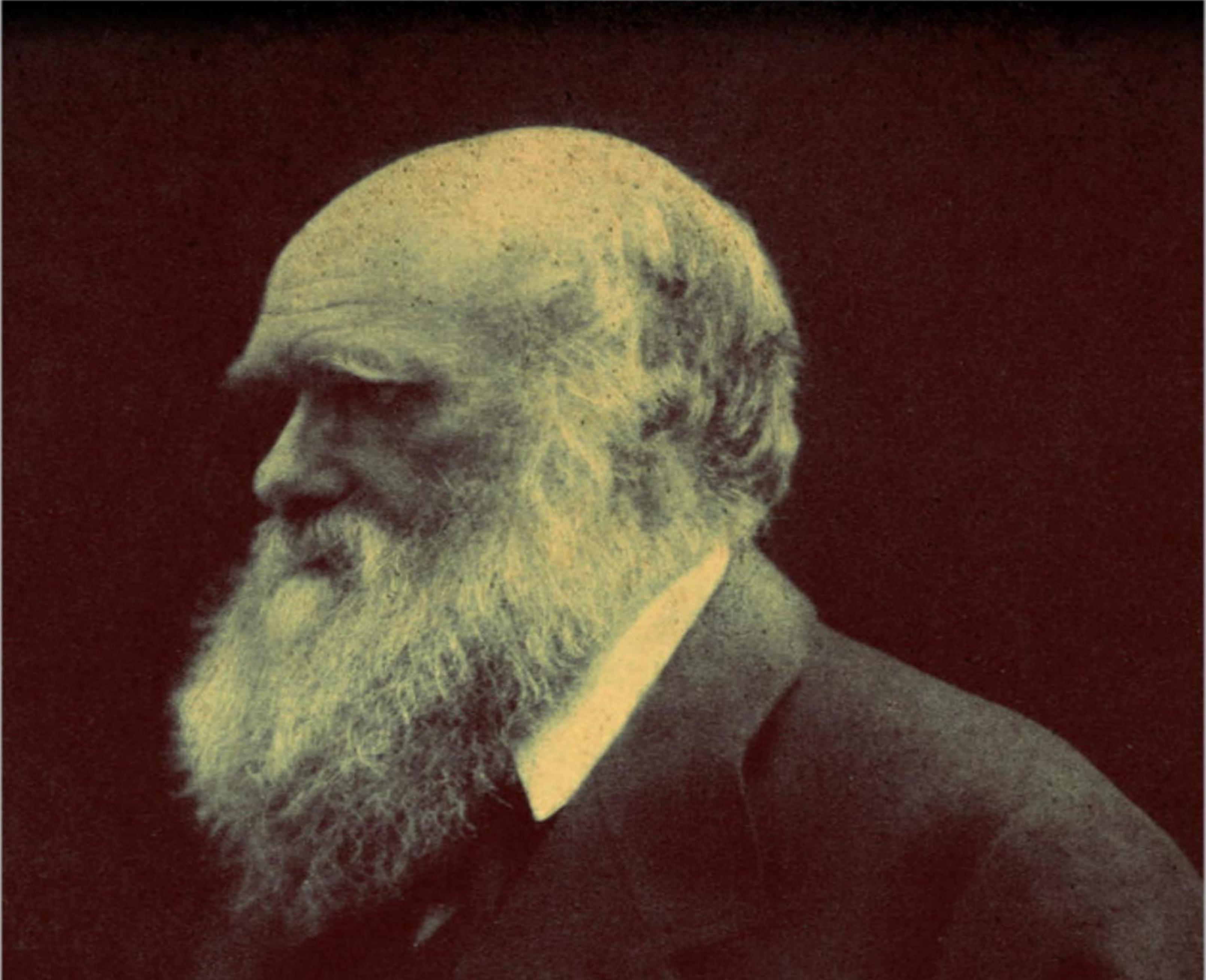

THE FOUNDATIONS OF THE ORIGIN OF SPECIES

CHARLES DARWIN

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INTRODUCTION

We know from the contents of Charles Darwin’s Note Book of 1837 that he was at that time a convinced Evolutionist [1]. 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 [2]. But believe he did, and it is certain (as Huxley has forcibly pointed out [3]) 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 [4]:—

“ Propagation explains why modern animals same type as extinct, which is law almost proved. 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 [5] 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 [6] held that these facts could not have produced their essential effect until the voyage had 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 [7] 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 fossilcollecting 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 {8}” had not yet attained any 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* {9}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 {10}” 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 naturally more vivid to him. In his letter [11] 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 [12] 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, 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 [13] of the importance of the survival of the fittest. "With respect to extinction, we can easily see that a variety of the ostrich (Petise [14]), may not be well adapted, and thus perish out; or on the other hand, like Orpheus [15], 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 [16]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 [17], 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* .

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 fathers 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 [18].” 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 [19]. 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 [20](Jan. 25, 1859) he speaks of his own contributions to the Linnean paper [21]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 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,—fœtal 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 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 [22]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 [23]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.

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 [24]. 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 «?». 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 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 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 [25] 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 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 [26].” 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 [27] 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 [28] to Asa Gray, as well as an extract [29] 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. 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 [30], and “from memory,” as Darwin says elsewhere [31], 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:

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 [32], 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 the work. I consider that for this the editor is bound to get the sketch published either at a publisher's or his own risk. Many of the scraps in the portfolios contain mere rude suggestions and early views, now useless, and many of the facts will probably turn out as having no bearing on my theory.

“ With respect to editors, Mr Lyell would be the best if he would undertake it; I believe he would find the work pleasant, and he would learn some facts new to him. As the editor must be a geologist as well as a naturalist, the next best editor would be Professor Forbes of London. The next best (and quite best in many respects) would be Professor Henslow. Dr Hooker would be *very* good. The next, Mr Strickland {33}. If none of these would undertake it, I would request you to consult with Mr Lyell, or some other capable man, for some editor, a geologist and naturalist. Should one other hundred pounds make the difference of procuring a good editor, I request earnestly that you will raise £500.

“ My remaining collections in Natural History may be given to any one or any museum where «they» would be accepted....”

«The following note seems to have formed part of the original letter, but may have been of later date:»

“ Lyell, especially with the aid of Hooker (and of any good zoological aid), would be best of all. Without an editor will pledge himself to give up time to it, it would be of no use paying such a sum.

“ If there should be any difficulty in getting an editor who would go thoroughly into the subject, and think of the bearing of the passages marked in the books and copied out of scraps of paper, then let my sketch be published as it is, stating that it was done several years ago {34}, and from memory without consulting any works, and with no intention of publication in its present form.”

The idea that the sketch of 1844 might remain, in the event of his death, as the only record of his work, seems to have been long in his mind, for in August, 1854, when he had finished with the Cirripedes, and was thinking of beginning his "species work," he added on the back of the above letter, "Hooker by far best man to edit my species volume. August 1854."

I have called attention in footnotes to many points in which the *Origin* agrees with the *Foundations* . One of the most interesting is the final sentence, practically the same in the Essays of 1842 and 1844, and almost identical with the concluding words of the *Origin* . I have elsewhere pointed out [35]that the ancestry of this eloquent passage may be traced one stage further back,—to the Note Book of 1837. I have given this sentence as an appropriate motto for the *Foundations* in its character of a study of general laws. It will be remembered that a corresponding motto from Whewell's *Bridgewater Treatise* is printed opposite the title-page of the *Origin of Species* .

Mr Huxley who, about the year 1887, read the Essay of 1844, remarked that "much more weight is attached to the influence of external conditions in producing variation and to the inheritance of acquired habits than in the *Origin* ." In the *Foundations* the effect of conditions is frequently mentioned, and Darwin seems to have had constantly in mind the need of referring each variation to a cause. But I gain the impression that the slighter prominence given to this view in the *Origin* was not due to change of opinion, but rather because he had gradually come to take this view for granted; so that in the scheme of that book, it was overshadowed by considerations which then seemed to him more pressing. With regard to the inheritance of acquired characters I am not inclined to agree with Huxley. It is certain that the *Foundations* contains strong recognition of the importance of germinal variation, that is of external conditions acting indirectly through the “reproductive functions.” He evidently considered this as more important than the inheritance of habit or other acquired peculiarities.

Another point of interest is the weight he attached in 1842-4 to “sports” or what are now called “mutations.” This is I think more prominent in the *Foundations* than in the first edition of the *Origin* , and certainly than in the fifth and sixth editions.

Among other interesting points may be mentioned the “good effects of crossing” being “possibly analogous to good effects of change in condition,”—a principle which he upheld on experimental grounds in his *Cross and Self-Fertilisation* in 1876.

In conclusion, I desire to express my thanks to Mr Wallace for a footnote he was good enough to supply: and to Professor Bateson, Sir W. Thiselton-Dyer, Dr Gadow, Professor Judd, Dr Marr, Col. Prain and Dr Stapf for information on various points. I am also indebted to Mr Rutherford, of the University Library, for his careful copy of the manuscript of 1842.

PART I.

§ I. «On Variation under Domestication, and on the Principles of Selection.»

An individual organism placed under new conditions [often] sometimes varies in a small degree and in very trifling respects such as stature, fatness, sometimes colour, health, habits in animals and probably disposition. Also habits of life develope certain parts. Disuse atrophies. [Most of these slight variations tend to become hereditary.]

When the individual is multiplied for long periods by buds the variation is yet small, though greater and occasionally a single bud or individual departs widely from its type (example) [36]and continues steadily to propagate, by buds, such new kind.

When the organism is bred for several generations under new or varying conditions, the variation is greater in amount and endless in kind [especially [37]holds good when individuals have long been exposed to new conditions]. The nature of the external conditions tends to effect some definite change in all or greater part of offspring,—little food, small size—certain foods harmless &c. &c. organs affected and diseases—extent unknown. A certain degree of variation (Müller's twins) [38]seems inevitable effect of process of reproduction. But more important is that simple «?» generation, especially under new conditions [when no crossing] «causes» infinite variation and not direct effect of external conditions, but only in as much as it affects the reproductive functions [39]. There seems to be no part (*beau ideal* of liver) [40]of body, internal or external, or mind or habits, or instincts which does not vary in some small degree and [often] some «?» to a great amount.

[All such] variations [being congenital] or those very slowly acquired of all kinds [decidedly evince a tendency to become hereditary], when not so become simple variety, when it does a race. Each {41}parent transmits its peculiarities, therefore if varieties allowed freely to cross, except by the *chance* of two characterized by same peculiarity happening to marry, such varieties will be constantly demolished {42}. All bisexual animals must cross, hermaphrodite plants do cross, it seems very possible that hermaphrodite animals do cross,—conclusion strengthened: ill effects of breeding in and in, good effects of crossing possibly analogous to good effects of change in condition «?» {43}.

Therefore if in any country or district all animals of one species be allowed freely to cross, any small tendency in them to vary will be constantly counteracted. Secondly reversion to parent form—analogue of *vis medicatrix* {44}. But if man selects, then new races rapidly formed,—of late years systematically followed,—in most ancient times often practically followed {45}. By such selection make race-horse, dray-horse—one cow good for tallow, another for eating &c.—one plant's good lay «illegible» in leaves another in fruit &c. &c.: the same plant to supply his wants at different times of year. By former means animals become adapted, as a direct effect to a cause, to external conditions, as size of body to amount of food. By this latter means they may also be so adapted, but further they may be adapted to ends and pursuits, which by no possibility can affect growth, as existence of tallow-chandler cannot tend to make fat. In such selected races, if not removed to new conditions, and «if» preserved from all cross, after several generations become very true, like each other and not varying. But man {46}selects only «?» what is useful and curious—has bad judgment, is capricious,—grudges to destroy those that do not come up to his pattern,—has no [knowledge] power of selecting according to internal variations,—can hardly keep his conditions uniform,—

[cannot] does not select those best adapted to the conditions under which «the» form «?» lives, but those most useful to him. This might all be otherwise.

§ II. «On Variation in a State of Nature and on the Natural Means of Selection.»

Let us see how far above principles of variation apply to wild animals. Wild animals vary exceedingly little—yet they are known as individuals {47}. British Plants, in many genera number quite uncertain of varieties and species: in shells chiefly external conditions {48}. Primrose and cowslip. Wild animals from different [countries can be recognized]. Specific character gives some organs as varying. Variations analogous in kind, but less in degree with domesticated animals—chiefly external and less important parts.

Our experience would lead us to expect that any and every one of these organisms would vary if «the organism were» taken away «?» and placed under new conditions. Geology proclaims a constant round of change, bringing into play, by every possible «?» change of climate and the death of pre-existing inhabitants, endless variations of new conditions. These «?» generally very slow, doubtful though «illegible» how far the slowness «?» would produce tendency to vary. But Geolog«ists» show change in configuration which, together with the accidents of air and water and the means of transportal which every being possesses, must occasionally bring, rather suddenly, organism to new conditions and «?» expose it for several generations. Hence «?» we should expect every now and then a wild form to vary {49}; possibly this may be cause of some species varying more than others.

According to nature of new conditions, so we might expect all or majority of organisms born under them to vary in some definite way. Further we might expect that the mould in which they are cast would likewise vary in some small degree. But is there any means of selecting those offspring which vary in the same manner, crossing them and keeping their offspring separate and thus producing selected races: otherwise as the wild animals freely cross, so must such small heterogeneous varieties be constantly counter-balanced and lost, and a uniformity of character [kept up] preserved. The former variation as the direct and necessary effects of causes, which we can see can act on them, as size of body from amount of food, effect of certain kinds of food on certain parts of bodies &c. &c.; such new varieties may then become adapted to those external [natural] agencies which act on them. But can varieties be produced adapted to end, which cannot possibly influence their structure and which it is absurd to look «at» as effects of chance. Can varieties like some vars of domesticated animals, like almost all wild species be produced adapted by exquisite means to prey on one animal or to escape from another,—or rather, as it puts out of question effects of intelligence and habits, can a plant become adapted to animals, as a plant which cannot be impregnated without agency of insect; or hooked seeds depending on animal"s existence: woolly animals cannot have any direct effect on seeds of plant. This point which all theories about climate adapting woodpecker [50] to crawl «?» up trees, «illegible» miseltoe, «sentence incomplete». But if every part of a plant or animal was to vary «illegible», and if a being infinitely more sagacious than man (not an omniscient creator) during thousands and thousands of years were to select all the variations which tended towards certain ends ([or were to produce causes «?» which tended to the same end]), for instance, if he foresaw a canine animal would be better off, owing to the country producing more hares, if he were longer legged and keener sight,—greyhound produced [51]. If he saw that aquatic «animal would need» skinned toes. If for some unknown cause he found it would advantage a plant, which «?» like most plants is occasionally visited by bees &c.: if that plant's seed were occasionally eaten by birds and were then carried on to rotten trees, he might select trees with fruit more agreeable to such birds as perched, to ensure their being carried to trees; if he perceived those birds more often dropped the seeds, he might well have selected a bird who would «illegible» rotten trees or [gradually select plants which «he» had proved to live on less and less rotten trees]. Who, seeing how plants vary in garden, what blind foolish man has done {52}in a few years, will deny an all-seeing being in thousands of years could effect (if the Creator chose to do so), either by his own direct foresight or by intermediate means,—which will represent «?» the creator of this universe. Seems usual means. Be it remembered I have nothing to say about life and mind and *all* forms descending from one common type {53}. I speak of the variation of the existing great divisions of the organised kingdom, how far I would go, hereafter to be seen.

Before considering whether «there» be any natural means of selection, and secondly (which forms the 2nd Part of this sketch) the far more important point whether the characters and relations of animated «things» are such as favour the idea of wild species being races «?» descended from a common stock, as the varieties of potato or dahlia or cattle having so descended, let us consider probable character of [selected races] wild varieties.

Natural Selection. De Candolle's war of nature,—seeing contented face of nature,—may be well at first doubted; we see it on borders of perpetual cold {54}. But considering the enormous geometrical power of increase in every organism and as «?» every country, in ordinary cases «countries» must be stocked to full extent, reflection will show that this is the case. Malthus on man,—in animals no moral [check] restraint «?»—they breed in time of year when provision most abundant, or season most favourable, every country has its seasons,—calculate robins,—oscillating from years of destruction [55]. If proof were wanted let any singular change of climate «occur» here «?», how astoundingly some tribes «?» increase, also introduced animals [56], the pressure is always ready,—capacity of alpine plants to endure other climates,—think of endless seeds scattered abroad,—forests regaining their percentage [57],—a thousand wedges [58]are being forced into the œconomy of nature. This requires much reflection; study Malthus and calculate rates of increase and remember the resistance,—only periodical.

The unavoidable effect of this «is» that many of every species are destroyed either in egg or [young or mature (the former state the more common)]. In the course of a thousand generations infinitesimally small differences must inevitably tell [59]; when unusually cold winter, or hot or dry summer comes, then out of the whole body of individuals of any species, if there be the smallest differences in their structure, habits, instincts [senses], health &c., «it» will on an average tell; as conditions change a rather larger proportion will be preserved: so if the chief check to increase falls on seeds or eggs, so will, in the course of 1000 generations or ten thousand, those seeds (like one with down to fly [60]) which fly furthest and get scattered most ultimately rear most plants, and such small differences tend to be hereditary like shades of expression in human countenance. So if one parent «?» fish deposits its egg in infinitesimally different circumstances, as in rather shallower or deeper water &c., it will then «?» tell.

Let hares [61]increase very slowly from change of climate affecting peculiar plants, and some other «illegible» rabbit decrease in same proportion [let this unsettle organisation of], a canine animal, who formerly derived its chief sustenance by springing on rabbits or running them by scent, must decrease too and might thus readily become exterminated. But if its form varied very slightly, the long legged fleet ones, during a thousand years being selected, and the less fleet rigidly destroyed must, if no law of nature be opposed to it, alter forms.

Remember how soon Bakewell on the same principle altered cattle and Western, sheep,—carefully avoiding a cross (pigeons) with any breed. We cannot suppose that one plant tends to vary in fruit and another in flower, and another in flower and foliage,—some have been selected for both fruit and flower: that one animal varies in its covering and another not,—another in its milk. Take any organism and ask what is it useful for and on that point it will be found to vary,—cabbages in their leaf,—corn in size «and» quality of grain, both in times of year,—kidney beans for young pod and cotton for envelope of seeds &c. &c.: dogs in intellect, courage, fleetness and smell «?»: pigeons in peculiarities approaching to monsters. This requires consideration,—should be introduced in first chapter if it holds, I believe it does. It is hypothetical at best [62].

Nature's variation far less, but such selection far more rigid and scrutinising. Man's races not [even so well] only not better adapted to conditions than other races, but often not «?» one race adapted to its conditions, as man keeps and propagates some alpine plants in garden. Nature lets «an» animal live, till on actual proof it is found less able to do the required work to serve the desired end, man judges solely by his eye, and knows not whether nerves, muscles, arteries, are developed in proportion to the change of external form.

Besides selection by death, in bisexual animals «illegible» the selection in time of fullest vigour, namely struggle of males; even in animals which pair there seems a surplus