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Autobiographical Chapter, Volume 2
Charles Darwin
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LIFE AND LETTERS
OF
CHARLES DARWIN.

—♦—
CHAPTER I.

THE FOUNDATIONS OF THE 'ORIGIN OF SPECIES.'

[IN the first volume, p. 82, the growth of the 'Origin of Species' has been briefly described in my father's words. The letters given in the present and following chapters will illustrate and amplify the history thus sketched out.

It is clear that, in the early part of the voyage of the *Beagle* he did not feel it inconsistent with his views to express himself in thoroughly orthodox language as to the genesis of new species. Thus in 1834 he wrote* at Valparaiso: "I have already found beds of recent shells yet retaining their colour at an elevation of 1300 feet, and beneath the level country is strewn with them. It seems not a very improbable conjecture that the want of animals may be owing to none having been created since this country was raised from the sea."

This passage does not occur in the published 'Journal,' the last proof of which was finished in 1837; and this fact harmonizes with the change we know to have been proceeding in his views. But in the published 'Journal' we find passages which show a point of view more in accordance with orthodox

* MS. Journals, p. 468.

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theological natural history than with his later views. Thus, in speaking of the birds *Synallaxis* and *Scytalopus* (1st edit. p. 353; 2nd edit. p. 289), he says: "When finding, as in this case, any animal which seems to play so insignificant a part in the great scheme of nature, one is apt to wonder why a distinct species should have been created."

A comparison of the two editions of the 'Journal' is instructive, as giving some idea of the development of his views on evolution. It does not give us a true index of the mass of conjecture which was taking shape in his mind, but it shows us that he felt sure enough of the truth of his belief to allow a stronger tinge of evolution to appear in the second edition. He has mentioned in the Autobiography (p. 83), that it was not until he read Malthus that he got a clear view of the potency of natural selection. This was in 1838—a year after he finished the first edition (it was not published until 1839), and seven years before the second edition was written (1845). Thus the turning-point in the formation of his theory took place between the writing of the two editions.

I will first give a few passages which are practically the same in the two editions, and which are, therefore, chiefly of interest as illustrating his frame of mind in 1837.

The case of the two species of *Molothrus* (1st edit. p. 61; 2nd edit. p. 53) must have been one of the earliest instances noticed by him of the existence of representative species—a phenomenon which we know ('Autobiography,' p. 83) struck him deeply. The discussion on introduced animals (1st edit. p. 139; 2nd edit. p. 120) shows how much he was impressed by the complicated interdependence of the inhabitants of a given area.

An analogous point of view is given in the discussion (1st edit. p. 98; 2nd edit. p. 85) of the mistaken belief that large animals require, for their support, a luxuriant vegetation; the incorrectness of this view is illustrated by the com-

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parison of the fauna of South Africa and South America, and the vegetation of the two continents. The interest of the discussion is that it shows clearly our *à priori* ignorance of the conditions of life suitable to any organism.

There is a passage which has been more than once quoted as bearing on the origin of his views. It is where he discusses the striking difference between the species of mice on the east and west of the Andes (1st edit. p. 399): "Unless we suppose the same species to have been created in two different countries, we ought not to expect any closer similarity between the organic beings on the opposite sides of the Andes than on shores separated by a broad strait of the sea." In the 2nd edit. p. 327, the passage is almost verbally identical, and is practically the same.

There are other passages again which are more strongly evolutionary in the 2nd edit., but otherwise are similar to the corresponding passages in the 1st edition. Thus, in describing the blind Tuco-tuco (1st edit. p. 60; 2nd edit. p. 52), in the first edition he makes no allusion to what Lamarck might have thought, nor is the instance used as an example of modification, as in the edition of 1845.

A striking passage occurs in the 2nd edit. (p. 173) on the relationship between the "extinct edentata and the living sloths, ant-eaters, and armadillos."

"This wonderful relationship in the same continent between the dead and the living, will, I do not doubt, hereafter throw more light on the appearance of organic beings on our earth, and their disappearance from it, than any other class of facts."

This sentence does not occur in the 1st edit., but he was evidently profoundly struck by the disappearance of the gigantic forerunners of the present animals. The difference between the discussions in the two editions is most instructive. In both, our ignorance of the conditions of life is insisted on, but in the second edition, the discussion is made to lead up to a strong statement of the intensity of the struggle for life.

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Then follows a comparison between rarity* and extinction, which introduces the idea that the preservation and dominance of existing species depend on the degree in which they are adapted to surrounding conditions. In the first edition, he is merely "tempted to believe in such simple relations as variation of climate and food, or introduction of enemies, or the increased number of other species, as the cause of the succession of races." But finally (1st edit.) he ends the chapter by comparing the extinction of a species to the exhaustion and disappearance of varieties of fruit-trees, as though he thought that a mysterious term of life was impressed on each species at its creation.

The difference of treatment of the Galapagos problem is of some interest. In the earlier book, the American type of the productions of the islands is noticed, as is the fact that the different islands possess forms specially their own, but the importance of the whole problem is not so strongly put forward. Thus, in the first edition, he merely says:—

"This similarity of type between distant islands and continents, while the species are distinct, has scarcely been sufficiently noticed. The circumstance would be explained, according to the views of some authors, by saying that the creative power had acted according to the same law over a wide area."—(1st edit. p. 474.)

This passage is not given in the second edition, and the generalisations on geographical distribution are much wider and fuller. Thus he asks:—

"Why were their aboriginal inhabitants, associated . . . in different proportions both in kind and number from those on the Continent, and therefore acting on each other in a different manner—why were they created on American types of organisation?"—(2nd edit. p. 393.)

* In the second edition, p. 146, of our ignorance of the causes of the destruction of Niata cattle by droughts is given as a good example of rarity or extinction. The passage does not occur in the first edition.

The same difference of treatment is shown elsewhere in this chapter. Thus the gradation in the form of beak presented by the thirteen allied species of finch is described in the first edition (p. 461) without comment. Whereas in the second edition (p. 380) he concludes:—

“One might really fancy that from an original paucity of birds in this Archipelago, one species has been taken and modified for different ends.”

On the whole it seems to me remarkable that the difference between the two editions is not greater; it is another proof of the author's caution and self-restraint in the treatment of his theory. After reading the second edition of the ‘Journal,’ we find with a strong sense of surprise how far developed were his views in 1837. We are enabled to form an opinion on this point from the note-books in which he wrote down detached thoughts and queries. I shall quote from the first note-book, completed between July 1837 and February 1838: and this is the more worth doing, as it gives us an insight into the condition of his thoughts before the reading of Malthus. The notes are written in his most hurried style, so many words being omitted, that it is often difficult to arrive at the meaning. With a few exceptions (indicated by square brackets)* I have printed the extracts as written; the punctuation, however, has been altered, and a few obvious slips corrected where it seemed necessary. The extracts are not printed in order, but are roughly classified.†

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

“We can see why structure is common in certain countries

* In the extracts from the note-book ordinary brackets represent my father's parentheses.

† On the first page of the note-book, is written “Zoonomia”; this seems to refer to the first few pages in which reproduction by gemma-

tion is discussed, and where the “Zoonomia” is mentioned. Many pages have been cut out of the note-book, probably for use in writing the Sketch of 1844, and these would have no doubt contained the most interesting extracts.

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when we can hardly believe necessary, but if it was necessary to one forefather, the result would be as it is. Hence antelopes at Cape of Good Hope ; marsupials at Australia."

"Countries longest separated greatest differences—if separated from immersage, possibly two distinct types, but each having its representatives—as in Australia."

"Will this apply to whole organic kingdom when our planet first cooled?"

The two following extracts show that he applied the theory of evolution to the "whole organic kingdom" from plants to man.

"If we choose to let conjecture run wild, then animals, our fellow brethren in pain, disease, death, suffering and famine—our slaves in the most laborious works, our companions in our amusements—they may partake [of?] our origin in one common ancestor—we may be all melted together."

"The different intellects of man and animals not so great as between living things without thought (plants), and living things with thought (animals)."

The following extracts are again concerned with an *à priori* view of the probability of the origin of species by descent—"propagation," as he called it.

"The tree of life should perhaps be called the coral of life, base of branches dead ; so that passages cannot be seen."

"There never may have been grade between pig and tapir, yet from some common progenitor. Now if the intermediate ranks had produced infinite species, probably the series would have been more perfect."

At another place, speaking of intermediate forms, he says :—

"Cuvier objects to propagation of species by saying, why have not some intermediate forms been discovered between Palæotherium, Megalonyx, Mastodon, and the species now living? Now according to my view (in S. America) parent of all Armadilloes might be brother to Megatherium—uncle now dead."

Speaking elsewhere of intermediate forms, he remarks:—

“Opponents will say—*show them me*. I will answer yes, if you will show me every step between bulldog and greyhound.”

Here we see that the case of domestic animals was already present in his mind as bearing on the production of natural species. The disappearance of intermediate forms naturally leads up to the subject of extinction, with which the next extract begins.

“It is a wonderful fact, horse, elephant, and mastodon, dying out about same time in such different quarters.

“Will Mr. Lyell say that some [same?] circumstance killed it over a tract from Spain to South America?—(Never.)

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

“Why does individual die? To perpetuate certain peculiarities (therefore adaptation), and obliterate accidental varieties, and to accommodate itself to change (for, of course, change, even in varieties, is accommodation). Now this argument applies to species.

“If individual cannot propagate he has no issue—so with species.

“If *species* generate other *species*, their race is not utterly cut off:—like golden pippins, if produced by seed, go on—otherwise all die.

“The fossil horse generated, in South Africa, zebra—and continued—perished in America.

“All animals of same species are bound together just like buds of plants, which die at one time, though produced either sooner or later. Prove animals like plants—trace gradation between associated and non-associated animals—and the story will be complete.”

Here we have the view already alluded to of a term of life impressed on a species.

But in the following note we get extinction connected with

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unfavourable variation, and thus a hint is given of natural selection :—

“ With respect to extinction, we can easily see that [a] variety of [the] ostrich (Petise), may not be well adapted, and thus perish out ; or, on the other hand, like Orpheus [a Galapagos bird], 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 from America) of non-adaptation of circumstances.”

The first part of the next extract has a similar bearing. The end of the passage is of much interest, as showing that he had at this early date visions of the far-reaching character of his speculations :—

“ With belief of transmutation and geographical grouping, we are led to endeavour to discover *causes* of change ; the manner of adaptation (wish of parents??), instinct and structure becomes full of speculation and lines of observation. View of generation being condensation,* test of highest organisation intelligible My theory would give zest to recent and fossil comparative anatomy ; it would lead to the study of instincts, heredity, and mind-heredity, whole [of] metaphysics.

“ It would lead to closest examination of hybridity, regeneration, causes of change in order to know what we have come from and to what we tend—to what circumstances favour crossing and what prevents it—this, and direct examination of direct passages of structure in species, might lead to laws of change, which would then be the main object of study, to guide our speculations.”

The following two extracts have a similar interest ; the

* I imagine him to mean that each generation is “ condensed ” to a small number of the best organized individuals.

second is especially interesting, as it contains the germ of the concluding sentence of the 'Origin of Species': *—

"Before the attraction of gravity discovered it might have been said it was as great a difficulty to account for the movement of all [planets] by one law, as to account for each separate one; so to say that all mammalia were born from one stock, and since distributed by such means as we can recognise, may be thought to explain nothing.

"Astronomers might formerly have said that God fore-ordered each planet to move in its particular destiny. In the same manner God orders each animal created with certain forms in certain countries; but how much more simple and sublime [a] power—let attraction act according to certain law, such are inevitable consequences—let animals be created, then by the fixed laws of generation, such will be their successors.

"Let the powers of transportal be such, and so will be the forms of one country to another—let geological changes go at such a rate, so will be the number and distribution of the species!!"

The three next extracts are of miscellaneous interest :—

"When one sees nipple on man's breast, one does not say some use, but sex not having been determined—so with useless wings under elytra of beetles—born from beetles with wings, and modified—if simple creation merely, would have been born without them."

"In a decreasing population at any one moment fewer closely related (few species of genera); ultimately few genera (for otherwise the relationship would converge sooner), and lastly, perhaps, some one single one. Will not this account

* 'Origin of Species' (edit. i.), p. 490 :—"There is a grandeur in this view of life, with its several powers, having been originally breathed into a few forms or into one; and that whilst this planet has gone cycling on according to the fixed law of gravity, from so simple a beginning endless forms most beautiful and most wonderful have been, and are being evolved."

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for the odd genera with few species which stand between great groups, which we are bound to consider the increasing ones?"

The last extract which I shall quote gives the germ of his theory of the relation between alpine plants in various parts of the world, in the publication of which he was forestalled by E. Forbes (see Vol. I. p. 88). He says, in the 1837 note-book, that alpine plants, "formerly descended lower, therefore [they are] species of lower genera altered, or northern plants."

When we turn to the Sketch of his theory, written in 1844 (still therefore before the second edition of the 'Journal' was completed), we find an enormous advance made on the note-book of 1837. The Sketch is in fact a surprisingly complete presentation of the argument afterwards familiar to us in the 'Origin of Species.' There is some obscurity as to the date of the short Sketch which formed the basis of the 1844 Essay. We know from his own words (Vol. I. p. 184), that it was in June 1842 that he first wrote out a short sketch of his views.* This statement is given with so much circumstance that it is almost impossible to suppose that it contains an error of date. It agrees also with the following extract from his 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 species theory."

Again in the introduction to the 'Origin,' p. 1, he writes, "after an interval of five years' work," [from 1837, *i.e.* in 1842,] "I allowed myself to speculate on the subject, and drew up some short notes."

Nevertheless in the letter signed by Sir C. Lyell and Sir J. D. Hooker, which serves as an introduction to the joint paper of Messrs. C. Darwin and A. Wallace on the 'Tendency

* This version I cannot find, and much of his MS., after it had been enlarged and re-copied in 1844. it was probably destroyed, like so