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Invention of a Historical Puzzle: Darwin's Procrastination

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Abstract

Much ink has been spilled to explain why Darwin avoided publishing his views for many years. Although a general consensus was never achieved on any one reason, not much doubt has been raised as to the existence of such a delay. In this article I argue that there was no delay. Darwin published his views as soon as he developed a defensible theory. I argue that the appearance of a delay emerged as a consequence of reading Darwin out of context. Once we distinguish what would constitute a satisfactory account of transmutation for Darwin from what it would be for us, there will be no plausible case to argue that Darwin delayed publication.

Keywords: Charles Darwin, Darwin's Delay, Principle of Divergence, Natural Selection

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Tarihsel Bir Problemin İcadı: Darwin'in Siftinmesi

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Öz

Darwin'in görüşlerini neden yıllarca yayınlamadığını açıklamaya yönelik çok fazla çalışma vardır. Belirli bir neden konusunda uzlaşım sağlanamamış olsa da, böyle bir gecikmenin olduğuna ilişkin pek kuşku ifade edilmemiştir. Bu yazıda böyle bir gecikmenin olmadığını öne sürüyorum. Darwin görüşlerini savunulabilir bir kuram elde eder etmez yayınlamıştır. Gecikme olduğu izleniminin Darwin'i bağlamı dışında okumaktan kaynaklandığını iddia ediyorum. Darwin için neyin tatmin edici bir kuram olduğunu, bizim için neyin yeterli bir kuram olacağından ayırdığımızda Darwin'in çalışmasını yayınlamayı geciktirdiğini öne sürmek için makul bir argüman kalmayacaktır.

Anahtar sözcükler: Charles Darwin, Darwin'in Bekleyişi, Farklılaşma İlkesi, Doğal Seçilim

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Invention of a Historical Puzzle: Darwin's Procrastination

Cem Kamözüt

1. Introduction

There is a widespread belief that Darwin avoided publishing his theory for about two decades. Such a long delay attracted a lot of attention from Darwin scholars and several explanations have been suggested for the delay. Two of these are very popular. One is that Darwin needed more evidence and spent this period collecting them. The second one is that he was scared of reactions, for his theory was contrary to religion. In this work I aim to show that neither reason is plausible. The first misrepresents Darwin's method and the second misrepresents the intellectual atmosphere of nineteenth century England. Rather I will argue that there was no delay, and this historical puzzle emerged after the development of modern evolutionary synthesis—which created a gap between what we call “Darwin's theory” and what Darwin and his contemporaries thought it was.

In the first part of this paper, I will discuss the reasons that led many to think that Darwin had his theory much earlier than his publication of it. I will argue that only one may give us some pause, but others are clearly implausible. In the second part I will discuss the explanations provided to understand the delay; again, I will argue that none of them are plausible. In the third part I will argue that this myth of delay emerged due to an anachronistic reading of Darwin. I will also support my view by arguing that if my approach is accepted it will make sense of several other minor puzzles about Darwin and Wallace, such as “Why the 1858 session at the Linnean Society did not receive any reactions?” or “Why Wallace never made priority claims?”

2. Evidence that Supposedly Indicate Darwin Avoided Publication

Darwin returned from his voyage around the world in 1836. But he published his book where he explained his theory of evolution in 1859. It is generally believed that he discovered his theory much earlier than this publication date—if not during his voyage—and intentionally withheld publication. Moreover, even when he published his theory in 1859, his book *Origin* is considered to lack many of his important ideas reinforcing the belief that he was unwilling to uncover his views. Finally, eight years spent on the taxonomy of an obscure group of creatures before the publication of the *Origin* is taken to indicate his reluctance. I will now evaluate the strength of each of these evidences and claim that they are far from demonstrating a gap between discovery and publication.

a. Date of Discovery

The myth that Darwin discovered his theory at Galapagos during his voyage is convincingly demolished by Frank Sulloway (1982). Yet there is still a general consensus that Darwin discovered his theory much earlier than his publication of it. According to most of the popular literature on Darwin, there is approximately a 20-year gap between discovery and publication. This is based on an often-quoted statement from the autobiography of Darwin:

In October 1838, that is, fifteen months after I had begun my systematic enquiry, I happened to read for amusement Malthus on Population, and being well prepared to appreciate the struggle for existence which everywhere goes on from long-continued observation of the habits of animals and plants, it at once struck me that under these circumstances favourable variations would tend to be preserved, and unfavourable ones to be destroyed. The result of this would be the formation of new species (Barlow 1985, 120).

What finally made him publish was apparently a letter from Wallace who also discovered *exactly the same theory in exactly the same way*—after reading Malthus! This story of discovery is so simplified that if true one wonders why Malthus himself or any of his other readers failed to discover natural selection. Not only there are reasons to suspect that Darwin's reconstruction of the events almost four decades later may be unreliable but also the above quote is followed by clear statements that indicate Darwin believed that there was still a lot of work to do before anything could be published:

Here, then, I had at last got a theory by which to work with...

But at that time I overlooked one problem of great importance; and it is astonishing to me, except on the principle of Columbus and his egg, how I could have overlooked it and its solution (Barlow 1985, 120).

The “solution” mentioned here is his “Principle of Divergence” which he will develop much later. Hence for Darwin, although Malthus provided some valuable ideas, they were far short of providing a complete theory that could be published. A lot of changes occurred in his ideas before he finally formulated a publishable theory.

It is certainly not an easy task to pinpoint an exact date of discovery. Yet as Ospovat (1981) clearly demonstrates even in late 1854 Darwin did not formulate his principle of divergence. The more likely date is the summer of 1857. Here what I claim to show is only that the discovery story that is repeatedly told in popular literature is unreliable. Hence it in no way makes a plausible case for Darwin’s reluctance for publication.

b. Humans in the *Origin*

Even though simplified discovery myths are largely limited to popular literature, claims about Darwin’s late publication is by no means limited to popular accounts. One major argument supposedly indicating this is that even when he published the *Origin* he was still hiding many of his “controversial” views. The view that is considered to be intentionally omitted from the book was that humans were also subject to laws of nature and they evolved from other animals. The omission is implied by the presence of only a single sentence about humans in the whole book.

The belief [that humans descended from other animals] was so heretical that Darwin even sidestepped it in *The Origin of Species* (1859), where he ventured only the cryptic comment that “light will be thrown on the origin of man and his history” (Gould 1977, 25).

The *Origin* is not directly about humans. The only explicit reference is an almost throwaway passage at the end of the book. ‘Light will be thrown on the origin of man and his history.’ But no one was fooled (Ruse and Richards 2009, xvii).

Clearly the authors of these passages believe that Darwin intentionally tried to hide his views about humans. It is clear from his notebooks that by the time the *Origin* was published he was in no doubt about the applicability of transmutation to humans and had considerable amount of material that may be published about human origins. Yet if one reads the whole paragraph, which is indeed quite short, one can hardly call it a “cryptic comment” or a “throwaway passage”:

In the distant future I see open fields for far more important researches. Psychology will be based on a new foundation, that of the necessary acquirement of each mental power and capacity by gradation. Light will be thrown on the origin of man and his history (Darwin 1859, 488).

This passage from the first edition clearly shows that Darwin, although not discussing human evolution in detail, made no attempt to hide his general views on

the issue. Even mental powers are expected to be explained by natural selection. Certainly “no one was fooled” but it is dubious at least that Darwin wanted to fool anyone and yet claimed that psychology would be a branch of evolution. The paragraph might be short, but it leaves no doubt about Darwin’s position on the matter. He thinks that not only our bodies but also our minds are a product of evolution. This was a view which even Wallace did not fully accept. Darwin’s autobiography provides a more plausible explanation for why a detailed discussion of humans is missing from the *Origin*:

As soon as I had become, in the year 1837 or 1838, convinced that species were mutable productions, I could not avoid the belief that man must come under the same law... Although in the *Origin of Species*, the derivation of any particular species is never discussed, yet I thought it best, in order that no honourable man should accuse me of concealing my views, to add that by the work in question “light will be thrown on the origin of man and his history” (Barlow 1958, 130).

Moreover, both in the *Origin* and in his paper read in 1858 Darwin quite explicitly mentions the fact that humans are subject to the same laws as all the other animals:

The same kind of calculation applied to all plants and animals affords results more or less striking, but in very few instances more striking than in man (Darwin 1858, 259–260).

It is the doctrine of Malthus applied with manifold force to the whole animal and vegetable kingdoms (Darwin 1859, 63).

Not only humans but also no other species received explicit derivation in the *Origin*. This was not because he wanted to hide his views but because the book aims to present the theory clearly. It was “one long argument” and the story of a particular species would both complicate and unnecessarily lengthen the book, which was in Darwin’s eyes only an ‘abstract.’ Yet he made sure that his views about humans were known. Contrary to common belief, I think, Darwin was quite comfortable arguing that humans descended from other animals at least after 1858. There is no mention to orchids in the *Origin* as well. He certainly did not fear that including orchids would cause a public outrage; nor did he lack a detailed knowledge about them. Rather he wrote a separate book on them—just like he did for humans. And humans received a very lengthy treatment.

My book on the Expression of the Emotions in Men and Animals was published in the autumn of 1872. I had intended to give only a chapter on the subject in the *Descent of Man*, but as soon as I began to put my notes together, I saw that it would require a separate Treatise (Darwin 1958, 131).

So clearly Darwin had a lot to write and certainly cannot fill everything in a single book. The so-called “throwaway” remark at the end of the book was actually placed

there to clearly state his view at a stage where he writes about the significance of his theory. The book ends with reference to Newtonian laws which were the paradigm of scientific work at the time. And Darwin was claiming that he was the Newton of natural history. He mentions ‘man’ in this context, underlying that his laws apply even to man just in case anyone is ignorant enough not to realize that.

If, as some claimed, Darwin was hiding some of his views even when he published the *Origin*, then one might have said that he avoided publishing them as long as he could. Yet in my opinion there is no indication that he was avoiding humans in the *Origin*, hence this alone would not be sufficient to argue that at any time he was intentionally delaying publication.

c. Barnacles

One final “evidence” I will consider is his work on the taxonomy of barnacles. Clearly someone who discovered a major theory would not have spent eight years on an irrelevant research rather than publishing it. Hence his work on barnacles received a variety of interpretations. It was argued that he needed a detailed study of a species before proposing a general theory. He needed to establish his place in the scientific society so that his views would receive due attention. Or that he actually thought that it would not take much time but since he was “obsessive” the project got out of hand. Or even that he was procrastinating and was comfortable since there were very few researchers back then and he never thought someone else might hit the idea. Neither of these is plausible, and they do not cohere with many other clear facts about Darwin. Yet I will not evaluate any of these claims. Rather I will indicate that his work on barnacles is by itself not enough to indicate any avoidance of publication. All the above-mentioned explanations on his work on barnacles simply assumed that he already had a theory that was ready for publication. It makes sense to ask why he worked on barnacles rather than publishing the *Origin only if* he had the theory ready. I will argue in part III that it was not.

3. Explanations for the Delay

Since the belief that there is a gap of many years between discovery and publication is widespread there are a number of explanations for Darwin’s long delay. I will now try to argue that even if there was a delay none of the proposed explanations of it are plausible. The most popular explanation is his fear of adverse reactions. Another is that he was collecting more evidence to support his theory and tying up loose ends. After I argue their implausibility, I will finally consider a much less widespread explanation by Mayr—that he was simply procrastinating—and my criticism of Mayr’s claim will provide a portrayal of the intellectual climate of the era.

a. Reception of the *Vestiges*

Among the popular reasons for Darwin’s supposed delay is his fear of reactions. The expected reaction is illustrated by the reaction towards another publication entitled *Vestiges of the Natural History of Creation*.

It is very likely that Darwin was shocked by the venomous rejection of [*Vestiges*] a transmutatory text that had been published anonymously in 1844; and correspondingly elected in his future work to avoid any discussion of humans emerging from apes... Darwin probably delayed publication of his own 'species' book in order to avoid the same kind of response (Browne 2013, x-xi).

Since today there is some hostile reaction to evolutionary theory from certain religious circles, many simply assume that this should have been a much serious issue in the past. Yet in the 19th century England not only the scientific community but also much of the general public thought science and religion could co-exist. In their view God created the laws of nature, he was the primary cause of everything, and science was a pursuit of secondary causes. They believed that many passages of the sacred texts were metaphorical. Hence *Vestiges* was very warmly received by the public. Within the first year of publication, it made a second printing and sold 1750 copies. During the next year in 1845, it went to press twice and sold 3500 copies. At a time when 1250 copies (number of copies of the first edition of the *Origin*) for a scientific book was considered success, *Vestiges* sold over 15 000 by the year 1850. In six years it sold more than what *Origin* sold in 15. Prince Albert read it aloud to Queen Victoria in 1845, assuming that it was cutting edge science. So, unlike what most popular accounts of Darwin suggest, any fear that *Vestiges* might have induced in Darwin must have come from academic circles and not from religious authorities or general public. Yet even though Browne acknowledges the positive public reaction towards *Vestiges*, she presents the reaction against it as a religious attack and not a scientific one:

the Victorian politician Benjamin Disraeli caught the heart of the matter when he asked his contemporaries: are we apes or are we angels?... These were the questions that raged through the public world when Darwin published his *On the Origin of Species* in 1859. In that book Darwin alluded only briefly to human origins and made it plain that he was not prepared to speculate in print. Others before him had volunteered theories about the evolution of human beings and had been ridiculed by scientific experts (Browne 2013, ix).

Yet certainly Disraeli was far from representing the views of the academic community. A better source to understand the hostile reaction of the scientific community towards *Vestiges* is Sedgwick's (1845) 85-page review.

Sedgwick, like many other respected scientists of his day such as Owen and Lyell, accepted that the earth was millions of years old if not infinitely old. For several years the Geological Society had studied the geology of the earth and there had been some lively debates on many issues but at no one time they suspected that the earth was 6000 years old. This was also well known to Darwin who was secretary to the Geological Society for some years. So as Darwin would expect, the negative reaction

of respected scientists to *Vestiges* was not just quotes from the Bible. Literal readings of the sacred texts were not considered to be authoritative on geological debates.

In mid-19th-century England a deity worthy of our praise should have created the universe and the laws so perfectly that he need not constantly interfere. There might have been a religious literalist here and there who thought that the world was created in six 24-hour days, but this was not the general sentiment and there was no such respected geologist at the time. Transmutation was deemed perfectly compatible with Christianity. Surely Sedgwick also indicated some religious problems about *Vestiges* in his bitter review; yet the bulk of the criticism was about sloppy reasoning, inadequate methods of research and lack of evidence.

The real hostility against *Vestiges* among the academic elite was due to its reliance on phrenology and galvanism, both of which were highly popular yet scientifically dubious to say the least. The claim of the book that humans are part of nature and obey natural laws and that most if not all living beings have a common origin was not the main focus of criticism. The objections were against the way these views were defended and against the mechanism by which diversity and complexity emerged from a simple origin. A typical criticism of Sedgwick is not a quote from bible or not even a criticism of using galvanism. Rather he focused on the invalidity of the arguments provided: “Allowing that some of the functions of the brain resembles galvanism, are we to conclude that all of its functions are galvanic (Sedgwick 1845, 5)?” Sedgwick does not conclude that the author of the text must be an atheist, rather he concludes—as a means of humiliation—that the author must be a woman, falsely assuming that women have inadequate intellectual skills for scientific inquiry. Other than his sexism this shows that the major problem of *Vestiges* is not what it defends but rather how it defends it.¹

Hence Darwin must have only been assured that his theory would be warmly received based on the reaction of the scientific community against *Vestiges*. His views made absolutely no reference to either galvanism or phrenology. And what he considers to be his really creative contribution was the mechanism by which transmutation takes place—natural selection. Certainly, he might have been worried that his arguments might not stand against scrutiny. Yet this is a kind of fear every researcher has before submitting a work, and there is nothing special about Darwin’s case that might lead to a decades-long avoidance of publication.

The publication of the *Descent* took place 12 years after the *Origin*, which, for some, is another indicator that Darwin was avoiding publication due to fear from reactions. Gould, after insisting that there is almost no mention of humans in the *Origin*, argues that “Latter editions [of the *Origin*] added the intensifier “much” before the sentence [about humans]. Only in 1871 did he gather the courage to publish *The Descent of Man* (Gould 1977, 50).” Whereas Edward O. Wilson thinks that “The *Origin* won the day quickly... so much so that Darwin could confidently publish

¹ The fact that *Vestiges* was published anonymously should also not be considered as evidence that its author was afraid of reactions and that transmutation was a “dangerous” topic. Sedgwick’s review was also anonymously published. This was quite a common practice at the time. William Whewell also published his book entitled *Of the Plurality of Worlds* anonymously.

The Descent of Man only twelve years later (Wilson 2009, xv).” In both cases the claim is that the publication of anything about humans required courage even after the publication *and success* of the *Origin*. Hence, we are expected to conclude that fear should have overwhelmed Darwin earlier. The myth of fear and avoidance is most intensely dramatized by Browne in her introduction to a recent edition of the *Descent*. Even when Darwin finally had the courage, the publisher was still worried according to Browne:

John Murray, ... flinched a little at the subject matter... asked his friend Whitwell Elwin for his professional opinion. ... Elwin ... served as a useful barometer of public opinion. ... Elwin roared, ‘ ... The arguments in the sheets you have sent me appear to me to be little more than drivel’ (Browne 2013, xv).

Yet the story does not cohere with what took place afterwards. If Murray really needed some encouragement to publish the *Descent* and considered Elwin to be a “barometer of public opinion” then surely Elwin’s report would have prevented publication. One may at the very least expect that Murray consulted another referee and decided only after the encouragement of this second referee. Yet Browne mentions no such referee.² Even more troublesome is the fact that Murray decided to print 2500 copies of the *Descent*, which is twice as large as the first printing of the *Origin*. Finally, from the exchanges between Murray and Darwin about the publication of the *Origin*, it is clear that Murray was ready to publish whatever his illustrious author sent even before he saw the manuscript.³

The general pattern of the fear myth is that Darwin was afraid to voice his views until he realized Wallace was about to publish the same theory. At this point Darwin, by the help of his powerful connections, made it known that he got the idea much earlier than Wallace. This story—although sometimes backed by some out of context quotations—also is problematic. If due to such fear Darwin avoided publication, then why did he claim priority for these dangerous views once he saw Wallace’s letter? How come he suddenly realized that being the discoverer of natural selection would be a great honor rather than a source of mockery? Wouldn’t it be more fitting to the fear story if he simply had waited to see the reaction against Wallace before publishing his views? Hence though I believe that Darwin wanted to publish a well-developed, defensible theory rather than simply publishing vague ideas, it is implausible to say the least that he feared to the extent of hiding his views for years, after those views were ready to be published. Especially because, he views it important enough to elevate him to being the Newton of natural history.

² It seems, for the *Origin*, the actual decision was left to Charles Lyell. See (McClay 2009).

³ Darwin and Murray’s relation is documented in McClay (2009). From their exchanges it is clear that Murray—to Darwin’s surprise—was ready to publish whatever Darwin wrote even without seeing the manuscript. McClay cites Murray’s letter to Darwin dating April 1, 1859: “On the Strength of this information and my knowledge of your former publications, I can have no hesitation in swerving from my usual routine and in stating at once even without seeing the MS. that I shall be most happy to publish it for you on the same terms as those on which I publish for Sir Charles Lyell” (quoted in McClay 2009, 221).

b. Tying Loose Ends

A less popular excuse for Darwin's delay is based on the realization of the implausibility of fear myth. Instead, it is argued that the period between discovery and publication is spent to polish the theory that is to solve some empirical problems that one encounters in its applications. Here it makes sense that Darwin tried to maintain his priority because the problems that are supposedly causing the delay were relatively minor applications of it, such as the evolution of sterility. For example, Darwin "corresponded, beginning in late 1857, with the entomologist Fredrick Smith of the British Museum, a specialist on the Hymenoptera (the ants, bees and wasps), on subjects including the morphology of ant casts" (Lustig 2009, 124). The date of the correspondence is significant since this shows Darwin still had problems with explaining hymenoptera as late as 1857, hence the delay is thought to be explained. This view can be found for example in Richards (2009).

However, we know that Darwin does not wait until he manages to explain every phenomenon under the domain of his theory before publication. Indeed, he does not shy away from publishing problematic views *under certain conditions*. For example, his theory of pangenesis published in 1868 received harsh criticisms yet he kept it even in the "corrected" edition of the *Descent* published in 1875. In the autobiography Darwin explains this as follows:

Towards the end of the work, I give my well-abused hypothesis of Pangenesis. An unverified hypothesis is of little or no value. But if anyone should hereafter be led to make observations by which some such hypothesis could be established, I shall have done good service, as an astonishing number of isolated facts can thus be connected together and rendered intelligible (Barlow 1958, 130).

So, Darwin's approach was far from an attempt to explain every phenomenon that is known. Rather he was comfortable with a case where his theory was better than an alternative in explaining many diverse phenomena with a single idea.⁴ This was probably due to the influence of Whewell, who was the president of the Geological Society when Darwin was its secretary. This attitude was not something that he developed late in his career and used in the autobiography to reconstruct his earlier works. We may find it in the *Origin* as well:

But if the same species can be produced at two separate points, why do we not find a single mammal common to Europe and Australia or South America? The conditions of life are nearly the same... The answer, as I believe, is, that mammals have not been able to migrate... across the vast and broken interspace.

Undoubtedly many cases occur, in which we cannot explain how the same species could have passed from one point to other. But the geographical and climatal changes, which have certainly

⁴ This was probably due to the influence of Whewell, who was the president of the Geological Society when Darwin was its secretary.

occurred within recent geological times, must have interrupted or rendered discontinuous the formerly continuous range of many species.... *It would be hopelessly tedious to discuss all the exceptional cases of the same species, now living at distant and separated points; nor do I for a moment pretend that any explanation could be offered of many such cases* (Darwin 1859, 352–354, my emphasis).

Hence, I believe Darwin was quite comfortable with *some* problems of some applications of his theory. Hymenoptera poses no more fundamental problems than unexplained instances of biodiversity. Darwin was well aware of the fact that minor problems could never be completely removed to the satisfaction of every critique: “Any one whose dispositions leads him to attach more weight to unexplained difficulties than to the explanation of a certain number of facts will certainly reject my theory” (Darwin 1859, 482).

c. Procrastination

Finally, I want to discuss another attempt to explain the delay not because it is popular (indeed it is not) but because it reflects a general misconception about the scientific community of early nineteenth century England. According to Mayr, Darwin’s delay was due to neither collecting more data, nor perfecting the applications. He sure was doing these but it would be—as I agree—implausible to think that these were considered by him important enough to wait for two decades before publishing. The fear myth is also not a sufficient explanation for Mayr. Hence, he argues that Darwin was simply procrastinating: “So few people were working in the different branches of biology that Darwin thought he could afford to wait twenty years before publishing his theory of natural selection” (Mayr 1982, 111).

The main problem with this view is not that it misrepresents Darwin’s views. The real problem is that it gave the impression that there was no significant scientist at the time working on some form of transmutation theory. This could not be farther from the truth. Not only transmutation was a popular idea at the time but also one of the most prominent figures on natural history was on his way of developing some form of theory of descent. Richard Owen, although today he is often portrayed as a creationist for his opposition to Darwin, was an obvious candidate for such a discovery. He did not rule out the possibility that humans evolved from other apes. Moreover, he even mentioned natural selection as a possible source of transmutation. Owen’s resistance to Darwin’s view was not due to a belief in the fixity of species, but rather he considered six possible mechanisms guiding transmutation and one of them—natural selection—was deemed far too weak to be a significant factor. Owen thought that natural selection could never be the creative force of transmutation.⁵

⁵ For an evaluation of Owen’s views see Rupke (2009). Ospovat also indicates this point: “There is an obvious sense in which Owen’s conception of the history of life is protoevolutionary. It might be said to be ready-made for reinterpretation in harmony with the doctrine of descent” (Ospovat 1981, 138). Neither Ospovat nor I claim that Owen or anyone else for that matter would have developed exactly the same theory that Darwin came up with if he never published. Peter J. Bowler made a convincing

Since Owen, who was considered at the time the “English Cuvier” published his views much earlier; Darwin well knew that sooner or later someone would come up with a convincing mechanism for transmutation. Darwin surely was in no delusion of being the only researcher looking for a mechanism for transmutation.

4. Anachronism

In section I, while discussing Darwin’s work on barnacles, I argued that Darwin did not have a theory worthy of publication at the time, hence his work on barnacles should not be considered as evidence that he avoided publication. Yet many think otherwise. My main reason for thinking so is that Darwin lacked the principle of divergence which he mentioned in his autobiography. Indeed, especially after Ospovat’s work on the subject, there is a general consensus that Darwin’s views developed significantly during the years after his reading of Malthus. Yet it is one thing to claim that Darwin’s views have developed and quite another to say that he did not have a publishable theory in 1838 or 1844. And of course since he wrote extensively on the subject one may hope to find a written text that was worthy of publication. Those arguing that Darwin unnecessarily delayed publication aim to do just that. In *The Development of Darwin’s Theory: Natural History, Natural Theology, and Natural Selection, 1838-1859*, Ospovat (1981) convincingly shows that Darwin developed his principle of divergence at a time much closer to the publication of the *Origin*. Yet the debate is whether lacking this principle was serious enough to explain Darwin’s delay. Was there a defensible theory even without this principle? For example, Ruse writes: “And let us not forget the ‘principle of divergence,’... I fully accept that Darwin did not really realize the problem and the solution until much later” (Ruse 2009, 6). Yet he goes on to say that this cannot justify the delay: “I cannot find all of that much difference between the Essay of 1844 and the Origin of 1859” (Ruse 2009, 7). Similarly, Browne cites the late discovery of principle of divergence but considers this to be a better way of presenting the theory rather than an essential aspect of it (Browne 2002, 39).

Yet if we accept this view, not only we need to explain why Darwin waited for two decades but also why there was almost no reaction to their papers read at the Linnaean Society in 1858, and why Wallace never made any priority claims. Even more importantly one cannot help but wonder why until late 1930’s Darwin’s delay was not a topic of discussion, nor why the claim that his views were kept a secret was asserted only after a century from the publication of the *Origin*.⁶

The *Origin* is “one long argument” according to Darwin. Yet even this is disputed by scholars today. Naturally we have plenty of room to disagree with Darwin on a number of points like this. We may assess to what extent Darwin understood the significance of his achievements or we may argue that the book would have been better if organized in some other way and so on. The general tendency to use the

case that that would not be so (Bowler 2008). However it would be highly misleading to claim that Darwin was not aware of others working on some form of theory of descent.

⁶ van Wyhe convincingly argues that the debate on Darwin’s delay is a relatively recent invention. See John van Wyhe (2007).

first edition of his *Origin* is also similar. His many “corrections” made during several later editions seem to damage the text from our point of view rather than improving it.

I do not argue against any of these. Yet in order to claim that Darwin avoided publication intentionally it wouldn't be enough just to show that his earlier draft contained a sufficiently strong case for natural selection *from our perspective for our purposes*. We should also evaluate what his contemporaries and Darwin himself believed. I will now try to show that neither for Darwin nor for his contemporaries the 1844 essay would mean much. Without the principle of divergence the theory was indefensible. The essay of 1844 became a sufficiently clear and convincing text only after modern synthesis. Hence the puzzle of delay and myths of secrecy emerged almost a century after the *Origin*.

a. Swamping Argument

Darwin and his contemporaries were convinced about blending inheritance. This caused a significant problem for some forms of transmutation theories. Today the most popular version of such a criticism against transmutation is due to Fleeming Jenkin. His anonymously published review of the *Origin* attracted much attention from historians for there is some evidence that Darwin valued this review.

According to this argument any differentiation of an individual from its group would soon be removed from the population since blending inheritance would ensure that in each generation the difference would decrease. In a few generations the population would return to its state before the differentiation occurred. This would be the case even if the differentiation is hugely advantageous. Hence blending inheritance is an obstacle to the emergence of new species by means of natural selection.

Some attributed modifications to later additions of the *Origin* to the swamping argument presented here. However, since Vorzimmer's article (1963) it is generally accepted that the swamping argument did not cause significant alterations in Darwin's views. Lack of significant alterations to overcome the swamping argument in later editions of the *Origin* is an indication that Darwin solved the problem to his own satisfaction in the first edition.

The problem—hence the solution—is no longer relevant for debates on evolution since the argument rests on a long-dismissed premise. Without blending inheritance, the argument loses its teeth. Hence any modern reader will tend to undervalue the significance of Darwin's solution to it.

Today natural selection is often presented in a way that Malthus would appear as its discoverer. The description of natural selection below would pass as a simple but clear statement of natural selection in a contemporary popular text on the subject:

Those individuals of any species which are most adapted to the life they lead, live on an average longer than those which are less adapted to the circumstances in which the species is placed. The

individuals which live the longest will have the most numerous offspring, and as the offspring on the whole resemble their parents, the descendants from any given generation will on the whole resemble the more favoured rather than the less favoured individuals of the species. So much of the theory of natural selection will hardly be denied (Jenkin, 1867).

Yet it is quoted from Jenkin's review which aims to show that not only Darwin's theory is not proven but *proven to be false*. Hence, we cannot conclude from the fact that some aspects of the theory such as the principle of divergence does not seem essential to us, to the erroneous view that it was not essential to Darwin or his contemporaries.

In essence the problem was that natural selection did not seem to be a creative force that could drive transmutation. Its function as the eliminator of disadvantageous traits was not disputed. Yet this negative function would only cause extinction, not emergence of new species. The swamping argument was pointing this out. Similarly, Owen rejected the theory largely because he never accepted natural selection as a creative force even though he referred to it as a source of change in his own works before the publication of the *Origin*. Jenkin was certainly not the first to have thought of this hence Susan W. Morris (1994) argued—quite convincingly—that the swamping argument was not even the main point of Jenkin's review. In the following I will argue that Darwin failed to present natural selection as a creative force before this development of the principle of divergence. And not even immediately after having it. It was certainly lacking in his 1844 Essay and also from Wallace's letter.

b. Wallace's Letter

It is well known that there are some differences between Wallace's and Darwin's approaches to evolution. Even though some—like Mayr—argues that they came up with the same theory, three points of departure are evident. First and most famously Wallace rejects the idea that human capacities for morality, mathematics and arts are unique and could not have emerged via natural selection or by any other natural means. Secondly, they had a disagreement about coloring in birds. The disagreement is essentially the same as the above mentioned one. Since Wallace does not accept the presence of aesthetics in any life form other than humans, he cannot come to accept sexual selection. Female birds could not shape male birds by means of their attraction to the beautiful ones. Whatever the importance of these disagreements for claiming that their theories were different, a third disagreement is much more critical for my purposes.

In his paper which was read at the Linnean Society, Wallace points out the fact that in domesticated animals reversion to original type occurs, which causes people to believe that species are fixed. In order to convince his readers that this is not the case, he draws a sharp distinction between what happens in nature and under domestication. Since domesticated animals are in such a condition that they have no chance to survive in nature they revert to the original type if left unchecked.

Whereas in nature species constantly evolve to better adapt to their environment, they never lose their advantageous traits. Wallace writes:

We believe we have now shown that there is a tendency in nature to the continued progression of certain classes of varieties further and further from the original type—a progression to which there appears no reason to assign any definite limits (Wallace 1858, 278–279).

Selection under domestication is also an important point of discussion for Darwin. But unlike Wallace, Darwin tries to pass from domestication to wild species seamlessly. For Darwin what helps a wild animal survive in nature is similar to what helps a domesticated pig to survive in a farm. Hence there is no point in indicating that a domesticated pig would not survive in the wild. In the domesticated environment domestic pigs are much better adapted than their wild cousins.

Their different understandings of the relation between domestic and wild species have important consequences for their respected theories. Wallace was satisfied by his proposal, since the obvious criticism towards it was avoided by arguing that species under domestication could not model what happened in the wild. Yet for Darwin, such a move is not available. Wallace's explanation did not satisfy anyone. For one, reversion to original stock does not occur only when the domesticated species are left unattended. On the contrary even with the best efforts of the breeders that is unavoidable. Moreover, it is far easier under domestication to prevent them from mating with the original stock. Wallace's demand for a balanced development is also an unrealistic condition. Surely as Darwin would well know there are species which developed extreme traits in nature. It is also certainly not clear what constitutes a "balanced" development.

Hence after the reading of excerpts from Darwin's letters and Wallace's paper there was not much debate. Also, the president of the Linnean Society of London, Thomas Bell, wrote in his annual presidential report that "The year which has passed has not, indeed, been marked by any of those striking discoveries which at once revolutionize, so to speak, the department of science on which they bear" (quoted in Browne 2002, 42).

It was evident for everyone that under domestication species change significantly. Yet even with the best efforts no one managed to create a new species and it became more and more difficult to alter the members of the species as they diverge more and more from the original stock. Hence it was quite implausible to accept Wallace's suggestion that the species change indefinitely in the wild. Wallace's approach was surely insufficient for Darwin as expressed in his *Origin*:

varieties, even strongly-marked ones, though having somewhat of the character of species—as is shown by the hopeless doubts in many cases how to rank them—yet certainly differ from each other far less than do good and distinct species. ... How, then, does the lesser difference between varieties become augmented into the

greater difference between species? ... Mere chance, as we may call it, might cause one variety to differ in some character from its parents, and the offspring of this variety again to differ from its parent in the very same character and in a greater degree; but this alone would never account for so habitual and large an amount of difference as that between varieties of the same species and species of the same genus. After the example of pigeons where humans prevent desirable versions to mix with undesirable ones. ... But how, it may be asked, can any analogous principle apply in nature (Darwin, 1859: 111-112)?

To repeat once again the effects of natural selection were not in doubt if it was understood only as a destructive force. Yet there was nothing to convince the scientific community that blending inheritance would not eliminate advantageous varieties much faster and much effectively than natural selection would eliminate the disadvantageous ones. Darwin recognized the problem quite early and failed to find a solution until much later. The classification work on barnacles was not directly a task about improving his Essay of 1844 in any sense. Yet his work led to the principle of divergence which turned out to be well suited to explain evolution to his satisfaction. As Roderick D. Buchanan and James Bradley argued: “The evidence and insights Darwin gained from it [his barnacle project] were largely incidental and came after his decision to tackle the whole group” (Buchanan and Bradley 2017, 529).⁷

c. Development of Darwin’s Ideas

If we are to agree that Darwin needed a mechanism that would explain how the creation of new species could occur despite blending inheritance, it at once became obvious why he was not satisfied with his Essay of 1844. It also became apparent why the first reading of Wallace’s paper did not create any reaction at all even though the publication of the *Origin* caused a lively debate. Hence it is no wonder that Wallace called the theory “Darwinism.”

Principle of divergence is the creative force of evolution. It is the equivalent of what breeders do to the domestic breeds, in the wild.

But how, it may be asked, can any analogous principle apply in nature? I believe it can and does apply most efficiently, from the simple circumstance that the more diversified the descendants from any one species become in structure, constitution, and habits, by so much will they be better enabled to seize on many and widely diversified places in the polity of nature, and so be enabled to increase in numbers (Darwin, 1859: 112).

⁷ Nevertheless, they also claim that even if the fear story is generally exaggerated one should not dismiss it altogether. So they add: “However, the credentialing motivations behind it [working on barnacles for eight years] were driven by field-generated self-doubts that are difficult to separate from fear” (Buchanan and Bradley 2017, 529).

Breeders, by preventing diversified descendants from mating with the original stock, enable the development of the desired trait. This cannot be done by natural selection alone for it can only eliminate those which are unfit to survive. However, unless some major change occurs overnight, the initial difference between the original stock and the new variety will not be so dramatic as to render only one of them to be able to survive. Leaving out such major saltations Darwin's only option was to find a means to separate the variety from the parent stock even though both are capable of surviving. Hence, without this principle Darwin claimed that small populations in confined areas are much more likely to evolve into different species. In a large population reversion to the original stock was unavoidable. But later Darwin realized not only this is not in accordance with his empirical data but also that adaptation is a matter of degree. There is no such thing as perfect adaptation. Moreover, he altered his views on the source of variability. When initially he was thinking in terms of environmental effects as the cause of variability, at this later stage he came to believe that variations occur regardless of the presence of any external input. So now in addition to the eliminative force of natural selection he was able to suggest a creative force. In Ospovat's words:

Darwin had previously reached an understanding of how divergence must result from the struggle for existence, which is all that was required for his explanation of classification. But he had not considered the reverse relationship, that the principle of divergence might influence the process of selection. Now [during the summer of 1957] he concluded that it must do so. It is divergence that converts incipient species into good and distinct species (Ospovat 1981, 187).

Both of these points are so clear to us that when reading Darwin, we tend to overlook the development of his views. The presence of variations among the species even at the same location and at the same time period—as accepted in the *Origin*—is so obvious for us that we do not imagine Darwin having different ideas about these. For example, Mayr completely ignores Darwin's changing ideas on the source of variation and simply states his position in the *Origin* as a result of his "keen ability to observe":

To be sure, Darwin was vague and confused about the origin of genetic variation ... But Darwin with his customary keen ability to observe, concluded that the abundant variability always present in nature resulted not from major saltations but from the accumulation of small changes occurring at random with respect to environmental conditions (Mayr 2003, xvi).

With perfect adaptation of species and only source of variation being environmental change, natural selection would not be a plausible explanation of speciation especially for someone holding the view of blending inheritance. Hence a minor group left in an island or somehow lost contact to the original stock and drifted to a

different environment are the only possible sources of speciation. Yet Darwin's observations do not support such a view.

Without a change in all these views and the principle of divergence one either needs to invoke saltations and explain all speciation based on complete annihilation of parent species; or introduce some such mechanism in an ad-hoc manner. The first was more like what Thomas Huxley attempted, while the second was Wallace's attempt in his first paper. I have already mentioned that Wallace's paper did not arouse any discussion (just like Darwin's works read on the same day) hence that was not a plausible theory. Huxley's version was also not to Darwin's satisfaction. After listening to Huxley's exposition of his theory in 1860, Darwin said "as an exposition of the doctrine the lecture seems to me an entire failure" (quoted in Young 1992, 151). Huxley being always completely at ease with saltations never bothered with the principle of divergence.

Ospovat argues that the principle of divergence emerged as Darwin tackled problems of classification and not speciation (1981, 170-173). It only later turned out to be a good tool to tackle the speciation problem. Nevertheless, it did provide the means to present a creative force hence transformed Darwin's theory. Species occupying a given territory will have a variation despite the fact that they are exposed to very similar environmental conditions. Yet different varieties will be slightly better adapted to occupy slightly varied niches. Hence, they will diverge and occupy niches distinct from the parent stock. There is now no need for a small, isolated population for natural selection to act effectively. On the contrary if the population is large and occupies an open area where it could populate a variety of niches, more of its variants will differentiate and fill those niches. This way Darwin has a creative force that drives evolution and a better fit with his observations.

5. Conclusion

Without the principle of divergence, the *Origin* is not about speciation. However, the principle is rarely studied and even when it is studied, it's generally considered being of minor importance. It is not really understood as a key ingredient for Darwin's explanation of speciation. Kohn cites Mayr, Sulloway, Coyne and Orr when he wrote: "[h]owever, there is an irony in the historical fate of the principle [of divergence]. Much of twentieth-century evolutionary biology rejected Darwin's explanation of 'speciation' as muddled." (Kohn 2009, 87).

Today overlooking the principle of divergence caused some to argue that Darwin's book has an inappropriate title. For example, Coyne writes: "Despite the title of his famous book, Darwin was notably unsuccessful in solving the real problem of organic diversity: why plants and animals in a habitat fall into discrete, nonoverlapping packages" (Coyne 1994, 19). Coyne then goes on to argue that the first step in approaching the problem of speciation should be to determine the boundaries of species. Hence in his article "Ernst Mayr and the Origin of Species" he refers to Mayr's biological species concept and its utility in handling the speciation problem. Coyne also notes that not all researchers view species as real units: "Those entertaining this view [that species are not real units], of course, require no theory of speciation beyond that given by Darwin" (Coyne 1994, 20).

It is certainly true that Darwin rejected species as well-defined distinct units. According to Darwin our view of distinct species emerges as a result of extinctions. Even though in practice there are species, in theory there could have been only a continuous group. Yet Darwin did use the concept and did attempt to explain speciation. Yet he was living in a very different scientific environment with radically different background assumptions than we now have.

From a contemporary perspective whatever there is interesting in Darwin's *Origin* was already contained in his Essay of 1844, as almost any biologists after modern synthesis would agree. Hence the question why Darwin avoided publishing his theory for more than a decade naturally arises—but only from a contemporary perspective. What I am trying to argue is that the problem of speciation was not unimportant for Darwin and his title *On the Origin of Species by means of Natural Selection* was by no means a carelessly chosen one. Yet erroneous theories of inheritance led him to solve problems that did not emerge today. Hence his solution that took so much time and effort on his part seems to us as a minor issue. Yet Ospovat states the importance of the principle for Darwin very clearly: “Apart from writing of *Natural Selection*, solving this problem and constructing an argument to support his solution became Darwin's largest single project—gauged by the amount of his own time spent on it and by the number of assistants he recruited—during the period 1854–8” (Ospovat, 1981: 170).

Our understanding of evolution is quite different from Darwin's understanding of it. For many issues the difference may be insignificant. Even at times it might be satisfactory to sweep aside Darwin's own views regarding them as “muddled.” However, if we are to answer the question why Darwin published his theory in 1859 rather than, say, 1844, we should try to understand what merit Darwin *himself* saw in his Essay of 1844 and not just what we think about it.

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