ON THE ORIGIN OF SPECIES

(ILLUSTRATED)

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[EVOLUTION GLOSSARY ADDED]

By

Charles Darwin

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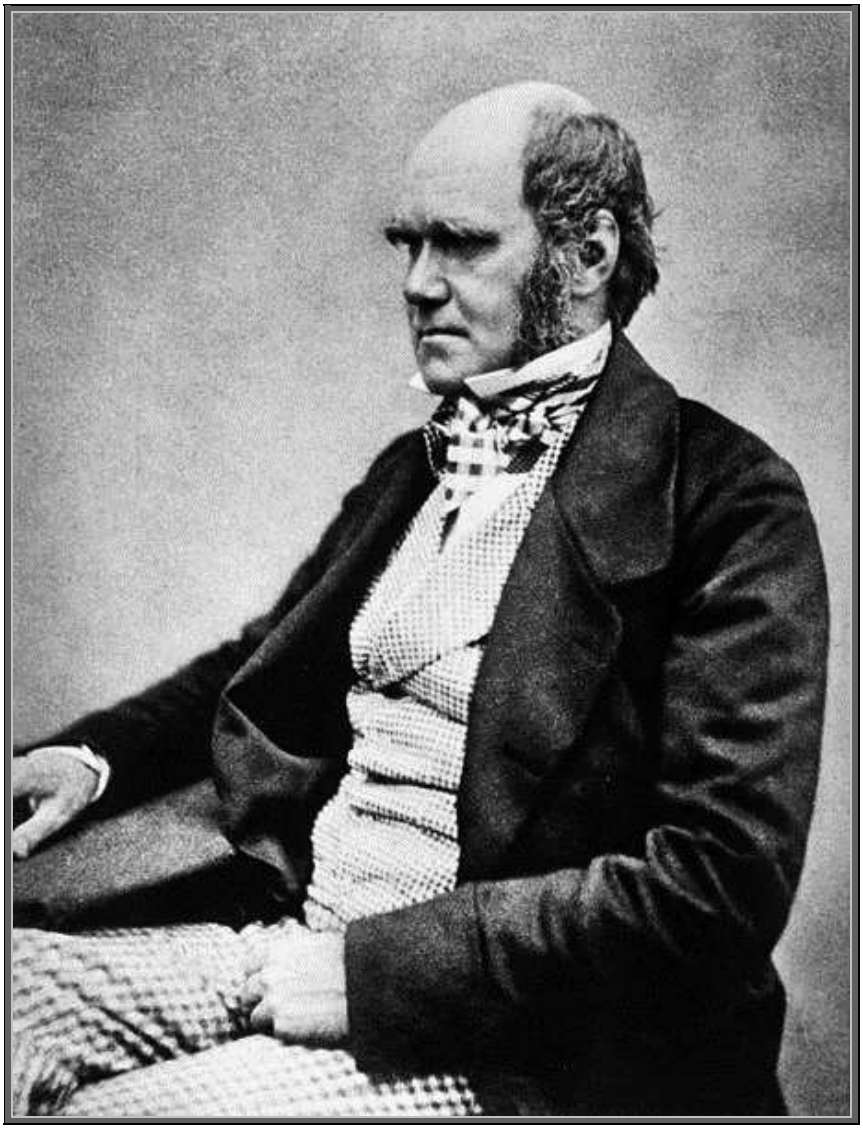
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ABOUT AUTHOR



Darwin, aged 45 in 1854, by then working towards publication of *On the Origin of Species*

Charles Robert Darwin, (12 February 1809 – 19 April 1882) was an English naturalist and geologist, best known for his contributions to evolutionary theory.

He established that all species of life have descended over time from common ancestors, and in a joint publication with Alfred Russel Wallace introduced his scientific theory that this branching pattern of evolution resulted from a process that he called natural selection, in which the struggle for existence has a similar effect to the artificial selection involved in selective breeding.

Darwin published his theory of evolution with compelling evidence in his 1859 book *On the Origin of Species*, overcoming scientific rejection of earlier concepts of transmutation of species. By the 1870s the scientific community and much of the general public had accepted evolution as a fact. However, many favoured competing explanations and it was not until the emergence of the modern evolutionary synthesis from the 1930s to the 1950s that a broad consensus developed in which natural selection was the basic mechanism of evolution. In modified form, Darwin's scientific discovery is the unifying theory of the life sciences, explaining the diversity of life.

Darwin's early interest in nature led him to neglect his medical education at the University of Edinburgh; instead, he helped to investigate marine invertebrates. Studies at the University of Cambridge encouraged his passion for natural science. His five-year voyage on HMS *Beagle* established him as an eminent geologist whose observations and theories supported Charles Lyell's uniformitarian ideas, and publication of his journal of the voyage made him famous as a popular author.

Puzzled by the geographical distribution of wildlife and fossils he collected on the voyage, Darwin began detailed investigations and in 1838 conceived his theory of natural selection. Although he discussed his ideas with several naturalists, he needed time for extensive research and his geological work had priority. He was writing up his theory in 1858 when Alfred Russel Wallace sent him an essay which described the same idea, prompting immediate joint publication of both of their theories. Darwin's work established evolutionary descent with modification as the dominant scientific explanation of diversification in nature. In 1871 he examined human evolution and sexual selection in *The Descent of Man, and Selection in Relation to Sex*, followed by *The Expression of the Emotions in Man and Animals*. His research

on plants was published in a series of books, and in his final book, he examined earthworms and their effect on soil.

Darwin became internationally famous, and his pre-eminence as a scientist was honoured by burial in Westminster Abbey. Darwin has been described as one of the most influential figures in human history.

Biography

Early life and education

Charles Robert Darwin was born in Shrewsbury, Shropshire, England, on 12 February 1809 at his family home, The Mount. He was the fifth of six children of wealthy society doctor and financier Robert Darwin, and Susannah Darwin (*née* Wedgwood). He was the grandson of two prominent abolitionists: Erasmus Darwin on his father's side, and of Josiah Wedgwood on his mother's side.



The seven-year-old Charles Darwin in 1816.

Both families were largely Unitarian, though the Wedgwoods were adopting Anglicanism. Robert Darwin, himself quietly a freethinker, had baby Charles baptised in November 1809 in the Anglican St Chad's Church, Shrewsbury, but Charles and his siblings attended the Unitarian chapel with their mother. The eight-year-old Charles already had a taste for natural history and collecting when he joined the day school run by its preacher in 1817. That July, his mother died. From September 1818 he joined his older brother Erasmus attending the nearby Anglican Shrewsbury School as a boarder.

Darwin spent the summer of 1825 as an apprentice doctor, helping his father treat the poor of Shropshire, before going to the University of Edinburgh Medical School, at the time the best medical school in the UK, with his brother Erasmus in October 1825. He found lectures dull and surgery distressing, so neglected his studies. He learned taxidermy from John Edmonstone, a freed black slave who had accompanied Charles Waterton in the South American rainforest, and often sat with this "very pleasant and intelligent man".

In Darwin's second year he joined the Plinian Society, a student natural history group whose debates strayed into radical materialism. He assisted Robert Edmond Grant's investigations of the anatomy and life cycle of marine invertebrates in the Firth of Forth, and on 27 March 1827 presented at the Plinian his own discovery that black spores found in oyster shells were the eggs of a skate leech. One day, Grant praised Lamarck's evolutionary ideas. Darwin was astonished by Grant's audacity, but had recently read similar ideas in his grandfather Erasmus' journals. Darwin was rather bored by Robert Jameson's natural history course which covered geology including the debate between Neptunism and Plutonism. He learned classification of plants, and assisted with work on the collections of the University Museum, one of the largest museums in Europe at the time.

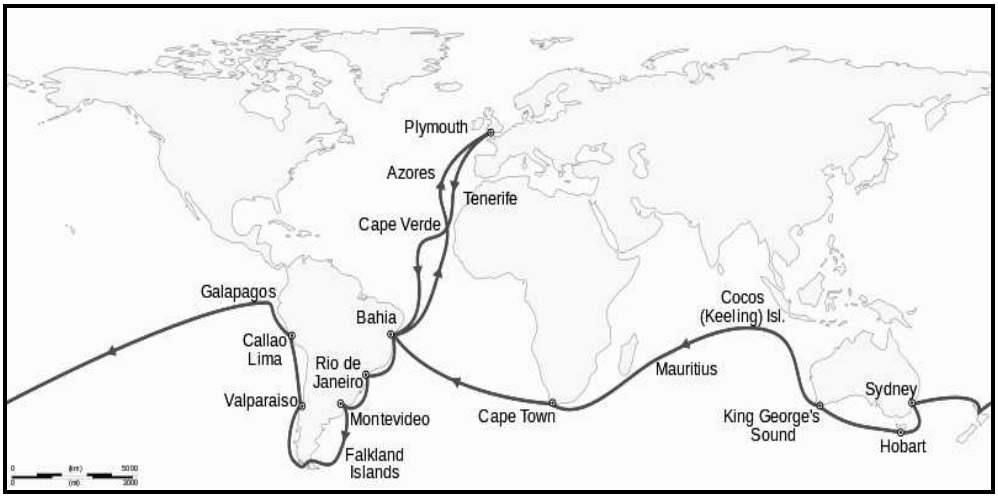
This neglect of medical studies annoyed his father, who shrewdly sent him to Christ's College, Cambridge, for a Bachelor of Arts degree as the first step towards becoming an Anglican parson. As Darwin was unqualified for the *Tripos*, he joined the *ordinary* degree course in January 1828. He preferred riding and shooting to studying. His cousin William Darwin Fox introduced him to the

popular craze for beetle collecting; Darwin pursued this zealously, getting some of his finds published in Stevens' *Illustrations of British entomology*. He became a close friend and follower of botany professor John Stevens Henslow and met other leading naturalists who saw scientific work as religious natural theology, becoming known to these dons as "the man who walks with Henslow". When his own exams drew near, Darwin focused on his studies and was delighted by the language and logic of William Paley's *Evidences of Christianity*. In his final examination in January 1831 Darwin did well, coming tenth out of 178 candidates for the *ordinary* degree.

Darwin had to stay at Cambridge until June. He studied Paley's *Natural Theology*, which made an argument for divine design in nature, explaining adaptation as God acting through laws of nature. He read John Herschel's new book, which described the highest aim of natural philosophy as understanding such laws through inductive reasoning based on observation, and Alexander von Humboldt's *Personal Narrative* of scientific travels. Inspired with "a burning zeal" to contribute, Darwin planned to visit Tenerife with some classmates after graduation to study natural history in the tropics. In preparation, he joined Adam Sedgwick's geology course, then travelled with him in the summer for a fortnight, in order to map strata in Wales.

Voyage of the *Beagle*

After a week with student friends at Barmouth, Darwin returned home on 29 August to find a letter from Henslow proposing him as a suitable (if unfinished) gentleman naturalist for a self-funded supernumerary place on HMS *Beagle* with captain Robert FitzRoy, more as a companion than a mere collector. The ship was to leave in four weeks on an expedition to chart the coastline of South America. Robert Darwin objected to his son's planned two-year voyage, regarding it as a waste of time, but was persuaded by his brother-in-law, Josiah Wedgwood, to agree to (and fund) his son's participation.



The voyage of the *Beagle*

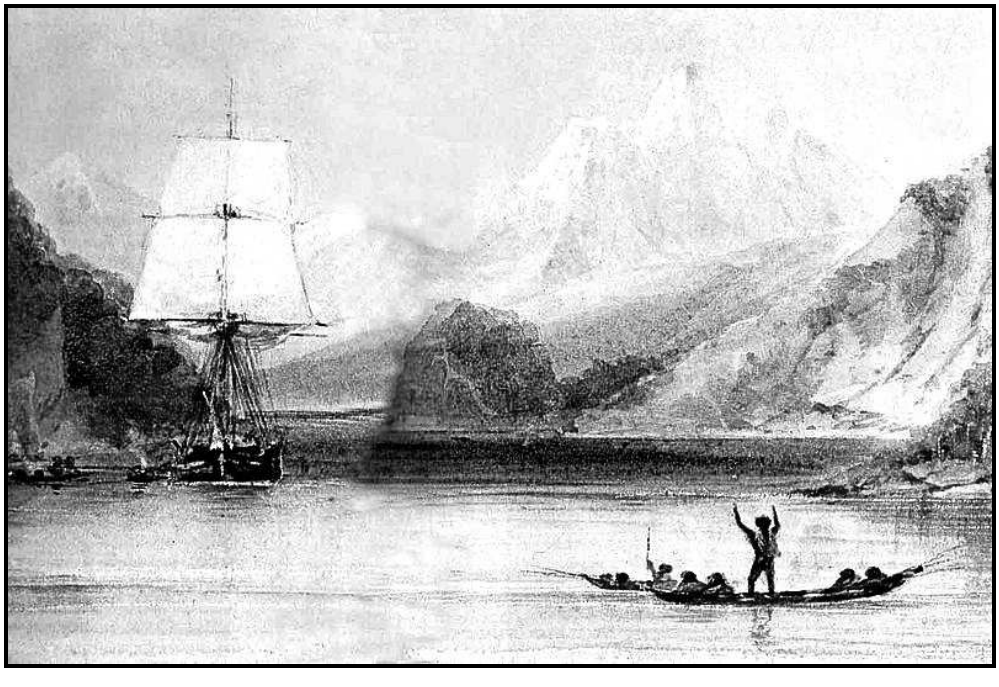
After delays, the voyage began on 27 December 1831; it lasted almost five years. As FitzRoy had intended, Darwin spent most of that time on land investigating geology and making natural history collections, while the *Beagle* surveyed and charted coasts. He kept careful notes of his observations and theoretical speculations, and at intervals during the voyage his specimens were sent to Cambridge together with letters including a copy of his journal for his family. He had some expertise in geology, beetle collecting and dissecting marine invertebrates, but in all other areas was a novice and ably collected specimens for expert appraisal. Despite suffering badly from seasickness, Darwin wrote copious notes while on board the ship. Most of his zoology notes are about marine invertebrates, starting with plankton collected in a calm spell.

On their first stop ashore at St. Jago, Darwin found that a white band high in the volcanic rock cliffs included seashells. FitzRoy had given him the first volume of Charles Lyell's *Principles of Geology* which set out uniformitarian concepts of land slowly rising or falling over immense periods, and Darwin saw things Lyell's way, theorising and thinking of writing a book on geology.

When they reached Brazil Darwin was delighted by the tropical forest, but detested the sight of slavery. The survey continued to the south in Patagonia. They stopped at Bahía Blanca, and in cliffs near Punta Alta Darwin made a major find of fossil bones of huge extinct mammals beside modern seashells, indicating

recent extinction with no signs of change in climate or catastrophe. He identified the little-known *Megatherium* by a tooth and its association with bony armour which had at first seemed to him like a giant version of the armour on local armadillos. The finds brought great interest when they reached England.

On rides with gauchos into the interior to explore geology and collect more fossils, Darwin gained social, political and anthropological insights into both native and colonial people at a time of revolution, and learnt that two types of rhea had separate but overlapping territories. Further south he saw stepped plains of shingle and seashells as raised beaches showing a series of elevations. He read Lyell's second volume and accepted its view of "centres of creation" of species, but his discoveries and theorising challenged Lyell's ideas of smooth continuity and of extinction of species.



As *Beagle* surveyed the coasts of South America, Darwin theorised about geology and extinction of giant mammals.

Three Fuegians on board, who had been seized during the first *Beagle* voyage and had spent a year in England, were taken back to Tierra del Fuego as missionaries. Darwin found them friendly and civilised, yet their relatives seemed "miserable, degraded savages", as different as wild from domesticated animals. To Darwin the difference showed cultural advances, not racial inferiority. Unlike his scientist friends, he now thought there was no unbridgeable gap between humans and animals. A year on, the mission had been abandoned. The Fuegian they had named Jemmy Button lived like the other natives, had a wife, and had no wish to return to England.

Darwin experienced an earthquake in Chile and saw signs that the land had just been raised, including mussel-beds stranded above high tide. High in the Andes he saw seashells, and several fossil trees that had grown on a sand beach. He theorised that as the land rose, oceanic islands sank, and coral reefs round them grew to form atolls.

On the geologically new Galápagos Islands Darwin looked for evidence attaching wildlife to an older "centre of creation", and found mockingbirds allied to those in Chile but differing from island to island. He heard that slight variations in the shape of tortoise shells showed which island they came from, but failed to collect them, even after eating tortoises taken on board as food. In Australia the marsupial rat-kangaroo and the platypus seemed so unusual that Darwin thought it was almost as though two distinct Creators had been at work. He found the Aborigines "good-humoured & pleasant", and noted their depletion by European settlement.

The *Beagle* investigated how the atolls of the Cocos (Keeling) Islands had formed, and the survey supported Darwin's theorising. FitzRoy began writing the official *Narrative* of the *Beagle* voyages, and after reading Darwin's diary he proposed incorporating it into the account. Darwin's *Journal* was eventually rewritten as a separate third volume, on natural history.

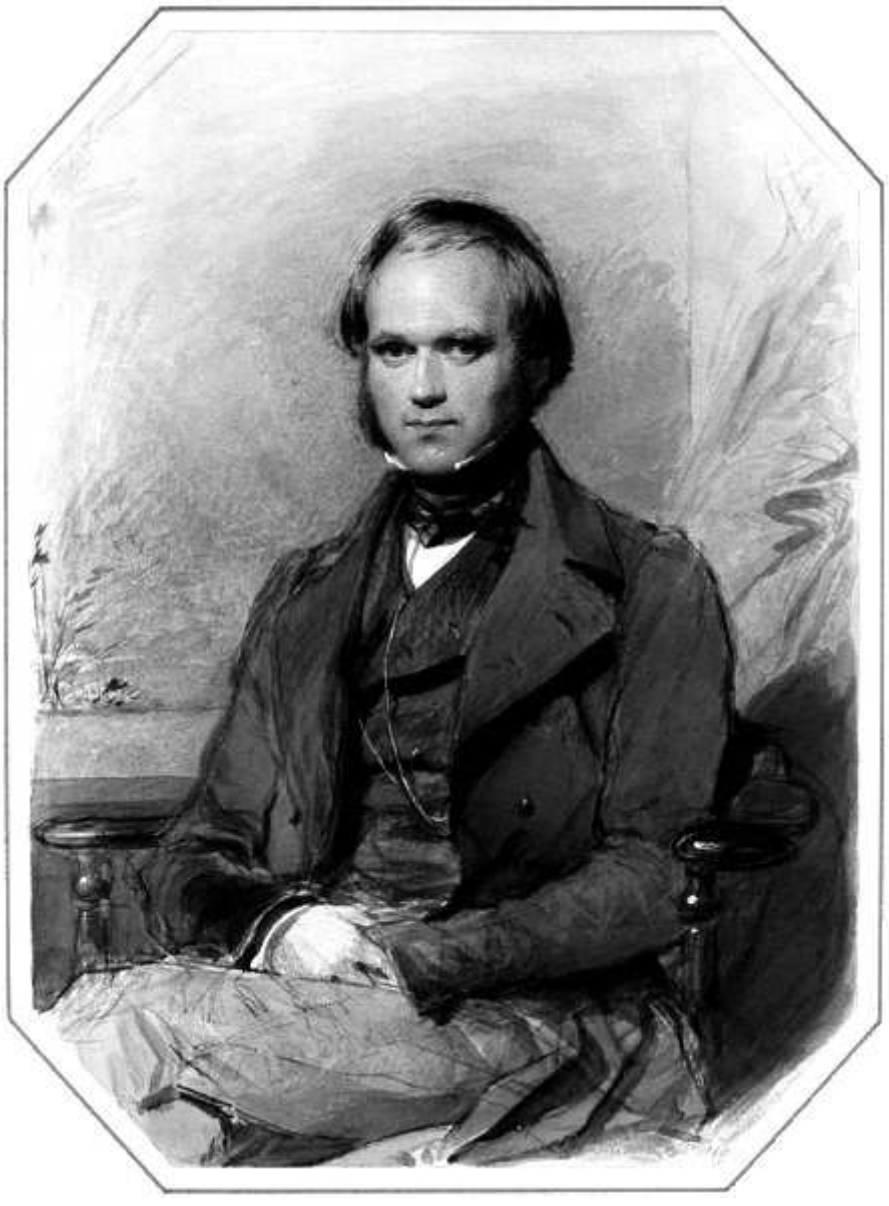
In Cape Town Darwin and FitzRoy met John Herschel, who had recently written to Lyell praising his uniformitarianism as opening bold speculation on "that mystery of mysteries, the replacement of extinct species by others" as "a natural in contradistinction to a miraculous process". When organising his notes

as the ship sailed home, Darwin wrote that if his growing suspicions about the mockingbirds, the tortoises and the Falkland Islands Fox were correct, "such facts undermine the stability of Species", then cautiously added "would" before "undermine". He later wrote that such facts "seemed to me to throw some light on the origin of species".

Inception of Darwin's evolutionary theory

When the *Beagle* reached Falmouth, Cornwall, on 2 October 1836, Darwin was already a celebrity in scientific circles as in December 1835 Henslow had fostered his former pupil's reputation by giving selected naturalists a pamphlet of Darwin's geological letters. Darwin visited his home in Shrewsbury and saw relatives, then hurried to Cambridge to see Henslow, who advised on finding naturalists available to catalogue the collections and agreed to take on the botanical specimens. Darwin's father organised investments, enabling his son to be a self-funded gentleman scientist, and an excited Darwin went round the London institutions being fêted and seeking experts to describe the collections. Zoologists had a huge backlog of work, and there was a danger of specimens just being left in storage.

Charles Lyell eagerly met Darwin for the first time on 29 October and soon introduced him to the up-and-coming anatomist Richard Owen, who had the facilities of the Royal College of Surgeons to work on the fossil bones collected by Darwin. Owen's surprising results included other gigantic extinct ground sloths as well as the *Megatherium*, a near complete skeleton of the unknown *Scelidotherium* and a hippopotamus-sized rodent-like skull named *Toxodon* resembling a giant capybara. The armour fragments were actually from *Glyptodon*, a huge armadillo-like creature as Darwin had initially thought. These extinct creatures were related to living species in South America.



While still a young man, Charles Darwin joined the scientific elite

In mid-December Darwin took lodgings in Cambridge to organise work on his collections and rewrite his *Journal*. He wrote his first paper, showing that the South American landmass was slowly rising, and with Lyell's enthusiastic backing read it to the Geological Society of London on 4 January 1837. On the same day,

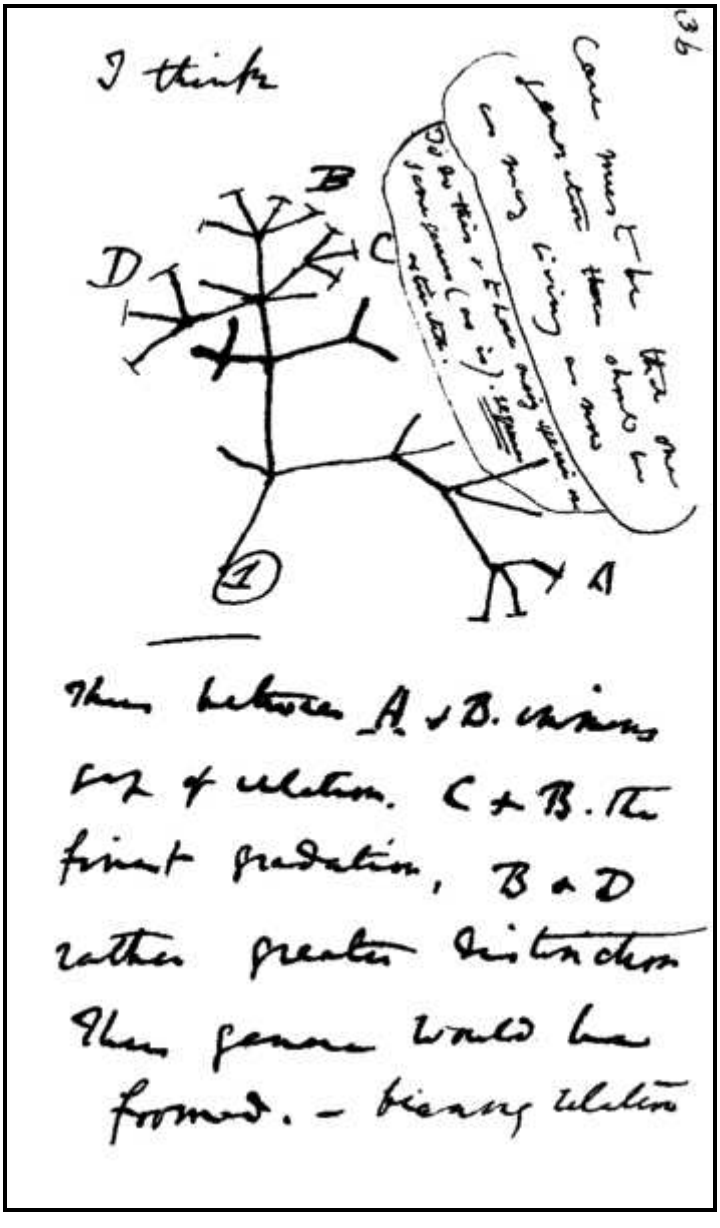
he presented his mammal and bird specimens to the Zoological Society. The ornithologist John Gould soon announced that the Galapagos birds that Darwin had thought a mixture of blackbirds, "gros-beaks" and finches, were, in fact, twelve separate species of finches. On 17 February Darwin was elected to the Council of the Geological Society, and Lyell's presidential address presented Owen's findings on Darwin's fossils, stressing geographical continuity of species as supporting his uniformitarian ideas.

Early in March, Darwin moved to London to be near this work, joining Lyell's social circle of scientists and experts such as Charles Babbage, who described God as a programmer of laws. Darwin stayed with his freethinking brother Erasmus, part of this Whig circle and close friend of writer Harriet Martineau who promoted Malthusianism underlying the controversial Whig Poor Law reforms to stop welfare from causing overpopulation and more poverty. As a Unitarian she welcomed the radical implications of transmutation of species, promoted by Grant and younger surgeons influenced by Geoffroy. Transmutation was anathema to Anglicans defending social order, but reputable scientists openly discussed the subject and there was wide interest in John Herschel's letter praising Lyell's approach as a way to find a natural cause of the origin of new species.

Gould met Darwin and told him that the Galápagos mockingbirds from different islands were separate species, not just varieties, and what Darwin had thought was a "wren" was also in the finch group. Darwin had not labelled the finches by island, but from the notes of others on the *Beagle*, including FitzRoy, he allocated species to islands. The two rheas were also distinct species, and on 14 March Darwin announced how their distribution changed going southwards.

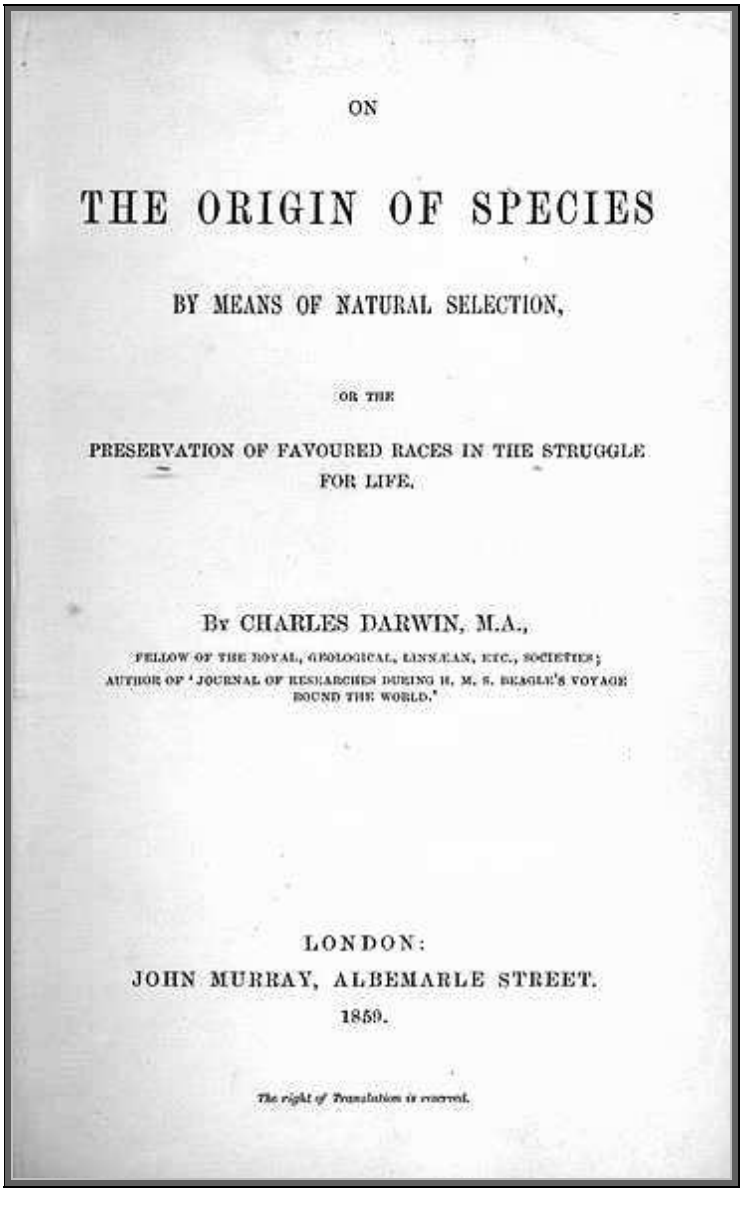
By mid-March, Darwin was speculating in his *Red Notebook* on the possibility that "one species does change into another" to explain the geographical distribution of living species such as the rheas, and extinct ones such as the strange *Macrauchenia* which resembled a giant guanaco. His thoughts on lifespan, asexual reproduction and sexual reproduction developed in his "B" notebook around mid-July on to variation in offspring "to adapt & alter the race to *changing* world" explaining the Galápagos tortoises, mockingbirds and rheas. He sketched branching descent, then a genealogical branching of a single evolutionary tree, in

which "It is absurd to talk of one animal being higher than another", discarding Lamarck's independent lineages progressing to higher forms.



In mid-July 1837 Darwin started his "B" notebook on *Transmutation of Species*, and on page 36 wrote : "I think" above his first evolutionary tree."

PREFACE (ABOUT THE BOOK)



ON

THE ORIGIN OF SPECIES

BY MEANS OF NATURAL SELECTION,

OR THE

PRESERVATION OF FAVOURED RACES IN THE STRUGGLE FOR LIFE.

By CHARLES DARWIN, M.A.,

FELLOW OF THE ROYAL, GEOLOGICAL, LINNÆAN, ETC., SOCIETIES;
AUTHOR OF 'JOURNAL OF RESEARCHES DURING H. M. S. BEAGLE'S VOYAGE ROUND THE WORLD.'

LONDON:
JOHN MURRAY, ALBEMARLE STREET.
1859.

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On the Origin of Species, published on 24 November 1859, is a work of scientific literature by Charles Darwin which is considered to be the foundation of evolutionary biology. Its full title was ***On the Origin of Species by Means of***

Natural Selection, or the Preservation of Favoured Races in the Struggle for Life. For the sixth edition of 1872, the short title was changed to ***The Origin of Species***. Darwin's book introduced the scientific theory that populations evolve over the course of generations through a process of natural selection. It presented a body of evidence that the diversity of life arose by common descent through a branching pattern of evolution. Darwin included evidence that he had gathered on the *Beagle* expedition in the 1830s and his subsequent findings from research, correspondence, and experimentation.

Various evolutionary ideas had already been proposed to explain new findings in biology. There was growing support for such ideas among dissident anatomists and the general public, but during the first half of the 19th century the English scientific establishment was closely tied to the Church of England, while science was part of natural theology. Ideas about the transmutation of species were controversial as they conflicted with the beliefs that species were unchanging parts of a designed hierarchy and that humans were unique, unrelated to other animals. The political and theological implications were intensely debated, but transmutation was not accepted by the scientific mainstream.

The book was written for non-specialist readers and attracted widespread interest upon its publication. As Darwin was an eminent scientist, his findings were taken seriously and the evidence he presented generated scientific, philosophical, and religious discussion. The debate over the book contributed to the campaign by T. H. Huxley and his fellow members of the X Club to secularise science by promoting scientific naturalism. Within two decades there was widespread scientific agreement that evolution, with a branching pattern of common descent, had occurred, but scientists were slow to give natural selection the significance that Darwin thought appropriate. During the "eclipse of Darwinism" from the 1880s to the 1930s, various other mechanisms of evolution were given more credit. With the development of the modern evolutionary synthesis in the 1930s and 1940s, Darwin's concept of evolutionary adaptation through natural selection became central to modern evolutionary theory, and it has now become the unifying concept of the life sciences.

Summary of Darwin's theory

Darwin's theory of evolution is based on key facts and the inferences drawn from them, which biologist Ernst Mayr summarised as follows:

- Every species is fertile enough that if all offspring survived to reproduce the population would grow (fact).
- Despite periodic fluctuations, populations remain roughly the same size (fact).
- Resources such as food are limited and are relatively stable over time (fact).
- A struggle for survival ensues (inference).
- Individuals in a population vary significantly from one another (fact).
- Much of this variation is inheritable (fact).
- Individuals less suited to the environment are less likely to survive and less likely to reproduce; individuals more suited to the environment are more likely to survive and more likely to reproduce and leave their inheritable traits to future generations, which produces the process of natural selection (inference).
- This slowly effected process results in populations changing to adapt to their environments, and ultimately, these variations accumulate over time to form new species (inference).

* * * * *

TABLE OF CONTENTS

On the Origin of Species (Illustrated) 1

About Author 5

Preface (About the Book) 17

Table of Contents 20

Introduction 25

1. Variation Under Domestication 30

On the Breeds of the Domestic Pigeon 41

Selection 48

2. Variation Under Nature 59

3. Struggle For Existence 72

4. Natural Selection 87

Sexual Selection 93

Illustrations of the Action of Natural Selection 95

On the Intercrossing of Individuals 100

Circumstances Favourable to Natural Selection 105

Extinction 111

Divergence of Character 113

Summary of Chapter 125

5. Laws of Variation 128

Effects of Use And Disuse 131

Acclimatisation 135

Correlation of Growth 138

Summary 157

6. Difficulties On Theory 160

On the Absence Or Rarity of Transitional Varieties ____ 162

On the Origin And Transitions of Organic Beings With Peculiar Habits And Structure ____ 167

Organs of Extreme Perfection And Complication ____ 173

Organs of Little Apparent Importance ____ 180

Summary of Chapter ____ 187

7. Instinct ____ *191*

Slave-Making Instinct ____ 201

Cell-Making Instinct of the Hive-Bee ____ 205

Summary ____ 219

8. Hybridism ____ *221*

Laws Governing the Sterility of First Crosses And of Hybrids ____ 229

Causes of the Sterility of First Crosses And of Hybrids ____ 236

Fertility of Varieties When Crossed And of Their Mongrel Offspring ____ 239

Hybrids And Mongrels Compared Independently of Their Fertility ____ 243

Summary of Chapter ____ 246

9. On the Imperfection of the Geological Record ____ *248*

On the Lapse of Time ____ 251

On the Poorness of Our Palaeontological Collections ____ 255

On the Sudden Appearance of Whole Groups of Allied Species ____ 267

On the Sudden Appearance of Groups of Allied Species In the Lowest Known Fossiliferous Strata ____ 271

10. On the Geological Succession of Organic Beings ____ *275*

On Extinction ____ 279

On the Forms of Life Changing Almost Simultaneously Throughout the World ____ 283

On the Affinities of Extinct Species to Each Other And to Living Forms ____ 289

On the State of Development of Ancient Forms ____ 295

On the Succession of the Same Types Within the Same Are As During the Later Tertiary Periods ____ 297

Summary of the Preceding And Present Chapters ____ 299

11. Geographical Distribution ____ *303*

Means of Dispersal ____ 311

Dispersal During the Glacial Period ____ 318

12. Geographical Distribution—Continued ____ *332*

On the Inhabitants of Oceanic Islands ____ 337

Summary of Last And Present Chapters ____ 350

13. Mutual Affinities of Organic Beings: Morphology ____ *354*

Embryology: Rudimentary Organs ____ 354

Morphology ____ 372

Embryology ____ 376

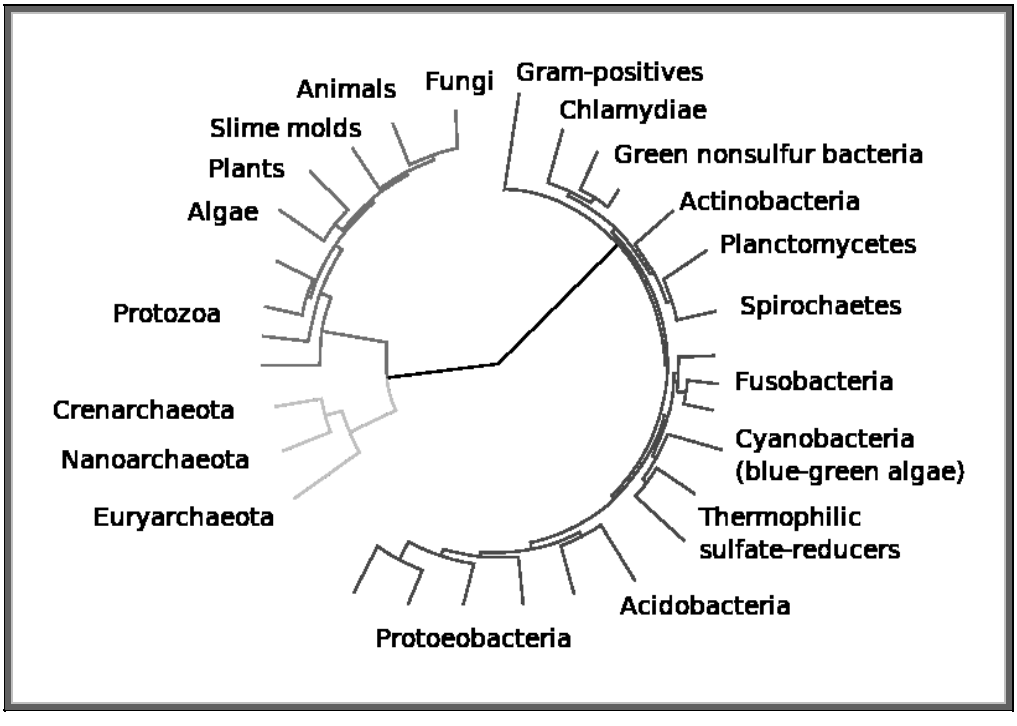
Rudimentary, Atrophied, Or Aborted Organs ____ 385

Summary ____ 389

14. Recapitulation And Conclusion ____ *391*

Evolution Glossarry ____ *416*





Biological Evolution Map:

Part of a series on Evolutionary Biology

ON THE ORIGIN OF SPECIES.

OR THE PRESERVATION OF FAVOURED RACES

IN THE STRUGGLE FOR LIFE.

By

Charles Darwin, M.A.,

Fellow Of The Royal, Geological, Linnaean, Etc., Societies;

Author Of 'Journal Of Researches During H.M.S. Beagle's Voyage Round The World.'

From the First Edition

LONDON:

1859.



ON THE ORIGIN OF SPECIES

INTRODUCTİON

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 facts seemed to me to throw some light on the origin of species—that mystery of mysteries, as it has been called by one of our greatest philosophers. On my return home, it occurred to me, in 1837, that something might perhaps be made out on this question by patiently accumulating and reflecting on all sorts of facts which could possibly have any bearing on it. After five years' work I allowed myself to speculate on the subject, and drew up some short notes; these I enlarged in 1844 into a sketch of the conclusions, which then seemed to me probable: from that period to the present day I have steadily pursued the same object. I hope that I may be excused for entering on these personal details, as I give them to show that I have not been hasty in coming to a decision.

My work is now nearly finished; but as it will take me two or three more years to complete it, and as my health is far from strong, I have been urged to publish this Abstract. I have more especially been induced to do this, as Mr. Wallace, who is now studying the natural history of the Malay archipelago, has arrived at almost exactly the same general conclusions that I have on the origin of species. Last year he sent to me a memoir on this subject, with a request that I would forward it to Sir Charles Lyell, who sent it to the Linnean Society, and it is published in the third volume of the Journal of that Society. Sir C. Lyell and Dr. Hooker, who both knew of my work—the latter having read my sketch of 1844—honoured me by thinking it advisable to publish, with Mr. Wallace's excellent memoir, some brief extracts from my manuscripts.

This Abstract, which I now publish, must necessarily be imperfect. I cannot here give references and authorities for my several statements; and I must trust to the reader reposing some confidence in my accuracy. No doubt errors will have crept in, though I hope I have always been cautious in trusting to good authorities alone. I can here give only the general conclusions at which I have arrived, with a few facts in illustration, but which, I hope, in most cases will suffice. No one can feel more sensible than I do of the necessity of hereafter publishing in detail all the facts, with references, on which my conclusions have been grounded; and I hope in a future work to do this. For I am well aware that scarcely a single point is discussed in this volume on which facts cannot be adduced, often apparently leading to conclusions directly opposite to those at which I have arrived. A fair result can be obtained only by fully stating and balancing the facts and arguments on both sides of each question; and this cannot possibly be here done.

I much regret that want of space prevents my having the satisfaction of acknowledging the generous assistance which I have received from very many naturalists, some of them personally unknown to me. I cannot, however, let this opportunity pass without expressing my deep obligations to Dr. Hooker, who for the last fifteen years has aided me in every possible way by his large stores of knowledge and his excellent judgment.

In considering the Origin of Species, it is quite conceivable that a naturalist, reflecting on the mutual affinities of organic beings, on their embryological relations, their geographical distribution, geological succession, and other such facts, might come to the conclusion that each species had not been independently created, but had descended, like varieties, from other species. Nevertheless, such a conclusion, even if well founded, would be unsatisfactory, until it could be shown how the innumerable species inhabiting this world have been modified, so as to acquire that perfection of structure and coadaptation which most justly excites our admiration. Naturalists continually refer to external conditions, such as climate, food, etc., as the only possible cause of variation. In one very limited sense, as we shall hereafter see, this may be true; but it is preposterous to attribute to mere external conditions, the structure, for instance, of the woodpecker, with its feet,

tail, beak, and tongue, so admirably adapted to catch insects under the bark of trees. In the case of the misseltoe, which draws its nourishment from certain trees, which has seeds that must be transported by certain birds, and which has flowers with separate sexes absolutely requiring the agency of certain insects to bring pollen from one flower to the other, it is equally preposterous to account for the structure of this parasite, with its relations to several distinct organic beings, by the effects of external conditions, or of habit, or of the volition of the plant itself.

The author of the 'Vestiges of Creation' would, I presume, say that, after a certain unknown number of generations, some bird had given birth to a woodpecker, and some plant to the misseltoe, and that these had been produced perfect as we now see them; but this assumption seems to me to be no explanation, for it leaves the case of the coadaptations of organic beings to each other and to their physical conditions of life, untouched and unexplained.

It is, therefore, of the highest importance to gain a clear insight into the means of modification and coadaptation. At the commencement of my observations it seemed to me probable that a careful study of domesticated animals and of cultivated plants would offer the best chance of making out this obscure problem. Nor have I been disappointed; in this and in all other perplexing cases I have invariably found that our knowledge, imperfect though it be, of variation under domestication, afforded the best and safest clue. I may venture to express my conviction of the high value of such studies, although they have been very commonly neglected by naturalists.

From these considerations, I shall devote the first chapter of this Abstract to Variation under Domestication. We shall thus see that a large amount of hereditary modification is at least possible, and, what is equally or more important, we shall see how great is the power of man in accumulating by his Selection successive slight variations. I will then pass on to the variability of species in a state of nature; but I shall, unfortunately, be compelled to treat this subject far too briefly, as it can be treated properly only by giving long catalogues of facts. We shall, however, be enabled to discuss what circumstances are most favourable to

variation. In the next chapter the Struggle for Existence amongst all organic beings throughout the world, which inevitably follows from their high geometrical powers of increase, will be treated of. This is the doctrine of Malthus, applied to the whole animal and vegetable kingdoms. As many more individuals of each species are born than can possibly survive; and as, consequently, there is a frequently recurring struggle for existence, it follows that any being, if it vary however slightly in any manner profitable to itself, under the complex and sometimes varying conditions of life, will have a better chance of surviving, and thus be NATURALLY SELECTED. From the strong principle of inheritance, any selected variety will tend to propagate its new and modified form.

This fundamental subject of Natural Selection will be treated at some length in the fourth chapter; and we shall then see how Natural Selection almost inevitably causes much Extinction of the less improved forms of life and induces what I have called Divergence of Character. In the next chapter I shall discuss the complex and little known laws of variation and of correlation of growth. In the four succeeding chapters, the most apparent and gravest difficulties on the theory will be given: namely, first, the difficulties of transitions, or in understanding how a simple being or a simple organ can be changed and perfected into a highly developed being or elaborately constructed organ; secondly the subject of Instinct, or the mental powers of animals, thirdly, Hybridism, or the infertility of species and the fertility of varieties when intercrossed; and fourthly, the imperfection of the Geological Record. In the next chapter I shall consider the geological succession of organic beings throughout time; in the eleventh and twelfth, their geographical distribution throughout space; in the thirteenth, their classification or mutual affinities, both when mature and in an embryonic condition. In the last chapter I shall give a brief recapitulation of the whole work, and a few concluding remarks.

No one ought to feel surprise at much remaining as yet unexplained in regard to the origin of species and varieties, if he makes due allowance for our profound ignorance in regard to the mutual relations of all the beings which live around us. Who can explain why one species ranges widely and is very numerous, and why

another allied species has a narrow range and is rare? Yet these relations are of the highest importance, for they determine the present welfare, and, as I believe, the future success and modification of every inhabitant of this world. Still less do we know of the mutual relations of the innumerable inhabitants of the world during the many past geological epochs in its history. Although much remains obscure, and will long remain obscure, I can entertain no doubt, after the most deliberate study and dispassionate judgment of which I am capable, that the view which most naturalists entertain, and which I formerly entertained—namely, that each species has been independently created—is erroneous. I am fully convinced that species are not immutable; but that those belonging to what are called the same genera are lineal descendants of some other and generally extinct species, in the same manner as the acknowledged varieties of any one species are the descendants of that species. Furthermore, I am convinced that Natural Selection has been the main but not exclusive means of modification.

1. VARİATİON UNDER DOMESTİCATİON



Causes of Variability. Effects of Habit. Correlation of Growth. Inheritance. Character of Domestic Varieties. Difficulty of distinguishing between Varieties and Species. Origin of Domestic Varieties from one or more Species. Domestic Pigeons, their Differences and Origin. Principle of Selection anciently followed, its Effects. Methodical and Unconscious Selection. Unknown Origin of our Domestic Productions. Circumstances favourable to Man's power of Selection.

When we look to the individuals of the same variety or sub-variety of our older cultivated plants and animals, one of the first points which strikes us, is, that they generally differ much more from each other, than do the individuals of any one species or variety in a state of nature. When we reflect on the vast diversity of the

plants and animals which have been cultivated, and which have varied during all ages under the most different climates and treatment, I think we are driven to conclude that this greater variability is simply due to our domestic productions having been raised under conditions of life not so uniform as, and somewhat different from, those to which the parent-species have been exposed under nature. There is, also, I think, some probability in the view propounded by Andrew Knight, that this variability may be partly connected with excess of food. It seems pretty clear that organic beings must be exposed during several generations to the new conditions of life to cause any appreciable amount of variation; and that when the organisation has once begun to vary, it generally continues to vary for many generations. No case is on record of a variable being ceasing to be variable under cultivation. Our oldest cultivated plants, such as wheat, still often yield new varieties: our oldest domesticated animals are still capable of rapid improvement or modification.

It has been disputed at what period of life the causes of variability, whatever they may be, generally act; whether during the early or late period of development of the embryo, or at the instant of conception. Geoffroy St. Hilaire's experiments show that unnatural treatment of the embryo causes monstrosities; and monstrosities cannot be separated by any clear line of distinction from mere variations. But I am strongly inclined to suspect that the most frequent cause of variability may be attributed to the male and female reproductive elements having been affected prior to the act of conception. Several reasons make me believe in this; but the chief one is the remarkable effect which confinement or cultivation has on the functions of the reproductive system; this system appearing to be far more susceptible than any other part of the organisation, to the action of any change in the conditions of life. Nothing is more easy than to tame an animal, and few things more difficult than to get it to breed freely under confinement, even in the many cases when the male and female unite. How many animals there are which will not breed, though living long under not very close confinement in their native country! This is generally attributed to vitiated instincts; but how many cultivated plants display the utmost vigour, and yet rarely or never seed! In some few such cases it has been found out that very trifling changes, such as a little

more or less water at some particular period of growth, will determine whether or not the plant sets a seed. I cannot here enter on the copious details which I have collected on this curious subject; but to show how singular the laws are which determine the reproduction of animals under confinement, I may just mention that carnivorous animals, even from the tropics, breed in this country pretty freely under confinement, with the exception of the plantigrades or bear family; whereas, carnivorous birds, with the rarest exceptions, hardly ever lay fertile eggs. Many exotic plants have pollen utterly worthless, in the same exact condition as in the most sterile hybrids. When, on the one hand, we see domesticated animals and plants, though often weak and sickly, yet breeding quite freely under confinement; and when, on the other hand, we see individuals, though taken young from a state of nature, perfectly tamed, long-lived, and healthy (of which I could give numerous instances), yet having their reproductive system so seriously affected by unperceived causes as to fail in acting, we need not be surprised at this system, when it does act under confinement, acting not quite regularly, and producing offspring not perfectly like their parents or variable.

Sterility has been said to be the bane of horticulture; but on this view we owe variability to the same cause which produces sterility; and variability is the source of all the choicest productions of the garden. I may add, that as some organisms will breed most freely under the most unnatural conditions (for instance, the rabbit and ferret kept in hutches), showing that their reproductive system has not been thus affected; so will some animals and plants withstand domestication or cultivation, and vary very slightly—perhaps hardly more than in a state of nature.

A long list could easily be given of "sporting plants;" by this term gardeners mean a single bud or offset, which suddenly assumes a new and sometimes very different character from that of the rest of the plant. Such buds can be propagated by grafting, etc., and sometimes by seed. These "sports" are extremely rare under nature, but far from rare under cultivation; and in this case we see that the treatment of the parent has affected a bud or offset, and not the ovules or pollen. But it is the opinion of most physiologists that there is no essential difference between a bud and an ovule in their earliest stages of formation; so that, in fact,

"sports" support my view, that variability may be largely attributed to the ovules or pollen, or to both, having been affected by the treatment of the parent prior to the act of conception. These cases anyhow show that variation is not necessarily connected, as some authors have supposed, with the act of generation.

Seedlings from the same fruit, and the young of the same litter, sometimes differ considerably from each other, though both the young and the parents, as Muller has remarked, have apparently been exposed to exactly the same conditions of life; and this shows how unimportant the direct effects of the conditions of life are in comparison with the laws of reproduction, and of growth, and of inheritance; for had the action of the conditions been direct, if any of the young had varied, all would probably have varied in the same manner. To judge how much, in the case of any variation, we should attribute to the direct action of heat, moisture, light, food, etc., is most difficult: my impression is, that with animals such agencies have produced very little direct effect, though apparently more in the case of plants. Under this point of view, Mr. Buckman's recent experiments on plants seem extremely valuable. When all or nearly all the individuals exposed to certain conditions are affected in the same way, the change at first appears to be directly due to such conditions; but in some cases it can be shown that quite opposite conditions produce similar changes of structure. Nevertheless some slight amount of change may, I think, be attributed to the direct action of the conditions of life—as, in some cases, increased size from amount of food, colour from particular kinds of food and from light, and perhaps the thickness of fur from climate.

Habit also has a decided influence, as in the period of flowering with plants when transported from one climate to another. In animals it has a more marked effect; for instance, I find in the domestic duck that the bones of the wing weigh less and the bones of the leg more, in proportion to the whole skeleton, than do the same bones in the wild-duck; and I presume that this change may be safely attributed to the domestic duck flying much less, and walking more, than its wild parent. The great and inherited development of the udders in cows and goats in countries where they are habitually milked, in comparison with the state of these

organs in other countries, is another instance of the effect of use. Not a single domestic animal can be named which has not in some country drooping ears; and the view suggested by some authors, that the drooping is due to the disuse of the muscles of the ear, from the animals not being much alarmed by danger, seems probable.

There are many laws regulating variation, some few of which can be dimly seen, and will be hereafter briefly mentioned. I will here only allude to what may be called correlation of growth. Any change in the embryo or larva will almost certainly entail changes in the mature animal. In monstrosities, the correlations between quite distinct parts are very curious; and many instances are given in Isidore Geoffroy St. Hilaire's great work on this subject. Breeders believe that long limbs are almost always accompanied by an elongated head. Some instances of correlation are quite whimsical; thus cats with blue eyes are invariably deaf; colour and constitutional peculiarities go together, of which many remarkable cases could be given amongst animals and plants. From the facts collected by Heusinger, it appears that white sheep and pigs are differently affected from coloured individuals by certain vegetable poisons. Hairless dogs have imperfect teeth; long-haired and coarse-haired animals are apt to have, as is asserted, long or many horns; pigeons with feathered feet have skin between their outer toes; pigeons with short beaks have small feet, and those with long beaks large feet. Hence, if man goes on selecting, and thus augmenting, any peculiarity, he will almost certainly unconsciously modify other parts of the structure, owing to the mysterious laws of the correlation of growth.

The result of the various, quite unknown, or dimly seen laws of variation is infinitely complex and diversified. It is well worth while carefully to study the several treatises published on some of our old cultivated plants, as on the hyacinth, potato, even the dahlia, etc.; and it is really surprising to note the endless points in structure and constitution in which the varieties and sub-varieties differ slightly from each other. The whole organisation seems to have become plastic, and tends to depart in some small degree from that of the parental type.

Any variation which is not inherited is unimportant for us. But the number and diversity of inheritable deviations of structure, both those of slight and those of considerable physiological importance, is endless. Dr. Prosper Lucas's treatise, in two large volumes, is the fullest and the best on this subject. No breeder doubts how strong is the tendency to inheritance: like produces like is his fundamental belief: doubts have been thrown on this principle by theoretical writers alone. When a deviation appears not unfrequently, and we see it in the father and child, we cannot tell whether it may not be due to the same original cause acting on both; but when amongst individuals, apparently exposed to the same conditions, any very rare deviation, due to some extraordinary combination of circumstances, appears in the parent—say, once amongst several million individuals—and it reappears in the child, the mere doctrine of chances almost compels us to attribute its reappearance to inheritance. Every one must have heard of cases of albinism, prickly skin, hairy bodies, etc., appearing in several members of the same family. If strange and rare deviations of structure are truly inherited, less strange and commoner deviations may be freely admitted to be inheritable. Perhaps the correct way of viewing the whole subject, would be, to look at the inheritance of every character whatever as the rule, and non-inheritance as the anomaly.

The laws governing inheritance are quite unknown; no one can say why the same peculiarity in different individuals of the same species, and in individuals of different species, is sometimes inherited and sometimes not so; why the child often reverts in certain characters to its grandfather or grandmother or other much more remote ancestor; why a peculiarity is often transmitted from one sex to both sexes or to one sex alone, more commonly but not exclusively to the like sex. It is a fact of some little importance to us, that peculiarities appearing in the males of our domestic breeds are often transmitted either exclusively, or in a much greater degree, to males alone. A much more important rule, which I think may be trusted, is that, at whatever period of life a peculiarity first appears, it tends to appear in the offspring at a corresponding age, though sometimes earlier. In many cases this could not be otherwise: thus the inherited peculiarities in the horns of cattle could appear only in the offspring when nearly mature; peculiarities in the silkworm are known to appear at the corresponding caterpillar

or cocoon stage. But hereditary diseases and some other facts make me believe that the rule has a wider extension, and that when there is no apparent reason why a peculiarity should appear at any particular age, yet that it does tend to appear in the offspring at the same period at which it first appeared in the parent. I believe this rule to be of the highest importance in explaining the laws of embryology. These remarks are of course confined to the first APPEARANCE of the peculiarity, and not to its primary cause, which may have acted on the ovules or male element; in nearly the same manner as in the crossed offspring from a short-horned cow by a long-horned bull, the greater length of horn, though appearing late in life, is clearly due to the male element.

Having alluded to the subject of reversion, I may here refer to a statement often made by naturalists—namely, that our domestic varieties, when run wild, gradually but certainly revert in character to their aboriginal stocks. Hence it has been argued that no deductions can be drawn from domestic races to species in a state of nature. I have in vain endeavoured to discover on what decisive facts the above statement has so often and so boldly been made. There would be great difficulty in proving its truth: we may safely conclude that very many of the most strongly-marked domestic varieties could not possibly live in a wild state. In many cases we do not know what the aboriginal stock was, and so could not tell whether or not nearly perfect reversion had ensued. It would be quite necessary, in order to prevent the effects of intercrossing, that only a single variety should be turned loose in its new home. Nevertheless, as our varieties certainly do occasionally revert in some of their characters to ancestral forms, it seems to me not improbable, that if we could succeed in naturalising, or were to cultivate, during many generations, the several races, for instance, of the cabbage, in very poor soil (in which case, however, some effect would have to be attributed to the direct action of the poor soil), that they would to a large extent, or even wholly, revert to the wild aboriginal stock. Whether or not the experiment would succeed, is not of great importance for our line of argument; for by the experiment itself the conditions of life are changed. If it could be shown that our domestic varieties manifested a strong tendency to reversion,—that is, to lose their acquired characters, whilst kept under unchanged conditions, and whilst kept in a

considerable body, so that free intercrossing might check, by blending together, any slight deviations of structure, in such case, I grant that we could deduce nothing from domestic varieties in regard to species. But there is not a shadow of evidence in favour of this view: to assert that we could not breed our cart and race-horses, long and short-horned cattle, and poultry of various breeds, and esculent vegetables, for an almost infinite number of generations, would be opposed to all experience. I may add, that when under nature the conditions of life do change, variations and reversions of character probably do occur; but natural selection, as will hereafter be explained, will determine how far the new characters thus arising shall be preserved.

When we look to the hereditary varieties or races of our domestic animals and plants, and compare them with species closely allied together, we generally perceive in each domestic race, as already remarked, less uniformity of character than in true species. Domestic races of the same species, also, often have a somewhat monstrous character; by which I mean, that, although differing from each other, and from the other species of the same genus, in several trifling respects, they often differ in an extreme degree in some one part, both when compared one with another, and more especially when compared with all the species in nature to which they are nearest allied. With these exceptions (and with that of the perfect fertility of varieties when crossed,—a subject hereafter to be discussed), domestic races of the same species differ from each other in the same manner as, only in most cases in a lesser degree than, do closely-allied species of the same genus in a state of nature. I think this must be admitted, when we find that there are hardly any domestic races, either amongst animals or plants, which have not been ranked by some competent judges as mere varieties, and by other competent judges as the descendants of aboriginally distinct species. If any marked distinction existed between domestic races and species, this source of doubt could not so perpetually recur. It has often been stated that domestic races do not differ from each other in characters of generic value. I think it could be shown that this statement is hardly correct; but naturalists differ most widely in determining what characters are of generic value; all such valuations being at present empirical. Moreover, on the view of the origin of genera which I shall

presently give, we have no right to expect often to meet with generic differences in our domesticated productions.

When we attempt to estimate the amount of structural difference between the domestic races of the same species, we are soon involved in doubt, from not knowing whether they have descended from one or several parent-species. This point, if it could be cleared up, would be interesting; if, for instance, it could be shown that the greyhound, bloodhound, terrier, spaniel, and bull-dog, which we all know propagate their kind so truly, were the offspring of any single species, then such facts would have great weight in making us doubt about the immutability of the many very closely allied and natural species—for instance, of the many foxes—inhabiting different quarters of the world. I do not believe, as we shall presently see, that all our dogs have descended from any one wild species; but, in the case of some other domestic races, there is presumptive, or even strong, evidence in favour of this view.

It has often been assumed that man has chosen for domestication animals and plants having an extraordinary inherent tendency to vary, and likewise to withstand diverse climates. I do not dispute that these capacities have added largely to the value of most of our domesticated productions; but how could a savage possibly know, when he first tamed an animal, whether it would vary in succeeding generations, and whether it would endure other climates? Has the little variability of the ass or guinea-fowl, or the small power of endurance of warmth by the rein-deer, or of cold by the common camel, prevented their domestication? I cannot doubt that if other animals and plants, equal in number to our domesticated productions, and belonging to equally diverse classes and countries, were taken from a state of nature, and could be made to breed for an equal number of generations under domestication, they would vary on an average as largely as the parent species of our existing domesticated productions have varied.

In the case of most of our anciently domesticated animals and plants, I do not think it is possible to come to any definite conclusion, whether they have descended from one or several species. The argument mainly relied on by those

who believe in the multiple origin of our domestic animals is, that we find in the most ancient records, more especially on the monuments of Egypt, much diversity in the breeds; and that some of the breeds closely resemble, perhaps are identical with, those still existing. Even if this latter fact were found more strictly and generally true than seems to me to be the case, what does it show, but that some of our breeds originated there, four or five thousand years ago? But Mr. Horner's researches have rendered it in some degree probable that man sufficiently civilized to have manufactured pottery existed in the valley of the Nile thirteen or fourteen thousand years ago; and who will pretend to say how long before these ancient periods, savages, like those of Tierra del Fuego or Australia, who possess a semi-domestic dog, may not have existed in Egypt?

The whole subject must, I think, remain vague; nevertheless, I may, without here entering on any details, state that, from geographical and other considerations, I think it highly probable that our domestic dogs have descended from several wild species. In regard to sheep and goats I can form no opinion. I should think, from facts communicated to me by Mr. Blyth, on the habits, voice, and constitution, etc., of the humped Indian cattle, that these had descended from a different aboriginal stock from our European cattle; and several competent judges believe that these latter have had more than one wild parent. With respect to horses, from reasons which I cannot give here, I am doubtfully inclined to believe, in opposition to several authors, that all the races have descended from one wild stock. Mr. Blyth, whose opinion, from his large and varied stores of knowledge, I should value more than that of almost any one, thinks that all the breeds of poultry have proceeded from the common wild Indian fowl (Gallus bankiva). In regard to ducks and rabbits, the breeds of which differ considerably from each other in structure, I do not doubt that they all have descended from the common wild duck and rabbit.

The doctrine of the origin of our several domestic races from several aboriginal stocks, has been carried to an absurd extreme by some authors. They believe that every race which breeds true, let the distinctive characters be ever so slight, has had its wild prototype. At this rate there must have existed at least a score of

species of wild cattle, as many sheep, and several goats in Europe alone, and several even within Great Britain. One author believes that there formerly existed in Great Britain eleven wild species of sheep peculiar to it! When we bear in mind that Britain has now hardly one peculiar mammal, and France but few distinct from those of Germany and conversely, and so with Hungary, Spain, etc., but that each of these kingdoms possesses several peculiar breeds of cattle, sheep, etc., we must admit that many domestic breeds have originated in Europe; for whence could they have been derived, as these several countries do not possess a number of peculiar species as distinct parent-stocks? So it is in India. Even in the case of the domestic dogs of the whole world, which I fully admit have probably descended from several wild species, I cannot doubt that there has been an immense amount of inherited variation. Who can believe that animals closely resembling the Italian greyhound, the bloodhound, the bull-dog, or Blenheim spaniel, etc.—so unlike all wild Canidae—ever existed freely in a state of nature? It has often been loosely said that all our races of dogs have been produced by the crossing of a few aboriginal species; but by crossing we can get only forms in some degree intermediate between their parents; and if we account for our several domestic races by this process, we must admit the former existence of the most extreme forms, as the Italian greyhound, bloodhound, bull-dog, etc., in the wild state. Moreover, the possibility of making distinct races by crossing has been greatly exaggerated. There can be no doubt that a race may be modified by occasional crosses, if aided by the careful selection of those individual mongrels, which present any desired character; but that a race could be obtained nearly intermediate between two extremely different races or species, I can hardly believe. Sir J. Sebright expressly experimentised for this object, and failed. The offspring from the first cross between two pure breeds is tolerably and sometimes (as I have found with pigeons) extremely uniform, and everything seems simple enough; but when these mongrels are crossed one with another for several generations, hardly two of them will be alike, and then the extreme difficulty, or rather utter hopelessness, of the task becomes apparent. Certainly, a breed intermediate between TWO VERY DISTINCT breeds could not be got without extreme care and long-continued selection; nor can I find a single case on record of a permanent race having been thus formed.

ON THE BREEDS OF THE DOMESTİC PİGEON

Believing that it is always best to study some special group, I have, after deliberation, taken up domestic pigeons. I have kept every breed which I could purchase or obtain, and have been most kindly favoured with skins from several quarters of the world, more especially by the Honourable W. Elliot from India, and by the Honourable C. Murray from Persia. Many treatises in different languages have been published on pigeons, and some of them are very important, as being of considerable antiquity. I have associated with several eminent fanciers, and have been permitted to join two of the London Pigeon Clubs. The diversity of the breeds is something astonishing. Compare the English carrier and the short-faced tumbler, and see the wonderful difference in their beaks, entailing corresponding differences in their skulls. The carrier, more especially the male bird, is also remarkable from the wonderful development of the carunculated skin about the head, and this is accompanied by greatly elongated eyelids, very large external orifices to the nostrils, and a wide gape of mouth. The short-faced tumbler has a beak in outline almost like that of a finch; and the common tumbler has the singular and strictly inherited habit of flying at a great height in a compact flock, and tumbling in the air head over heels. The runt is a bird of great size, with long, massive beak and large feet; some of the sub-breeds of runts have very long necks, others very long wings and tails, others singularly short tails. The barb is allied to the carrier, but, instead of a very long beak, has a very short and very broad one. The pouter has a much elongated body, wings, and legs; and its enormously developed crop, which it glories in inflating, may well excite astonishment and even laughter. The turbit has a very short and conical beak, with a line of reversed feathers down the breast; and it has the habit of continually expanding slightly the upper part of the oesophagus. The Jacobin has the feathers so much reversed along the back of the neck that they form a hood, and it has, proportionally to its size, much elongated wing and tail feathers. The trumpeter and laugher, as their names express, utter a very different coo from the other breeds. The fantail has thirty or even forty tail-feathers, instead of twelve or fourteen, the normal number in all members of the great pigeon family; and these

feathers are kept expanded, and are carried so erect that in good birds the head and tail touch; the oil-gland is quite aborted. Several other less distinct breeds might have been specified.

In the skeletons of the several breeds, the development of the bones of the face in length and breadth and curvature differs enormously. The shape, as well as the breadth and length of the ramus of the lower jaw, varies in a highly remarkable manner. The number of the caudal and sacral vertebrae vary; as does the number of the ribs, together with their relative breadth and the presence of processes. The size and shape of the apertures in the sternum are highly variable; so is the degree of divergence and relative size of the two arms of the furcula. The proportional width of the gape of mouth, the proportional length of the eyelids, of the orifice of the nostrils, of the tongue (not always in strict correlation with the length of beak), the size of the crop and of the upper part of the oesophagus; the development and abortion of the oil-gland; the number of the primary wing and caudal feathers; the relative length of wing and tail to each other and to the body; the relative length of leg and of the feet; the number of scutellae on the toes, the development of skin between the toes, are all points of structure which are variable. The period at which the perfect plumage is acquired varies, as does the state of the down with which the nestling birds are clothed when hatched. The shape and size of the eggs vary. The manner of flight differs remarkably; as does in some breeds the voice and disposition. Lastly, in certain breeds, the males and females have come to differ to a slight degree from each other.

Altogether at least a score of pigeons might be chosen, which if shown to an ornithologist, and he were told that they were wild birds, would certainly, I think, be ranked by him as well-defined species. Moreover, I do not believe that any ornithologist would place the English carrier, the short-faced tumbler, the runt, the barb, pouter, and fantail in the same genus; more especially as in each of these breeds several truly-inherited sub-breeds, or species as he might have called them, could be shown him.

Great as the differences are between the breeds of pigeons, I am fully convinced that the common opinion of naturalists is correct, namely, that all have descended from the rock-pigeon (Columba livia), including under this term several geographical races or sub-species, which differ from each other in the most trifling respects. As several of the reasons which have led me to this belief are in some degree applicable in other cases, I will here briefly give them. If the several breeds are not varieties, and have not proceeded from the rock-pigeon, they must have descended from at least seven or eight aboriginal stocks; for it is impossible to make the present domestic breeds by the crossing of any lesser number: how, for instance, could a pouter be produced by crossing two breeds unless one of the parent-stocks possessed the characteristic enormous crop? The supposed aboriginal stocks must all have been rock-pigeons, that is, not breeding or willingly perching on trees. But besides C. livia, with its geographical sub-species, only two or three other species of rock-pigeons are known; and these have not any of the characters of the domestic breeds. Hence the supposed aboriginal stocks must either still exist in the countries where they were originally domesticated, and yet be unknown to ornithologists; and this, considering their size, habits, and remarkable characters, seems very improbable; or they must have become extinct in the wild state. But birds breeding on precipices, and good fliers, are unlikely to be exterminated; and the common rock-pigeon, which has the same habits with the domestic breeds, has not been exterminated even on several of the smaller British islets, or on the shores of the Mediterranean. Hence the supposed extermination of so many species having similar habits with the rock-pigeon seems to me a very rash assumption. Moreover, the several above-named domesticated breeds have been transported to all parts of the world, and, therefore, some of them must have been carried back again into their native country; but not one has ever become wild or feral, though the dovecot-pigeon, which is the rock-pigeon in a very slightly altered state, has become feral in several places. Again, all recent experience shows that it is most difficult to get any wild animal to breed freely under domestication; yet on the hypothesis of the multiple origin of our pigeons, it must be assumed that at least seven or eight species were so thoroughly domesticated in ancient times by half-civilized man, as to be quite prolific under confinement.

An argument, as it seems to me, of great weight, and applicable in several other cases, is, that the above-specified breeds, though agreeing generally in constitution, habits, voice, colouring, and in most parts of their structure, with the wild rock-pigeon, yet are certainly highly abnormal in other parts of their structure: we may look in vain throughout the whole great family of Columbidae for a beak like that of the English carrier, or that of the short-faced tumbler, or barb; for reversed feathers like those of the jacobin; for a crop like that of the pouter; for tail-feathers like those of the fantail. Hence it must be assumed not only that half-civilized man succeeded in thoroughly domesticating several species, but that he intentionally or by chance picked out extraordinarily abnormal species; and further, that these very species have since all become extinct or unknown. So many strange contingencies seem to me improbable in the highest degree.

Some facts in regard to the colouring of pigeons well deserve consideration. The rock-pigeon is of a slaty-blue, and has a white rump (the Indian sub-species, C. intermedia of Strickland, having it bluish); the tail has a terminal dark bar, with the bases of the outer feathers externally edged with white; the wings have two black bars; some semi-domestic breeds and some apparently truly wild breeds have, besides the two black bars, the wings chequered with black. These several marks do not occur together in any other species of the whole family. Now, in every one of the domestic breeds, taking thoroughly well-bred birds, all the above marks, even to the white edging of the outer tail-feathers, sometimes concur perfectly developed. Moreover, when two birds belonging to two distinct breeds are crossed, neither of which is blue or has any of the above-specified marks, the mongrel offspring are very apt suddenly to acquire these characters; for instance, I crossed some uniformly white fantails with some uniformly black barbs, and they produced mottled brown and black birds; these I again crossed together, and one grandchild of the pure white fantail and pure black barb was of as beautiful a blue colour, with the white rump, double black wing-bar, and barred and white-edged tail-feathers, as any wild rock-pigeon! We can understand these facts, on the well-known principle of reversion to ancestral characters, if all the domestic breeds have descended from the rock-pigeon. But if we deny this, we must make one of

the two following highly improbable suppositions. Either, firstly, that all the several imagined aboriginal stocks were coloured and marked like the rock-pigeon, although no other existing species is thus coloured and marked, so that in each separate breed there might be a tendency to revert to the very same colours and markings. Or, secondly, that each breed, even the purest, has within a dozen or, at most, within a score of generations, been crossed by the rock-pigeon: I say within a dozen or twenty generations, for we know of no fact countenancing the belief that the child ever reverts to some one ancestor, removed by a greater number of generations. In a breed which has been crossed only once with some distinct breed, the tendency to reversion to any character derived from such cross will naturally become less and less, as in each succeeding generation there will be less of the foreign blood; but when there has been no cross with a distinct breed, and there is a tendency in both parents to revert to a character, which has been lost during some former generation, this tendency, for all that we can see to the contrary, may be transmitted undiminished for an indefinite number of generations. These two distinct cases are often confounded in treatises on inheritance.

Lastly, the hybrids or mongrels from between all the domestic breeds of pigeons are perfectly fertile. I can state this from my own observations, purposely made on the most distinct breeds. Now, it is difficult, perhaps impossible, to bring forward one case of the hybrid offspring of two animals CLEARLY DISTINCT being themselves perfectly fertile. Some authors believe that long-continued domestication eliminates this strong tendency to sterility: from the history of the dog I think there is some probability in this hypothesis, if applied to species closely related together, though it is unsupported by a single experiment. But to extend the hypothesis so far as to suppose that species, aboriginally as distinct as carriers, tumblers, pouters, and fantails now are, should yield offspring perfectly fertile, inter se, seems to me rash in the extreme.

From these several reasons, namely, the improbability of man having formerly got seven or eight supposed species of pigeons to breed freely under domestication; these supposed species being quite unknown in a wild state, and

their becoming nowhere feral; these species having very abnormal characters in certain respects, as compared with all other Columbidae, though so like in most other respects to the rock-pigeon; the blue colour and various marks occasionally appearing in all the breeds, both when kept pure and when crossed; the mongrel offspring being perfectly fertile;—from these several reasons, taken together, I can feel no doubt that all our domestic breeds have descended from the Columba livia with its geographical sub-species.

In favour of this view, I may add, firstly, that C. livia, or the rock-pigeon, has been found capable of domestication in Europe and in India; and that it agrees in habits and in a great number of points of structure with all the domestic breeds. Secondly, although an English carrier or short-faced tumbler differs immensely in certain characters from the rock-pigeon, yet by comparing the several sub-breeds of these breeds, more especially those brought from distant countries, we can make an almost perfect series between the extremes of structure. Thirdly, those characters which are mainly distinctive of each breed, for instance the wattle and length of beak of the carrier, the shortness of that of the tumbler, and the number of tail-feathers in the fantail, are in each breed eminently variable; and the explanation of this fact will be obvious when we come to treat of selection. Fourthly, pigeons have been watched, and tended with the utmost care, and loved by many people. They have been domesticated for thousands of years in several quarters of the world; the earliest known record of pigeons is in the fifth Aegyptian dynasty, about 3000 B.C., as was pointed out to me by Professor Lepsius; but Mr. Birch informs me that pigeons are given in a bill of fare in the previous dynasty. In the time of the Romans, as we hear from Pliny, immense prices were given for pigeons; "nay, they are come to this pass, that they can reckon up their pedigree and race." Pigeons were much valued by Akber Khan in India, about the year 1600; never less than 20,000 pigeons were taken with the court. "The monarchs of Iran and Turan sent him some very rare birds;" and, continues the courtly historian, "His Majesty by crossing the breeds, which method was never practised before, has improved them astonishingly." About this same period the Dutch were as eager about pigeons as were the old Romans. The paramount importance of these considerations in explaining the immense amount