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# The Foundation of the Origin of Species

*Two Essays Written in 1842 and 1844 by  
Charles Darwin*

CHARLES DARWIN  
EDITED BY FRANCIS DARWIN



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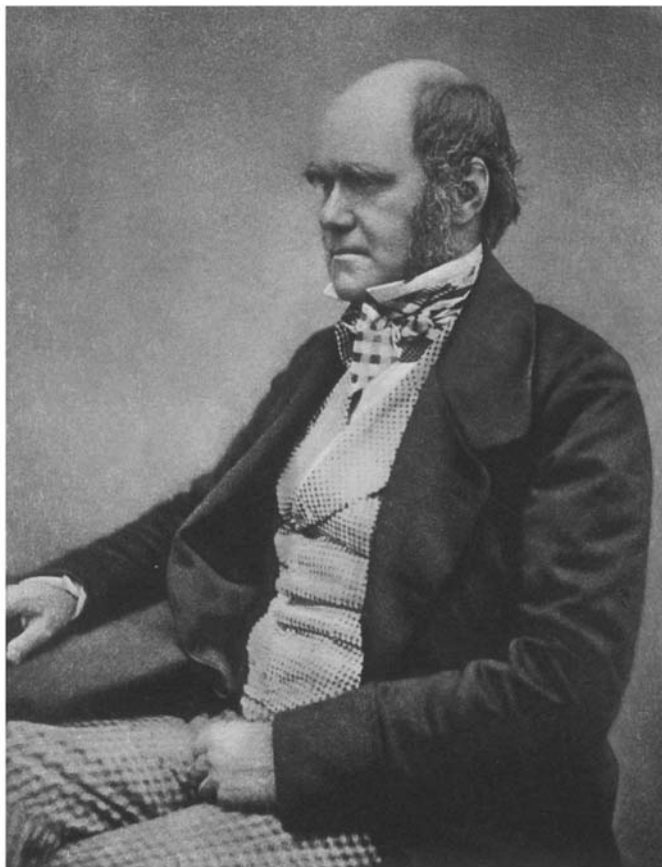
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*Charles Darwin*

*from a photograph by Maul & Fox. circ. 1854*

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THE FOUNDATIONS OF THE  
ORIGIN OF SPECIES

TWO ESSAYS  
WRITTEN IN 1842 AND 1844

by

CHARLES DARWIN

Edited by his son

FRANCIS DARWIN

Honorary Fellow of Christ's College

Cambridge:  
at the University Press

1909



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Astronomers might formerly have said that God ordered each planet to move in its particular destiny. In same manner God orders each animal created with certain form in certain country. But how much more simple and sublime power,—let attraction act according to certain law, such are inevitable consequences,—let animal(s) be created, then by the fixed laws of generation, such will be their successors.

From DARWIN'S *Note Book*, 1837, p. 101.

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TO THE MASTER AND FELLOWS  
OF CHRIST'S COLLEGE, THIS  
BOOK IS DEDICATED BY THE  
EDITOR IN TOKEN OF RESPECT  
AND GRATITUDE

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WE know from the contents of Charles Darwin's Note Book of 1837 that he was at that time a convinced Evolutionist<sup>1</sup>. Nor can there be any doubt that, when he started on board the *Beagle*, such opinions as he had were on the side of immutability. When therefore did the current of his thoughts begin to set in the direction of Evolution?

We have first to consider the factors that made for such a change. On his departure in 1831, Henslow gave him vol. I. of Lyell's *Principles*, then just published, with the warning that he was not to believe what he read<sup>2</sup>. But believe he did, and it is certain (as Huxley has forcibly pointed out<sup>3</sup>) that the doctrine of uniformitarianism when applied to Biology leads of necessity to Evolution. If the extermination of a species is no more catastrophic than the natural death of an individual, why should the birth of a species be any more miraculous than the birth of an individual? It is quite clear that this thought was vividly present to Darwin when he was writing out his early thoughts in the 1837 Note Book<sup>4</sup>:—

“Propagation explains why modern animals same type as extinct, which is law almost proved.

<sup>1</sup> See the extracts in *Life and Letters of Charles Darwin*, ii. p. 5.

<sup>2</sup> The second volume,—especially important in regard to Evolution,—reached him in the autumn of 1832, as Prof. Judd has pointed out in his most interesting paper in *Darwin and Modern Science*. Cambridge, 1909.

<sup>3</sup> Obituary Notice of C. Darwin, *Proc. R. Soc.* vol. 44. Reprinted in Huxley's *Collected Essays*. See also *Life and Letters of C. Darwin*, ii. p. 179.

<sup>4</sup> See the extracts in the *Life and Letters*, ii. p. 5.

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They die, without they change, like golden pippins; it is a *generation of species* like generation of *individuals*."

"If *species* generate other *species* their race is not utterly cut off."

These quotations show that he was struggling to see in the origin of species a process just as scientifically comprehensible as the birth of individuals. They show, I think, that he recognised the two things not merely as similar but as identical.

It is impossible to know how soon the ferment of uniformitarianism began to work, but it is fair to suspect that in 1832 he had already begun to see that mutability was the logical conclusion of Lyell's doctrine, though this was not acknowledged by Lyell himself.

There were however other factors of change. In his Autobiography<sup>1</sup> he wrote:—"During the voyage of the *Beagle* I had been deeply impressed by discovering in the Pampean formation great fossil animals covered with armour like that on the existing armadillos; secondly, by the manner in which closely allied animals replace one another in proceeding southward over the Continent; and thirdly, by the South American character of most of the productions of the Galapagos archipelago, and more especially by the manner in which they differ slightly on each island of the group; none of the islands appearing to be very ancient in a geological sense. It was evident that such facts as these, as well as many others, could only be explained on the supposition that species gradually become modified; and the subject haunted me."

Again we have to ask: how soon did any of these influences produce an effect on Darwin's mind? Different answers have been attempted. Huxley<sup>2</sup> held that these facts could not have produced their essential effect until the voyage had

<sup>1</sup> *Life and Letters*, i. p. 82.

<sup>2</sup> *Obituary Notice*, *loc. cit.*



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come to an end, and the “relations of the existing with the extinct species and of the species of the different geographical areas with one another were determined with some exactness.” He does not therefore allow that any appreciable advance towards evolution was made during the actual voyage of the *Beagle*.

Professor Judd<sup>1</sup> takes a very different view. He holds that November 1832 may be given with some confidence as the “date at which Darwin commenced that long series of observations and reasonings which eventually culminated in the preparation of the *Origin of Species*.”

Though I think these words suggest a more direct and continuous march than really existed between fossil-collecting in 1832 and writing the *Origin of Species* in 1859, yet I hold that it was during the voyage that Darwin’s mind began to be turned in the direction of Evolution, and I am therefore in essential agreement with Prof. Judd, although I lay more stress than he does on the latter part of the voyage.

Let us for a moment confine our attention to the passage, above quoted, from the Autobiography and to what is said in the Introduction to the *Origin*, Ed. i., viz. “When on board H.M.S. ‘Beagle,’ as naturalist, I was much struck with certain facts in the distribution of the inhabitants of South America, and in the geological relations of the present to the past inhabitants of that continent.” These words, occurring where they do, can only mean one thing,—namely that the facts suggested an evolutionary interpretation. And this being so it must be true that his thoughts *began to flow in the direction of Descent* at this early date.

I am inclined to think that the “new light which was rising in his mind<sup>2</sup>” had not yet attained any

<sup>1</sup> *Darwin and Modern Science*.

<sup>2</sup> Huxley, *Obituary*, p. xi.

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effective degree of steadiness or brightness. I think so because in his Pocket Book under the date 1837 he wrote, “In July opened first note-book on ‘transmutation of species.’ Had been greatly struck *from about month of previous March*<sup>1</sup> on character of South American fossils, and species on Galapagos Archipelago. These facts origin (*especially latter*), of all my views.” But he did not visit the Galapagos till 1835 and I therefore find it hard to believe that his evolutionary views attained any strength or permanence until at any rate quite late in the voyage. The Galapagos facts are strongly against Huxley’s view, for Darwin’s attention was “thoroughly aroused<sup>2</sup>” by comparing the birds shot by himself and by others on board. The case must have struck him at once,—without waiting for accurate determinations,—as a microcosm of evolution.

It is also to be noted, in regard to the remains of extinct animals, that, in the above quotation from his Pocket Book, he speaks of March 1837 as the time at which he began to be “greatly struck on character of South American fossils,” which suggests at least that the impression made in 1832 required reinforcement before a really powerful effect was produced.

We may therefore conclude, I think, that the evolutionary current in my father’s thoughts had continued to increase in force from 1832 onwards, being especially reinforced at the Galapagos in 1835 and again in 1837 when he was overhauling the results, mental and material, of his travels. And that when the above record in the Pocket Book was made he unconsciously minimised the earlier beginnings of his theorisings, and laid more stress on the recent thoughts which were

<sup>1</sup> In this citation the italics are mine.

<sup>2</sup> *Journal of Researches*, Ed. 1860, p. 394.

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naturally more vivid to him. In his letter<sup>1</sup> to Otto Zacharias (1877) he wrote, “On my return home in the autumn of 1836, I immediately began to prepare my Journal for publication, and then saw how many facts indicated the common descent of species.” This again is evidence in favour of the view that the later growths of his theory were the essentially important parts of its development.

In the same letter to Zacharias he says, “When I was on board the *Beagle* I believed in the permanence of species, but as far as I can remember vague doubts occasionally flitted across my mind.” Unless Prof. Judd and I are altogether wrong in believing that late or early in the voyage (it matters little which) a definite approach was made to the evolutionary standpoint, we must suppose that in 40 years such advance had shrunk in his recollection to the dimensions of “vague doubts.” The letter to Zacharias shows, I think, some forgetting of the past where the author says, “But I did not become convinced that species were mutable until, I think, two or three years had elapsed.” It is impossible to reconcile this with the contents of the evolutionary Note Book of 1837. I have no doubt that in his retrospect he felt that he had not been “convinced that species were mutable” until he had gained a clear conception of the mechanism of natural selection, *i.e.* in 1838—9.

But even on this last date there is some room, not for doubt, but for surprise. The passage in the Autobiography<sup>2</sup> is quite clear, namely that in October 1838 he read Malthus’s *Essay on the principle of Population* and “being well prepared to appreciate the struggle for existence..., it at once struck me that under these circumstances favourable variations would tend to be preserved,

<sup>1</sup> F. Darwin’s *Life of Charles Darwin* (in one volume), 1892, p. 166.

<sup>2</sup> *Life and Letters*, i. p. 83.

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and unfavourable ones to be destroyed. The result of this would be the formation of new species. Here then I had at last got a theory by which to work.”

It is surprising that Malthus should have been needed to give him the clue, when in the Note Book of 1837 there should occur—however obscurely expressed—the following forecast<sup>1</sup> of the importance of the survival of the fittest. “With respect to extinction, we can easily see that a variety of the ostrich (Petise<sup>2</sup>), may not be well adapted, and thus perish out; or on the other hand, like Orpheus<sup>3</sup>, being favourable, many might be produced. This requires the principle that the permanent variations produced by confined breeding and changing circumstances are continued and produce(d) according to the adaptation of such circumstances, and therefore that death of species is a consequence (contrary to what would appear in America) of non-adaptation of circumstances.”

I can hardly doubt, that with his knowledge of the interdependence of organisms and the tyranny of conditions, his experience would have crystallized out into “a theory by which to work” even without the aid of Malthus.

In my father’s Autobiography<sup>4</sup> he writes, “In June 1842 I first allowed myself the satisfaction of writing a very brief abstract of my theory in pencil in 35 pages; and this was enlarged during the summer of 1844 into one of 230 pages<sup>5</sup>, which I had fairly copied out and still possess.” These two Essays, of 1842 and 1844, are now printed under the title *The Foundations of the Origin of Species*.

<sup>1</sup> *Life and Letters*, ii. p. 8.

<sup>2</sup> Avestruz Petise; *i.e.* Rhea Darwin.

<sup>3</sup> A bird.

<sup>4</sup> *Life and Letters*, i. p. 84.

<sup>5</sup> It contains as a fact 231 pp. It is a strongly bound folio, interleaved with blank pages, as though for notes and additions. His own ms. from which it was copied contains 189 pp.

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It will be noted that in the above passage he does not mention the MS. of 1842 as being in existence, and when I was at work on *Life and Letters* I had not seen it. It only came to light after my mother's death in 1896 when the house at Down was vacated. The MS. was hidden in a cupboard under the stairs which was not used for papers of any value, but rather as an overflow for matter which he did not wish to destroy.

The statement in the Autobiography that the MS. was written in 1842 agrees with an entry in my father's Diary:—

“1842. May 18th went to Maer. June 15th to Shrewsbury, and on 18th to Capel Curig....During my stay at Maer and Shrewsbury (five years after commencement) wrote pencil sketch of my species theory.” Again in a letter to Lyell (June 18, 1858) he speaks of his “MS. sketch written out in 1842<sup>1</sup>.” In the *Origin of Species*, Ed. i. p. 1, he speaks of beginning his speculations in 1837 and of allowing himself to draw up some “short notes” after “five years' work,” *i.e.* in 1842. So far there seems no doubt as to 1842 being the date of the first sketch; but there is evidence in favour of an earlier date<sup>2</sup>. Thus across the Table of Contents of the bound copy of the 1844 MS. is written in my father's hand “This was sketched in 1839.” Again in a letter to Mr Wallace<sup>3</sup> (Jan. 25, 1859) he speaks of his own contributions to the Linnean paper<sup>4</sup> of July 1, 1858, as “written in 1839, now just twenty years ago.” This statement as it stands is undoubtedly incorrect, since the extracts are from the MS. of 1844, about the date of which no doubt exists; but even if it could be supposed to refer to the 1842 Essay, it must, I think, be rejected. I can only account for his mistake by the supposition that my father had in

<sup>1</sup> *Life and Letters*, ii. p. 116.<sup>3</sup> *Life and Letters*, ii. p. 146.<sup>2</sup> *Life and Letters*, ii. p. 10.<sup>4</sup> *J. Linn. Soc. Zool.* iii. p. 45.

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mind the date (1839) at which the framework of his theory was laid down. It is worth noting that in his *Autobiography* (p. 88) he speaks of the time “about 1839, when the theory was clearly conceived.” However this may be there can be no doubt that 1842 is the correct date. Since the publication of *Life and Letters* I have gained fresh evidence on this head. A small packet containing 13 pp. of MS. came to light in 1896. On the outside is written “First Pencil Sketch of Species Theory. Written at Maer and Shrewsbury during May and June 1842.” It is not however written in pencil, and it consists of a single chapter on *The Principles of Variation in Domestic Organisms*. A single unnumbered page is written in pencil, and is headed “Maer, May 1842, useless”; it also bears the words “This page was thought of as introduction.” It consists of the briefest sketch of the geological evidence for evolution, together with words intended as headings for discussion,—such as “Affinity,—unity of type,—foetal state,—abortive organs.”

The back of this “useless” page is of some interest, although it does not bear on the question of date,—the matter immediately before us.

It seems to be an outline of the Essay or sketch of 1842, consisting of the titles of the three chapters of which it was to have consisted.

“I. The Principles of Var. in domestic organisms.

“II. The possible and probable application of these same principles to wild animals and consequently the possible and probable production of wild races, analogous to the domestic ones of plants and animals.

“III. The reasons for and against believing that such races have really been produced, forming what are called species.”

It will be seen that Chapter III as originally

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designed corresponds to Part II (p. 22) of the Essay of 1842, which is (p. 7) defined by the author as discussing “whether the characters and relations of animated things are such as favour the idea of wild species being races descended from a common stock.” Again at p. 23 the author asks “What then is the evidence in favour of it (the theory of descent) and what the evidence against it.” The generalised section of his Essay having been originally Chapter III<sup>1</sup> accounts for the curious error which occurs in pp. 18 and 22 where the second Part of the Essay is called Part III.

The division of the Essay into two parts is maintained in the enlarged Essay of 1844, in which he writes: “The Second Part of this work is devoted to the general consideration of how far the general economy of nature justifies or opposes the belief that related species and genera are descended from common stocks.” The *Origin of Species* however is not so divided.

We may now return to the question of the date of the Essay. I have found additional evidence in favour of 1842 in a sentence written on the back of the Table of Contents of the 1844 MS.—not the copied version but the original in my father’s writing: “This was written and enlarged from a sketch in 37 pages<sup>2</sup> in Pencil (the latter written in summer of 1842 at Maer and Shrewsbury) in beginning of 1844, and finished it (*sic*) in July; and finally corrected the copy by Mr Fletcher in the last week in September.” On the whole it is impossible to doubt that 1842 is the date of the earlier of the two Essays.

<sup>1</sup> It is evident that *Parts* and *Chapters* were to some extent interchangeable in the author’s mind, for p. 1 (of the MS. we have been discussing) is headed in ink Chapter I, and afterwards altered in pencil to Part I.

<sup>2</sup> On p. 23 of the MS. of the *Foundations* is a reference to the “back of p. 21 bis”: this suggests that additional pages had been interpolated in the MS. and that it may once have had 37 in place of 35 pp.



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The sketch of 1842 is written on bad paper with a soft pencil, and is in many parts extremely difficult to read, many of the words ending in mere scrawls and being illegible without context. It is evidently written rapidly, and is in his most elliptical style, the articles being frequently omitted, and the sentences being loosely composed and often illogical in structure. There is much erasure and correction, apparently made at the moment of writing, and the MS. does not give the impression of having been re-read with any care. The whole is more like hasty memoranda of what was clear to himself, than material for the convincing of others.

Many of the pages are covered with writing on the back, an instance of his parsimony in the matter of paper<sup>1</sup>. This matter consists partly of passages marked for insertion in the text, and these can generally (though by no means always) be placed where he intended. But he also used the back of one page for a preliminary sketch to be rewritten on a clean sheet. These parts of the work have been printed as footnotes, so as to allow what was written on the front of the pages to form a continuous text. A certain amount of repetition is unavoidable, but much of what is written on the backs of the pages is of too much interest to be omitted. Some of the matter here given in footnotes may, moreover, have been intended as the final text and not as the preliminary sketch.

When a word cannot be deciphered, it is replaced by:—(illegible), the angular brackets being, as already explained, a symbol for an insertion by the editor. More commonly, however, the context makes the interpretation of a word reasonably sure although the word is not strictly legible. Such words are followed by an inserted mark of interrogation (?).

<sup>1</sup> *Life and Letters*, i. p. 153.



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Lastly, words inserted by the editor, of which the appropriateness is doubtful, are printed thus (variation?).

Two kinds of erasure occur in the MS. of 1842. One by vertical lines which seem to have been made when the 35 pp. MS. was being expanded into that of 1844, and merely imply that such a page is done with: and secondly the ordinary erasures by horizontal lines. I have not been quite consistent in regard to these: I began with the intention of printing (in square brackets) all such erasures. But I ultimately found that the confusion introduced into the already obscure sentences was greater than any possible gain; and many such erasures are altogether omitted. In the same way I have occasionally omitted hopelessly obscure and incomprehensible fragments, which if printed would only have burthened the text with a string of (illegible)s and queried words. Nor have I printed the whole of what is written on the backs of the pages, where it seemed to me that nothing but unnecessary repetition would have been the result.

In the matter of punctuation I have given myself a free hand. I may no doubt have misinterpreted the author's meaning in so doing, but without such punctuation, the number of repellantly crabbed sentences would have been even greater than at present. In dealing with the Essay of 1844, I have corrected some obvious slips without indicating such alterations, because the MS. being legible, there is no danger of changing the author's meaning.

The sections into which the Essay of 1842 is divided are in the original merely indicated by a gap in the MS. or by a line drawn across the page. No titles are given except in the case of § VIII.; and § II. is the only section which has a number in the original. I might equally well have made sections of what are now subsections, *e.g.* *Natural*

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*Selection* p. 7, or *Extermination* p. 28. But since the present sketch is the germ of the Essay of 1844, it seemed best to preserve the identity between the two works, by using such of the author's divisions as correspond to the chapters of the enlarged version of 1844. The geological discussion with which Part II begins corresponds to two chapters (IV and V) of the 1844 Essay. I have therefore described it as §§ iv. and v., although I cannot make sure of its having originally consisted of two sections. With this exception the ten sections of the Essay of 1842 correspond to the ten chapters of that of 1844.

The *Origin of Species* differs from the sketch of 1842 in not being divided into two parts. But the two volumes resemble each other in general structure. Both begin with a statement of what may be called the mechanism of evolution,—variation and selection: in both the argument proceeds from the study of domestic organisms to that of animals and plants in a state of nature. This is followed in both by a discussion of the *Difficulties on Theory* and this by a section *Instinct* which in both cases is treated as a special case of difficulty.

If I had to divide the *Origin* (first edition) into two parts without any knowledge of earlier MS., I should, I think, make Part II begin with Ch. VI, *Difficulties on Theory*. A possible reason why this part of the argument is given in Part I of the Essay of 1842 may be found in the Essay of 1844, where it is clear that the chapter on instinct is placed in Part I because the author thought it of importance to show that heredity and variation occur in mental attributes. The whole question is perhaps an instance of the sort of difficulty which made the author give up the division of his argument into two Parts when he wrote the *Origin*. As matters stand §§ iv. and v. of the 1842 Essay correspond to

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the geological chapters, IX and X, in the *Origin*. From this point onwards the material is grouped in the same order in both works: geographical distribution; affinities and classification; unity of type and morphology; abortive or rudimentary organs; recapitulation and conclusion.

In enlarging the Essay of 1842 into that of 1844, the author retained the sections of the sketch as chapters in the completer presentment. It follows that what has been said of the relation of the earlier Essay to the *Origin* is generally true of the 1844 Essay. In the latter, however, the geological discussion is, clearly instead of obscurely, divided into two chapters, which correspond roughly with Chapters IX and X of the *Origin*. But part of the contents of Chapter X (*Origin*) occurs in Chapter VI (1844) on Geographical Distribution. The treatment of distribution is particularly full and interesting in the 1844 Essay, but the arrangement of the material, especially the introduction of § III. p. 183, leads to some repetition which is avoided in the *Origin*. It should be noted that Hybridism, which has a separate chapter (VIII) in the *Origin*, is treated in Chapter II of the Essay. Finally that Chapter XIII (*Origin*) corresponds to Chapters VII, VIII and IX of the work of 1844.

The fact that in 1842, seventeen years before the publication of the *Origin*, my father should have been able to write out so full an outline of his future work, is very remarkable. In his Autobiography<sup>1</sup> he writes of the 1844 Essay, "But at that time I overlooked one problem of great importance... This problem is the tendency in organic beings descended from the same stock to diverge in character as they become modified." The absence of the principle of divergence is of course also a characteristic of the

<sup>1</sup> *Life and Letters*, i. p. 84.

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sketch of 1842. But at p. 37, the author is not far from this point of view. The passage referred to is: “If any species, *A*, in changing gets an advantage and that advantage...is inherited, *A* will be the progenitor of several genera or even families in the hard struggle of nature. *A* will go on beating out other forms, it might come that *A* would people (the) earth,—we may now not have one descendant on our globe of the one or several original creations<sup>1</sup>.” But if the descendants of *A* have peopled the earth by beating out other forms, they must have diverged in occupying the innumerable diverse modes of life from which they expelled their predecessors. What I wrote<sup>2</sup> on this subject in 1887 is I think true: “Descent with modification implies divergence, and we become so habituated to a belief in descent, and therefore in divergence, that we do not notice the absence of proof that divergence is in itself an advantage.”

The fact that there is no set discussion on the principle of divergence in the 1844 Essay, makes it clear why the joint paper read before the Linnean Society on July 1, 1858, included a letter<sup>3</sup> to Asa Gray, as well as an extract<sup>4</sup> from the Essay of 1844. It is clearly because the letter to Gray includes a discussion on divergence, and was thus, probably, the only document, including this subject, which could be appropriately made use of. It shows once more how great was the importance attached by its author to the principle of divergence.

I have spoken of the hurried and condensed manner in which the sketch of 1842 is written; the style of the later Essay (1844) is more finished.

<sup>1</sup> In the footnotes to the Essay of 1844 attention is called to similar passages.

<sup>2</sup> *Life and Letters*, ii. p. 15.

<sup>3</sup> The passage is given in the *Life and Letters*, ii. p. 124.

<sup>4</sup> The extract consists of the section on *Natural Means of Selection*, p. 87.

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It has, however, the air of an uncorrected MS. rather than of a book which has gone through the ordeal of proof sheets. It has not all the force and conciseness of the *Origin*, but it has a certain freshness which gives it a character of its own. It must be remembered that the *Origin* was an abstract or condensation of a much bigger book, whereas the Essay of 1844 was an expansion of the sketch of 1842. It is not therefore surprising that in the *Origin* there is occasionally evident a chafing against the author's self-imposed limitation. Whereas in the 1844 Essay there is an air of freedom, as if the author were letting himself go, rather than applying the curb. This quality of freshness and the fact that some questions were more fully discussed in 1844 than in 1859, makes the earlier work good reading even to those who are familiar with the *Origin*.

The writing of this Essay "during the summer of 1844," as stated in the Autobiography<sup>1</sup>, and "from memory," as Darwin says elsewhere<sup>2</sup>, was a remarkable achievement, and possibly renders more conceivable the still greater feat of the writing of the *Origin* between July 1858 and September 1859.

It is an interesting subject for speculation: what influence on the world the Essay of 1844 would have exercised, had it been published in place of the *Origin*. The author evidently thought of its publication in its present state as an undesirable expedient, as appears clearly from the following extracts from the *Life and Letters*, vol. ii. pp. 16—18:

<sup>1</sup> *Life and Letters*, i. p. 84.

<sup>2</sup> *Life and Letters*, ii. p. 18.

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*C. Darwin to Mrs Darwin.*Down, *July 5, 1844.*

“...I have just finished my sketch of my species theory. If, as I believe, my theory in time be accepted even by one competent judge, it will be a considerable step in science.

“I therefore write this in case of my sudden death, as my most solemn and last request, which I am sure you will consider the same as if legally entered in my will, that you will devote £400 to its publication, and further will yourself, or through Hensleigh<sup>1</sup>, take trouble in promoting it. I wish that my sketch be given to some competent person, with this sum to induce him to take trouble in its improvement and enlargement. I give to him all my books on Natural History, which are either scored or have references at the end to the pages, begging him carefully to look over and consider such passages as actually bearing, or by possibility bearing, on this subject. I wish you to make a list of all such books as some temptation to an editor. I also request that you will hand over (to) him all those scraps roughly divided into eight or ten brown paper portfolios. The scraps, with copied quotations from various works, are those which may aid my editor. I also request that you, or some amanuensis, will aid in deciphering any of the scraps which the editor may think possibly of use. I leave to the editor’s judgment whether to interpolate these facts in the text, or as notes, or under appendices. As the looking over the references and scraps will be a long labour, and as the *correcting* and enlarging and altering my sketch will also take considerable time, I leave this sum of £400 as some remuneration, and any profits from

<sup>1</sup> Mrs Darwin’s brother.