autumn of 1847, Mr. A. R. Wallace ... proposed to me a joint expedition to the river Amazonas, for the purpose of exploring the Natural History of its banks; the plan being to make for ourselves a collection of objects, dispose of the duplicates in London to pay our expenses, *and gather facts*, *as Mr. Wallace expressed it in one of his letters, "towards solving the problem of the origin of the species," a subject on which we had conversed and corresponded much together*. (emphasis added)

These are the first lines of the preface of Henry Walter Bates' (1825-1892) book, *The naturalist on the river Amazonas* (1863; 1962; 1984). Bates' statement is very important, since it demonstrates that he and Wallace were looking for evidence of how species had originated. The only way that this venture could be carried out was by analyzing the relations between patterns of affinity and distribution among closely related species, based on the study of morphology.

Wallace observed that the Amazon River and its principal tributaries functioned as frontiers for various groups of animals, particularly monkeys. The final paragraph of an article he published in the London Zoological Society declared:

> During my residence in the Amazon district I took every opportunity of determining the limits of species, and I soon found that the Amazon, the Rio Negro and the Madeira formed the limits beyond which certain species never passed.

> The native hunters are perfectly acquainted with this fact, and always cross over the river when they want to procure particular animals, which are found even on the river's bank on one side, but never by any chance on the other. On approaching the sources of the rivers they cease to be a boundary, and most of the species are found on both sides of them. (Wallace, 1852, pp.109-110)

We should note that Wallace had already realized during his first trips the significance of rivers as faunistic frontiers. However, he had not paid great attention to this fact until he began to explore the Rio Negro. After this, the question of physical barriers came to be constant in the works Wallace published. In an article about the distribution of monkeys, Wallace seems to have understood the significance of physical barriers when he asks:

> On this accurate determination of an animal's range many interesting questions depend. Are very closely allied species ever separated by a wide interval of country? What physical features determine the boundaries of species and of genera? Do the isothermal lines ever accurately bound the range of species, or are they altogether independent of them? (Wallace, 1852, p.110)

Wallace probably conceived the great Amazon rivers as insuperable barriers to the dispersion of species, although *not* as a barrier which had divided an ancestral population or biota into two ancestral descendants, which over time had converted into distinct species. Nevertheless, this subject seems to have never been outside of his attention, since in an article about butterflies in the Amazon valley, presented to the London Entomological Society in December 1853, Wallace argued that the diversity of these insects was directly related to physical frontiers (Wallace, 1853a). New species can originate when an ancestral species, originally living in higher lands (such as those which inhabit plateaus and mountains for example) disperses to lower lying lands (more recent fro the geological point of view). The populations of the lower lands will be modified by the influence of new *habitats*, generating varieties and finally new species. The data obtained about the distribution of butterflies seemed to point in this direction.

In his work *A Narrative of Travels on the Amazon and Rio Negro* (1972), Wallace presented a general overview of the geography and geology, vegetation, zoology and anthropology of the Amazon region. One of the most relevant questions in this book can be found in the chapters about the general characteristics of the geological history of the Amazon basin and consequently the distribution patterns of species in highlands which generated species in

low lands in certain groups of animals (Wallace, 1853b, pp.425-427; 1889, pp.294-296).

The Malay Archipelago

Returning from South America, Wallace began to make preparations for another voyage which would compensate him for his Amazonian journey. As the latter had not done so, at least in terms of the material collected, due to the tragedies which occurred on the return. The ship he was returning to Europe caught fire and practically all his collection and his notes were burnt.

His search for unexplored places in the tropics led him to the conclusion that the Malay Archipelago was the most promising place to collect scientific samples. A year and a half after returning from the Amazon, Wallace and Charles Allen, his field assistant for three years, left for the Malay Archipelago. Thanks to the intervention of Sir Roderick Murchison (1792-1871), president of the Royal Geographic Society, transport for Wallace and his assistant in a government ship was arranged. He reached Singapore on 20 April 1854, after a voyage of 45 days. Many years later Wallace wrote in his autobiography (1905) that this trip was the most important event of his life.

He spent eight years in the archipelago, travelling more than 22,000 kilometers; an area equivalent to that of South America. Wallace changed residence at least eighty times, almost once per month. During this period, he collected more than 125,000 specimens, many of which he studied on his return to Great Britain.

Taking advantage of the time he spent as a recluse, due to the climatic conditions or the various infirmities which attacked him, Wallace wrote several of his most important articles, especially those related to the theory of Evolution. The book related to this voyage, entitled *The Malay Archipelago* (1962; 1986), emerged six years after he returned to Britain in 1869. The book is geographically ordered, which creates some confusion in chronological terms. The 31 chapters are written almost with the same narrative that appeared in his field diary. In each section there is a chapter that summarizes the natural history of each group of islands. The author also prepared an introductory chapter with the complete description of the geography of the archipelago and a final chapter summarizing his anthropological observations about the different races living in the archipelago. In the introduction he defined the limits of the distribution of the biota of Borneo, Sumatra and Java (with an

Asiatic affinity) Celebes [now Sulawesi] and other islands (with an Australian affinity). This imaginary line between them is now called the *Wallace Line*.

A little less than a year after arriving in Singapore, Wallace wrote his first contribution to the theory of organic evolution. In his autobiography (Wallace, 1905, pp.354-355) remembered what had occurred 50 years previously:

> Always having been interested in the geographic distribution of animals and plant, as I had studied Swainson and Humboldt, and now having a vivid impression of the fundamental differences between the tropics of the East and West; having also read books such as *Conspectus* by Bonaparte ... and various insect and reptile catalogues in the British Museum (which I knew almost from memory) which gave me a great volume of data about the distribution of animals throughout the world, it occurred to me that this data had never been used as the indicators of the manner in which species had come to exist. Lyell's great work had given me the principal aspects of the succession of species in time, and, combining the two, I thought that I could reach some valuable conclusion. As a result, I put my ideas and data on paper, and the result – which appeared to be of some importance – I sent to *Annals and Magazine of Natural History*, where it appeared the following September. (Wallace, 1855b)

The result which Wallace, in his immense modesty, said, *appeared to be of some importance*, is the paper entitled *On the law which has regulated the introduction of new species*, where he clearly and succinctly outlines the theory of Evolution. This article was so well written, so clear and transparent that Sir Charles Lyell, in a letter written to Wallace on 4 April 1867, declared:

> I was reading, once again, his work published in 1855 in the *Annals*, "On the law which has regulated the introduction of new species," because I want to cite some of his passages, not in reference to his *priority of publication, but simply because there are some points expressed more clearly than in Darwin's own work in relation to the importance of geological and zoological evidence for the geographical distribution and the origin of the species*. (Marchant, 1916, pp.279-280, emphasis added)

An important source of inspiration for Wallace, which deserves highlighting, was the work of the English geologist, ornithologist and taxonomist Hugh Edwin Strickland (1811-1853). Of all of Strickland's ideas (1841), perhaps what most influenced Wallace's thought was the one that suggested the scheme of a tree as a useful analogy for a classification system:

> Again, if we consider that we only have fragments of this vast system, the stem and main branches being represented by extinct species of which we have no knowledge, while a vast mass of limbs and boughs and minute twigs and scattered leaves is what we have to place in order, and determine the true position each originally occupied in relation to the others, the whole difficulty of the true Natural System of classification becomes apparent to us. (Wallace, 1855b, p.187)

The analogy of the ordering of species within a system similar to the branches of a tree was well noted and described by Wallace, although in history this discovery in most often credited to Charles Darwin alone.

Darwin and the Galapagos Archipelago

One of the first works to contain the name of Charles Darwin was the result of his voyage on HMS Beagle. Published in 1839, the book *The narrative of the voyages of H.M. Ships Adventure and Beagle* was a book version of the diaries and notes Darwin had written during the three years and three months on land and 18 months at sea during what was a cartographic survey voyage along the southern coasts of South America. The Beagle, a brig commanded by Captain Robert FitzRoy (1805-1865), a young English navy officer with a strict character, sailed from Plymouth on 27 December 1831, only returning to Great Britain on 20 February 1836.

In his diary Darwin made observations about the Cape Verde islands, Rio de Janeiro, Maldonado, Bahía Blanca, Buenos Aires, Patagonia, Tierra del Fuego, the Strait of Magellan, Chile, Peru, the Galapagos islands, Tahiti, New Zealand, Australia, Mauritius, and finally, Britain. Of all the places visited by Darwin, perhaps the one that became the most famous (thanks to his visit) was the Galapagos Archipelago, now belonging to Ecuador. Many authors have stated that the Galapagos Islands supplied Darwin with a considerable range of information to formulation his theory of evolution.

However, for him the giant tortoises (Galapagos) had been brought to the archipelago by pirates, to be used as a source of food. On the penal colony of Ilha de Santa Maria, he was told that the tortoises had small differences from island to island and that the natives knew which species was from which island just by observing the shell. Darwin did not give this information any particular importance, nor was he concerned with collecting specimens of these chelonians on the various islands which formed the Galapagos Archipelago.

In relation to the Galapagos iguanas, he thought that these (unique) reptiles were one of numerous species found in South America. In relation to the birds, especially the songbirds, Darwin noted that each island was inhabited by a different, unique, species. Nevertheless, when he collected these animals, Darwin did not precisely label the various species of finch which inhabited each of the islands on the Galapagos Archipelago. Curiously, it was the finches and their varied forms of beaks which presented the greatest proof of how a species, based on a common ancestor, could diversify and produce new species (Darwin, 1937, pp.355-380).

In 1839 Darwin published the first edition of his *Journal of Researches into the Geology and Natural History of various Countries visited by H. M. S. Beagle* (1839). Six years later the second edition of this appeared, in which Darwin declared:

> Seeing this gradation and diversity of structure in one small, intimately related group of birds, one might really fancy that from an original paucity of birds in this archipelago, one species had been taken and modified for different ends. (Darwin, 1845, pp.345-356)

Also in this second edition of the *Journal*, when he finished his discussion of the Galapagos Islands, Darwin indicated the aspect of the islands which most intrigued him in relation to the biota:

> The distribution of the tenants of this archipelago would not be nearly so wonderful, if, for instance, one island had a mocking-thrush, and a second island some other quite distinct genus: – if one island had its genus of lizard, and a second island another distinct genus, or none whatever –, or if the different islands were inhabited, not by representative species of the same genera of plants, but by totally different genera … But it is the circumstance, that several of the islands possess their own species of the tortoise, mocking-thrush, finches, and numerous plants, these species having the same general habits, occupying analogous situations, and obviously filling the same place in the natural economy of this archipelago, that strikes me with wonder. It may be suspected that some of these representative species, at least in the case of the tortoise and of some of the birds, *may hereafter prove to be only well-marked races*; but this would be of equally great interest to the philosophical naturalist. (Darwin, 1845, p.362, emphasis added)

On the other hand, in his 1855 work, Wallace clearly argued that his law of the gradual modification of species was the response to the problem of the

fauna and flora of the Galapagos Islands, as we can see in the following passage:

> Such phenomena as are exhibited by the Galapagos Islands, which contain little groups of plants and animals peculiar to themselves, but most nearly allied to those of South America, *have not hitherto received any, even a conjectural explanation*. The Galapagos are a volcanic group of high antiquity, and have probably never been more closely connected with the continent than they are at present. They must have been first peopled, like other newly-formed islands, by the action of winds and currents, and at a period sufficiently remote *to have had the original species die out, and the modified prototypes only remain*. In the same way we can account for the separate islands having each their peculiar species, either on the supposition that the same original emigration peopled the whole of the islands with the same species from which differently modified prototypes were created, or that the islands were successively peopled from each other, but that new species have been created in each on the plan of the pre-existing ones. (Wallace, 1855b, p.188, emphasis added)

Wallace's perceptions about the diversity and relationship among the species found in the Galapagos Islands was much more profound and close to an explanation which was moving towards an understanding of the origin and diversity of the species than that of Darwin, when he discussed the same phenomenon.

Wallace and types of butterflies

The search for evidence to confirm the theory of evolution was successfully completed by Wallace in the Malay Archipelago. Various cases indicated that in a very general manner the disjointed distributions of species is due to the extinction of intermediate forms. For example, in relation to lepidopterons[1] of the *Euploea* genre, Wallace noted, upon his arrival in Singapore, that *The Euploea here occupy the place of the Heliconidae*[2] *of the Amazon and are exactly similar to them in their habits* (Wallace, 1854a, p.4396). Later summarizing the entomology of Singapore and Malacca, he compared various groups of moths from the Orient with those of the Americas, concluding that *The Euploea, despite being very beautiful, cannot compete with the strange Heliconidae, with whom they are intimately related...* (Wallace, 1854b, p.4637).

However, Wallace need more conclusive evidence to prove his theory. This emerged in 1855, when he had the possibility of describing a new species of *Ornithoptera*[3] totally distinct from the others. The example came from the extreme northwest of Borneo, and Wallace called it *Ornithoptera brookiana*:

> This magnificent insect is a most interesting addition to the genus Ornithoptera. The green-marked species have hitherto been found only in North Australia, New Guinea and the Moluccas, and all those yet known so much resemble each other in their style of marking, that most of them have been considered as varieties of the original *Papilio Priamus* of Linnæus. Our new species is therefore remarkable on two accounts; first, as offering a quite new style of colouring in the genus to which it belongs; and, secondly, by extending the range of the green--marked Ornithopteræ to the Northwest extremity of Borneo. As it has not been met with by the Dutch naturalists, who have explored much of the South and Southwest of the island, it is probably confined to the Northwest coast. (Wallace, 1855a, pp.104-105)

For Wallace this finding was evidence in favor of his theory: such a distribution of *Ornithoptera* with green wing colorings was explained by admitting in a hypothetical form that previously it had occupied the rest of Borneo. This form, which became extinct after the formation of *O. brookiana*, was an intermediate form between this species and the forms related to *O. priamus* on the islands in the southeast. Therefore, his law of gradual modifications was valid. Finally in 1855, Wallace decided to publish *On the law which has regulated the introduction of new species* (1885b).

A later case confirmed, once again, his theory. In 1857, after landing in the Aru Islands, he was able to collect three examples (one male and two female) of a new form related to *Ornithoptera priamus*. This new native form from Aru was precisely intermediate between *O. priamus* from Amboina (on the Moluccas Islands) and *O. poseidon* from New Guinea. Effectively, *O. priamus* had four black colorings on its underwing, while the forewing lacked the central green longitudinal vein. *O. poseidon* had two black colorings on its underwing and a central green longitudinal vein on the forewing. The new form discovered by Wallace in Aru had three black colorings on the underwing, and the green vein on the forewing has a length exactly intermediate between *O. poseidon* and *O. priamus*.

According to Wallace, this was clear evidence of the formation process of a species. A predecessor species had completely occupied the area then

occupied by these three forms, which had become differentiated into localized populations due to environmental influence. However, the intermediate form (from the Aru Islands) existed. If the form found in Aru disappeared, *O. priamus* and *O. poseidon* would remain isolated and separated species, as Wallace had proposed for so many varied zoological groups.

Darwin and the classification of barnacles

In his autobiography, published by his son Francis Darwin (1848-1925) in 1887, Charles Darwin stated:

> In October, 1846, I began to work on 'Cirripedia.'[4] When on the coast of Chile, I found a most curious form, which burrowed into the shells of Concholepas, and which differed so much from all other Cirripedes that I had to form a new sub--order for its sole reception. Lately an allied burrowing genus has been found on the shores of Portugal. To understand the structure of my new Cirripede I had to examine and dissect many of the common forms; and this gradually led me on to take up the whole group. (Darwin, 1887, p.80)

As a result of these studies, between 1851 and 1854 Darwin published at least four papers about this class of crustacean, with a little over 1200 pages, and 89 plates with precious drawings (Darwin, 1851a; 1851b; 1854a; 1854b).

A more detailed examination of Darwin's writings about the *Cirripedia* shows that they were based on the classical atemporal Aristotelian-Linnean taxonomy, in other words, we cannot find the most insignificant trace of evolutionism – something very different from what Wallace proposed in his studies of *Ornithoptera*. Perhaps because at this time (before 1858) Darwin had still not understood how species were formed. What better occasion would it have been to demonstrate his theory of the *origin of the species* than a paper about taxonomy?

Wallace and his *On the tendencies of varieties to depart indefinitely from the original type*

Almost at the end of his life, in his book *The Wonderful Century* (1898; 1903), Wallace referred to the discovery of the theory of natural selection,

which had happened in the middle of an attack of fever brought on by Malaria in Ternate:

> That same evening I sketched out the draft of a paper; in the two succeeding evenings I wrote it out, and sent it by the next post to Mr. Darwin. I fully expected it would be as new to him as it was to myself, because he had informed me by letter that he was engaged on a work intended to show in what way species and varieties differ from each other, adding, "my work will not fix or settle anything." I was therefore surprised to find that he had really arrived at the very same theory as mine long before (in 1844). (Wallace, 1898, p.140)

Darwin really had found the theory of natural selection; he had not, however, understood the *origin of the species*. In a letter dated 1 May 1857, Darwin told Wallace that for almost 20 years he had worked on how species and varieties differed from each other, insisting on the impossibility of explaining his theory in a simple letter. Despite this in September of the same year, Darwin had sent (it is not known why) a letter to the US botanist Asa Gray, continuing the fundamental part of his theory of natural selection, advising him not to spread this information, as someone like the British Robert Chambers (1802-1871) could easily hear it and develop it. A question emerges here: why send this information to Asa Gray, who considered the letter as something very hypothetical, instead of to Wallace, who would have understood its content better? Or was the person Darwin feared, this *someone like Chambers,* actually Wallace?

Many years later, Wallace discovered the impact his letter and manuscript had had on Darwin. In a letter to Francis Darwin, dated 1887, Wallace said: "It was not well known that your father has been so anguished – or better disturbed – by my sending him my essay when I was in Ternate...".

When he received Wallace's manuscript, Darwin notified his friends, Charles Lyell and Joseph Dalton Hooker (1817-1811). They were charged with presenting these contributions to the members of the Linnean Society of London and deciding the order when they would be presented. These were: a note by Darwin, supposedly written in 1839 and copied afterwards in 1844; a fragment of a letter which Darwin wrote to Asa Gray in September 1857; and the work of Wallace *On the tendencies of varieties to depart indefinitely from the original type* (Darwin; Wallace, 1858), written in February 1858, in Ternate, in the Moluccas Islands. Wallace's essay remained at the end. Darwin inserted a note clarifying that the essay was not written to be published and therefore

was not written carefully (which can be noted in the reading). Nevertheless, as Beddal (1968) pointed out, the content of this note was not totally correct, since Darwin had a bound copy of the same essay, and had given instructions to his wife that it be published in the event of his premature death.

In relation to Darwin's contribution to the 'joint publication,' Brooks (1984) clarifies that Darwin's sketch, dated 1844, was actually written in 1842 (Darwin, 1859, p.1) and not five years previously, as implied by the letter of Lyell and Hooker to the secretary of the Linnean Society. In the draft, Darwin made no mention of the principle of *divergence*. The latter, in other words, the cause the of the *origin of species*, is, however, mentioned in Darwin's second contribution, in the extract of the letter to Asa Gray in 1857. Nevertheless, Dupree clarifies that in the extract published in the *Journal of the Linnean Society,* "the copy sent to Gray has handwriting which is not Darwin's, although it is corrected by him... it varies in detail with the version published by the Linnean Society" (Dupree, 1968, p.459). According to Dupree, the copy of the letter to Asa Gray essentially contains the same arguments which Lyell and Hooker knew in 1844 and 1856, with the addition of the *principle of divergence*. Dupree infers that his principle was only a vague statement, which Darwin later expanded in the first chapters of *Origin of the Species* (Dupree, 1968).

Unfortunately, there is known published copy, as far as we know, of the original version received by Asa Gray. Strangely, other very important documents are also still lost: Wallace's original manuscript, written in Ternate; Wallace's letter to Darwin which accompanied this manuscript, as well as the content of these two documents; the letters between Darwin, Hooker and Lyell in June 1858. Also missing are the letters of Darwin to Asa Gray about the theme of the letters which Darwin and Hooker sent to Wallace after these events. The losses and coincidences are many...

Nevertheless, after the publication of Darwin's abstracts and Wallace's essay in the *Journal of the Linnean Society* in 1858, Darwin definitely abandoned the writing of his *big book on species*, entitled *Natural Selection*, and the same year began to feverishly write a new book, a *summary* of his *big book on species*, published in 1859, under the title *Origin of Species*.

More *unexplained mysteries*

In his *Diary,* published by Sir Gavin de Beer (1959), Darwin said that on 31 March 1857, he had finished Chapter 6 about *Natural Selection*. It was this chapter he sent to Asa Gray in September 1857.

However, a year later in the same diary the following entry occurs: "April 14th Discussion on large general & small & on Divergence & correcting Ch. 6 (Moor Park) finished June 12th & Bee Cells" (De Beer, 1959, p.14). Moor Park was a hydrotherapic station where Darwin stayed from 20 April to 4 May 1858. This may indicate that Darwin wrote (or rewrote) *Divergence*, as well as the corrections of Chapter 6 of his *Natural Selection*, between 6 May and 12 June 1858. The original manuscript of *Natural Selection*, the *Big Book on Species* by Darwin, was presumed to have disappeared until the Second World War when in 1942 its discovery was announced in the magazine *Nature*. However, that was not the proper occasion for studies of that type. Finally, Stauffer (1959) made known the contents of the unpublished manuscript. An examination of Brooks (1984) of a copy of the manuscript in Stauffer's power showed that each page of the manuscript was numbered in a consecutive form; in the need for some correction or insertion in the manuscript, the corresponding page has an asterisk. For example, the pages added after page 10 were numbered as follows: 10*, 10a, 10b, etc. If more than 27 pages were inserted, the numbering followed the pattern: 10aa, 10bb, etc. Brooks' discovery was surprising: after page 26 forty-one pages were inserted, until page 26nn. At the bottom of page 26, there is a heading entitled *Extinction*, and at the top of page 26b another header, *Principle of Divergence*. These pages were thus what Darwin wrote between 6 May and 12 June 1858.

On 8 June 1858, Darwin wrote to Hooker saying that finally he had understood how species diverged in nature. This letter was thus apparently written four days before he ended writing his new 41 page version of the *principle of divergence*.

The letter in which Darwin announced to Charles Lyell to arrival of the correspondence and manuscript of Wallace is simply dated "Down, 18th." It is possible that later the son of Darwin, Francis, editor of his letters, had added between inverted commas "June 1858". Thus, it seems that Darwin had finished writing his new version of the *principle of divergence* six days before the arrival of Wallace's manuscript about the same subject. A notable coincidence.

All these coincidences, including the presentation of data in the *joint contribution* of Wallace and Darwin, meant that various authors have investigated in greater detail this extraordinary case of *convergence*. Just citing some, we can mention Beddal (1968; 1969; 1972) and Brackman (1980). However, the most detailed and documented study of this strange situation was carried out by Brooks (1984).

It is known that Wallace sent, on the same day that he sent the letter with the manuscript from Ternate to Darwin, also from Ternate, a letter to Frederick Bates (1777-1825), the younger brother of Henry Walter Bates (1825-1892), who lived in Leicester. This letter, addressed to Frederick Bates, is dated 2 March 1858. The letter is in the possession of the Wallace family. McKinney (1972, pp.140-141) reproduced it in his book, where we can see the postmark "Via Southampton," with the date of 21 April in Singapore and 3 June in London. The problem seems to reside in how many days a letter took to travel from Ternate to London. This was what Brooks undertook to investigate, carrying out extensive research in the Post Office Museum and in the Postal Archives in the Hague, as well as in the archives of the P&O Steamship Navigation Company in London. Armed with the information obtained in these documentation centers, Brooks found that there were only two possibilities.

If the letter with the manuscript was posted in Ternate on 9 March 1858, it would have reached Singapore on 21 April and, passing through several ports, would have arrived in Malta, on 23 May. If it had been marked "Via Southampton," it would have reached London on the same day that the letter of Frederick Bates reached Leicester, in other words, 3 June. If it had been marked "Via Marseille" and "Overland," it would have reached London on 28 May.

The second possibility is that the letter addressed to Darwin, written in February, went in a post bag *prior* to 9 March, which would have been on 23 February. In this case, it reached Singapore on 7 April and Malta on 10 May. If it had continued "Via Southampton," it would have arrived in London on 20 May; if it had gone "Overland, Via Marseille," if would have arrived in the English capital on 14 May. This was a Friday. According to Brooks, the letter reached Darwin either on Monday 17 or Tuesday 18, May 1858.

Brooks believed that, with Wallace's manuscript in his hands, Darwin reread Wallace's 1855 paper and, finally, had an 'epiphany' about the *principle of divergence*. He wrote the letter to Lyell announcing that the manuscript had arrived on *18 May*, however, *he did not send it*. He thus had at least 25 days to rewrite the 41 new pages about the *principle of divergence*, which he announced to Hooker on 12 June. Finally, it can be interpreted that he sent the letter to Lyell in *June*, and perhaps this is why Francis Darwin, his son and editor of his letters, wrote in hand after "Down, 18 th", "June 1858".

The other possibility is that the letter reached Darwin on 28 May (or 29). This would have given Darwin *two weeks* to write the 41 pages of the *principle*

of divergence. Brooks says that we have to recognize that despair made the pen move quickly. In this case the letter written by Darwin to Lyell would really have been dated 18 June.

Nevertheless, Darwin had between two and four weeks to write a new chapter about the *principle of divergence*. He published an idea distinct from that of Wallace, and made the same error as Maupertuis (1698-1759) – since without geographic isolation (Darwin only admitted competition between varieties formed from a predecessor species), how could the formation of morphospecies be explained? How is it possible that *evolutionary innovations* that fortuitously emerge are not disseminated amongst all populations? Why, in place of killing them, did the varieties that emerged from a predecessor species not copulate before? Is it that for a Victorian like Darwin, incest was more serious that fratricide in the case of varieties originating from a common *mother species*?

Separating what Hooker and Lyell joined together

Finally, by analyzing separately in detail the contributions of Wallace and Darwin in the joint publication orchestrated by Hooker and Lyell, we can see that Wallace's is considerably better written than Darwin's summaries. Wallace, for example, started by saying that the varieties produced in the state of domestication are very distinct from those which occur in a natural state – the total opposite of Darwin's point of view, who believed that the process of artificial selection, caused by domestication, were a faithful analogy of natural selection occurring in nature. According to Wallace, when abandoned domesticated varieties have a tendency to revert to the *normal form of their predecessor species*. In this way, Wallace firmly rejected the validity of this analogy. Darwin, like so many other naturalists, had begun with a consideration of domestic animals and an analogy with the natural condition. However, he made an analogy of the known results of domestic forms with possible results of a more powerful selective force that what he proposed acted in nature.

Wallace also stated that "The life of wild animals is a struggle for existence" (Darwin; Wallace, 1858, p.54), in other words, they all used their energies and faculties to the maximum to preserve their own existence and their offspring. Depending on how successful a species was, its members would be more or less numerous: "The general proportion that there should be in certain animal groups is easily visible. Large animals cannot be as abundant as small ones; carnivores have to be less numerous that herbivores" (ibidem). Despite

the fecundity which allowed each species to widely expand their numbers, it is evident that the animal population of the globe had to be stationary, or perhaps, due to the influence of man, decreasing (ibidem). Of course, fluctuations were evident everywhere. After a simple calculation, based on the fecundity of birds, Wallace concluded that "it is therefore evident that every years an immense number of beings should perish – as much in fact as are born" (Darwin; Wallace, 1858, p.55), this for the population to remain in balance.

Afterwards, Wallace wonders:

> The numbers that die annually must be immense; and as the individual existence of each animal depends upon itself, those that die must be the weakest – the very young, the aged, and the diseased, – while those that prolong their existence can only be the most perfect in health and vigour – those who are best able to obtain food regularly, and avoid their numerous enemies. It is, as we commenced by remarking, "a struggle for existence," in which the weakest and least perfectly organized must always succumb. (Darwin; Wallace, 1858, pp.56-57)

Up to this point Darwin and Wallace's arguments are notably similar. However, the following logical step taken by Wallace clearly has no correspondence in the formulation previewed by Darwin (1844), nor in the – very different – concept expressed in his 1857 letter to Asa Gray (1810-1888).

According to Wallace, the majority of variations of a typical form of a species, or perhaps all, must have some defined effect – despite being small – about the habits and capacities of individuals. Equally a change in color can affect their security, as it can leave them more or less indistinguishable (Darwin; Wallace, 1858, p.58). Furthermore, it is evident that the majority of changes will affect, favorably or unfavorably, the faculties slightly expanded to prolong their existence. This variety will inevitably with time acquire numerical superiority (ibidem). Thus, in general,

> All varieties will therefore fall into two classes – those which under the same conditions would never reach the population of the parent species, and those which would in time obtain and keep a numerical superiority. Now, let some alteration of physical conditions occur in the district... it is evident that, of all the individuals composing the species, those forming the least numerous and most feebly organized variety would suffer first, and, were the pressure severe, must soon become extinct. (ibidem)

If this extreme environmental crisis is continuous, the individuals of the parent species will also die, thereby reducing the typical population of the *species* to the point of extinction: "The superior variety will be the only one left, and with the return of favorable circumstances will rapidly increase in numbers and occupy the place of the extinct varieties" (ibidem).

Darwin had not explained in his 1844 paper, how new species emerged. Equally about the formation of varieties in nature, he had simply said "Who can state that it [natural selection] does not have any effect?"

How Lyell and Hooker could assert in the introduction of the joint work Darwin and Wallace had independently arrived at the *same* "ingenious theory to explain the appearance and perpetuation of varieties and forms in our planet," when this was not correct?

In another part of the letter to Gray, Darwin presented his idea more clearly: "Each new variety or species, when formed, will generally take the place of, and thus exterminate, its less adapted parent group" (Darwin; Wallace, 1858, pp.51-52). The difference between the ideas of the two *indefatigable naturalists* is clear. Wallace postulated that the superior variety would expand to take the place of the parent species *after* this population succumbed to some environmental crisis. Darwin postulated *direct* competition between the superior variety and the population of the parent *species*, ending with the elimination of the latter.

Wallace's theory, though the *principle of divergence,* had the additional advantage of explaining other phenomenon. This term, used around 1829, signified *deviation from a continuous or standard norm*, while the fossil register left clear the divergence of later representative forms when compared to the previous ones, in each group of organisms.

On the other hand, Darwin's *principle of divergence* did not offer any explanation for *continuous separation*. He does not explain either the formation nor the lines of *morpospecies*. Darwin only stated that "the variety of offspring of each species will try to (and only a few will manage) to occupy as many and as different places in the economy of nature as possible." This conjecture contradicted Wallace's experience, who knew after a decade studying animals in the field that despite the varieties occupying different localities from the parent *species*, all the differences which they presented with small and in both the variety and *typical species* occupied the same *place in the economy of nature*. Darwin's conjecture could only be seen by Wallace as the speculation of someone who knew little about the variation which occurred in nature (Brooks, 1984, p.211).

Finally, if we take into account the differences existing between the work of Darwin and Wallace, which can easily be noted in their writings, Wallace could have asked if Lyell and Hooker understood what the theory of each meant. If they had, they certainly would not have stated in the joint publication that the theories of Wallace and Darwin *were the same* (Brooks, 1984, pp.211-212).

More than 150 years after the joint publication of Darwin and Wallace, the latter, despite having intuited *natural selection* and the *origin of the species* more quickly and in greater detail than Darwin, is still relegated to a secondary level in the history of science. Much of what we see nowadays in relation to the stardom of Darwin and the anonymity of Wallace can be explained by the academic industry which turns around the figure of Darwin. Nevertheless, part of the response to this phenomenon can also be found in the somewhat careless manner in which students, teachers, and researchers have studies the history of evolutionism.

REFERENCES

BATES, Henry Walter. *El naturalista por el Amazonas*. Barcelona: Ed. Laertes, 1984.

_____. *The naturalist on the river Amazonas*. Berkeley & Los Angeles: University of California Press, 1962.

_____. *The naturalist on the river Amazonas*. London: s.n., 1863.

BEDDAL, B. *Wallace and Bates in the tropics*: An introduction to the theory of natural selection. London: Collier-Macmillan, 1969.

_____. Wallace, Darwin and Edward Blyth: Further notes on the development of evolution theory. *Journal of the History of Biology*, Vol. 5, no. 1, pp.153-158, 1972.

_____. Wallace, Darwin, and the theory of natural selection: A study in the development of ideas and attitudes. *Journal of the History of Biology*, Vol. 1, no. 2, pp.261-323, 1968.

BRACKMAN, A. C. *A Delicate Arrangement*: The Strange Case of Charles Darwin and Alfred Russel Wallace. New York: Times Books, 1980.

BROOKS, J. L. *Just before the origin*: Alfred Russel Wallace's theory of evolution. New York: Columbia University Press, 1984.

DARWIN, C. A monograph of the Fossil Balanidae and Verrucidae of Great Britain. *Palaeontographical Society*, London, 1854a.

_____. *A monograph of the Fossil Lepadidae, or Pedunculated Cirripedes, of Great Britain*. London: Palaeontographical Society, 1851a.

DARWIN, C. *A monograph of the Sub-Class Cirripedia, with figures of all the species. The Lepadidae; Or, Pedunculated Cirripedes*. London: The Ray Society, 1851b.

_____. *A monograph of the Sub-Class Cirripedia, with Figures of all the Species. The Balanidae, (or sessile Cirripedes); The Verrucidae, etc., etc., etc*. London: The Ray Society, 1854b.

_____. *As cartas de Charles Darwin*: Uma seleta, 1825-1859. São Paulo: Ed. Unesp, 2000.

_____. *El Origen de las Especies*. México, D.F.: Editorial Porrúa, 1992.

_____. *Journal of Researches into the Natural History and Geology of the Countries visited during the Voyage of H. M. S. Beagle*. London: Murray, 1845.

_____. *On the Origin of Species by means of Natural Selection, or the preservation of favoured races in the struggle for life*. London: John Murray, 1859.

_____. *On the Origin of Species by means of Natural Selection*. [Fac-simile edition (1859) with introduction by E. Mayr]. Cambridge, Mass: Harvard University Press, 1964.

_____. *Viagem de um naturalista ao redor do mundo*. Rio de Janeiro: Cia. do Brasil Ed., 1937.

DARWIN, C.; WALLACE, A. R. On the tendency of species to form varieties; and the perpetuation of varieties and species by natural selection. *Journal of the Proceedings of the Linnean Society, Zoology*, Vol. 3, pp.53-62, 1858.

DARWIN, F. (Ed.) *Life and letters of Charles Darwin, including an autobiographical chapter*. 2v. New York: Appleton-Century-Crofts, 1887.

DE BEER, G. Darwin's Journal. *Bulletin of the British Museum (Natural History)*, Historical Series, Vol. 2, no. 1, pp.1-21, 1959.

DUPREE, A. H. *Asa Gray, 1810-1888*. New York: Atheneum, 1968.

MARCHANT, J. (Ed.) *Alfred Russel Wallace*: Letters and Reminiscences. 2v. New York; London: Harper & Brothers, 1916.

McKINNEY, H. L. *Wallace and natural selection*. New Haven; London: Yale University Press, 1972.

STAUFFER, R. C. On Origin of Species: An unpublished version. *Science*, Vol. 130, no. 3387, pp.1449-1452, 1959.

STRICKLAND, H. E. On the true method of discovering the natural system in zoology and botany. *Annals and Magazine of Natural History*, Vol. 6, pp.184-194, 1841.

WALLACE, A. R. *A Narrative of Travels on the Amazon and Rio Negro*. New York: Dover, 1972.

_____. *A Narrative of Travels on the Amazon and Rio Negro*: with an Account of the Native Tribes, and Observations on the Climate, Geology, and Natural History of the Amazon. 1st. ed. London: Reeve & Co., 1853b.

WALLACE, A. R. *A Narrative of Travels on the Amazon and Rio Negro*: with an Account of the Native Tribes, and Observations on the Climate, Geology, and Natural History of the Amazon. 2nd. ed. London; New York; Melbourne: Ward Lock & Co., 1889.

_____. Description of a new species of *Ornithoptera*. *Transactions of the Entomological Society of London*, s.n., Vol. 3, pp.104-105, 1855a.

_____. Letter, May 9, 1854, from Singapore. *Zoologist*, Vol. 12, pp.4395-4396, 1854a.

_____. *My Life*. 2v. London: Chapman & Hall, 1905.

_____. On the monkeys of the Amazon. *Proceedings of the Zoological Society of London*, Vol. 20, pp.107-110, 1852.

_____. On the Habits of the butterflies of the Amazon Valley. *The Transactions of the Entomological Society of London*, Vol. 2, pp.241-244, 1853a.

_____. On the law which has regulated the introduction of new species. *Annals and Magazine of Natural History*, Vol. 16, second series, pp.184-196, 1855b.

_____. The Entomology of Malacca. *Zoologist*, Vol. 13, pp.4636-4637, 1854b.

_____. *The Malay Archipelago*. New York: Dover, 1962.

_____. *The Wonderful Century*: The age of new ideas in science and invention. New York: Dood & Mead, 1898.

_____. *The Wonderful Century*: The age of new ideas in science and invention. London: G. Allen & Unwin Ltd., 1903.

_____. *Viaje al Archipiélago Malayo*. Barcelona: Ed. Laertes, 1986.

NOTES

[1] Butterflies and moths.

[2] Subfamily Heliconiinae (family Nymphalidae), with dark wings with orange or yellow spots.

[3] Butterflies from the Papilionidae family, brightly colored and relatively large, much estimated by collectors.

[4] Class of marine crustaceans, which include the barnacles amongst others, with ger. sessile adults, fixed to rocks, shells, corals, and freely born larvae; barnacles. (Some species are eaten by whales, turtles, and fish, and many others are parasites.)

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