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Produced by Sue Asscher

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.'

LONDON:

JOHN MURRAY, ALBEMARLE STREET.

1859.

Down, Bromley, Kent,

October 1st, 1859.

"But with regard to the material world, we can at least go so far as

this--we can perceive that events are brought about not by insulated

interpositions of Divine power, exerted in each particular case, but by

the establishment of general laws."

W. Whewell: Bridgewater Treatise.

"To conclude, therefore, let no man out of a weak conceit of sobriety,

or an ill-applied moderation, think or maintain, that a man can search

too far or be too well studied in the book of God's word, or in the book

of God's works; divinity or philosophy; but rather let men endeavour an

endless progress or proficience in both."

Bacon: Advancement of Learning.

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ON THE ORIGIN OF SPECIES.

INTRODUCTION.

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. VARIATION UNDER DOMESTICATION.

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 DOMESTIC PIGEON.

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 of variation which pigeons have undergone, will be

obvious when we treat of Selection. We shall then, also, see how it is

that the breeds so often have a somewhat monstrous character. It is also

a most favourable circumstance for the production of distinct breeds,

that male and female pigeons can be easily mated for life; and thus

different breeds can be kept together in the same aviary.

I have discussed the probable origin of domestic pigeons at some,

yet quite insufficient, length; because when I first kept pigeons and

watched the several kinds, knowing well how true they bred, I felt fully

as much difficulty in believing that they could ever have descended

from a common parent, as any naturalist could in coming to a similar

conclusion in regard to the many species of finches, or other large

groups of birds, in nature. One circumstance has struck me much;

namely, that all the breeders of the various domestic animals and

the cultivators of plants, with whom I have ever conversed, or whose

treatises I have read, are firmly convinced that the several breeds

to which each has attended, are descended from so many aboriginally

distinct species. Ask, as I have asked, a celebrated raiser of Hereford

cattle, whether his cattle might not have descended from long horns, and

he will laugh you to scorn. I have never met a pigeon, or poultry, or

duck, or rabbit fancier, who was not fully convinced that each main

breed was descended from a distinct species. Van Mons, in his treatise

on pears and apples, shows how utterly he disbelieves that the several

sorts, for instance a Ribston-pippin or Codlin-apple, could ever have

proceeded from the seeds of the same tree. Innumerable other examples

could be given. The explanation, I think, is simple: from long-continued

study they are strongly impressed with the differences between the

several races; and though they well know that each race varies slightly,

for they win their prizes by selecting such slight differences, yet they

ignore all general arguments, and refuse to sum up in their minds slight

differences accumulated during many successive generations. May not

those naturalists who, knowing far less of the laws of inheritance than

does the breeder, and knowing no more than he does of the intermediate

links in the long lines of descent, yet admit that many of our domestic

races have descended from the same parents--may they not learn a lesson

of caution, when they deride the idea of species in a state of nature

being lineal descendants of other species?

SELECTION.

Let us now briefly consider the steps by which domestic races have been

produced, either from one or from several allied species. Some little

effect may, perhaps, be attributed to the direct action of the external

conditions of life, and some little to habit; but he would be a bold

man who would account by such agencies for the differences of a dray and

race horse, a greyhound and bloodhound, a carrier and tumbler pigeon.

One of the most remarkable features in our domesticated races is that we

see in them adaptation, not indeed to the animal's or plant's own good,

but to man's use or fancy. Some variations useful to him have probably

arisen suddenly, or by one step; many botanists, for instance, believe

that the fuller's teazle, with its hooks, which cannot be rivalled by

any mechanical contrivance, is only a variety of the wild Dipsacus; and

this amount of change may have suddenly arisen in a seedling. So it has

probably been with the turnspit dog; and this is known to have been

the case with the ancon sheep. But when we compare the dray-horse and

race-horse, the dromedary and camel, the various breeds of sheep fitted

either for cultivated land or mountain pasture, with the wool of one

breed good for one purpose, and that of another breed for another

purpose; when we compare the many breeds of dogs, each good for man in

very different ways; when we compare the game-cock, so pertinacious

in battle, with other breeds so little quarrelsome, with "everlasting

layers" which never desire to sit, and with the bantam so small and

elegant; when we compare the host of agricultural, culinary, orchard,

and flower-garden races of plants, most useful to man at different

seasons and for different purposes, or so beautiful in his eyes, we

must, I think, look further than to mere variability. We cannot suppose

that all the breeds were suddenly produced as perfect and as useful as

we now see them; indeed, in several cases, we know that this has not

been their history. The key is man's power of accumulative selection:

nature gives successive variations; man adds them up in certain

directions useful to him. In this sense he may be said to make for

himself useful breeds.

The great power of this principle of selection is not hypothetical.

It is certain that several of our eminent breeders have, even within a

single lifetime, modified to a large extent some breeds of cattle and

sheep. In order fully to realise what they have done, it is almost

necessary to read several of the many treatises devoted to this subject,

and to inspect the animals. Breeders habitually speak of an animal's

organisation as something quite plastic, which they can model almost

as they please. If I had space I could quote numerous passages to this

effect from highly competent authorities. Youatt, who was probably

better acquainted with the works of agriculturalists than almost any

other individual, and who was himself a very good judge of an animal,

speaks of the principle of selection as "that which enables the

agriculturist, not only to modify the character of his flock, but to

change it altogether. It is the magician's wand, by means of which

he may summon into life whatever form and mould he pleases." Lord

Somerville, speaking of what breeders have done for sheep, says:--"It

would seem as if they had chalked out upon a wall a form perfect in

itself, and then had given it existence." That most skilful breeder,

Sir John Sebright, used to say, with respect to pigeons, that "he would

produce any given feather in three years, but it would take him

six years to obtain head and beak." In Saxony the importance of the

principle of selection in regard to merino sheep is so fully recognised,

that men follow it as a trade: the sheep are placed on a table and are

studied, like a picture by a connoisseur; this is done three times at

intervals of months, and the sheep are each time marked and classed, so

that the very best may ultimately be selected for breeding.

What English breeders have actually effected is proved by the enormous

prices given for animals with a good pedigree; and these have now been

exported to almost every quarter of the world. The improvement is by no

means generally due to crossing different breeds; all the best breeders

are strongly opposed to this practice, except sometimes amongst closely

allied sub-breeds. And when a cross has been made, the closest selection

is far more indispensable even than in ordinary cases. If selection

consisted merely in separating some very distinct variety, and breeding

from it, the principle would be so obvious as hardly to be worth

notice; but its importance consists in the great effect produced by

the accumulation in one direction, during successive generations, of

differences absolutely inappreciable by an uneducated eye--differences

which I for one have vainly attempted to appreciate. Not one man in

a thousand has accuracy of eye and judgment sufficient to become an

eminent breeder. If gifted with these qualities, and he studies his

subject for years, and devotes his lifetime to it with indomitable

perseverance, he will succeed, and may make great improvements; if he

wants any of these qualities, he will assuredly fail. Few would readily

believe in the natural capacity and years of practice requisite to

become even a skilful pigeon-fancier.

The same principles are followed by horticulturists; but the variations

are here often more abrupt. No one supposes that our choicest

productions have been produced by a single variation from the aboriginal

stock. We have proofs that this is not so in some cases, in which exact

records have been kept; thus, to give a very trifling instance, the

steadily-increasing size of the common gooseberry may be quoted. We see

an astonishing improvement in many florists' flowers, when the flowers

of the present day are compared with drawings made only twenty or thirty

years ago. When a race of plants is once pretty well established, the

seed-raisers do not pick out the best plants, but merely go over their

seed-beds, and pull up the "rogues," as they call the plants that

deviate from the proper standard. With animals this kind of selection

is, in fact, also followed; for hardly any one is so careless as to

allow his worst animals to breed.

In regard to plants, there is another means of observing the accumulated

effects of selection--namely, by comparing the diversity of flowers in

the different varieties of the same species in the flower-garden; the

diversity of leaves, pods, or tubers, or whatever part is valued, in the

kitchen-garden, in comparison with the flowers of the same varieties;

and the diversity of fruit of the same species in the orchard, in

comparison with the leaves and flowers of the same set of varieties. See

how different the leaves of the cabbage are, and how extremely alike the

flowers; how unlike the flowers of the heartsease are, and how alike the

leaves; how much the fruit of the different kinds of gooseberries differ

in size, colour, shape, and hairiness, and yet the flowers present very

slight differences. It is not that the varieties which differ largely

in some one point do not differ at all in other points; this is hardly

ever, perhaps never, the case. The laws of correlation of growth,

the importance of which should never be overlooked, will ensure some

differences; but, as a general rule, I cannot doubt that the continued

selection of slight variations, either in the leaves, the flowers, or

the fruit, will produce races differing from each other chiefly in these

characters.

It may be objected that the principle of selection has been reduced to

methodical practice for scarcely more than three-quarters of a century;

it has certainly been more attended to of late years, and many treatises

have been published on the subject; and the result, I may add, has been,

in a corresponding degree, rapid and important. But it is very far from

true that the principle is a modern discovery. I could give several

references to the full acknowledgment of the importance of the principle

in works of high antiquity. In rude and barbarous periods of English

history choice animals were often imported, and laws were passed to

prevent their exportation: the destruction of horses under a certain

size was ordered, and this may be compared to the "roguing" of plants

by nurserymen. The principle of selection I find distinctly given in an

ancient Chinese encyclopaedia. Explicit rules are laid down by some of

the Roman classical writers. From passages in Genesis, it is clear that

the colour of domestic animals was at that early period attended to.

Savages now sometimes cross their dogs with wild canine animals, to

improve the breed, and they formerly did so, as is attested by passages

in Pliny. The savages in South Africa match their draught cattle by

colour, as do some of the Esquimaux their teams of dogs. Livingstone

shows how much good domestic breeds are valued by the negroes of the

interior of Africa who have not associated with Europeans. Some of these

facts do not show actual selection, but they show that the breeding of

domestic animals was carefully attended to in ancient times, and is now

attended to by the lowest savages. It would, indeed, have been a strange

fact, had attention not been paid to breeding, for the inheritance of

good and bad qualities is so obvious.

At the present time, eminent breeders try by methodical selection, with

a distinct object in view, to make a new strain or sub-breed, superior

to anything existing in the country. But, for our purpose, a kind of

Selection, which may be called Unconscious, and which results from every

one trying to possess and breed from the best individual animals, is

more important. Thus, a man who intends keeping pointers naturally tries

to get as good dogs as he can, and afterwards breeds from his own best

dogs, but he has no wish or expectation of permanently altering the

breed. Nevertheless I cannot doubt that this process, continued during

centuries, would improve and modify any breed, in the same way as

Bakewell, Collins, etc., by this very same process, only carried on more

methodically, did greatly modify, even during their own lifetimes, the

forms and qualities of their cattle. Slow and insensible changes of this

kind could never be recognised unless actual measurements or careful

drawings of the breeds in question had been made long ago, which might

serve for comparison. In some cases, however, unchanged or but little

changed individuals of the same breed may be found in less civilised

districts, where the breed has been less improved. There is reason to

believe that King Charles's spaniel has been unconsciously modified to

a large extent since the time of that monarch. Some highly competent

authorities are convinced that the setter is directly derived from the

spaniel, and has probably been slowly altered from it. It is known that

the English pointer has been greatly changed within the last century,

and in this case the change has, it is believed, been chiefly effected

by crosses with the fox-hound; but what concerns us is, that the change

has been effected unconsciously and gradually, and yet so effectually,

that, though the old Spanish pointer certainly came from Spain, Mr.

Borrow has not seen, as I am informed by him, any native dog in Spain

like our pointer.

By a similar process of selection, and by careful training, the whole

body of English racehorses have come to surpass in fleetness and size

the parent Arab stock, so that the latter, by the regulations for the

Goodwood Races, are favoured in the weights they carry. Lord Spencer and

others have shown how the cattle of England have increased in weight

and in early maturity, compared with the stock formerly kept in this

country. By comparing the accounts given in old pigeon treatises of

carriers and tumblers with these breeds as now existing in Britain,

India, and Persia, we can, I think, clearly trace the stages through

which they have insensibly passed, and come to differ so greatly from

the rock-pigeon.

Youatt gives an excellent illustration of the effects of a course of

selection, which may be considered as unconsciously followed, in so far

that the breeders could never have expected or even have wished to have

produced the result which ensued--namely, the production of two distinct

strains. The two flocks of Leicester sheep kept by Mr. Buckley and Mr.

Burgess, as Mr. Youatt remarks, "have been purely bred from the original

stock of Mr. Bakewell for upwards of fifty years. There is not a

suspicion existing in the mind of any one at all acquainted with

the subject that the owner of either of them has deviated in any one

instance from the pure blood of Mr. Bakewell's flock, and yet the

difference between the sheep possessed by these two gentlemen is so

great that they have the appearance of being quite different varieties."

If there exist savages so barbarous as never to think of the inherited

character of the offspring of their domestic animals, yet any one animal

particularly useful to them, for any special purpose, would be carefully

preserved during famines and other accidents, to which savages are

so liable, and such choice animals would thus generally leave more

offspring than the inferior ones; so that in this case there would be a

kind of unconscious selection going on. We see the value set on animals

even by the barbarians of Tierra del Fuego, by their killing and

devouring their old women, in times of dearth, as of less value than

their dogs.

In plants the same gradual process of improvement, through the

occasional preservation of the best individuals, whether or not

sufficiently distinct to be ranked at their first appearance as distinct

varieties, and whether or not two or more species or races have become

blended together by crossing, may plainly be recognised in the increased

size and beauty which we now see in the varieties of the heartsease,

rose, pelargonium, dahlia, and other plants, when compared with the

older varieties or with their parent-stocks. No one would ever expect to

get a first-rate heartsease or dahlia from the seed of a wild plant. No

one would expect to raise a first-rate melting pear from the seed of a

wild pear, though he might succeed from a poor seedling growing wild,

if it had come from a garden-stock. The pear, though cultivated in

classical times, appears, from Pliny's description, to have been a

fruit of very inferior quality. I have seen great surprise expressed

in horticultural works at the wonderful skill of gardeners, in having

produced such splendid results from such poor materials; but the art,

I cannot doubt, has been simple, and, as far as the final result is

concerned, has been followed almost unconsciously. It has consisted in

always cultivating the best known variety, sowing its seeds, and, when

a slightly better variety has chanced to appear, selecting it, and so

onwards. But the gardeners of the classical period, who cultivated

the best pear they could procure, never thought what splendid fruit we

should eat; though we owe our excellent fruit, in some small degree,

to their having naturally chosen and preserved the best varieties they

could anywhere find.

A large amount of change in our cultivated plants, thus slowly and

unconsciously accumulated, explains, as I believe, the well-known fact,

that in a vast number of cases we cannot recognise, and therefore do

not know, the wild parent-stocks of the plants which have been longest

cultivated in our flower and kitchen gardens. If it has taken centuries

or thousands of years to improve or modify most of our plants up to

their present standard of usefulness to man, we can understand how it

is that neither Australia, the Cape of Good Hope, nor any other region

inhabited by quite uncivilised man, has afforded us a single plant worth

culture. It is not that these countries, so rich in species, do not by

a strange chance possess the aboriginal stocks of any useful plants, but

that the native plants have not been improved by continued selection up

to a standard of perfection comparable with that given to the plants in

countries anciently civilised.

In regard to the domestic animals kept by uncivilised man, it should

not be overlooked that they almost always have to struggle for their

own food, at least during certain seasons. And in two countries very

differently circumstanced, individuals of the same species, having

slightly different constitutions or structure, would often succeed

better in the one country than in the other, and thus by a process of

"natural selection," as will hereafter be more fully explained, two

sub-breeds might be formed. This, perhaps, partly explains what has been

remarked by some authors, namely, that the varieties kept by savages

have more of the character of species than the varieties kept in

civilised countries.

On the view here given of the all-important part which selection by

man has played, it becomes at once obvious, how it is that our domestic

races show adaptation in their structure or in their habits to man's

wants or fancies. We can, I think, further understand the frequently

abnormal character of our domestic races, and likewise their differences

being so great in external characters and relatively so slight in

internal parts or organs. Man can hardly select, or only with much

difficulty, any deviation of structure excepting such as is externally

visible; and indeed he rarely cares for what is internal. He can never

act by selection, excepting on variations which are first given to

him in some slight degree by nature. No man would ever try to make

a fantail, till he saw a pigeon with a tail developed in some slight

degree in an unusual manner, or a pouter till he saw a pigeon with a

crop of somewhat unusual size; and the more abnormal or unusual any

character was when it first appeared, the more likely it would be to

catch his attention. But to use such an expression as trying to make a

fantail, is, I have no doubt, in most cases, utterly incorrect. The man

who first selected a pigeon with a slightly larger tail, never dreamed

what the descendants of that pigeon would become through long-continued,

partly unconscious and partly methodical selection. Perhaps the parent

bird of all fantails had only fourteen tail-feathers somewhat expanded,

like the present Java fantail, or like individuals of other and distinct

breeds, in which as many as seventeen tail-feathers have been counted.

Perhaps the first pouter-pigeon did not inflate its crop much more than

the turbit now does the upper part of its oesophagus,--a habit which

is disregarded by all fanciers, as it is not one of the points of the

breed.

Nor let it be thought that some great deviation of structure would

be necessary to catch the fancier's eye: he perceives extremely small

differences, and it is in human nature to value any novelty, however

slight, in one's own possession. Nor must the value which would formerly

be set on any slight differences in the individuals of the same species,

be judged of by the value which would now be set on them, after several

breeds have once fairly been established. Many slight differences might,

and indeed do now, arise amongst pigeons, which are rejected as faults

or deviations from the standard of perfection of each breed. The common

goose has not given rise to any marked varieties; hence the Thoulouse

and the common breed, which differ only in colour, that most fleeting of

characters, have lately been exhibited as distinct at our poultry-shows.

I think these views further explain what has sometimes been

noticed--namely that we know nothing about the origin or history of

any of our domestic breeds. But, in fact, a breed, like a dialect of

a language, can hardly be said to have had a definite origin. A man

preserves and breeds from an individual with some slight deviation of

structure, or takes more care than usual in matching his best animals

and thus improves them, and the improved individuals slowly spread in

the immediate neighbourhood. But as yet they will hardly have a distinct

name, and from being only slightly valued, their history will be

disregarded. When further improved by the same slow and gradual process,

they will spread more widely, and will get recognised as something

distinct and valuable, and will then probably first receive a provincial

name. In semi-civilised countries, with little free communication, the

spreading and knowledge of any new sub-breed will be a slow process.

As soon as the points of value of the new sub-breed are once fully

acknowledged, the principle, as I have called it, of unconscious

selection will always tend,--perhaps more at one period than at another,

as the breed rises or falls in fashion,--perhaps more in one district

than in another, according to the state of civilisation of the

inhabitants--slowly to add to the characteristic features of the breed,

whatever they may be. But the chance will be infinitely small of any

record having been preserved of such slow, varying, and insensible

changes.

I must now say a few words on the circumstances, favourable, or the

reverse, to man's power of selection. A high degree of variability is

obviously favourable, as freely giving the materials for selection to

work on; not that mere individual differences are not amply sufficient,

with extreme care, to allow of the accumulation of a large amount

of modification in almost any desired direction. But as variations

manifestly useful or pleasing to man appear only occasionally, the

chance of their appearance will be much increased by a large number

of individuals being kept; and hence this comes to be of the highest

importance to success. On this principle Marshall has remarked, with

respect to the sheep of parts of Yorkshire, that "as they generally

belong to poor people, and are mostly IN SMALL LOTS, they never can be

improved." On the other hand, nurserymen, from raising large stocks

of the same plants, are generally far more successful than amateurs in

getting new and valuable varieties. The keeping of a large number of

individuals of a species in any country requires that the species should

be placed under favourable conditions of life, so as to breed freely in

that country. When the individuals of any species are scanty, all the

individuals, whatever their quality may be, will generally be allowed

to breed, and this will effectually prevent selection. But probably the

most important point of all, is, that the animal or plant should be

so highly useful to man, or so much valued by him, that the closest

attention should be paid to even the slightest deviation in the

qualities or structure of each individual. Unless such attention be paid

nothing can be effected. I have seen it gravely remarked, that it was

most fortunate that the strawberry began to vary just when gardeners

began to attend closely to this plant. No doubt the strawberry had

always varied since it was cultivated, but the slight varieties had been

neglected. As soon, however, as gardeners picked out individual plants

with slightly larger, earlier, or better fruit, and raised seedlings

from them, and again picked out the best seedlings and bred from them,

then, there appeared (aided by some crossing with distinct species)

those many admirable varieties of the strawberry which have been raised

during the last thirty or forty years.

In the case of animals with separate sexes, facility in preventing

crosses is an important element of success in the formation of new

races,--at least, in a country which is already stocked with other

races. In this respect enclosure of the land plays a part. Wandering

savages or the inhabitants of open plains rarely possess more than one

breed of the same species. Pigeons can be mated for life, and this is a

great convenience to the fancier, for thus many races may be kept true,

though mingled in the same aviary; and this circumstance must have

largely favoured the improvement and formation of new breeds. Pigeons,

I may add, can be propagated in great numbers and at a very quick rate,

and inferior birds may be freely rejected, as when killed they serve

for food. On the other hand, cats, from their nocturnal rambling habits,

cannot be matched, and, although so much valued by women and children,

we hardly ever see a distinct breed kept up; such breeds as we do

sometimes see are almost always imported from some other country, often

from islands. Although I do not doubt that some domestic animals vary

less than others, yet the rarity or absence of distinct breeds of the

cat, the donkey, peacock, goose, etc., may be attributed in main part

to selection not having been brought into play: in cats, from the

difficulty in pairing them; in donkeys, from only a few being kept by

poor people, and little attention paid to their breeding; in peacocks,

from not being very easily reared and a large stock not kept; in geese,

from being valuable only for two purposes, food and feathers, and more

especially from no pleasure having been felt in the display of distinct

breeds.

To sum up on the origin of our Domestic Races of animals and plants.

I believe that the conditions of life, from their action on the

reproductive system, are so far of the highest importance as causing

variability. I do not believe that variability is an inherent and

necessary contingency, under all circumstances, with all organic beings,

as some authors have thought. The effects of variability are modified by

various degrees of inheritance and of reversion. Variability is governed

by many unknown laws, more especially by that of correlation of growth.

Something may be attributed to the direct action of the conditions of

life. Something must be attributed to use and disuse. The final result

is thus rendered infinitely complex. In some cases, I do not doubt

that the intercrossing of species, aboriginally distinct, has played an

important part in the origin of our domestic productions. When in

any country several domestic breeds have once been established, their

occasional intercrossing, with the aid of selection, has, no doubt,

largely aided in the formation of new sub-breeds; but the importance of

the crossing of varieties has, I believe, been greatly exaggerated, both

in regard to animals and to those plants which are propagated by seed.

In plants which are temporarily propagated by cuttings, buds, etc., the

importance of the crossing both of distinct species and of varieties

is immense; for the cultivator here quite disregards the extreme

variability both of hybrids and mongrels, and the frequent sterility of

hybrids; but the cases of plants not propagated by seed are of little

importance to us, for their endurance is only temporary. Over all

these causes of Change I am convinced that the accumulative action

of Selection, whether applied methodically and more quickly, or

unconsciously and more slowly, but more efficiently, is by far the

predominant Power.

2. VARIATION UNDER NATURE.

Variability. Individual differences. Doubtful species. Wide ranging,

much diffused, and common species vary most. Species of the larger

genera in any country vary more than the species of the smaller genera.

Many of the species of the larger genera resemble varieties in being

very closely, but unequally, related to each other, and in having

restricted ranges.

Before applying the principles arrived at in the last chapter to organic

beings in a state of nature, we must briefly discuss whether these

latter are subject to any variation. To treat this subject at all

properly, a long catalogue of dry facts should be given; but these I

shall reserve for my future work. Nor shall I here discuss the various

definitions which have been given of the term species. No one definition

has as yet satisfied all naturalists; yet every naturalist knows vaguely

what he means when he speaks of a species. Generally the term includes

the unknown element of a distinct act of creation. The term "variety"

is almost equally difficult to define; but here community of descent is

almost universally implied, though it can rarely be proved. We have also

what are called monstrosities; but they graduate into varieties. By a

monstrosity I presume is meant some considerable deviation of structure

in one part, either injurious to or not useful to the species, and

not generally propagated. Some authors use the term "variation" in a

technical sense, as implying a modification directly due to the physical

conditions of life; and "variations" in this sense are supposed not to

be inherited: but who can say that the dwarfed condition of shells in

the brackish waters of the Baltic, or dwarfed plants on Alpine summits,

or the thicker fur of an animal from far northwards, would not in some

cases be inherited for at least some few generations? and in this case I

presume that the form would be called a variety.

Again, we have many slight differences which may be called individual

differences, such as are known frequently to appear in the offspring

from the same parents, or which may be presumed to have thus arisen,

from being frequently observed in the individuals of the same species

inhabiting the same confined locality. No one supposes that all the

individuals of the same species are cast in the very same mould. These

individual differences are highly important for us, as they afford

materials for natural selection to accumulate, in the same manner as

man can accumulate in any given direction individual differences in his

domesticated productions. These individual differences generally affect

what naturalists consider unimportant parts; but I could show by a long

catalogue of facts, that parts which must be called important, whether

viewed under a physiological or classificatory point of view, sometimes

vary in the individuals of the same species. I am convinced that the

most experienced naturalist would be surprised at the number of the

cases of variability, even in important parts of structure, which he

could collect on good authority, as I have collected, during a course of

years. It should be remembered that systematists are far from pleased at

finding variability in important characters, and that there are not

many men who will laboriously examine internal and important organs, and

compare them in many specimens of the same species. I should never

have expected that the branching of the main nerves close to the great

central ganglion of an insect would have been variable in the same

species; I should have expected that changes of this nature could have

been effected only by slow degrees: yet quite recently Mr. Lubbock has

shown a degree of variability in these main nerves in Coccus, which may

almost be compared to the irregular branching of the stem of a tree.

This philosophical naturalist, I may add, has also quite recently shown

that the muscles in the larvae of certain insects are very far from

uniform. Authors sometimes argue in a circle when they state that

important organs never vary; for these same authors practically rank

that character as important (as some few naturalists have honestly

confessed) which does not vary; and, under this point of view, no

instance of an important part varying will ever be found: but under any

other point of view many instances assuredly can be given.

There is one point connected with individual differences, which seems

to me extremely perplexing: I refer to those genera which have sometimes

been called "protean" or "polymorphic," in which the species present

an inordinate amount of variation; and hardly two naturalists can agree

which forms to rank as species and which as varieties. We may instance

Rubus, Rosa, and Hieracium amongst plants, several genera of insects,

and several genera of Brachiopod shells. In most polymorphic genera

some of the species have fixed and definite characters. Genera which

are polymorphic in one country seem to be, with some few exceptions,

polymorphic in other countries, and likewise, judging from Brachiopod

shells, at former periods of time. These facts seem to be very

perplexing, for they seem to show that this kind of variability is

independent of the conditions of life. I am inclined to suspect that we

see in these polymorphic genera variations in points of structure which

are of no service or disservice to the species, and which consequently

have not been seized on and rendered definite by natural selection, as

hereafter will be explained.

Those forms which possess in some considerable degree the character of

species, but which are so closely similar to some other forms, or are so

closely linked to them by intermediate gradations, that naturalists do

not like to rank them as distinct species, are in several respects the

most important for us. We have every reason to believe that many of

these doubtful and closely-allied forms have permanently retained their

characters in their own country for a long time; for as long, as far as

we know, as have good and true species. Practically, when a naturalist

can unite two forms together by others having intermediate characters,

he treats the one as a variety of the other, ranking the most common,

but sometimes the one first described, as the species, and the other

as the variety. But cases of great difficulty, which I will not here

enumerate, sometimes occur in deciding whether or not to rank one

form as a variety of another, even when they are closely connected by

intermediate links; nor will the commonly-assumed hybrid nature of the

intermediate links always remove the difficulty. In very many cases,

however, one form is ranked as a variety of another, not because the

intermediate links have actually been found, but because analogy leads

the observer to suppose either that they do now somewhere exist, or may

formerly have existed; and here a wide door for the entry of doubt and

conjecture is opened.

Hence, in determining whether a form should be ranked as a species or

a variety, the opinion of naturalists having sound judgment and wide

experience seems the only guide to follow. We must, however, in many

cases, decide by a majority of naturalists, for few well-marked and

well-known varieties can be named which have not been ranked as species

by at least some competent judges.

That varieties of this doubtful nature are far from uncommon cannot be

disputed. Compare the several floras of Great Britain, of France or

of the United States, drawn up by different botanists, and see what

a surprising number of forms have been ranked by one botanist as good

species, and by another as mere varieties. Mr. H. C. Watson, to whom I

lie under deep obligation for assistance of all kinds, has marked for

me 182 British plants, which are generally considered as varieties, but

which have all been ranked by botanists as species; and in making this

list he has omitted many trifling varieties, but which nevertheless have

been ranked by some botanists as species, and he has entirely omitted

several highly polymorphic genera. Under genera, including the most

polymorphic forms, Mr. Babington gives 251 species, whereas Mr. Bentham

gives only 112,--a difference of 139 doubtful forms! Amongst animals

which unite for each birth, and which are highly locomotive, doubtful

forms, ranked by one zoologist as a species and by another as a variety,

can rarely be found within the same country, but are common in separated

areas. How many of those birds and insects in North America and Europe,

which differ very slightly from each other, have been ranked by one

eminent naturalist as undoubted species, and by another as varieties,

or, as they are often called, as geographical races! Many years ago,

when comparing, and seeing others compare, the birds from the separate

islands of the Galapagos Archipelago, both one with another, and with

those from the American mainland, I was much struck how entirely vague

and arbitrary is the distinction between species and varieties. On the

islets of the little Madeira group there are many insects which are

characterized as varieties in Mr. Wollaston's admirable work, but

which it cannot be doubted would be ranked as distinct species by many

entomologists. Even Ireland has a few animals, now generally regarded

as varieties, but which have been ranked as species by some zoologists.

Several most experienced ornithologists consider our British red grouse

as only a strongly-marked race of a Norwegian species, whereas the

greater number rank it as an undoubted species peculiar to Great

Britain. A wide distance between the homes of two doubtful forms leads

many naturalists to rank both as distinct species; but what distance, it

has been well asked, will suffice? if that between America and Europe

is ample, will that between the Continent and the Azores, or Madeira, or

the Canaries, or Ireland, be sufficient? It must be admitted that many

forms, considered by highly-competent judges as varieties, have so

perfectly the character of species that they are ranked by other

highly-competent judges as good and true species. But to discuss whether

they are rightly called species or varieties, before any definition of

these terms has been generally accepted, is vainly to beat the air.

Many of the cases of strongly-marked varieties or doubtful species well

deserve consideration; for several interesting lines of argument, from

geographical distribution, analogical variation, hybridism, etc., have

been brought to bear on the attempt to determine their rank. I will here

give only a single instance,--the well-known one of the primrose and

cowslip, or Primula veris and elatior. These plants differ considerably

in appearance; they have a different flavour and emit a different

odour; they flower at slightly different periods; they grow in somewhat

different stations; they ascend mountains to different heights; they

have different geographical ranges; and lastly, according to very

numerous experiments made during several years by that most careful

observer Gartner, they can be crossed only with much difficulty.

We could hardly wish for better evidence of the two forms being

specifically distinct. On the other hand, they are united by many

intermediate links, and it is very doubtful whether these links are

hybrids; and there is, as it seems to me, an overwhelming amount of

experimental evidence, showing that they descend from common parents,

and consequently must be ranked as varieties.

Close investigation, in most cases, will bring naturalists to an

agreement how to rank doubtful forms. Yet it must be confessed, that it

is in the best-known countries that we find the greatest number of forms

of doubtful value. I have been struck with the fact, that if any animal

or plant in a state of nature be highly useful to man, or from any cause

closely attract his attention, varieties of it will almost universally

be found recorded. These varieties, moreover, will be often ranked by

some authors as species. Look at the common oak, how closely it has

been studied; yet a German author makes more than a dozen species out

of forms, which are very generally considered as varieties; and in

this country the highest botanical authorities and practical men can be

quoted to show that the sessile and pedunculated oaks are either good

and distinct species or mere varieties.

When a young naturalist commences the study of a group of organisms

quite unknown to him, he is at first much perplexed to determine what

differences to consider as specific, and what as varieties; for he

knows nothing of the amount and kind of variation to which the group

is subject; and this shows, at least, how very generally there is some

variation. But if he confine his attention to one class within one

country, he will soon make up his mind how to rank most of the doubtful

forms. His general tendency will be to make many species, for he will

become impressed, just like the pigeon or poultry-fancier before alluded

to, with the amount of difference in the forms which he is continually

studying; and he has little general knowledge of analogical variation

in other groups and in other countries, by which to correct his first

impressions. As he extends the range of his observations, he will meet

with more cases of difficulty; for he will encounter a greater number

of closely-allied forms. But if his observations be widely extended, he

will in the end generally be enabled to make up his own mind which to

call varieties and which species; but he will succeed in this at the

expense of admitting much variation,--and the truth of this admission

will often be disputed by other naturalists. When, moreover, he comes to

study allied forms brought from countries not now continuous, in which

case he can hardly hope to find the intermediate links between his

doubtful forms, he will have to trust almost entirely to analogy, and

his difficulties will rise to a climax.

Certainly no clear line of demarcation has as yet been drawn between

species and sub-species--that is, the forms which in the opinion of some

naturalists come very near to, but do not quite arrive at the rank of

species; or, again, between sub-species and well-marked varieties, or

between lesser varieties and individual differences. These differences

blend into each other in an insensible series; and a series impresses

the mind with the idea of an actual passage.

Hence I look at individual differences, though of small interest to

the systematist, as of high importance for us, as being the first step

towards such slight varieties as are barely thought worth recording

in works on natural history. And I look at varieties which are in any

degree more distinct and permanent, as steps leading to more strongly

marked and more permanent varieties; and at these latter, as leading to

sub-species, and to species. The passage from one stage of difference

to another and higher stage may be, in some cases, due merely to the

long-continued action of different physical conditions in two different

regions; but I have not much faith in this view; and I attribute the

passage of a variety, from a state in which it differs very slightly

from its parent to one in which it differs more, to the action of

natural selection in accumulating (as will hereafter be more fully

explained) differences of structure in certain definite directions.

Hence I believe a well-marked variety may be justly called an incipient

species; but whether this belief be justifiable must be judged of by

the general weight of the several facts and views given throughout this

work.

It need not be supposed that all varieties or incipient species

necessarily attain the rank of species. They may whilst in this

incipient state become extinct, or they may endure as varieties for very

long periods, as has been shown to be the case by Mr. Wollaston with the

varieties of certain fossil land-shells in Madeira. If a variety were

to flourish so as to exceed in numbers the parent species, it would then

rank as the species, and the species as the variety; or it might come to

supplant and exterminate the parent species; or both might co-exist, and

both rank as independent species. But we shall hereafter have to return

to this subject.

From these remarks it will be seen that I look at the term species,

as one arbitrarily given for the sake of convenience to a set of

individuals closely resembling each other, and that it does not

essentially differ from the term variety, which is given to less

distinct and more fluctuating forms. The term variety, again,

in comparison with mere individual differences, is also applied

arbitrarily, and for mere convenience sake.

Guided by theoretical considerations, I thought that some interesting

results might be obtained in regard to the nature and relations of the

species which vary most, by tabulating all the varieties in several

well-worked floras. At first this seemed a simple task; but Mr. H. C.

Watson, to whom I am much indebted for valuable advice and assistance

on this subject, soon convinced me that there were many difficulties, as

did subsequently Dr. Hooker, even in stronger terms. I shall reserve

for my future work the discussion of these difficulties, and the tables

themselves of the proportional numbers of the varying species.

Dr. Hooker permits me to add, that after having carefully read my

manuscript, and examined the tables, he thinks that the following

statements are fairly well established. The whole subject, however,

treated as it necessarily here is with much brevity, is rather

perplexing, and allusions cannot be avoided to the "struggle for

existence," "divergence of character," and other questions, hereafter to

be discussed.

Alph. De Candolle and others have shown that plants which have very wide

ranges generally present varieties; and this might have been expected,

as they become exposed to diverse physical conditions, and as they

come into competition (which, as we shall hereafter see, is a far more

important circumstance) with different sets of organic beings. But my

tables further show that, in any limited country, the species which are

most common, that is abound most in individuals, and the species

which are most widely diffused within their own country (and this is a

different consideration from wide range, and to a certain extent from

commonness), often give rise to varieties sufficiently well-marked to

have been recorded in botanical works. Hence it is the most flourishing,

or, as they may be called, the dominant species,--those which range

widely over the world, are the most diffused in their own country,

and are the most numerous in individuals,--which oftenest produce

well-marked varieties, or, as I consider them, incipient species. And

this, perhaps, might have been anticipated; for, as varieties, in order

to become in any degree permanent, necessarily have to struggle with the

other inhabitants of the country, the species which are already dominant

will be the most likely to yield offspring which, though in some slight

degree modified, will still inherit those advantages that enabled their

parents to become dominant over their compatriots.

If the plants inhabiting a country and described in any Flora be divided

into two equal masses, all those in the larger genera being placed

on one side, and all those in the smaller genera on the other side, a

somewhat larger number of the very common and much diffused or dominant

species will be found on the side of the larger genera. This, again,

might have been anticipated; for the mere fact of many species of the

same genus inhabiting any country, shows that there is something in the

organic or inorganic conditions of that country favourable to the genus;

and, consequently, we might have expected to have found in the larger

genera, or those including many species, a large proportional number of

dominant species. But so many causes tend to obscure this result, that

I am surprised that my tables show even a small majority on the side of

the larger genera. I will here allude to only two causes of obscurity.

Fresh-water and salt-loving plants have generally very wide ranges and

are much diffused, but this seems to be connected with the nature of the

stations inhabited by them, and has little or no relation to the size of

the genera to which the species belong. Again, plants low in the scale

of organisation are generally much more widely diffused than plants

higher in the scale; and here again there is no close relation to the

size of the genera. The cause of lowly-organised plants ranging widely

will be discussed in our chapter on geographical distribution.

From looking at species as only strongly-marked and well-defined

varieties, I was led to anticipate that the species of the larger genera

in each country would oftener present varieties, than the species of the

smaller genera; for wherever many closely related species (i.e. species

of the same genus) have been formed, many varieties or incipient species

ought, as a general rule, to be now forming. Where many large trees

grow, we expect to find saplings. Where many species of a genus have

been formed through variation, circumstances have been favourable

for variation; and hence we might expect that the circumstances would

generally be still favourable to variation. On the other hand, if we

look at each species as a special act of creation, there is no apparent

reason why more varieties should occur in a group having many species,

than in one having few.

To test the truth of this anticipation I have arranged the plants of

twelve countries, and the coleopterous insects of two districts, into

two nearly equal masses, the species of the larger genera on one side,

and those of the smaller genera on the other side, and it has invariably

proved to be the case that a larger proportion of the species on the

side of the larger genera present varieties, than on the side of the

smaller genera. Moreover, the species of the large genera which present

any varieties, invariably present a larger average number of varieties

than do the species of the small genera. Both these results follow when

another division is made, and when all the smallest genera, with from

only one to four species, are absolutely excluded from the tables.

These facts are of plain signification on the view that species are only

strongly marked and permanent varieties; for wherever many species of

the same genus have been formed, or where, if we may use the expression,

the manufactory of species has been active, we ought generally to find

the manufactory still in action, more especially as we have every reason

to believe the process of manufacturing new species to be a slow one.

And this certainly is the case, if varieties be looked at as incipient

species; for my tables clearly show as a general rule that, wherever

many species of a genus have been formed, the species of that genus

present a number of varieties, that is of incipient species, beyond the

average. It is not that all large genera are now varying much, and are

thus increasing in the number of their species, or that no small genera

are now varying and increasing; for if this had been so, it would have

been fatal to my theory; inasmuch as geology plainly tells us that small

genera have in the lapse of time often increased greatly in size;

and that large genera have often come to their maxima, declined, and

disappeared. All that we want to show is, that where many species of a

genus have been formed, on an average many are still forming; and this

holds good.

There are other relations between the species of large genera and their

recorded varieties which deserve notice. We have seen that there is no

infallible criterion by which to distinguish species and well-marked

varieties; and in those cases in which intermediate links have not been

found between doubtful forms, naturalists are compelled to come to

a determination by the amount of difference between them, judging by

analogy whether or not the amount suffices to raise one or both to the

rank of species. Hence the amount of difference is one very important

criterion in settling whether two forms should be ranked as species or

varieties. Now Fries has remarked in regard to plants, and Westwood in

regard to insects, that in large genera the amount of difference between

the species is often exceedingly small. I have endeavoured to test this

numerically by averages, and, as far as my imperfect results go, they

always confirm the view. I have also consulted some sagacious and most

experienced observers, and, after deliberation, they concur in this

view. In this respect, therefore, the species of the larger genera

resemble varieties, more than do the species of the smaller genera.

Or the case may be put in another way, and it may be said, that in

the larger genera, in which a number of varieties or incipient species

greater than the average are now manufacturing, many of the species

already manufactured still to a certain extent resemble varieties, for

they differ from each other by a less than usual amount of difference.

Moreover, the species of the large genera are related to each other, in

the same manner as the varieties of any one species are related to

each other. No naturalist pretends that all the species of a genus are

equally distinct from each other; they may generally be divided into

sub-genera, or sections, or lesser groups. As Fries has well remarked,

little groups of species are generally clustered like satellites around

certain other species. And what are varieties but groups of forms,

unequally related to each other, and clustered round certain forms--that

is, round their parent-species? Undoubtedly there is one most important

point of difference between varieties and species; namely, that the

amount of difference between varieties, when compared with each other or

with their parent-species, is much less than that between the species of

the same genus. But when we come to discuss the principle, as I call it,

of Divergence of Character, we shall see how this may be explained, and

how the lesser differences between varieties will tend to increase into

the greater differences between species.

There is one other point which seems to me worth notice. Varieties

generally have much restricted ranges: this statement is indeed scarcely

more than a truism, for if a variety were found to have a wider range

than that of its supposed parent-species, their denominations ought to

be reversed. But there is also reason to believe, that those species

which are very closely allied to other species, and in so far resemble

varieties, often have much restricted ranges. For instance, Mr. H. C.

Watson has marked for me in the well-sifted London Catalogue of plants

(4th edition) 63 plants which are therein ranked as species, but which

he considers as so closely allied to other species as to be of doubtful

value: these 63 reputed species range on an average over 6.9 of the

provinces into which Mr. Watson has divided Great Britain. Now, in this

same catalogue, 53 acknowledged varieties are recorded, and these range

over 7.7 provinces; whereas, the species to which these varieties belong

range over 14.3 provinces. So that the acknowledged varieties have very

nearly the same restricted average range, as have those very closely

allied forms, marked for me by Mr. Watson as doubtful species, but which

are almost universally ranked by British botanists as good and true

species.

Finally, then, varieties have the same general characters as species,

for they cannot be distinguished from species,--except, firstly, by

the discovery of intermediate linking forms, and the occurrence of

such links cannot affect the actual characters of the forms which they

connect; and except, secondly, by a certain amount of difference, for

two forms, if differing very little, are generally ranked as varieties,

notwithstanding that intermediate linking forms have not been

discovered; but the amount of difference considered necessary to give to

two forms the rank of species is quite indefinite. In genera having more

than the average number of species in any country, the species of these

genera have more than the average number of varieties. In large genera

the species are apt to be closely, but unequally, allied together,

forming little clusters round certain species. Species very closely

allied to other species apparently have restricted ranges. In all these

several respects the species of large genera present a strong analogy

with varieties. And we can clearly understand these analogies, if

species have once existed as varieties, and have thus originated:

whereas, these analogies are utterly inexplicable if each species has

been independently created.

We have, also, seen that it is the most flourishing and dominant species

of the larger genera which on an average vary most; and varieties, as

we shall hereafter see, tend to become converted into new and distinct

species. The larger genera thus tend to become larger; and throughout

nature the forms of life which are now dominant tend to become still

more dominant by leaving many modified and dominant descendants. But by

steps hereafter to be explained, the larger genera also tend to break up

into smaller genera. And thus, the forms of life throughout the universe

become divided into groups subordinate to groups.

3. STRUGGLE FOR EXISTENCE.

Bears on natural selection. The term used in a wide sense. Geometrical

powers of increase. Rapid increase of naturalised animals and plants.

Nature of the checks to increase. Competition universal. Effects of

climate. Protection from the number of individuals. Complex relations of

all animals and plants throughout nature. Struggle for life most severe

between individuals and varieties of the same species; often severe

between species of the same genus. The relation of organism to organism

the most important of all relations.

Before entering on the subject of this chapter, I must make a few

preliminary remarks, to show how the struggle for existence bears on

Natural Selection. It has been seen in the last chapter that

amongst organic beings in a state of nature there is some individual

variability; indeed I am not aware that this has ever been disputed.

It is immaterial for us whether a multitude of doubtful forms be called

species or sub-species or varieties; what rank, for instance, the two or

three hundred doubtful forms of British plants are entitled to hold,

if the existence of any well-marked varieties be admitted. But the

mere existence of individual variability and of some few well-marked

varieties, though necessary as the foundation for the work, helps us but

little in understanding how species arise in nature. How have all those

exquisite adaptations of one part of the organisation to another part,

and to the conditions of life, and of one distinct organic being to

another being, been perfected? We see these beautiful co-adaptations

most plainly in the woodpecker and missletoe; and only a little

less plainly in the humblest parasite which clings to the hairs of a

quadruped or feathers of a bird; in the structure of the beetle which

dives through the water; in the plumed seed which is wafted by the

gentlest breeze; in short, we see beautiful adaptations everywhere and

in every part of the organic world.

Again, it may be asked, how is it that varieties, which I have called

incipient species, become ultimately converted into good and distinct

species, which in most cases obviously differ from each other far

more than do the varieties of the same species? How do those groups of

species, which constitute what are called distinct genera, and which

differ from each other more than do the species of the same genus,

arise? All these results, as we shall more fully see in the next

chapter, follow inevitably from the struggle for life. Owing to this

struggle for life, any variation, however slight and from whatever cause

proceeding, if it be in any degree profitable to an individual of any

species, in its infinitely complex relations to other organic beings and

to external nature, will tend to the preservation of that individual,

and will generally be inherited by its offspring. The offspring,

also, will thus have a better chance of surviving, for, of the many

individuals of any species which are periodically born, but a small

number can survive. I have called this principle, by which each slight

variation, if useful, is preserved, by the term of Natural Selection,

in order to mark its relation to man's power of selection. We have seen

that man by selection can certainly produce great results, and can adapt

organic beings to his own uses, through the accumulation of slight

but useful variations, given to him by the hand of Nature. But Natural

Selection, as we shall hereafter see, is a power incessantly ready for

action, and is as immeasurably superior to man's feeble efforts, as the

works of Nature are to those of Art.

We will now discuss in a little more detail the struggle for existence.

In my future work this subject shall be treated, as it well deserves,

at much greater length. The elder De Candolle and Lyell have largely

and philosophically shown that all organic beings are exposed to severe

competition. In regard to plants, no one has treated this subject with

more spirit and ability than W. Herbert, Dean of Manchester, evidently

the result of his great horticultural knowledge. Nothing is easier than

to admit in words the truth of the universal struggle for life, or more

difficult--at least I have found it so--than constantly to bear this

conclusion in mind. Yet unless it be thoroughly engrained in the mind,

I am convinced that the whole economy of nature, with every fact on

distribution, rarity, abundance, extinction, and variation, will be

dimly seen or quite misunderstood. We behold the face of nature bright

with gladness, we often see superabundance of food; we do not see, or

we forget, that the birds which are idly singing round us mostly live on

insects or seeds, and are thus constantly destroying life; or we forget

how largely these songsters, or their eggs, or their nestlings, are

destroyed by birds and beasts of prey; we do not always bear in mind,

that though food may be now superabundant, it is not so at all seasons

of each recurring year.

I should premise that I use the term Struggle for Existence in a large

and metaphorical sense, including dependence of one being on another,

and including (which is more important) not only the life of the

individual, but success in leaving progeny. Two canine animals in a time

of dearth, may be truly said to struggle with each other which shall get

food and live. But a plant on the edge of a desert is said to struggle

for life against the drought, though more properly it should be said to

be dependent on the moisture. A plant which annually produces a thousand

seeds, of which on an average only one comes to maturity, may be more

truly said to struggle with the plants of the same and other kinds which

already clothe the ground. The missletoe is dependent on the apple and a

few other trees, but can only in a far-fetched sense be said to struggle

with these trees, for if too many of these parasites grow on the same

tree, it will languish and die. But several seedling missletoes, growing

close together on the same branch, may more truly be said to struggle

with each other. As the missletoe is disseminated by birds, its

existence depends on birds; and it may metaphorically be said to

struggle with other fruit-bearing plants, in order to tempt birds to

devour and thus disseminate its seeds rather than those of other

plants. In these several senses, which pass into each other, I use for

convenience sake the general term of struggle for existence.

A struggle for existence inevitably follows from the high rate at which

all organic beings tend to increase. Every being, which during its

natural lifetime produces several eggs or seeds, must suffer destruction

during some period of its life, and during some season or occasional

year, otherwise, on the principle of geometrical increase, its numbers

would quickly become so inordinately great that no country could support

the product. Hence, as more individuals are produced than can possibly

survive, there must in every case be a struggle for existence, either

one individual with another of the same species, or with the individuals

of distinct species, or with the physical conditions of life. It is the

doctrine of Malthus applied with manifold force to the whole animal and

vegetable kingdoms; for in this case there can be no artificial increase

of food, and no prudential restraint from marriage. Although some

species may be now increasing, more or less rapidly, in numbers, all

cannot do so, for the world would not hold them.

There is no exception to the rule that every organic being naturally

increases at so high a rate, that if not destroyed, the earth would soon

be covered by the progeny of a single pair. Even slow-breeding man has

doubled in twenty-five years, and at this rate, in a few thousand years,

there would literally not be standing room for his progeny. Linnaeus has

calculated that if an annual plant produced only two seeds--and there is

no plant so unproductive as this--and their seedlings next year produced

two, and so on, then in twenty years there would be a million plants.

The elephant is reckoned to be the slowest breeder of all known animals,

and I have taken some pains to estimate its probable minimum rate of

natural increase: it will be under the mark to assume that it breeds

when thirty years old, and goes on breeding till ninety years old,

bringing forth three pair of young in this interval; if this be so,

at the end of the fifth century there would be alive fifteen million

elephants, descended from the first pair.

But we have better evidence on this subject than mere theoretical

calculations, namely, the numerous recorded cases of the astonishingly

rapid increase of various animals in a state of nature, when

circumstances have been favourable to them during two or three following

seasons. Still more striking is the evidence from our domestic animals

of many kinds which have run wild in several parts of the world: if the

statements of the rate of increase of slow-breeding cattle and horses

in South America, and latterly in Australia, had not been well

authenticated, they would have been quite incredible. So it is with

plants: cases could be given of introduced plants which have become

common throughout whole islands in a period of less than ten years.

Several of the plants now most numerous over the wide plains of La

Plata, clothing square leagues of surface almost to the exclusion of

all other plants, have been introduced from Europe; and there are plants

which now range in India, as I hear from Dr. Falconer, from Cape

Comorin to the Himalaya, which have been imported from America since its

discovery. In such cases, and endless instances could be given, no one

supposes that the fertility of these animals or plants has been

suddenly and temporarily increased in any sensible degree. The obvious

explanation is that the conditions of life have been very favourable,

and that there has consequently been less destruction of the old and

young, and that nearly all the young have been enabled to breed. In such

cases the geometrical ratio of increase, the result of which never fails

to be surprising, simply explains the extraordinarily rapid increase and

wide diffusion of naturalised productions in their new homes.

In a state of nature almost every plant produces seed, and amongst

animals there are very few which do not annually pair. Hence we may

confidently assert, that all plants and animals are tending to increase

at a geometrical ratio, that all would most rapidly stock every station

in which they could any how exist, and that the geometrical tendency

to increase must be checked by destruction at some period of life. Our

familiarity with the larger domestic animals tends, I think, to mislead

us: we see no great destruction falling on them, and we forget that

thousands are annually slaughtered for food, and that in a state of

nature an equal number would have somehow to be disposed of.

The only difference between organisms which annually produce eggs or

seeds by the thousand, and those which produce extremely few, is,

that the slow-breeders would require a few more years to people, under

favourable conditions, a whole district, let it be ever so large. The

condor lays a couple of eggs and the ostrich a score, and yet in the

same country the condor may be the more numerous of the two: the Fulmar

petrel lays but one egg, yet it is believed to be the most numerous bird

in the world. One fly deposits hundreds of eggs, and another, like the

hippobosca, a single one; but this difference does not determine how

many individuals of the two species can be supported in a district.

A large number of eggs is of some importance to those species, which

depend on a rapidly fluctuating amount of food, for it allows them

rapidly to increase in number. But the real importance of a large number

of eggs or seeds is to make up for much destruction at some period of

life; and this period in the great majority of cases is an early one. If

an animal can in any way protect its own eggs or young, a small number

may be produced, and yet the average stock be fully kept up; but if many

eggs or young are destroyed, many must be produced, or the species will

become extinct. It would suffice to keep up the full number of a tree,

which lived on an average for a thousand years, if a single seed were

produced once in a thousand years, supposing that this seed were never

destroyed, and could be ensured to germinate in a fitting place. So that

in all cases, the average number of any animal or plant depends only

indirectly on the number of its eggs or seeds.

In looking at Nature, it is most necessary to keep the foregoing

considerations always in mind--never to forget that every single organic

being around us may be said to be striving to the utmost to increase in

numbers; that each lives by a struggle at some period of its life; that

heavy destruction inevitably falls either on the young or old, during

each generation or at recurrent intervals. Lighten any check, mitigate

the destruction ever so little, and the number of the species will

almost instantaneously increase to any amount. The face of Nature may

be compared to a yielding surface, with ten thousand sharp wedges packed

close together and driven inwards by incessant blows, sometimes one

wedge being struck, and then another with greater force.

What checks the natural tendency of each species to increase in number

is most obscure. Look at the most vigorous species; by as much as it

swarms in numbers, by so much will its tendency to increase be still

further increased. We know not exactly what the checks are in even

one single instance. Nor will this surprise any one who reflects how

ignorant we are on this head, even in regard to mankind, so incomparably

better known than any other animal. This subject has been ably treated

by several authors, and I shall, in my future work, discuss some of the

checks at considerable length, more especially in regard to the feral

animals of South America. Here I will make only a few remarks, just to

recall to the reader's mind some of the chief points. Eggs or very young

animals seem generally to suffer most, but this is not invariably the

case. With plants there is a vast destruction of seeds, but, from some

observations which I have made, I believe that it is the seedlings which

suffer most from germinating in ground already thickly stocked with

other plants. Seedlings, also, are destroyed in vast numbers by various

enemies; for instance, on a piece of ground three feet long and two

wide, dug and cleared, and where there could be no choking from other

plants, I marked all the seedlings of our native weeds as they came up,

and out of the 357 no less than 295 were destroyed, chiefly by slugs

and insects. If turf which has long been mown, and the case would be the

same with turf closely browsed by quadrupeds, be let to grow, the more

vigorous plants gradually kill the less vigorous, though fully grown,

plants: thus out of twenty species growing on a little plot of turf

(three feet by four) nine species perished from the other species being

allowed to grow up freely.

The amount of food for each species of course gives the extreme limit

to which each can increase; but very frequently it is not the obtaining

food, but the serving as prey to other animals, which determines the

average numbers of a species. Thus, there seems to be little doubt that

the stock of partridges, grouse, and hares on any large estate depends

chiefly on the destruction of vermin. If not one head of game were shot

during the next twenty years in England, and, at the same time, if no

vermin were destroyed, there would, in all probability, be less game

than at present, although hundreds of thousands of game animals are now

annually killed. On the other hand, in some cases, as with the elephant

and rhinoceros, none are destroyed by beasts of prey: even the tiger in

India most rarely dares to attack a young elephant protected by its dam.

Climate plays an important part in determining the average numbers of a

species, and periodical seasons of extreme cold or drought, I believe

to be the most effective of all checks. I estimated that the winter of

1854-55 destroyed four-fifths of the birds in my own grounds; and this

is a tremendous destruction, when we remember that ten per cent. is an

extraordinarily severe mortality from epidemics with man. The action of

climate seems at first sight to be quite independent of the struggle for

existence; but in so far as climate chiefly acts in reducing food, it

brings on the most severe struggle between the individuals, whether of

the same or of distinct species, which subsist on the same kind of food.

Even when climate, for instance extreme cold, acts directly, it will

be the least vigorous, or those which have got least food through the

advancing winter, which will suffer most. When we travel from south to

north, or from a damp region to a dry, we invariably see some species

gradually getting rarer and rarer, and finally disappearing; and the

change of climate being conspicuous, we are tempted to attribute the

whole effect to its direct action. But this is a very false view: we

forget that each species, even where it most abounds, is constantly

suffering enormous destruction at some period of its life, from enemies

or from competitors for the same place and food; and if these enemies

or competitors be in the least degree favoured by any slight change of

climate, they will increase in numbers, and, as each area is already

fully stocked with inhabitants, the other species will decrease. When

we travel southward and see a species decreasing in numbers, we may feel

sure that the cause lies quite as much in other species being favoured,

as in this one being hurt. So it is when we travel northward, but in

a somewhat lesser degree, for the number of species of all kinds,

and therefore of competitors, decreases northwards; hence in going

northward, or in ascending a mountain, we far oftener meet with stunted

forms, due to the DIRECTLY injurious action of climate, than we do in

proceeding southwards or in descending a mountain. When we reach

the Arctic regions, or snow-capped summits, or absolute deserts, the

struggle for life is almost exclusively with the elements.

That climate acts in main part indirectly by favouring other species, we

may clearly see in the prodigious number of plants in our gardens

which can perfectly well endure our climate, but which never become

naturalised, for they cannot compete with our native plants, nor resist

destruction by our native animals.

When a species, owing to highly favourable circumstances, increases

inordinately in numbers in a small tract, epidemics--at least, this

seems generally to occur with our game animals--often ensue: and here

we have a limiting check independent of the struggle for life. But even

some of these so-called epidemics appear to be due to parasitic worms,

which have from some cause, possibly in part through facility of

diffusion amongst the crowded animals, been disproportionably favoured:

and here comes in a sort of struggle between the parasite and its prey.

On the other hand, in many cases, a large stock of individuals of the

same species, relatively to the numbers of its enemies, is absolutely

necessary for its preservation. Thus we can easily raise plenty of

corn and rape-seed, etc., in our fields, because the seeds are in great

excess compared with the number of birds which feed on them; nor can

the birds, though having a superabundance of food at this one season,

increase in number proportionally to the supply of seed, as their

numbers are checked during winter: but any one who has tried, knows how

troublesome it is to get seed from a few wheat or other such plants in

a garden; I have in this case lost every single seed. This view of the

necessity of a large stock of the same species for its preservation,

explains, I believe, some singular facts in nature, such as that of very

rare plants being sometimes extremely abundant in the few spots where

they do occur; and that of some social plants being social, that is,

abounding in individuals, even on the extreme confines of their range.

For in such cases, we may believe, that a plant could exist only where

the conditions of its life were so favourable that many could exist

together, and thus save each other from utter destruction. I should add

that the good effects of frequent intercrossing, and the ill effects of

close interbreeding, probably come into play in some of these cases; but

on this intricate subject I will not here enlarge.

Many cases are on record showing how complex and unexpected are the

checks and relations between organic beings, which have to struggle

together in the same country. I will give only a single instance, which,

though a simple one, has interested me. In Staffordshire, on the estate

of a relation where I had ample means of investigation, there was a

large and extremely barren heath, which had never been touched by the

hand of man; but several hundred acres of exactly the same nature had

been enclosed twenty-five years previously and planted with Scotch fir.

The change in the native vegetation of the planted part of the heath was

most remarkable, more than is generally seen in passing from one quite

different soil to another: not only the proportional numbers of the

heath-plants were wholly changed, but twelve species of plants (not

counting grasses and carices) flourished in the plantations, which could

not be found on the heath. The effect on the insects must have been

still greater, for six insectivorous birds were very common in the

plantations, which were not to be seen on the heath; and the heath was

frequented by two or three distinct insectivorous birds. Here we see how

potent has been the effect of the introduction of a single tree, nothing

whatever else having been done, with the exception that the land had

been enclosed, so that cattle could not enter. But how important an

element enclosure is, I plainly saw near Farnham, in Surrey. Here

there are extensive heaths, with a few clumps of old Scotch firs on

the distant hill-tops: within the last ten years large spaces have been

enclosed, and self-sown firs are now springing up in multitudes, so

close together that all cannot live.

When I ascertained that these young trees had not been sown or planted,

I was so much surprised at their numbers that I went to several points

of view, whence I could examine hundreds of acres of the unenclosed

heath, and literally I could not see a single Scotch fir, except the old

planted clumps. But on looking closely between the stems of the heath,

I found a multitude of seedlings and little trees, which had been

perpetually browsed down by the cattle. In one square yard, at a

point some hundred yards distant from one of the old clumps, I counted

thirty-two little trees; and one of them, judging from the rings of

growth, had during twenty-six years tried to raise its head above the

stems of the heath, and had failed. No wonder that, as soon as the land

was enclosed, it became thickly clothed with vigorously growing young

firs. Yet the heath was so extremely barren and so extensive that no

one would ever have imagined that cattle would have so closely and

effectually searched it for food.

Here we see that cattle absolutely determine the existence of the Scotch

fir; but in several parts of the world insects determine the existence

of cattle. Perhaps Paraguay offers the most curious instance of this;

for here neither cattle nor horses nor dogs have ever run wild, though

they swarm southward and northward in a feral state; and Azara and

Rengger have shown that this is caused by the greater number in Paraguay

of a certain fly, which lays its eggs in the navels of these animals

when first born. The increase of these flies, numerous as they are,

must be habitually checked by some means, probably by birds. Hence, if

certain insectivorous birds (whose numbers are probably regulated by

hawks or beasts of prey) were to increase in Paraguay, the flies would

decrease--then cattle and horses would become feral, and this would

certainly greatly alter (as indeed I have observed in parts of South

America) the vegetation: this again would largely affect the insects;

and this, as we just have seen in Staffordshire, the insectivorous

birds, and so onwards in ever-increasing circles of complexity. We began

this series by insectivorous birds, and we have ended with them. Not

that in nature the relations can ever be as simple as this. Battle

within battle must ever be recurring with varying success; and yet in

the long-run the forces are so nicely balanced, that the face of nature

remains uniform for long periods of time, though assuredly the merest

trifle would often give the victory to one organic being over another.

Nevertheless so profound is our ignorance, and so high our presumption,

that we marvel when we hear of the extinction of an organic being; and

as we do not see the cause, we invoke cataclysms to desolate the world,

or invent laws on the duration of the forms of life!

I am tempted to give one more instance showing how plants and animals,

most remote in the scale of nature, are bound together by a web of

complex relations. I shall hereafter have occasion to show that the

exotic Lobelia fulgens, in this part of England, is never visited by

insects, and consequently, from its peculiar structure, never can set a

seed. Many of our orchidaceous plants absolutely require the visits of

moths to remove their pollen-masses and thus to fertilise them. I

have, also, reason to believe that humble-bees are indispensable to the

fertilisation of the heartsease (Viola tricolor), for other bees do not

visit this flower. From experiments which I have tried, I have found

that the visits of bees, if not indispensable, are at least highly

beneficial to the fertilisation of our clovers; but humble-bees alone

visit the common red clover (Trifolium pratense), as other bees cannot

reach the nectar. Hence I have very little doubt, that if the whole

genus of humble-bees became extinct or very rare in England, the

heartsease and red clover would become very rare, or wholly disappear.

The number of humble-bees in any district depends in a great degree on

the number of field-mice, which destroy their combs and nests; and Mr.

H. Newman, who has long attended to the habits of humble-bees, believes

that "more than two thirds of them are thus destroyed all over England."

Now the number of mice is largely dependent, as every one knows, on the

number of cats; and Mr. Newman says, "Near villages and small towns I

have found the nests of humble-bees more numerous than elsewhere, which

I attribute to the number of cats that destroy the mice." Hence it is

quite credible that the presence of a feline animal in large numbers in

a district might determine, through the intervention first of mice and

then of bees, the frequency of certain flowers in that district!

In the case of every species, many different checks, acting at different

periods of life, and during different seasons or years, probably come

into play; some one check or some few being generally the most potent,

but all concurring in determining the average number or even

the existence of the species. In some cases it can be shown that

widely-different checks act on the same species in different districts.

When we look at the plants and bushes clothing an entangled bank, we

are tempted to attribute their proportional numbers and kinds to what we

call chance. But how false a view is this! Every one has heard that when

an American forest is cut down, a very different vegetation springs

up; but it has been observed that the trees now growing on the ancient

Indian mounds, in the Southern United States, display the same beautiful

diversity and proportion of kinds as in the surrounding virgin forests.

What a struggle between the several kinds of trees must here have gone

on during long centuries, each annually scattering its seeds by the

thousand; what war between insect and insect--between insects, snails,

and other animals with birds and beasts of prey--all striving to

increase, and all feeding on each other or on the trees or their seeds

and seedlings, or on the other plants which first clothed the ground and

thus checked the growth of the trees! Throw up a handful of feathers,

and all must fall to the ground according to definite laws; but how

simple is this problem compared to the action and reaction of the

innumerable plants and animals which have determined, in the course of

centuries, the proportional numbers and kinds of trees now growing on

the old Indian ruins!

The dependency of one organic being on another, as of a parasite on its

prey, lies generally between beings remote in the scale of nature. This

is often the case with those which may strictly be said to struggle with

each other for existence, as in the case of locusts and grass-feeding

quadrupeds. But the struggle almost invariably will be most severe

between the individuals of the same species, for they frequent the same

districts, require the same food, and are exposed to the same dangers.

In the case of varieties of the same species, the struggle will

generally be almost equally severe, and we sometimes see the contest

soon decided: for instance, if several varieties of wheat be sown

together, and the mixed seed be resown, some of the varieties which best

suit the soil or climate, or are naturally the most fertile, will beat

the others and so yield more seed, and will consequently in a few years

quite supplant the other varieties. To keep up a mixed stock of even

such extremely close varieties as the variously coloured sweet-peas,

they must be each year harvested separately, and the seed then mixed

in due proportion, otherwise the weaker kinds will steadily decrease in

numbers and disappear. So again with the varieties of sheep: it has

been asserted that certain mountain-varieties will starve out other

mountain-varieties, so that they cannot be kept together. The same

result has followed from keeping together different varieties of the

medicinal leech. It may even be doubted whether the varieties of any

one of our domestic plants or animals have so exactly the same strength,

habits, and constitution, that the original proportions of a mixed stock

could be kept up for half a dozen generations, if they were allowed to

struggle together, like beings in a state of nature, and if the seed or

young were not annually sorted.

As species of the same genus have usually, though by no means

invariably, some similarity in habits and constitution, and always in

structure, the struggle will generally be more severe between species

of the same genus, when they come into competition with each other, than

between species of distinct genera. We see this in the recent extension

over parts of the United States of one species of swallow having

caused the decrease of another species. The recent increase of the

missel-thrush in parts of Scotland has caused the decrease of the

song-thrush. How frequently we hear of one species of rat taking the

place of another species under the most different climates! In Russia

the small Asiatic cockroach has everywhere driven before it its great

congener. One species of charlock will supplant another, and so in

other cases. We can dimly see why the competition should be most severe

between allied forms, which fill nearly the same place in the economy

of nature; but probably in no one case could we precisely say why one

species has been victorious over another in the great battle of life.

A corollary of the highest importance may be deduced from the foregoing

remarks, namely, that the structure of every organic being is related,

in the most essential yet often hidden manner, to that of all other

organic beings, with which it comes into competition for food or

residence, or from which it has to escape, or on which it preys. This

is obvious in the structure of the teeth and talons of the tiger; and in

that of the legs and claws of the parasite which clings to the hair on

the tiger's body. But in the beautifully plumed seed of the dandelion,

and in the flattened and fringed legs of the water-beetle, the relation

seems at first confined to the elements of air and water. Yet the

advantage of plumed seeds no doubt stands in the closest relation to the

land being already thickly clothed by other plants; so that the

seeds may be widely distributed and fall on unoccupied ground. In the

water-beetle, the structure of its legs, so well adapted for diving,

allows it to compete with other aquatic insects, to hunt for its own

prey, and to escape serving as prey to other animals.

The store of nutriment laid up within the seeds of many plants seems at

first sight to have no sort of relation to other plants. But from the

strong growth of young plants produced from such seeds (as peas and

beans), when sown in the midst of long grass, I suspect that the chief

use of the nutriment in the seed is to favour the growth of the young

seedling, whilst struggling with other plants growing vigorously all

around.

Look at a plant in the midst of its range, why does it not double or

quadruple its numbers? We know that it can perfectly well withstand a

little more heat or cold, dampness or dryness, for elsewhere it ranges

into slightly hotter or colder, damper or drier districts. In this case

we can clearly see that if we wished in imagination to give the plant

the power of increasing in number, we should have to give it some

advantage over its competitors, or over the animals which preyed on it.

On the confines of its geographical range, a change of constitution with

respect to climate would clearly be an advantage to our plant; but we

have reason to believe that only a few plants or animals range so far,

that they are destroyed by the rigour of the climate alone. Not until

we reach the extreme confines of life, in the arctic regions or on the

borders of an utter desert, will competition cease. The land may be

extremely cold or dry, yet there will be competition between some few

species, or between the individuals of the same species, for the warmest

or dampest spots.

Hence, also, we can see that when a plant or animal is placed in a new

country amongst new competitors, though the climate may be exactly

the same as in its former home, yet the conditions of its life will

generally be changed in an essential manner. If we wished to increase

its average numbers in its new home, we should have to modify it in a

different way to what we should have done in its native country; for

we should have to give it some advantage over a different set of

competitors or enemies.

It is good thus to try in our imagination to give any form some

advantage over another. Probably in no single instance should we know

what to do, so as to succeed. It will convince us of our ignorance on

the mutual relations of all organic beings; a conviction as necessary,

as it seems to be difficult to acquire. All that we can do, is to keep

steadily in mind that each organic being is striving to increase at a

geometrical ratio; that each at some period of its life, during some

season of the year, during each generation or at intervals, has to

struggle for life, and to suffer great destruction. When we reflect on

this struggle, we may console ourselves with the full belief, that the

war of nature is not incessant, that no fear is felt, that death is

generally prompt, and that the vigorous, the healthy, and the happy

survive and multiply.

4.

NATURAL SELECTION.

Natural Selection: its power compared with man's selection, its power

on characters of trifling importance, its power at all ages and on

both sexes. Sexual Selection. On the generality of intercrosses

between individuals of the same species. Circumstances favourable and

unfavourable to Natural Selection, namely, intercrossing, isolation,

number of individuals. Slow action. Extinction caused by Natural

Selection. Divergence of Character, related to the diversity of

inhabitants of any small area, and to naturalisation. Action of Natural

Selection, through Divergence of Character and Extinction, on the

descendants from a common parent. Explains the Grouping of all organic

beings.

How will the struggle for existence, discussed too briefly in the last

chapter, act in regard to variation? Can the principle of selection,

which we have seen is so potent in the hands of man, apply in nature? I

think we shall see that it can act most effectually. Let it be borne

in mind in what an endless number of strange peculiarities our domestic

productions, and, in a lesser degree, those under nature, vary; and how

strong the hereditary tendency is. Under domestication, it may be truly

said that the whole organisation becomes in some degree plastic. Let it

be borne in mind how infinitely complex and close-fitting are the mutual

relations of all organic beings to each other and to their physical

conditions of life. Can it, then, be thought improbable, seeing

that variations useful to man have undoubtedly occurred, that other

variations useful in some way to each being in the great and complex

battle of life, should sometimes occur in the course of thousands of

generations? If such do occur, can we doubt (remembering that many more

individuals are born than can possibly survive) that individuals having

any advantage, however slight, over others, would have the best chance

of surviving and of procreating their kind? On the other hand, we may

feel sure that any variation in the least degree injurious would be

rigidly destroyed. This preservation of favourable variations and the

rejection of injurious variations, I call Natural Selection. Variations

neither useful nor injurious would not be affected by natural selection,

and would be left a fluctuating element, as perhaps we see in the

species called polymorphic.

We shall best understand the probable course of natural selection

by taking the case of a country undergoing some physical change, for

instance, of climate. The proportional numbers of its inhabitants would

almost immediately undergo a change, and some species might become

extinct. We may conclude, from what we have seen of the intimate and

complex manner in which the inhabitants of each country are bound

together, that any change in the numerical proportions of some of the

inhabitants, independently of the change of climate itself, would most

seriously affect many of the others. If the country were open on its

borders, new forms would certainly immigrate, and this also would

seriously disturb the relations of some of the former inhabitants. Let

it be remembered how powerful the influence of a single introduced tree

or mammal has been shown to be. But in the case of an island, or of a

country partly surrounded by barriers, into which new and better adapted

forms could not freely enter, we should then have places in the economy

of nature which would assuredly be better filled up, if some of the

original inhabitants were in some manner modified; for, had the area

been open to immigration, these same places would have been seized on by

intruders. In such case, every slight modification, which in the course

of ages chanced to arise, and which in any way favoured the individuals

of any of the species, by better adapting them to their altered

conditions, would tend to be preserved; and natural selection would thus

have free scope for the work of improvement.

We have reason to believe, as stated in the first chapter, that a change

in the conditions of life, by specially acting on the reproductive

system, causes or increases variability; and in the foregoing case the

conditions of life are supposed to have undergone a change, and this

would manifestly be favourable to natural selection, by giving a

better chance of profitable variations occurring; and unless profitable

variations do occur, natural selection can do nothing. Not that, as

I believe, any extreme amount of variability is necessary; as man can

certainly produce great results by adding up in any given direction

mere individual differences, so could Nature, but far more easily, from

having incomparably longer time at her disposal. Nor do I believe that

any great physical change, as of climate, or any unusual degree of

isolation to check immigration, is actually necessary to produce new

and unoccupied places for natural selection to fill up by modifying and

improving some of the varying inhabitants. For as all the inhabitants

of each country are struggling together with nicely balanced forces,

extremely slight modifications in the structure or habits of one

inhabitant would often give it an advantage over others; and still

further modifications of the same kind would often still further

increase the advantage. No country can be named in which all the native

inhabitants are now so perfectly adapted to each other and to the

physical conditions under which they live, that none of them could

anyhow be improved; for in all countries, the natives have been so far

conquered by naturalised productions, that they have allowed foreigners

to take firm possession of the land. And as foreigners have thus

everywhere beaten some of the natives, we may safely conclude that the

natives might have been modified with advantage, so as to have better

resisted such intruders.

As man can produce and certainly has produced a great result by his

methodical and unconscious means of selection, what may not nature

effect? Man can act only on external and visible characters: nature

cares nothing for appearances, except in so far as they may be useful

to any being. She can act on every internal organ, on every shade of

constitutional difference, on the whole machinery of life. Man selects

only for his own good; Nature only for that of the being which she

tends. Every selected character is fully exercised by her; and the being

is placed under well-suited conditions of life. Man keeps the natives

of many climates in the same country; he seldom exercises each selected

character in some peculiar and fitting manner; he feeds a long and a

short beaked pigeon on the same food; he does not exercise a long-backed

or long-legged quadruped in any peculiar manner; he exposes sheep with

long and short wool to the same climate. He does not allow the most

vigorous males to struggle for the females. He does not rigidly destroy

all inferior animals, but protects during each varying season, as far as

lies in his power, all his productions. He often begins his selection

by some half-monstrous form; or at least by some modification prominent

enough to catch his eye, or to be plainly useful to him. Under nature,

the slightest difference of structure or constitution may well turn the

nicely-balanced scale in the struggle for life, and so be preserved.

How fleeting are the wishes and efforts of man! how short his time!

and consequently how poor will his products be, compared with those

accumulated by nature during whole geological periods. Can we wonder,

then, that nature's productions should be far "truer" in character than

man's productions; that they should be infinitely better adapted to the

most complex conditions of life, and should plainly bear the stamp of

far higher workmanship?

It may be said that natural selection is daily and hourly scrutinising,

throughout the world, every variation, even the slightest; rejecting

that which is bad, preserving and adding up all that is good; silently

and insensibly working, whenever and wherever opportunity offers, at

the improvement of each organic being in relation to its organic and

inorganic conditions of life. We see nothing of these slow changes in

progress, until the hand of time has marked the long lapse of ages, and

then so imperfect is our view into long past geological ages, that

we only see that the forms of life are now different from what they

formerly were.

Although natural selection can act only through and for the good of each

being, yet characters and structures, which we are apt to consider as of

very trifling importance, may thus be acted on. When we see leaf-eating

insects green, and bark-feeders mottled-grey; the alpine ptarmigan white

in winter, the red-grouse the colour of heather, and the black-grouse

that of peaty earth, we must believe that these tints are of service to

these birds and insects in preserving them from danger. Grouse, if not

destroyed at some period of their lives, would increase in countless

numbers; they are known to suffer largely from birds of prey; and hawks

are guided by eyesight to their prey,--so much so, that on parts of the

Continent persons are warned not to keep white pigeons, as being the

most liable to destruction. Hence I can see no reason to doubt that

natural selection might be most effective in giving the proper colour

to each kind of grouse, and in keeping that colour, when once acquired,

true and constant. Nor ought we to think that the occasional destruction

of an animal of any particular colour would produce little effect: we

should remember how essential it is in a flock of white sheep to destroy

every lamb with the faintest trace of black. In plants the down on

the fruit and the colour of the flesh are considered by botanists

as characters of the most trifling importance: yet we hear from

an excellent horticulturist, Downing, that in the United States

smooth-skinned fruits suffer far more from a beetle, a curculio, than

those with down; that purple plums suffer far more from a certain

disease than yellow plums; whereas another disease attacks

yellow-fleshed peaches far more than those with other coloured flesh.

If, with all the aids of art, these slight differences make a great

difference in cultivating the several varieties, assuredly, in a state

of nature, where the trees would have to struggle with other trees and

with a host of enemies, such differences would effectually settle which

variety, whether a smooth or downy, a yellow or purple fleshed fruit,

should succeed.

In looking at many small points of difference between species, which, as

far as our ignorance permits us to judge, seem to be quite unimportant,

we must not forget that climate, food, etc., probably produce some

slight and direct effect. It is, however, far more necessary to bear in

mind that there are many unknown laws of correlation of growth, which,

when one part of the organisation is modified through variation, and the

modifications are accumulated by natural selection for the good of the

being, will cause other modifications, often of the most unexpected

nature.

As we see that those variations which under domestication appear at any

particular period of life, tend to reappear in the offspring at the

same period;--for instance, in the seeds of the many varieties of our

culinary and agricultural plants; in the caterpillar and cocoon stages

of the varieties of the silkworm; in the eggs of poultry, and in the

colour of the down of their chickens; in the horns of our sheep and

cattle when nearly adult;--so in a state of nature, natural selection

will be enabled to act on and modify organic beings at any age, by

the accumulation of profitable variations at that age, and by their

inheritance at a corresponding age. If it profit a plant to have its

seeds more and more widely disseminated by the wind, I can see no

greater difficulty in this being effected through natural selection,

than in the cotton-planter increasing and improving by selection the

down in the pods on his cotton-trees. Natural selection may modify

and adapt the larva of an insect to a score of contingencies,

wholly different from those which concern the mature insect. These

modifications will no doubt affect, through the laws of correlation, the

structure of the adult; and probably in the case of those insects which

live only for a few hours, and which never feed, a large part of their

structure is merely the correlated result of successive changes in the

structure of their larvae. So, conversely, modifications in the adult

will probably often affect the structure of the larva; but in all cases

natural selection will ensure that modifications consequent on other

modifications at a different period of life, shall not be in the least

degree injurious: for if they became so, they would cause the extinction

of the species.

Natural selection will modify the structure of the young in relation

to the parent, and of the parent in relation to the young. In social

animals it will adapt the structure of each individual for the benefit

of the community; if each in consequence profits by the selected change.

What natural selection cannot do, is to modify the structure of one

species, without giving it any advantage, for the good of another

species; and though statements to this effect may be found in works of

natural history, I cannot find one case which will bear investigation.

A structure used only once in an animal's whole life, if of high

importance to it, might be modified to any extent by natural selection;

for instance, the great jaws possessed by certain insects, and used

exclusively for opening the cocoon--or the hard tip to the beak of

nestling birds, used for breaking the egg. It has been asserted, that

of the best short-beaked tumbler-pigeons more perish in the egg than are

able to get out of it; so that fanciers assist in the act of hatching.

Now, if nature had to make the beak of a full-grown pigeon very short

for the bird's own advantage, the process of modification would be very

slow, and there would be simultaneously the most rigorous selection of

the young birds within the egg, which had the most powerful and hardest

beaks, for all with weak beaks would inevitably perish: or, more

delicate and more easily broken shells might be selected, the thickness

of the shell being known to vary like every other structure.

SEXUAL SELECTION.

Inasmuch as peculiarities often appear under domestication in one sex

and become hereditarily attached to that sex, the same fact probably

occurs under nature, and if so, natural selection will be able to modify

one sex in its functional relations to the other sex, or in relation to

wholly different habits of life in the two sexes, as is sometimes the

case with insects. And this leads me to say a few words on what I call

Sexual Selection. This depends, not on a struggle for existence, but on

a struggle between the males for possession of the females; the result

is not death to the unsuccessful competitor, but few or no offspring.

Sexual selection is, therefore, less rigorous than natural selection.

Generally, the most vigorous males, those which are best fitted for

their places in nature, will leave most progeny. But in many cases,

victory will depend not on general vigour, but on having special

weapons, confined to the male sex. A hornless stag or spurless cock

would have a poor chance of leaving offspring. Sexual selection by

always allowing the victor to breed might surely give indomitable

courage, length to the spur, and strength to the wing to strike in the

spurred leg, as well as the brutal cock-fighter, who knows well that he

can improve his breed by careful selection of the best cocks. How low

in the scale of nature this law of battle descends, I know not; male

alligators have been described as fighting, bellowing, and whirling

round, like Indians in a war-dance, for the possession of the females;

male salmons have been seen fighting all day long; male stag-beetles

often bear wounds from the huge mandibles of other males. The war is,

perhaps, severest between the males of polygamous animals, and these

seem oftenest provided with special weapons. The males of carnivorous

animals are already well armed; though to them and to others, special

means of defence may be given through means of sexual selection, as the

mane to the lion, the shoulder-pad to the boar, and the hooked jaw to

the male salmon; for the shield may be as important for victory, as the

sword or spear.

Amongst birds, the contest is often of a more peaceful character.

All those who have attended to the subject, believe that there is the

severest rivalry between the males of many species to attract by singing

the females. The rock-thrush of Guiana, birds of Paradise, and some

others, congregate; and successive males display their gorgeous plumage

and perform strange antics before the females, which standing by as

spectators, at last choose the most attractive partner. Those who have

closely attended to birds in confinement well know that they often take

individual preferences and dislikes: thus Sir R. Heron has described how

one pied peacock was eminently attractive to all his hen birds. It may

appear childish to attribute any effect to such apparently weak means: I

cannot here enter on the details necessary to support this view; but if

man can in a short time give elegant carriage and beauty to his bantams,

according to his standard of beauty, I can see no good reason to doubt

that female birds, by selecting, during thousands of generations,

the most melodious or beautiful males, according to their standard of

beauty, might produce a marked effect. I strongly suspect that some

well-known laws with respect to the plumage of male and female birds, in

comparison with the plumage of the young, can be explained on the view

of plumage having been chiefly modified by sexual selection, acting when

the birds have come to the breeding age or during the breeding season;

the modifications thus produced being inherited at corresponding ages or

seasons, either by the males alone, or by the males and females; but I

have not space here to enter on this subject.

Thus it is, as I believe, that when the males and females of any animal

have the same general habits of life, but differ in structure, colour,

or ornament, such differences have been mainly caused by sexual

selection; that is, individual males have had, in successive

generations, some slight advantage over other males, in their weapons,

means of defence, or charms; and have transmitted these advantages to

their male offspring. Yet, I would not wish to attribute all such

sexual differences to this agency: for we see peculiarities arising and

becoming attached to the male sex in our domestic animals (as the wattle

in male carriers, horn-like protuberances in the cocks of certain fowls,

etc.), which we cannot believe to be either useful to the males in

battle, or attractive to the females. We see analogous cases under

nature, for instance, the tuft of hair on the breast of the turkey-cock,

which can hardly be either useful or ornamental to this bird;--indeed,

had the tuft appeared under domestication, it would have been called a

monstrosity.

ILLUSTRATIONS OF THE ACTION OF NATURAL SELECTION.

In order to make it clear how, as I believe, natural selection acts, I

must beg permission to give one or two imaginary illustrations. Let us

take the case of a wolf, which preys on various animals, securing some

by craft, some by strength, and some by fleetness; and let us suppose

that the fleetest prey, a deer for instance, had from any change in

the country increased in numbers, or that other prey had decreased in

numbers, during that season of the year when the wolf is hardest pressed

for food. I can under such circumstances see no reason to doubt that the

swiftest and slimmest wolves would have the best chance of surviving,

and so be preserved or selected,--provided always that they retained

strength to master their prey at this or at some other period of the

year, when they might be compelled to prey on other animals. I can see

no more reason to doubt this, than that man can improve the fleetness

of his greyhounds by careful and methodical selection, or by that

unconscious selection which results from each man trying to keep the

best dogs without any thought of modifying the breed.

Even without any change in the proportional numbers of the animals on

which our wolf preyed, a cub might be born with an innate tendency to

pursue certain kinds of prey. Nor can this be thought very improbable;

for we often observe great differences in the natural tendencies of our

domestic animals; one cat, for instance, taking to catch rats, another

mice; one cat, according to Mr. St. John, bringing home winged game,

another hares or rabbits, and another hunting on marshy ground and

almost nightly catching woodcocks or snipes. The tendency to catch rats

rather than mice is known to be inherited. Now, if any slight innate

change of habit or of structure benefited an individual wolf, it would

have the best chance of surviving and of leaving offspring. Some of its

young would probably inherit the same habits or structure, and by the

repetition of this process, a new variety might be formed which would

either supplant or coexist with the parent-form of wolf. Or, again,

the wolves inhabiting a mountainous district, and those frequenting the

lowlands, would naturally be forced to hunt different prey; and from the

continued preservation of the individuals best fitted for the two sites,

two varieties might slowly be formed. These varieties would cross and

blend where they met; but to this subject of intercrossing we shall soon

have to return. I may add, that, according to Mr. Pierce, there are two

varieties of the wolf inhabiting the Catskill Mountains in the United

States, one with a light greyhound-like form, which pursues deer, and

the other more bulky, with shorter legs, which more frequently attacks

the shepherd's flocks.

Let us now take a more complex case. Certain plants excrete a sweet

juice, apparently for the sake of eliminating something injurious from

their sap: this is effected by glands at the base of the stipules in

some Leguminosae, and at the back of the leaf of the common laurel. This

juice, though small in quantity, is greedily sought by insects. Let us

now suppose a little sweet juice or nectar to be excreted by the inner

bases of the petals of a flower. In this case insects in seeking the

nectar would get dusted with pollen, and would certainly often transport

the pollen from one flower to the stigma of another flower. The flowers

of two distinct individuals of the same species would thus get crossed;

and the act of crossing, we have good reason to believe (as will

hereafter be more fully alluded to), would produce very vigorous

seedlings, which consequently would have the best chance of flourishing

and surviving. Some of these seedlings would probably inherit the

nectar-excreting power. Those individual flowers which had the largest

glands or nectaries, and which excreted most nectar, would be oftenest

visited by insects, and would be oftenest crossed; and so in the

long-run would gain the upper hand. Those flowers, also, which had their

stamens and pistils placed, in relation to the size and habits of the

particular insects which visited them, so as to favour in any degree

the transportal of their pollen from flower to flower, would likewise be

favoured or selected. We might have taken the case of insects visiting

flowers for the sake of collecting pollen instead of nectar; and as

pollen is formed for the sole object of fertilisation, its destruction

appears a simple loss to the plant; yet if a little pollen were carried,

at first occasionally and then habitually, by the pollen-devouring

insects from flower to flower, and a cross thus effected, although

nine-tenths of the pollen were destroyed, it might still be a great gain

to the plant; and those individuals which produced more and more pollen,

and had larger and larger anthers, would be selected.

When our plant, by this process of the continued preservation or natural

selection of more and more attractive flowers, had been rendered highly

attractive to insects, they would, unintentionally on their part,

regularly carry pollen from flower to flower; and that they can most

effectually do this, I could easily show by many striking instances.

I will give only one--not as a very striking case, but as likewise

illustrating one step in the separation of the sexes of plants,

presently to be alluded to. Some holly-trees bear only male flowers,

which have four stamens producing rather a small quantity of pollen, and

a rudimentary pistil; other holly-trees bear only female flowers; these

have a full-sized pistil, and four stamens with shrivelled anthers, in

which not a grain of pollen can be detected. Having found a female

tree exactly sixty yards from a male tree, I put the stigmas of twenty

flowers, taken from different branches, under the microscope, and

on all, without exception, there were pollen-grains, and on some a

profusion of pollen. As the wind had set for several days from the

female to the male tree, the pollen could not thus have been carried.

The weather had been cold and boisterous, and therefore not favourable

to bees, nevertheless every female flower which I examined had been

effectually fertilised by the bees, accidentally dusted with pollen,

having flown from tree to tree in search of nectar. But to return to

our imaginary case: as soon as the plant had been rendered so highly

attractive to insects that pollen was regularly carried from flower

to flower, another process might commence. No naturalist doubts the

advantage of what has been called the "physiological division of

labour;" hence we may believe that it would be advantageous to a plant

to produce stamens alone in one flower or on one whole plant, and

pistils alone in another flower or on another plant. In plants under

culture and placed under new conditions of life, sometimes the male

organs and sometimes the female organs become more or less impotent;

now if we suppose this to occur in ever so slight a degree under nature,

then as pollen is already carried regularly from flower to flower,

and as a more complete separation of the sexes of our plant would be

advantageous on the principle of the division of labour, individuals

with this tendency more and more increased, would be continually

favoured or selected, until at last a complete separation of the sexes

would be effected.

Let us now turn to the nectar-feeding insects in our imaginary case: we

may suppose the plant of which we have been slowly increasing the nectar

by continued selection, to be a common plant; and that certain insects

depended in main part on its nectar for food. I could give many facts,

showing how anxious bees are to save time; for instance, their habit of

cutting holes and sucking the nectar at the bases of certain flowers,

which they can, with a very little more trouble, enter by the mouth.

Bearing such facts in mind, I can see no reason to doubt that an

accidental deviation in the size and form of the body, or in the

curvature and length of the proboscis, etc., far too slight to be

appreciated by us, might profit a bee or other insect, so that an

individual so characterised would be able to obtain its food more

quickly, and so have a better chance of living and leaving descendants.

Its descendants would probably inherit a tendency to a similar slight

deviation of structure. The tubes of the corollas of the common red and

incarnate clovers (Trifolium pratense and incarnatum) do not on a hasty

glance appear to differ in length; yet the hive-bee can easily suck

the nectar out of the incarnate clover, but not out of the common red

clover, which is visited by humble-bees alone; so that whole fields of

the red clover offer in vain an abundant supply of precious nectar to

the hive-bee. Thus it might be a great advantage to the hive-bee to have

a slightly longer or differently constructed proboscis. On the other

hand, I have found by experiment that the fertility of clover greatly

depends on bees visiting and moving parts of the corolla, so as to push

the pollen on to the stigmatic surface. Hence, again, if humble-bees

were to become rare in any country, it might be a great advantage to the

red clover to have a shorter or more deeply divided tube to its corolla,

so that the hive-bee could visit its flowers. Thus I can understand how

a flower and a bee might slowly become, either simultaneously or one

after the other, modified and adapted in the most perfect manner to each

other, by the continued preservation of individuals presenting mutual

and slightly favourable deviations of structure.

I am well aware that this doctrine of natural selection, exemplified in

the above imaginary instances, is open to the same objections which were

at first urged against Sir Charles Lyell's noble views on "the modern

changes of the earth, as illustrative of geology;" but we now very

seldom hear the action, for instance, of the coast-waves, called a

trifling and insignificant cause, when applied to the excavation of

gigantic valleys or to the formation of the longest lines of inland

cliffs. Natural selection can act only by the preservation and

accumulation of infinitesimally small inherited modifications, each

profitable to the preserved being; and as modern geology has almost

banished such views as the excavation of a great valley by a single

diluvial wave, so will natural selection, if it be a true principle,

banish the belief of the continued creation of new organic beings, or of

any great and sudden modification in their structure.

ON THE INTERCROSSING OF INDIVIDUALS.

I must here introduce a short digression. In the case of animals

and plants with separated sexes, it is of course obvious that two

individuals must always unite for each birth; but in the case of

hermaphrodites this is far from obvious. Nevertheless I am strongly

inclined to believe that with all hermaphrodites two individuals, either

occasionally or habitually, concur for the reproduction of their kind.

This view, I may add, was first suggested by Andrew Knight. We shall

presently see its importance; but I must here treat the subject with

extreme brevity, though I have the materials prepared for an ample

discussion. All vertebrate animals, all insects, and some other large

groups of animals, pair for each birth. Modern research has much

diminished the number of supposed hermaphrodites, and of real

hermaphrodites a large number pair; that is, two individuals regularly

unite for reproduction, which is all that concerns us. But still there

are many hermaphrodite animals which certainly do not habitually pair,

and a vast majority of plants are hermaphrodites. What reason, it may be

asked, is there for supposing in these cases that two individuals ever

concur in reproduction? As it is impossible here to enter on details, I

must trust to some general considerations alone.

In the first place, I have collected so large a body of facts, showing,

in accordance with the almost universal belief of breeders, that with

animals and plants a cross between different varieties, or between

individuals of the same variety but of another strain, gives vigour

and fertility to the offspring; and on the other hand, that CLOSE

interbreeding diminishes vigour and fertility; that these facts alone

incline me to believe that it is a general law of nature (utterly

ignorant though we be of the meaning of the law) that no organic being

self-fertilises itself for an eternity of generations; but that a

cross with another individual is occasionally--perhaps at very long

intervals--indispensable.

On the belief that this is a law of nature, we can, I think, understand

several large classes of facts, such as the following, which on any

other view are inexplicable. Every hybridizer knows how unfavourable

exposure to wet is to the fertilisation of a flower, yet what a

multitude of flowers have their anthers and stigmas fully exposed to

the weather! but if an occasional cross be indispensable, the fullest

freedom for the entrance of pollen from another individual will explain

this state of exposure, more especially as the plant's own anthers and

pistil generally stand so close together that self-fertilisation seems

almost inevitable. Many flowers, on the other hand, have their organs

of fructification closely enclosed, as in the great papilionaceous or

pea-family; but in several, perhaps in all, such flowers, there is a

very curious adaptation between the structure of the flower and the

manner in which bees suck the nectar; for, in doing this, they either

push the flower's own pollen on the stigma, or bring pollen from another

flower. So necessary are the visits of bees to papilionaceous flowers,

that I have found, by experiments published elsewhere, that their

fertility is greatly diminished if these visits be prevented. Now, it

is scarcely possible that bees should fly from flower to flower, and not

carry pollen from one to the other, to the great good, as I believe,

of the plant. Bees will act like a camel-hair pencil, and it is quite

sufficient just to touch the anthers of one flower and then the stigma

of another with the same brush to ensure fertilisation; but it must not

be supposed that bees would thus produce a multitude of hybrids between

distinct species; for if you bring on the same brush a plant's own

pollen and pollen from another species, the former will have such a

prepotent effect, that it will invariably and completely destroy, as has

been shown by Gartner, any influence from the foreign pollen.

When the stamens of a flower suddenly spring towards the pistil, or

slowly move one after the other towards it, the contrivance seems

adapted solely to ensure self-fertilisation; and no doubt it is useful

for this end: but, the agency of insects is often required to cause the

stamens to spring forward, as Kolreuter has shown to be the case with

the barberry; and curiously in this very genus, which seems to have a

special contrivance for self-fertilisation, it is well known that if

very closely-allied forms or varieties are planted near each other, it

is hardly possible to raise pure seedlings, so largely do they

naturally cross. In many other cases, far from there being any aids for

self-fertilisation, there are special contrivances, as I could show

from the writings of C. C. Sprengel and from my own observations, which

effectually prevent the stigma receiving pollen from its own flower: for

instance, in Lobelia fulgens, there is a really beautiful and

elaborate contrivance by which every one of the infinitely numerous

pollen-granules are swept out of the conjoined anthers of each flower,

before the stigma of that individual flower is ready to receive them;

and as this flower is never visited, at least in my garden, by insects,

it never sets a seed, though by placing pollen from one flower on the

stigma of another, I raised plenty of seedlings; and whilst another

species of Lobelia growing close by, which is visited by bees, seeds

freely. In very many other cases, though there be no special mechanical

contrivance to prevent the stigma of a flower receiving its own pollen,

yet, as C. C. Sprengel has shown, and as I can confirm, either the

anthers burst before the stigma is ready for fertilisation, or the

stigma is ready before the pollen of that flower is ready, so that these

plants have in fact separated sexes, and must habitually be crossed.

How strange are these facts! How strange that the pollen and stigmatic

surface of the same flower, though placed so close together, as if

for the very purpose of self-fertilisation, should in so many cases be

mutually useless to each other! How simply are these facts explained

on the view of an occasional cross with a distinct individual being

advantageous or indispensable!

If several varieties of the cabbage, radish, onion, and of some other

plants, be allowed to seed near each other, a large majority, as I

have found, of the seedlings thus raised will turn out mongrels: for

instance, I raised 233 seedling cabbages from some plants of different

varieties growing near each other, and of these only 78 were true to

their kind, and some even of these were not perfectly true. Yet the

pistil of each cabbage-flower is surrounded not only by its own six

stamens, but by those of the many other flowers on the same plant. How,

then, comes it that such a vast number of the seedlings are mongrelized?

I suspect that it must arise from the pollen of a distinct VARIETY

having a prepotent effect over a flower's own pollen; and that this is

part of the general law of good being derived from the intercrossing

of distinct individuals of the same species. When distinct SPECIES are

crossed the case is directly the reverse, for a plant's own pollen

is always prepotent over foreign pollen; but to this subject we shall

return in a future chapter.

In the case of a gigantic tree covered with innumerable flowers, it may

be objected that pollen could seldom be carried from tree to tree, and

at most only from flower to flower on the same tree, and that flowers

on the same tree can be considered as distinct individuals only in a

limited sense. I believe this objection to be valid, but that nature has

largely provided against it by giving to trees a strong tendency to bear

flowers with separated sexes. When the sexes are separated, although

the male and female flowers may be produced on the same tree, we can see

that pollen must be regularly carried from flower to flower; and this

will give a better chance of pollen being occasionally carried from tree

to tree. That trees belonging to all Orders have their sexes more often

separated than other plants, I find to be the case in this country; and

at my request Dr. Hooker tabulated the trees of New Zealand, and Dr. Asa

Gray those of the United States, and the result was as I anticipated. On

the other hand, Dr. Hooker has recently informed me that he finds that

the rule does not hold in Australia; and I have made these few remarks

on the sexes of trees simply to call attention to the subject.

Turning for a very brief space to animals: on the land there are some

hermaphrodites, as land-mollusca and earth-worms; but these all pair.

As yet I have not found a single case of a terrestrial animal which

fertilises itself. We can understand this remarkable fact, which

offers so strong a contrast with terrestrial plants, on the view of an

occasional cross being indispensable, by considering the medium in which

terrestrial animals live, and the nature of the fertilising element; for

we know of no means, analogous to the action of insects and of the wind

in the case of plants, by which an occasional cross could be effected

with terrestrial animals without the concurrence of two individuals.

Of aquatic animals, there are many self-fertilising hermaphrodites;

but here currents in the water offer an obvious means for an occasional

cross. And, as in the case of flowers, I have as yet failed, after

consultation with one of the highest authorities, namely, Professor

Huxley, to discover a single case of an hermaphrodite animal with the

organs of reproduction so perfectly enclosed within the body, that

access from without and the occasional influence of a distinct

individual can be shown to be physically impossible. Cirripedes long

appeared to me to present a case of very great difficulty under this

point of view; but I have been enabled, by a fortunate chance, elsewhere

to prove that two individuals, though both are self-fertilising

hermaphrodites, do sometimes cross.

It must have struck most naturalists as a strange anomaly that, in the

case of both animals and plants, species of the same family and even of

the same genus, though agreeing closely with each other in almost their

whole organisation, yet are not rarely, some of them hermaphrodites,

and some of them unisexual. But if, in fact, all hermaphrodites do

occasionally intercross with other individuals, the difference between

hermaphrodites and unisexual species, as far as function is concerned,

becomes very small.

From these several considerations and from the many special facts which

I have collected, but which I am not here able to give, I am strongly

inclined to suspect that, both in the vegetable and animal kingdoms, an

occasional intercross with a distinct individual is a law of nature. I

am well aware that there are, on this view, many cases of difficulty,

some of which I am trying to investigate. Finally then, we may conclude

that in many organic beings, a cross between two individuals is an

obvious necessity for each birth; in many others it occurs perhaps only

at long intervals; but in none, as I suspect, can self-fertilisation go

on for perpetuity.

CIRCUMSTANCES FAVOURABLE TO NATURAL SELECTION.

This is an extremely intricate subject. A large amount of inheritable

and diversified variability is favourable, but I believe mere individual

differences suffice for the work. A large number of individuals, by

giving a better chance for the appearance within any given period

of profitable variations, will compensate for a lesser amount of

variability in each individual, and is, I believe, an extremely

important element of success. Though nature grants vast periods of time

for the work of natural selection, she does not grant an indefinite

period; for as all organic beings are striving, it may be said, to seize

on each place in the economy of nature, if any one species does

not become modified and improved in a corresponding degree with its

competitors, it will soon be exterminated.

In man's methodical selection, a breeder selects for some definite

object, and free intercrossing will wholly stop his work. But when many

men, without intending to alter the breed, have a nearly common standard

of perfection, and all try to get and breed from the best animals,

much improvement and modification surely but slowly follow from this

unconscious process of selection, notwithstanding a large amount of

crossing with inferior animals. Thus it will be in nature; for within a

confined area, with some place in its polity not so perfectly occupied

as might be, natural selection will always tend to preserve all the

individuals varying in the right direction, though in different degrees,

so as better to fill up the unoccupied place. But if the area be large,

its several districts will almost certainly present different conditions

of life; and then if natural selection be modifying and improving a

species in the several districts, there will be intercrossing with the

other individuals of the same species on the confines of each. And in

this case the effects of intercrossing can hardly be counterbalanced by

natural selection always tending to modify all the individuals in each

district in exactly the same manner to the conditions of each; for in a

continuous area, the conditions will generally graduate away insensibly

from one district to another. The intercrossing will most affect those

animals which unite for each birth, which wander much, and which do

not breed at a very quick rate. Hence in animals of this nature, for

instance in birds, varieties will generally be confined to separated

countries; and this I believe to be the case. In hermaphrodite organisms

which cross only occasionally, and likewise in animals which unite for

each birth, but which wander little and which can increase at a very

rapid rate, a new and improved variety might be quickly formed on any

one spot, and might there maintain itself in a body, so that whatever

intercrossing took place would be chiefly between the individuals of

the same new variety. A local variety when once thus formed might

subsequently slowly spread to other districts. On the above principle,

nurserymen always prefer getting seed from a large body of plants of

the same variety, as the chance of intercrossing with other varieties is

thus lessened.

Even in the case of slow-breeding animals, which unite for each birth,

we must not overrate the effects of intercrosses in retarding natural

selection; for I can bring a considerable catalogue of facts, showing

that within the same area, varieties of the same animal can long remain

distinct, from haunting different stations, from breeding at slightly

different seasons, or from varieties of the same kind preferring to pair

together.

Intercrossing plays a very important part in nature in keeping the

individuals of the same species, or of the same variety, true and

uniform in character. It will obviously thus act far more efficiently

with those animals which unite for each birth; but I have already

attempted to show that we have reason to believe that occasional

intercrosses take place with all animals and with all plants. Even if

these take place only at long intervals, I am convinced that the

young thus produced will gain so much in vigour and fertility over the

offspring from long-continued self-fertilisation, that they will have a

better chance of surviving and propagating their kind; and thus, in the

long run, the influence of intercrosses, even at rare intervals, will be

great. If there exist organic beings which never intercross, uniformity

of character can be retained amongst them, as long as their conditions

of life remain the same, only through the principle of inheritance, and

through natural selection destroying any which depart from the

proper type; but if their conditions of life change and they undergo

modification, uniformity of character can be given to their modified

offspring, solely by natural selection preserving the same favourable

variations.

Isolation, also, is an important element in the process of natural

selection. In a confined or isolated area, if not very large, the

organic and inorganic conditions of life will generally be in a great

degree uniform; so that natural selection will tend to modify all the

individuals of a varying species throughout the area in the same

manner in relation to the same conditions. Intercrosses, also, with the

individuals of the same species, which otherwise would have inhabited

the surrounding and differently circumstanced districts, will be

prevented. But isolation probably acts more efficiently in checking the

immigration of better adapted organisms, after any physical change, such

as of climate or elevation of the land, etc.; and thus new places in the

natural economy of the country are left open for the old inhabitants

to struggle for, and become adapted to, through modifications in their

structure and constitution. Lastly, isolation, by checking immigration

and consequently competition, will give time for any new variety to

be slowly improved; and this may sometimes be of importance in the

production of new species. If, however, an isolated area be very small,

either from being surrounded by barriers, or from having very peculiar

physical conditions, the total number of the individuals supported on it

will necessarily be very small; and fewness of individuals will greatly

retard the production of new species through natural selection, by

decreasing the chance of the appearance of favourable variations.

If we turn to nature to test the truth of these remarks, and look at

any small isolated area, such as an oceanic island, although the total

number of the species inhabiting it, will be found to be small, as we

shall see in our chapter on geographical distribution; yet of these

species a very large proportion are endemic,--that is, have been

produced there, and nowhere else. Hence an oceanic island at first sight

seems to have been highly favourable for the production of new species.

But we may thus greatly deceive ourselves, for to ascertain whether a

small isolated area, or a large open area like a continent, has been

most favourable for the production of new organic forms, we ought to

make the comparison within equal times; and this we are incapable of

doing.

Although I do not doubt that isolation is of considerable importance

in the production of new species, on the whole I am inclined to believe

that largeness of area is of more importance, more especially in the

production of species, which will prove capable of enduring for a long

period, and of spreading widely. Throughout a great and open area, not

only will there be a better chance of favourable variations arising from

the large number of individuals of the same species there supported, but

the conditions of life are infinitely complex from the large number

of already existing species; and if some of these many species

become modified and improved, others will have to be improved in a

corresponding degree or they will be exterminated. Each new form, also,

as soon as it has been much improved, will be able to spread over the

open and continuous area, and will thus come into competition with many

others. Hence more new places will be formed, and the competition to

fill them will be more severe, on a large than on a small and

isolated area. Moreover, great areas, though now continuous, owing to

oscillations of level, will often have recently existed in a broken

condition, so that the good effects of isolation will generally, to a

certain extent, have concurred. Finally, I conclude that, although small

isolated areas probably have been in some respects highly favourable for

the production of new species, yet that the course of modification

will generally have been more rapid on large areas; and what is more

important, that the new forms produced on large areas, which already

have been victorious over many competitors, will be those that will

spread most widely, will give rise to most new varieties and species,

and will thus play an important part in the changing history of the

organic world.

We can, perhaps, on these views, understand some facts which will

be again alluded to in our chapter on geographical distribution; for

instance, that the productions of the smaller continent of Australia

have formerly yielded, and apparently are now yielding, before those

of the larger Europaeo-Asiatic area. Thus, also, it is that continental

productions have everywhere become so largely naturalised on islands. On

a small island, the race for life will have been less severe, and there

will have been less modification and less extermination. Hence, perhaps,

it comes that the flora of Madeira, according to Oswald Heer, resembles

the extinct tertiary flora of Europe. All fresh-water basins, taken

together, make a small area compared with that of the sea or of the

land; and, consequently, the competition between fresh-water productions

will have been less severe than elsewhere; new forms will have been

more slowly formed, and old forms more slowly exterminated. And it is

in fresh water that we find seven genera of Ganoid fishes, remnants of

a once preponderant order: and in fresh water we find some of the most

anomalous forms now known in the world, as the Ornithorhynchus and

Lepidosiren, which, like fossils, connect to a certain extent orders now

widely separated in the natural scale. These anomalous forms may almost

be called living fossils; they have endured to the present day, from

having inhabited a confined area, and from having thus been exposed to

less severe competition.

To sum up the circumstances favourable and unfavourable to natural

selection, as far as the extreme intricacy of the subject permits. I

conclude, looking to the future, that for terrestrial productions a

large continental area, which will probably undergo many oscillations

of level, and which consequently will exist for long periods in a broken

condition, will be the most favourable for the production of many new

forms of life, likely to endure long and to spread widely. For the area

will first have existed as a continent, and the inhabitants, at this

period numerous in individuals and kinds, will have been subjected

to very severe competition. When converted by subsidence into large

separate islands, there will still exist many individuals of the same

species on each island: intercrossing on the confines of the range of

each species will thus be checked: after physical changes of any kind,

immigration will be prevented, so that new places in the polity of

each island will have to be filled up by modifications of the old

inhabitants; and time will be allowed for the varieties in each to

become well modified and perfected. When, by renewed elevation, the

islands shall be re-converted into a continental area, there will again

be severe competition: the most favoured or improved varieties will be

enabled to spread: there will be much extinction of the less improved

forms, and the relative proportional numbers of the various inhabitants

of the renewed continent will again be changed; and again there will

be a fair field for natural selection to improve still further the

inhabitants, and thus produce new species.

That natural selection will always act with extreme slowness, I fully

admit. Its action depends on there being places in the polity of nature,

which can be better occupied by some of the inhabitants of the country

undergoing modification of some kind. The existence of such places will

often depend on physical changes, which are generally very slow, and

on the immigration of better adapted forms having been checked. But the

action of natural selection will probably still oftener depend on some

of the inhabitants becoming slowly modified; the mutual relations of

many of the other inhabitants being thus disturbed. Nothing can be

effected, unless favourable variations occur, and variation itself is

apparently always a very slow process. The process will often be greatly

retarded by free intercrossing. Many will exclaim that these several

causes are amply sufficient wholly to stop the action of natural

selection. I do not believe so. On the other hand, I do believe that

natural selection will always act very slowly, often only at long

intervals of time, and generally on only a very few of the inhabitants

of the same region at the same time. I further believe, that this very

slow, intermittent action of natural selection accords perfectly

well with what geology tells us of the rate and manner at which the

inhabitants of this world have changed.

Slow though the process of selection may be, if feeble man can do much

by his powers of artificial selection, I can see no limit to the amount

of change, to the beauty and infinite complexity of the coadaptations

between all organic beings, one with another and with their physical

conditions of life, which may be effected in the long course of time by

nature's power of selection.

EXTINCTION.

This subject will be more fully discussed in our chapter on Geology; but

it must be here alluded to from being intimately connected with natural

selection. Natural selection acts solely through the preservation of

variations in some way advantageous, which consequently endure. But as

from the high geometrical powers of increase of all organic beings, each

area is already fully stocked with inhabitants, it follows that as

each selected and favoured form increases in number, so will the less

favoured forms decrease and become rare. Rarity, as geology tells us, is

the precursor to extinction. We can, also, see that any form represented

by few individuals will, during fluctuations in the seasons or in the

number of its enemies, run a good chance of utter extinction. But we may

go further than this; for as new forms are continually and slowly being

produced, unless we believe that the number of specific forms goes on

perpetually and almost indefinitely increasing, numbers inevitably must

become extinct. That the number of specific forms has not indefinitely

increased, geology shows us plainly; and indeed we can see reason why

they should not have thus increased, for the number of places in the

polity of nature is not indefinitely great,--not that we have any means

of knowing that any one region has as yet got its maximum of species.

Probably no region is as yet fully stocked, for at the Cape of Good

Hope, where more species of plants are crowded together than in any

other quarter of the world, some foreign plants have become naturalised,

without causing, as far as we know, the extinction of any natives.

Furthermore, the species which are most numerous in individuals will

have the best chance of producing within any given period favourable

variations. We have evidence of this, in the facts given in the second

chapter, showing that it is the common species which afford the greatest

number of recorded varieties, or incipient species. Hence, rare species

will be less quickly modified or improved within any given period, and

they will consequently be beaten in the race for life by the modified

descendants of the commoner species.

From these several considerations I think it inevitably follows, that as

new species in the course of time are formed through natural selection,

others will become rarer and rarer, and finally extinct. The forms which

stand in closest competition with those undergoing modification and

improvement, will naturally suffer most. And we have seen in the

chapter on the Struggle for Existence that it is the most closely-allied

forms,--varieties of the same species, and species of the same genus

or of related genera,--which, from having nearly the same structure,

constitution, and habits, generally come into the severest competition

with each other. Consequently, each new variety or species, during the

progress of its formation, will generally press hardest on its nearest

kindred, and tend to exterminate them. We see the same process of

extermination amongst our domesticated productions, through the

selection of improved forms by man. Many curious instances could

be given showing how quickly new breeds of cattle, sheep, and other

animals, and varieties of flowers, take the place of older and inferior

kinds. In Yorkshire, it is historically known that the ancient black

cattle were displaced by the long-horns, and that these "were swept away

by the short-horns" (I quote the words of an agricultural writer) "as if

by some murderous pestilence."

DIVERGENCE OF CHARACTER.

The principle, which I have designated by this term, is of high

importance on my theory, and explains, as I believe, several important

facts. In the first place, varieties, even strongly-marked ones, though

having somewhat of the character of species--as is shown by the hopeless

doubts in many cases how to rank them--yet certainly differ from

each other far less than do good and distinct species. Nevertheless,

according to my view, varieties are species in the process of formation,

or are, as I have called them, incipient species. How, then, does the

lesser difference between varieties become augmented into the greater

difference between species? That this does habitually happen, we must

infer from most of the innumerable species throughout nature presenting

well-marked differences; whereas varieties, the supposed prototypes and

parents of future well-marked species, present slight and ill-defined

differences. Mere chance, as we may call it, might cause one variety

to differ in some character from its parents, and the offspring of this

variety again to differ from its parent in the very same character and

in a greater degree; but this alone would never account for so habitual

and large an amount of difference as that between varieties of the same

species and species of the same genus.

As has always been my practice, let us seek light on this head from our

domestic productions. We shall here find something analogous. A fancier

is struck by a pigeon having a slightly shorter beak; another fancier is

struck by a pigeon having a rather longer beak; and on the acknowledged

principle that "fanciers do not and will not admire a medium standard,

but like extremes," they both go on (as has actually occurred with

tumbler-pigeons) choosing and breeding from birds with longer and longer

beaks, or with shorter and shorter beaks. Again, we may suppose that at

an early period one man preferred swifter horses; another stronger and

more bulky horses. The early differences would be very slight; in the

course of time, from the continued selection of swifter horses by some

breeders, and of stronger ones by others, the differences would become

greater, and would be noted as forming two sub-breeds; finally, after

the lapse of centuries, the sub-breeds would become converted into two

well-established and distinct breeds. As the differences slowly become

greater, the inferior animals with intermediate characters, being

neither very swift nor very strong, will have been neglected, and will

have tended to disappear. Here, then, we see in man's productions

the action of what may be called the principle of divergence, causing

differences, at first barely appreciable, steadily to increase, and

the breeds to diverge in character both from each other and from their

common parent.

But how, it may be asked, can any analogous principle apply in nature?

I believe it can and does apply most efficiently, from the simple

circumstance that the more diversified the descendants from any one

species become in structure, constitution, and habits, by so much will

they be better enabled to seize on many and widely diversified places in

the polity of nature, and so be enabled to increase in numbers.

We can clearly see this in the case of animals with simple habits. Take

the case of a carnivorous quadruped, of which the number that can be

supported in any country has long ago arrived at its full average. If

its natural powers of increase be allowed to act, it can succeed in

increasing (the country not undergoing any change in its conditions)

only by its varying descendants seizing on places at present occupied by

other animals: some of them, for instance, being enabled to feed on

new kinds of prey, either dead or alive; some inhabiting new stations,

climbing trees, frequenting water, and some perhaps becoming less

carnivorous. The more diversified in habits and structure the

descendants of our carnivorous animal became, the more places they would

be enabled to occupy. What applies to one animal will apply throughout

all time to all animals--that is, if they vary--for otherwise natural

selection can do nothing. So it will be with plants. It has been

experimentally proved, that if a plot of ground be sown with one species

of grass, and a similar plot be sown with several distinct genera of

grasses, a greater number of plants and a greater weight of dry herbage

can thus be raised. The same has been found to hold good when first

one variety and then several mixed varieties of wheat have been sown on

equal spaces of ground. Hence, if any one species of grass were to go

on varying, and those varieties were continually selected which differed

from each other in at all the same manner as distinct species and genera

of grasses differ from each other, a greater number of individual plants

of this species of grass, including its modified descendants, would

succeed in living on the same piece of ground. And we well know that

each species and each variety of grass is annually sowing almost

countless seeds; and thus, as it may be said, is striving its utmost to

increase its numbers. Consequently, I cannot doubt that in the course

of many thousands of generations, the most distinct varieties of any one

species of grass would always have the best chance of succeeding and

of increasing in numbers, and thus of supplanting the less distinct

varieties; and varieties, when rendered very distinct from each other,

take the rank of species.

The truth of the principle, that the greatest amount of life can be

supported by great diversification of structure, is seen under many

natural circumstances. In an extremely small area, especially if freely

open to immigration, and where the contest between individual and

individual must be severe, we always find great diversity in its

inhabitants. For instance, I found that a piece of turf, three feet by

four in size, which had been exposed for many years to exactly the same

conditions, supported twenty species of plants, and these belonged to

eighteen genera and to eight orders, which shows how much these plants

differed from each other. So it is with the plants and insects on small

and uniform islets; and so in small ponds of fresh water. Farmers find

that they can raise most food by a rotation of plants belonging to the

most different orders: nature follows what may be called a simultaneous

rotation. Most of the animals and plants which live close round any

small piece of ground, could live on it (supposing it not to be in

any way peculiar in its nature), and may be said to be striving to the

utmost to live there; but, it is seen, that where they come into the

closest competition with each other, the advantages of diversification

of structure, with the accompanying differences of habit and

constitution, determine that the inhabitants, which thus jostle each

other most closely, shall, as a general rule, belong to what we call

different genera and orders.

The same principle is seen in the naturalisation of plants through man's

agency in foreign lands. It might have been expected that the plants

which have succeeded in becoming naturalised in any land would generally

have been closely allied to the indigenes; for these are commonly looked

at as specially created and adapted for their own country. It might,

also, perhaps have been expected that naturalised plants would have

belonged to a few groups more especially adapted to certain stations in

their new homes. But the case is very different; and Alph. De Candolle

has well remarked in his great and admirable work, that floras gain by

naturalisation, proportionally with the number of the native genera and

species, far more in new genera than in new species. To give a single

instance: in the last edition of Dr. Asa Gray's 'Manual of the Flora of

the Northern United States,' 260 naturalised plants are enumerated, and

these belong to 162 genera. We thus see that these naturalised plants

are of a highly diversified nature. They differ, moreover, to a large

extent from the indigenes, for out of the 162 genera, no less than 100

genera are not there indigenous, and thus a large proportional addition

is made to the genera of these States.

By considering the nature of the plants or animals which have struggled

successfully with the indigenes of any country, and have there become

naturalised, we can gain some crude idea in what manner some of the

natives would have had to be modified, in order to have gained an

advantage over the other natives; and we may, I think, at least safely

infer that diversification of structure, amounting to new generic

differences, would have been profitable to them.

The advantage of diversification in the inhabitants of the same region

is, in fact, the same as that of the physiological division of labour in

the organs of the same individual body--a subject so well elucidated by

Milne Edwards. No physiologist doubts that a stomach by being adapted to

digest vegetable matter alone, or flesh alone, draws most nutriment from

these substances. So in the general economy of any land, the more widely

and perfectly the animals and plants are diversified for different

habits of life, so will a greater number of individuals be capable of

there supporting themselves. A set of animals, with their organisation

but little diversified, could hardly compete with a set more perfectly

diversified in structure. It may be doubted, for instance, whether

the Australian marsupials, which are divided into groups differing but

little from each other, and feebly representing, as Mr. Waterhouse and

others have remarked, our carnivorous, ruminant, and rodent mammals,

could successfully compete with these well-pronounced orders. In the

Australian mammals, we see the process of diversification in an early

and incomplete stage of development. After the foregoing discussion,

which ought to have been much amplified, we may, I think, assume that

the modified descendants of any one species will succeed by so much

the better as they become more diversified in structure, and are thus

enabled to encroach on places occupied by other beings. Now let us see

how this principle of great benefit being derived from divergence of

character, combined with the principles of natural selection and of

extinction, will tend to act.

The accompanying diagram will aid us in understanding this rather

perplexing subject. Let A to L represent the species of a genus large

in its own country; these species are supposed to resemble each other

in unequal degrees, as is so generally the case in nature, and as is

represented in the diagram by the letters standing at unequal distances.

I have said a large genus, because we have seen in the second chapter,

that on an average more of the species of large genera vary than of

small genera; and the varying species of the large genera present a

greater number of varieties. We have, also, seen that the species, which

are the commonest and the most widely-diffused, vary more than rare

species with restricted ranges. Let (A) be a common, widely-diffused,

and varying species, belonging to a genus large in its own country. The

little fan of diverging dotted lines of unequal lengths proceeding from

(A), may represent its varying offspring. The variations are supposed

to be extremely slight, but of the most diversified nature; they are not

supposed all to appear simultaneously, but often after long intervals of

time; nor are they all supposed to endure for equal periods. Only

those variations which are in some way profitable will be preserved or

naturally selected. And here the importance of the principle of benefit

being derived from divergence of character comes in; for this

will generally lead to the most different or divergent variations

(represented by the outer dotted lines) being preserved and accumulated

by natural selection. When a dotted line reaches one of the horizontal

lines, and is there marked by a small numbered letter, a sufficient

amount of variation is supposed to have been accumulated to have formed

a fairly well-marked variety, such as would be thought worthy of record

in a systematic work.

The intervals between the horizontal lines in the diagram, may represent

each a thousand generations; but it would have been better if each had

represented ten thousand generations. After a thousand generations,

species (A) is supposed to have produced two fairly well-marked

varieties, namely a1 and m1. These two varieties will generally continue

to be exposed to the same conditions which made their parents variable,

and the tendency to variability is in itself hereditary, consequently

they will tend to vary, and generally to vary in nearly the same manner

as their parents varied. Moreover, these two varieties, being only

slightly modified forms, will tend to inherit those advantages which

made their common parent (A) more numerous than most of the other

inhabitants of the same country; they will likewise partake of those

more general advantages which made the genus to which the parent-species

belonged, a large genus in its own country. And these circumstances we

know to be favourable to the production of new varieties.

If, then, these two varieties be variable, the most divergent of

their variations will generally be preserved during the next thousand

generations. And after this interval, variety a1 is supposed in the

diagram to have produced variety a2, which will, owing to the principle

of divergence, differ more from (A) than did variety a1. Variety m1 is

supposed to have produced two varieties, namely m2 and s2, differing

from each other, and more considerably from their common parent (A). We

may continue the process by similar steps for any length of time; some

of the varieties, after each thousand generations, producing only

a single variety, but in a more and more modified condition, some

producing two or three varieties, and some failing to produce any. Thus

the varieties or modified descendants, proceeding from the common

parent (A), will generally go on increasing in number and diverging

in character. In the diagram the process is represented up to the

ten-thousandth generation, and under a condensed and simplified form up

to the fourteen-thousandth generation.

But I must here remark that I do not suppose that the process ever goes

on so regularly as is represented in the diagram, though in itself

made somewhat irregular. I am far from thinking that the most divergent

varieties will invariably prevail and multiply: a medium form may

often long endure, and may or may not produce more than one modified

descendant; for natural selection will always act according to the

nature of the places which are either unoccupied or not perfectly

occupied by other beings; and this will depend on infinitely complex

relations. But as a general rule, the more diversified in structure the

descendants from any one species can be rendered, the more places they

will be enabled to seize on, and the more their modified progeny will

be increased. In our diagram the line of succession is broken at regular

intervals by small numbered letters marking the successive forms which

have become sufficiently distinct to be recorded as varieties. But

these breaks are imaginary, and might have been inserted anywhere, after

intervals long enough to have allowed the accumulation of a considerable

amount of divergent variation.

As all the modified descendants from a common and widely-diffused

species, belonging to a large genus, will tend to partake of the

same advantages which made their parent successful in life, they will

generally go on multiplying in number as well as diverging in character:

this is represented in the diagram by the several divergent branches

proceeding from (A). The modified offspring from the later and more

highly improved branches in the lines of descent, will, it is probable,

often take the place of, and so destroy, the earlier and less improved

branches: this is represented in the diagram by some of the lower

branches not reaching to the upper horizontal lines. In some cases I do

not doubt that the process of modification will be confined to a

single line of descent, and the number of the descendants will not be

increased; although the amount of divergent modification may have been

increased in the successive generations. This case would be represented

in the diagram, if all the lines proceeding from (A) were removed,

excepting that from a1 to a10. In the same way, for instance, the

English race-horse and English pointer have apparently both gone on

slowly diverging in character from their original stocks, without either

having given off any fresh branches or races.

After ten thousand generations, species (A) is supposed to have produced

three forms, a10, f10, and m10, which, from having diverged in character

during the successive generations, will have come to differ largely, but

perhaps unequally, from each other and from their common parent. If we

suppose the amount of change between each horizontal line in our diagram

to be excessively small, these three forms may still be only well-marked

varieties; or they may have arrived at the doubtful category of

sub-species; but we have only to suppose the steps in the process of

modification to be more numerous or greater in amount, to convert these

three forms into well-defined species: thus the diagram illustrates

the steps by which the small differences distinguishing varieties

are increased into the larger differences distinguishing species. By

continuing the same process for a greater number of generations (as

shown in the diagram in a condensed and simplified manner), we get eight

species, marked by the letters between a14 and m14, all descended from

(A). Thus, as I believe, species are multiplied and genera are formed.

In a large genus it is probable that more than one species would vary.

In the diagram I have assumed that a second species (I) has produced, by

analogous steps, after ten thousand generations, either two well-marked

varieties (w10 and z10) or two species, according to the amount of

change supposed to be represented between the horizontal lines. After

fourteen thousand generations, six new species, marked by the letters

n14 to z14, are supposed to have been produced. In each genus, the

species, which are already extremely different in character, will

generally tend to produce the greatest number of modified descendants;

for these will have the best chance of filling new and widely different

places in the polity of nature: hence in the diagram I have chosen the

extreme species (A), and the nearly extreme species (I), as those which

have largely varied, and have given rise to new varieties and species.

The other nine species (marked by capital letters) of our original

genus, may for a long period continue transmitting unaltered

descendants; and this is shown in the diagram by the dotted lines not

prolonged far upwards from want of space.

But during the process of modification, represented in the diagram,

another of our principles, namely that of extinction, will have played

an important part. As in each fully stocked country natural selection

necessarily acts by the selected form having some advantage in the

struggle for life over other forms, there will be a constant tendency in

the improved descendants of any one species to supplant and exterminate

in each stage of descent their predecessors and their original parent.

For it should be remembered that the competition will generally be most

severe between those forms which are most nearly related to each other

in habits, constitution, and structure. Hence all the intermediate forms

between the earlier and later states, that is between the less and more

improved state of a species, as well as the original parent-species

itself, will generally tend to become extinct. So it probably will be

with many whole collateral lines of descent, which will be conquered by

later and improved lines of descent. If, however, the modified offspring

of a species get into some distinct country, or become quickly adapted

to some quite new station, in which child and parent do not come into

competition, both may continue to exist.

If then our diagram be assumed to represent a considerable amount of

modification, species (A) and all the earlier varieties will have become

extinct, having been replaced by eight new species (a14 to m14); and (I)

will have been replaced by six (n14 to z14) new species.

But we may go further than this. The original species of our genus were

supposed to resemble each other in unequal degrees, as is so generally

the case in nature; species (A) being more nearly related to B, C, and

D, than to the other species; and species (I) more to G, H, K, L, than

to the others. These two species (A) and (I), were also supposed to be

very common and widely diffused species, so that they must originally

have had some advantage over most of the other species of the

genus. Their modified descendants, fourteen in number at the

fourteen-thousandth generation, will probably have inherited some of

the same advantages: they have also been modified and improved in

a diversified manner at each stage of descent, so as to have become

adapted to many related places in the natural economy of their country.

It seems, therefore, to me extremely probable that they will have taken

the places of, and thus exterminated, not only their parents (A) and

(I), but likewise some of the original species which were most nearly

related to their parents. Hence very few of the original species will

have transmitted offspring to the fourteen-thousandth generation. We may

suppose that only one (F), of the two species which were least closely

related to the other nine original species, has transmitted descendants

to this late stage of descent.

The new species in our diagram descended from the original eleven

species, will now be fifteen in number. Owing to the divergent tendency

of natural selection, the extreme amount of difference in character

between species a14 and z14 will be much greater than that between

the most different of the original eleven species. The new species,

moreover, will be allied to each other in a widely different manner. Of

the eight descendants from (A) the three marked a14, q14, p14, will be

nearly related from having recently branched off from a10; b14 and

f14, from having diverged at an earlier period from a5, will be in some

degree distinct from the three first-named species; and lastly, o14,

e14, and m14, will be nearly related one to the other, but from having

diverged at the first commencement of the process of modification, will

be widely different from the other five species, and may constitute a

sub-genus or even a distinct genus.

The six descendants from (I) will form two sub-genera or even genera.

But as the original species (I) differed largely from (A), standing

nearly at the extreme points of the original genus, the six descendants

from (I) will, owing to inheritance, differ considerably from the eight

descendants from (A); the two groups, moreover, are supposed to have

gone on diverging in different directions. The intermediate species,

also (and this is a very important consideration), which connected the

original species (A) and (I), have all become, excepting (F), extinct,

and have left no descendants. Hence the six new species descended from

(I), and the eight descended from (A), will have to be ranked as very

distinct genera, or even as distinct sub-families.

Thus it is, as I believe, that two or more genera are produced by

descent, with modification, from two or more species of the same genus.

And the two or more parent-species are supposed to have descended from

some one species of an earlier genus. In our diagram, this is indicated

by the broken lines, beneath the capital letters, converging in

sub-branches downwards towards a single point; this point representing a

single species, the supposed single parent of our several new sub-genera

and genera.

It is worth while to reflect for a moment on the character of the new

species F14, which is supposed not to have diverged much in character,

but to have retained the form of (F), either unaltered or altered only

in a slight degree. In this case, its affinities to the other fourteen

new species will be of a curious and circuitous nature. Having descended

from a form which stood between the two parent-species (A) and (I),

now supposed to be extinct and unknown, it will be in some degree

intermediate in character between the two groups descended from these

species. But as these two groups have gone on diverging in character

from the type of their parents, the new species (F14) will not be

directly intermediate between them, but rather between types of the two

groups; and every naturalist will be able to bring some such case before

his mind.

In the diagram, each horizontal line has hitherto been supposed to

represent a thousand generations, but each may represent a million or

hundred million generations, and likewise a section of the successive

strata of the earth's crust including extinct remains. We shall, when we

come to our chapter on Geology, have to refer again to this subject,

and I think we shall then see that the diagram throws light on the

affinities of extinct beings, which, though generally belonging to the

same orders, or families, or genera, with those now living, yet are

often, in some degree, intermediate in character between existing

groups; and we can understand this fact, for the extinct species lived

at very ancient epochs when the branching lines of descent had diverged

less.

I see no reason to limit the process of modification, as now explained,

to the formation of genera alone. If, in our diagram, we suppose the

amount of change represented by each successive group of diverging

dotted lines to be very great, the forms marked a14 to p14, those marked

b14 and f14, and those marked o14 to m14, will form three very distinct

genera. We shall also have two very distinct genera descended from

(I) and as these latter two genera, both from continued divergence of

character and from inheritance from a different parent, will differ

widely from the three genera descended from (A), the two little groups

of genera will form two distinct families, or even orders, according to

the amount of divergent modification supposed to be represented in the

diagram. And the two new families, or orders, will have descended from

two species of the original genus; and these two species are supposed

to have descended from one species of a still more ancient and unknown

genus.

We have seen that in each country it is the species of the larger genera

which oftenest present varieties or incipient species. This, indeed,

might have been expected; for as natural selection acts through one form

having some advantage over other forms in the struggle for existence,

it will chiefly act on those which already have some advantage; and

the largeness of any group shows that its species have inherited from

a common ancestor some advantage in common. Hence, the struggle for the

production of new and modified descendants, will mainly lie between the

larger groups, which are all trying to increase in number. One large

group will slowly conquer another large group, reduce its numbers, and

thus lessen its chance of further variation and improvement. Within the

same large group, the later and more highly perfected sub-groups, from

branching out and seizing on many new places in the polity of Nature,

will constantly tend to supplant and destroy the earlier and less

improved sub-groups. Small and broken groups and sub-groups will finally

tend to disappear. Looking to the future, we can predict that the groups

of organic beings which are now large and triumphant, and which are

least broken up, that is, which as yet have suffered least extinction,

will for a long period continue to increase. But which groups will

ultimately prevail, no man can predict; for we well know that many

groups, formerly most extensively developed, have now become extinct.

Looking still more remotely to the future, we may predict that, owing to

the continued and steady increase of the larger groups, a multitude

of smaller groups will become utterly extinct, and leave no modified

descendants; and consequently that of the species living at any one

period, extremely few will transmit descendants to a remote futurity. I

shall have to return to this subject in the chapter on Classification,

but I may add that on this view of extremely few of the more ancient

species having transmitted descendants, and on the view of all the

descendants of the same species making a class, we can understand how

it is that there exist but very few classes in each main division of

the animal and vegetable kingdoms. Although extremely few of the most

ancient species may now have living and modified descendants, yet at the

most remote geological period, the earth may have been as well peopled

with many species of many genera, families, orders, and classes, as at

the present day.

SUMMARY OF CHAPTER.

If during the long course of ages and under varying conditions of life,

organic beings vary at all in the several parts of their organisation,

and I think this cannot be disputed; if there be, owing to the high

geometrical powers of increase of each species, at some age, season, or

year, a severe struggle for life, and this certainly cannot be disputed;

then, considering the infinite complexity of the relations of all

organic beings to each other and to their conditions of existence,

causing an infinite diversity in structure, constitution, and habits, to

be advantageous to them, I think it would be a most extraordinary fact

if no variation ever had occurred useful to each being's own welfare, in

the same way as so many variations have occurred useful to man. But if

variations useful to any organic being do occur, assuredly individuals

thus characterised will have the best chance of being preserved in the

struggle for life; and from the strong principle of inheritance they

will tend to produce offspring similarly characterised. This principle

of preservation, I have called, for the sake of brevity, Natural

Selection. Natural selection, on the principle of qualities being

inherited at corresponding ages, can modify the egg, seed, or young, as

easily as the adult. Amongst many animals, sexual selection will give

its aid to ordinary selection, by assuring to the most vigorous and best

adapted males the greatest number of offspring. Sexual selection will

also give characters useful to the males alone, in their struggles with

other males.

Whether natural selection has really thus acted in nature, in modifying

and adapting the various forms of life to their several conditions

and stations, must be judged of by the general tenour and balance of

evidence given in the following chapters. But we already see how it

entails extinction; and how largely extinction has acted in the world's

history, geology plainly declares. Natural selection, also, leads to

divergence of character; for more living beings can be supported on the

same area the more they diverge in structure, habits, and constitution,

of which we see proof by looking at the inhabitants of any small spot

or at naturalised productions. Therefore during the modification of the

descendants of any one species, and during the incessant struggle of all

species to increase in numbers, the more diversified these descendants

become, the better will be their chance of succeeding in the battle of

life. Thus the small differences distinguishing varieties of the same

species, will steadily tend to increase till they come to equal the

greater differences between species of the same genus, or even of

distinct genera.

We have seen that it is the common, the widely-diffused, and

widely-ranging species, belonging to the larger genera, which vary

most; and these will tend to transmit to their modified offspring

that superiority which now makes them dominant in their own countries.

Natural selection, as has just been remarked, leads to divergence of

character and to much extinction of the less improved and intermediate

forms of life. On these principles, I believe, the nature of the

affinities of all organic beings may be explained. It is a truly

wonderful fact--the wonder of which we are apt to overlook from

familiarity--that all animals and all plants throughout all time and

space should be related to each other in group subordinate to group,

in the manner which we everywhere behold--namely, varieties of the same

species most closely related together, species of the same genus less

closely and unequally related together, forming sections and sub-genera,

species of distinct genera much less closely related, and genera

related in different degrees, forming sub-families, families, orders,

sub-classes, and classes. The several subordinate groups in any class

cannot be ranked in a single file, but seem rather to be clustered

round points, and these round other points, and so on in almost endless

cycles. On the view that each species has been independently created, I

can see no explanation of this great fact in the classification of all

organic beings; but, to the best of my judgment, it is explained through

inheritance and the complex action of natural selection, entailing

extinction and divergence of character, as we have seen illustrated in

the diagram.

The affinities of all the beings of the same class have sometimes been

represented by a great tree. I believe this simile largely speaks the

truth. The green and budding twigs may represent existing species; and

those produced during each former year may represent the long succession

of extinct species. At each period of growth all the growing twigs

have tried to branch out on all sides, and to overtop and kill the

surrounding twigs and branches, in the same manner as species and groups

of species have tried to overmaster other species in the great battle

for life. The limbs divided into great branches, and these into lesser

and lesser branches, were themselves once, when the tree was small,

budding twigs; and this connexion of the former and present buds by

ramifying branches may well represent the classification of all extinct

and living species in groups subordinate to groups. Of the many twigs

which flourished when the tree was a mere bush, only two or three, now

grown into great branches, yet survive and bear all the other branches;

so with the species which lived during long-past geological periods,

very few now have living and modified descendants. From the first growth

of the tree, many a limb and branch has decayed and dropped off; and

these lost branches of various sizes may represent those whole orders,

families, and genera which have now no living representatives, and which

are known to us only from having been found in a fossil state. As we

here and there see a thin straggling branch springing from a fork low

down in a tree, and which by some chance has been favoured and is

still alive on its summit, so we occasionally see an animal like the

Ornithorhynchus or Lepidosiren, which in some small degree connects by

its affinities two large branches of life, and which has apparently been

saved from fatal competition by having inhabited a protected station. As

buds give rise by growth to fresh buds, and these, if vigorous, branch

out and overtop on all sides many a feebler branch, so by generation I

believe it has been with the great Tree of Life, which fills with its

dead and broken branches the crust of the earth, and covers the surface

with its ever branching and beautiful ramifications.

5. LAWS OF VARIATION.

Effects of external conditions. Use and disuse, combined with natural

selection; organs of flight and of vision. Acclimatisation. Correlation

of growth. Compensation and economy of growth. False correlations.

Multiple, rudimentary, and lowly organised structures variable. Parts

developed in an unusual manner are highly variable: specific characters

more variable than generic: secondary sexual characters variable.

Species of the same genus vary in an analogous manner. Reversions to

long lost characters. Summary.

I have hitherto sometimes spoken as if the variations--so common and

multiform in organic beings under domestication, and in a lesser degree

in those in a state of nature--had been due to chance. This, of course,

is a wholly incorrect expression, but it serves to acknowledge plainly

our ignorance of the cause of each particular variation. Some authors

believe it to be as much the function of the reproductive system to

produce individual differences, or very slight deviations of structure,

as to make the child like its parents. But the much greater variability,

as well as the greater frequency of monstrosities, under domestication

or cultivation, than under nature, leads me to believe that deviations

of structure are in some way due to the nature of the conditions of

life, to which the parents and their more remote ancestors have been

exposed during several generations. I have remarked in the first

chapter--but a long catalogue of facts which cannot be here given would

be necessary to show the truth of the remark--that the reproductive

system is eminently susceptible to changes in the conditions of life;

and to this system being functionally disturbed in the parents, I

chiefly attribute the varying or plastic condition of the offspring. The

male and female sexual elements seem to be affected before that union

takes place which is to form a new being. In the case of "sporting"

plants, the bud, which in its earliest condition does not apparently

differ essentially from an ovule, is alone affected. But why, because

the reproductive system is disturbed, this or that part should vary more

or less, we are profoundly ignorant. Nevertheless, we can here and there

dimly catch a faint ray of light, and we may feel sure that there must

be some cause for each deviation of structure, however slight.

How much direct effect difference of climate, food, etc., produces on

any being is extremely doubtful. My impression is, that the effect is

extremely small in the case of animals, but perhaps rather more in that

of plants. We may, at least, safely conclude that such influences cannot

have produced the many striking and complex co-adaptations of structure

between one organic being and another, which we see everywhere

throughout nature. Some little influence may be attributed to climate,

food, etc.: thus, E. Forbes speaks confidently that shells at their

southern limit, and when living in shallow water, are more brightly

coloured than those of the same species further north or from greater

depths. Gould believes that birds of the same species are more brightly

coloured under a clear atmosphere, than when living on islands or near

the coast. So with insects, Wollaston is convinced that residence near

the sea affects their colours. Moquin-Tandon gives a list of plants

which when growing near the sea-shore have their leaves in some degree

fleshy, though not elsewhere fleshy. Several other such cases could be

given.

The fact of varieties of one species, when they range into the zone of

habitation of other species, often acquiring in a very slight degree

some of the characters of such species, accords with our view that

species of all kinds are only well-marked and permanent varieties. Thus

the species of shells which are confined to tropical and shallow seas

are generally brighter-coloured than those confined to cold and deeper

seas. The birds which are confined to continents are, according to

Mr. Gould, brighter-coloured than those of islands. The insect-species

confined to sea-coasts, as every collector knows, are often brassy or

lurid. Plants which live exclusively on the sea-side are very apt to

have fleshy leaves. He who believes in the creation of each species,

will have to say that this shell, for instance, was created with bright

colours for a warm sea; but that this other shell became bright-coloured

by variation when it ranged into warmer or shallower waters.

When a variation is of the slightest use to a being, we cannot tell how

much of it to attribute to the accumulative action of natural selection,

and how much to the conditions of life. Thus, it is well known to

furriers that animals of the same species have thicker and better fur

the more severe the climate is under which they have lived; but who

can tell how much of this difference may be due to the warmest-clad

individuals having been favoured and preserved during many generations,

and how much to the direct action of the severe climate? for it would

appear that climate has some direct action on the hair of our domestic

quadrupeds.

Instances could be given of the same variety being produced under

conditions of life as different as can well be conceived; and, on the

other hand, of different varieties being produced from the same species

under the same conditions. Such facts show how indirectly the conditions

of life must act. Again, innumerable instances are known to every

naturalist of species keeping true, or not varying at all, although

living under the most opposite climates. Such considerations as these

incline me to lay very little weight on the direct action of the

conditions of life. Indirectly, as already remarked, they seem to play

an important part in affecting the reproductive system, and in thus

inducing variability; and natural selection will then accumulate

all profitable variations, however slight, until they become plainly

developed and appreciable by us.

EFFECTS OF USE AND DISUSE.

From the facts alluded to in the first chapter, I think there can be

little doubt that use in our domestic animals strengthens and enlarges

certain parts, and disuse diminishes them; and that such modifications

are inherited. Under free nature, we can have no standard of comparison,

by which to judge of the effects of long-continued use or disuse, for we

know not the parent-forms; but many animals have structures which can

be explained by the effects of disuse. As Professor Owen has remarked,

there is no greater anomaly in nature than a bird that cannot fly; yet

there are several in this state. The logger-headed duck of South America

can only flap along the surface of the water, and has its wings in

nearly the same condition as the domestic Aylesbury duck. As the larger

ground-feeding birds seldom take flight except to escape danger, I

believe that the nearly wingless condition of several birds, which now

inhabit or have lately inhabited several oceanic islands, tenanted by

no beast of prey, has been caused by disuse. The ostrich indeed inhabits

continents and is exposed to danger from which it cannot escape by

flight, but by kicking it can defend itself from enemies, as well as any

of the smaller quadrupeds. We may imagine that the early progenitor

of the ostrich had habits like those of a bustard, and that as natural

selection increased in successive generations the size and weight of

its body, its legs were used more, and its wings less, until they became

incapable of flight.

Kirby has remarked (and I have observed the same fact) that the anterior

tarsi, or feet, of many male dung-feeding beetles are very often broken

off; he examined seventeen specimens in his own collection, and not one

had even a relic left. In the Onites apelles the tarsi are so habitually

lost, that the insect has been described as not having them. In some

other genera they are present, but in a rudimentary condition. In the

Ateuchus or sacred beetle of the Egyptians, they are totally deficient.

There is not sufficient evidence to induce us to believe that

mutilations are ever inherited; and I should prefer explaining the

entire absence of the anterior tarsi in Ateuchus, and their rudimentary

condition in some other genera, by the long-continued effects of disuse

in their progenitors; for as the tarsi are almost always lost in many

dung-feeding beetles, they must be lost early in life, and therefore

cannot be much used by these insects.

In some cases we might easily put down to disuse modifications of

structure which are wholly, or mainly, due to natural selection. Mr.

Wollaston has discovered the remarkable fact that 200 beetles, out of

the 550 species inhabiting Madeira, are so far deficient in wings that

they cannot fly; and that of the twenty-nine endemic genera, no less

than twenty-three genera have all their species in this condition!

Several facts, namely, that beetles in many parts of the world are very

frequently blown to sea and perish; that the beetles in Madeira, as

observed by Mr. Wollaston, lie much concealed, until the wind lulls and

the sun shines; that the proportion of wingless beetles is larger on

the exposed Dezertas than in Madeira itself; and especially the

extraordinary fact, so strongly insisted on by Mr. Wollaston, of the

almost entire absence of certain large groups of beetles, elsewhere

excessively numerous, and which groups have habits of life almost

necessitating frequent flight;--these several considerations have made

me believe that the wingless condition of so many Madeira beetles is

mainly due to the action of natural selection, but combined probably

with disuse. For during thousands of successive generations each

individual beetle which flew least, either from its wings having been

ever so little less perfectly developed or from indolent habit, will

have had the best chance of surviving from not being blown out to sea;

and, on the other hand, those beetles which most readily took to flight

will oftenest have been blown to sea and thus have been destroyed.

The insects in Madeira which are not ground-feeders, and which, as the

flower-feeding coleoptera and lepidoptera, must habitually use their

wings to gain their subsistence, have, as Mr. Wollaston suspects, their

wings not at all reduced, but even enlarged. This is quite compatible

with the action of natural selection. For when a new insect first

arrived on the island, the tendency of natural selection to enlarge

or to reduce the wings, would depend on whether a greater number of

individuals were saved by successfully battling with the winds, or

by giving up the attempt and rarely or never flying. As with mariners

shipwrecked near a coast, it would have been better for the good

swimmers if they had been able to swim still further, whereas it would

have been better for the bad swimmers if they had not been able to swim

at all and had stuck to the wreck.

The eyes of moles and of some burrowing rodents are rudimentary in size,

and in some cases are quite covered up by skin and fur. This state of

the eyes is probably due to gradual reduction from disuse, but aided

perhaps by natural selection. In South America, a burrowing rodent, the

tuco-tuco, or Ctenomys, is even more subterranean in its habits than the

mole; and I was assured by a Spaniard, who had often caught them, that

they were frequently blind; one which I kept alive was certainly in

this condition, the cause, as appeared on dissection, having been

inflammation of the nictitating membrane. As frequent inflammation of

the eyes must be injurious to any animal, and as eyes are certainly not

indispensable to animals with subterranean habits, a reduction in their

size with the adhesion of the eyelids and growth of fur over them,

might in such case be an advantage; and if so, natural selection would

constantly aid the effects of disuse.

It is well known that several animals, belonging to the most different

classes, which inhabit the caves of Styria and of Kentucky, are blind.

In some of the crabs the foot-stalk for the eye remains, though the eye

is gone; the stand for the telescope is there, though the telescope

with its glasses has been lost. As it is difficult to imagine that

eyes, though useless, could be in any way injurious to animals living in

darkness, I attribute their loss wholly to disuse. In one of the

blind animals, namely, the cave-rat, the eyes are of immense size; and

Professor Silliman thought that it regained, after living some days in

the light, some slight power of vision. In the same manner as in Madeira

the wings of some of the insects have been enlarged, and the wings of

others have been reduced by natural selection aided by use and disuse,

so in the case of the cave-rat natural selection seems to have struggled

with the loss of light and to have increased the size of the eyes;

whereas with all the other inhabitants of the caves, disuse by itself

seems to have done its work.

It is difficult to imagine conditions of life more similar than deep

limestone caverns under a nearly similar climate; so that on the

common view of the blind animals having been separately created for the

American and European caverns, close similarity in their organisation

and affinities might have been expected; but, as Schiodte and others

have remarked, this is not the case, and the cave-insects of the two

continents are not more closely allied than might have been anticipated

from the general resemblance of the other inhabitants of North America

and Europe. On my view we must suppose that American animals, having

ordinary powers of vision, slowly migrated by successive generations

from the outer world into the deeper and deeper recesses of the Kentucky

caves, as did European animals into the caves of Europe. We have some

evidence of this gradation of habit; for, as Schiodte remarks, "animals

not far remote from ordinary forms, prepare the transition from light to

darkness. Next follow those that are constructed for twilight; and, last

of all, those destined for total darkness." By the time that an animal

had reached, after numberless generations, the deepest recesses, disuse

will on this view have more or less perfectly obliterated its eyes, and

natural selection will often have effected other changes, such as an

increase in the length of the antennae or palpi, as a compensation for

blindness. Notwithstanding such modifications, we might expect still to

see in the cave-animals of America, affinities to the other inhabitants

of that continent, and in those of Europe, to the inhabitants of the

European continent. And this is the case with some of the American

cave-animals, as I hear from Professor Dana; and some of the European

cave-insects are very closely allied to those of the surrounding

country. It would be most difficult to give any rational explanation of

the affinities of the blind cave-animals to the other inhabitants of the

two continents on the ordinary view of their independent creation. That

several of the inhabitants of the caves of the Old and New Worlds should

be closely related, we might expect from the well-known relationship of

most of their other productions. Far from feeling any surprise that some

of the cave-animals should be very anomalous, as Agassiz has remarked

in regard to the blind fish, the Amblyopsis, and as is the case with

the blind Proteus with reference to the reptiles of Europe, I am only

surprised that more wrecks of ancient life have not been preserved,

owing to the less severe competition to which the inhabitants of these

dark abodes will probably have been exposed.

ACCLIMATISATION.

Habit is hereditary with plants, as in the period of flowering, in the

amount of rain requisite for seeds to germinate, in the time of sleep,

etc., and this leads me to say a few words on acclimatisation. As it is

extremely common for species of the same genus to inhabit very hot and

very cold countries, and as I believe that all the species of the same

genus have descended from a single parent, if this view be correct,

acclimatisation must be readily effected during long-continued descent.

It is notorious that each species is adapted to the climate of its own

home: species from an arctic or even from a temperate region cannot

endure a tropical climate, or conversely. So again, many succulent

plants cannot endure a damp climate. But the degree of adaptation of

species to the climates under which they live is often overrated. We

may infer this from our frequent inability to predict whether or not an

imported plant will endure our climate, and from the number of plants

and animals brought from warmer countries which here enjoy good health.

We have reason to believe that species in a state of nature are limited

in their ranges by the competition of other organic beings quite as much

as, or more than, by adaptation to particular climates. But whether or

not the adaptation be generally very close, we have evidence, in

the case of some few plants, of their becoming, to a certain

extent, naturally habituated to different temperatures, or becoming

acclimatised: thus the pines and rhododendrons, raised from seed

collected by Dr. Hooker from trees growing at different heights on the

Himalaya, were found in this country to possess different constitutional

powers of resisting cold. Mr. Thwaites informs me that he has observed

similar facts in Ceylon, and analogous observations have been made by

Mr. H. C. Watson on European species of plants brought from the Azores

to England. In regard to animals, several authentic cases could be given

of species within historical times having largely extended their

range from warmer to cooler latitudes, and conversely; but we do not

positively know that these animals were strictly adapted to their native

climate, but in all ordinary cases we assume such to be the case; nor

do we know that they have subsequently become acclimatised to their new

homes.

As I believe that our domestic animals were originally chosen by

uncivilised man because they were useful and bred readily under

confinement, and not because they were subsequently found capable

of far-extended transportation, I think the common and extraordinary

capacity in our domestic animals of not only withstanding the most

different climates but of being perfectly fertile (a far severer test)

under them, may be used as an argument that a large proportion of other

animals, now in a state of nature, could easily be brought to bear

widely different climates. We must not, however, push the foregoing

argument too far, on account of the probable origin of some of our

domestic animals from several wild stocks: the blood, for instance, of

a tropical and arctic wolf or wild dog may perhaps be mingled in our

domestic breeds. The rat and mouse cannot be considered as domestic

animals, but they have been transported by man to many parts of the

world, and now have a far wider range than any other rodent, living free

under the cold climate of Faroe in the north and of the Falklands in the

south, and on many islands in the torrid zones. Hence I am inclined to

look at adaptation to any special climate as a quality readily grafted

on an innate wide flexibility of constitution, which is common to most

animals. On this view, the capacity of enduring the most different

climates by man himself and by his domestic animals, and such facts

as that former species of the elephant and rhinoceros were capable

of enduring a glacial climate, whereas the living species are now all

tropical or sub-tropical in their habits, ought not to be looked at

as anomalies, but merely as examples of a very common flexibility of

constitution, brought, under peculiar circumstances, into play.

How much of the acclimatisation of species to any peculiar climate is

due to mere habit, and how much to the natural selection of varieties

having different innate constitutions, and how much to both means

combined, is a very obscure question. That habit or custom has some

influence I must believe, both from analogy, and from the incessant

advice given in agricultural works, even in the ancient Encyclopaedias

of China, to be very cautious in transposing animals from one district

to another; for it is not likely that man should have succeeded in

selecting so many breeds and sub-breeds with constitutions specially

fitted for their own districts: the result must, I think, be due to

habit. On the other hand, I can see no reason to doubt that natural

selection will continually tend to preserve those individuals which

are born with constitutions best adapted to their native countries. In

treatises on many kinds of cultivated plants, certain varieties are

said to withstand certain climates better than others: this is very

strikingly shown in works on fruit trees published in the United States,

in which certain varieties are habitually recommended for the northern,

and others for the southern States; and as most of these varieties are

of recent origin, they cannot owe their constitutional differences to

habit. The case of the Jerusalem artichoke, which is never propagated

by seed, and of which consequently new varieties have not been produced,

has even been advanced--for it is now as tender as ever it was--as

proving that acclimatisation cannot be effected! The case, also, of the

kidney-bean has been often cited for a similar purpose, and with

much greater weight; but until some one will sow, during a score of

generations, his kidney-beans so early that a very large proportion are

destroyed by frost, and then collect seed from the few survivors, with

care to prevent accidental crosses, and then again get seed from these

seedlings, with the same precautions, the experiment cannot be said to

have been even tried. Nor let it be supposed that no differences in the

constitution of seedling kidney-beans ever appear, for an account has

been published how much more hardy some seedlings appeared to be than

others.

On the whole, I think we may conclude that habit, use, and disuse, have,

in some cases, played a considerable part in the modification of the

constitution, and of the structure of various organs; but that the

effects of use and disuse have often been largely combined with, and

sometimes overmastered by, the natural selection of innate differences.

CORRELATION OF GROWTH.

I mean by this expression that the whole organisation is so tied

together during its growth and development, that when slight variations

in any one part occur, and are accumulated through natural selection,

other parts become modified. This is a very important subject, most

imperfectly understood. The most obvious case is, that modifications

accumulated solely for the good of the young or larva, will, it may

safely be concluded, affect the structure of the adult; in the same

manner as any malconformation affecting the early embryo, seriously

affects the whole organisation of the adult. The several parts of the

body which are homologous, and which, at an early embryonic period, are

alike, seem liable to vary in an allied manner: we see this in the right

and left sides of the body varying in the same manner; in the front and

hind legs, and even in the jaws and limbs, varying together, for the

lower jaw is believed to be homologous with the limbs. These tendencies,

I do not doubt, may be mastered more or less completely by natural

selection: thus a family of stags once existed with an antler only on

one side; and if this had been of any great use to the breed it might

probably have been rendered permanent by natural selection.

Homologous parts, as has been remarked by some authors, tend to cohere;

this is often seen in monstrous plants; and nothing is more common than

the union of homologous parts in normal structures, as the union of the

petals of the corolla into a tube. Hard parts seem to affect the form of

adjoining soft parts; it is believed by some authors that the diversity

in the shape of the pelvis in birds causes the remarkable diversity in

the shape of their kidneys. Others believe that the shape of the pelvis

in the human mother influences by pressure the shape of the head of the

child. In snakes, according to Schlegel, the shape of the body and

the manner of swallowing determine the position of several of the most

important viscera.

The nature of the bond of correlation is very frequently quite obscure.

M. Is. Geoffroy St. Hilaire has forcibly remarked, that certain

malconformations very frequently, and that others rarely coexist,

without our being able to assign any reason. What can be more singular

than the relation between blue eyes and deafness in cats, and the

tortoise-shell colour with the female sex; the feathered feet and skin

between the outer toes in pigeons, and the presence of more or less down

on the young birds when first hatched, with the future colour of their

plumage; or, again, the relation between the hair and teeth in the naked

Turkish dog, though here probably homology comes into play? With respect

to this latter case of correlation, I think it can hardly be accidental,

that if we pick out the two orders of mammalia which are most

abnormal in their dermal coverings, viz. Cetacea (whales) and Edentata

(armadilloes, scaly ant-eaters, etc.), that these are likewise the most

abnormal in their teeth.

I know of no case better adapted to show the importance of the laws of

correlation in modifying important structures, independently of utility

and, therefore, of natural selection, than that of the difference

between the outer and inner flowers in some Compositous and

Umbelliferous plants. Every one knows the difference in the ray and

central florets of, for instance, the daisy, and this difference is

often accompanied with the abortion of parts of the flower. But, in some

Compositous plants, the seeds also differ in shape and sculpture; and

even the ovary itself, with its accessory parts, differs, as has been

described by Cassini. These differences have been attributed by some

authors to pressure, and the shape of the seeds in the ray-florets in

some Compositae countenances this idea; but, in the case of the corolla

of the Umbelliferae, it is by no means, as Dr. Hooker informs me, in

species with the densest heads that the inner and outer flowers most

frequently differ. It might have been thought that the development of

the ray-petals by drawing nourishment from certain other parts of the

flower had caused their abortion; but in some Compositae there is a

difference in the seeds of the outer and inner florets without any

difference in the corolla. Possibly, these several differences may be

connected with some difference in the flow of nutriment towards the

central and external flowers: we know, at least, that in irregular

flowers, those nearest to the axis are oftenest subject to peloria, and

become regular. I may add, as an instance of this, and of a striking

case of correlation, that I have recently observed in some garden

pelargoniums, that the central flower of the truss often loses the

patches of darker colour in the two upper petals; and that when this

occurs, the adherent nectary is quite aborted; when the colour is

absent from only one of the two upper petals, the nectary is only much

shortened.

With respect to the difference in the corolla of the central and

exterior flowers of a head or umbel, I do not feel at all sure that C.

C. Sprengel's idea that the ray-florets serve to attract insects, whose

agency is highly advantageous in the fertilisation of plants of these

two orders, is so far-fetched, as it may at first appear: and if it be

advantageous, natural selection may have come into play. But in regard

to the differences both in the internal and external structure of the

seeds, which are not always correlated with any differences in the

flowers, it seems impossible that they can be in any way advantageous

to the plant: yet in the Umbelliferae these differences are of such

apparent importance--the seeds being in some cases, according to Tausch,

orthospermous in the exterior flowers and coelospermous in the central

flowers,--that the elder De Candolle founded his main divisions of

the order on analogous differences. Hence we see that modifications of

structure, viewed by systematists as of high value, may be wholly due to

unknown laws of correlated growth, and without being, as far as we can

see, of the slightest service to the species.

We may often falsely attribute to correlation of growth, structures

which are common to whole groups of species, and which in truth are

simply due to inheritance; for an ancient progenitor may have acquired

through natural selection some one modification in structure, and, after

thousands of generations, some other and independent modification; and

these two modifications, having been transmitted to a whole group

of descendants with diverse habits, would naturally be thought to be

correlated in some necessary manner. So, again, I do not doubt that some

apparent correlations, occurring throughout whole orders, are entirely

due to the manner alone in which natural selection can act. For

instance, Alph. De Candolle has remarked that winged seeds are never

found in fruits which do not open: I should explain the rule by the fact

that seeds could not gradually become winged through natural selection,

except in fruits which opened; so that the individual plants producing

seeds which were a little better fitted to be wafted further, might get

an advantage over those producing seed less fitted for dispersal; and

this process could not possibly go on in fruit which did not open.

The elder Geoffroy and Goethe propounded, at about the same period,

their law of compensation or balancement of growth; or, as Goethe

expressed it, "in order to spend on one side, nature is forced to

economise on the other side." I think this holds true to a certain

extent with our domestic productions: if nourishment flows to one part

or organ in excess, it rarely flows, at least in excess, to another

part; thus it is difficult to get a cow to give much milk and to fatten

readily. The same varieties of the cabbage do not yield abundant and

nutritious foliage and a copious supply of oil-bearing seeds. When the

seeds in our fruits become atrophied, the fruit itself gains largely in

size and quality. In our poultry, a large tuft of feathers on the head

is generally accompanied by a diminished comb, and a large beard by

diminished wattles. With species in a state of nature it can hardly

be maintained that the law is of universal application; but many good

observers, more especially botanists, believe in its truth. I will

not, however, here give any instances, for I see hardly any way of

distinguishing between the effects, on the one hand, of a part being

largely developed through natural selection and another and adjoining

part being reduced by this same process or by disuse, and, on the other

hand, the actual withdrawal of nutriment from one part owing to the

excess of growth in another and adjoining part.

I suspect, also, that some of the cases of compensation which have been

advanced, and likewise some other facts, may be merged under a more

general principle, namely, that natural selection is continually

trying to economise in every part of the organisation. If under changed

conditions of life a structure before useful becomes less useful, any

diminution, however slight, in its development, will be seized on by

natural selection, for it will profit the individual not to have its

nutriment wasted in building up an useless structure. I can thus

only understand a fact with which I was much struck when examining

cirripedes, and of which many other instances could be given: namely,

that when a cirripede is parasitic within another and is thus protected,

it loses more or less completely its own shell or carapace. This is the

case with the male Ibla, and in a truly extraordinary manner with the

Proteolepas: for the carapace in all other cirripedes consists of

the three highly-important anterior segments of the head enormously

developed, and furnished with great nerves and muscles; but in the

parasitic and protected Proteolepas, the whole anterior part of the

head is reduced to the merest rudiment attached to the bases of the

prehensile antennae. Now the saving of a large and complex structure,

when rendered superfluous by the parasitic habits of the Proteolepas,

though effected by slow steps, would be a decided advantage to each

successive individual of the species; for in the struggle for life to

which every animal is exposed, each individual Proteolepas would have

a better chance of supporting itself, by less nutriment being wasted in

developing a structure now become useless.

Thus, as I believe, natural selection will always succeed in the long

run in reducing and saving every part of the organisation, as soon as it

is rendered superfluous, without by any means causing some other part

to be largely developed in a corresponding degree. And, conversely, that

natural selection may perfectly well succeed in largely developing any

organ, without requiring as a necessary compensation the reduction of

some adjoining part.

It seems to be a rule, as remarked by Is. Geoffroy St. Hilaire, both in

varieties and in species, that when any part or organ is repeated many

times in the structure of the same individual (as the vertebrae in

snakes, and the stamens in polyandrous flowers) the number is variable;

whereas the number of the same part or organ, when it occurs in lesser

numbers, is constant. The same author and some botanists have further

remarked that multiple parts are also very liable to variation in

structure. Inasmuch as this "vegetative repetition," to use Professor

Owen's expression, seems to be a sign of low organisation; the foregoing

remark seems connected with the very general opinion of naturalists,

that beings low in the scale of nature are more variable than those

which are higher. I presume that lowness in this case means that the

several parts of the organisation have been but little specialised

for particular functions; and as long as the same part has to perform

diversified work, we can perhaps see why it should remain variable, that

is, why natural selection should have preserved or rejected each little

deviation of form less carefully than when the part has to serve for one

special purpose alone. In the same way that a knife which has to cut

all sorts of things may be of almost any shape; whilst a tool for

some particular object had better be of some particular shape. Natural

selection, it should never be forgotten, can act on each part of each

being, solely through and for its advantage.

Rudimentary parts, it has been stated by some authors, and I believe

with truth, are apt to be highly variable. We shall have to recur to the

general subject of rudimentary and aborted organs; and I will here only

add that their variability seems to be owing to their uselessness, and

therefore to natural selection having no power to check deviations in

their structure. Thus rudimentary parts are left to the free play of the

various laws of growth, to the effects of long-continued disuse, and to

the tendency to reversion.

A PART DEVELOPED IN ANY SPECIES IN AN EXTRAORDINARY DEGREE OR MANNER,

IN COMPARISON WITH THE SAME PART IN ALLIED SPECIES, TENDS TO BE HIGHLY

VARIABLE.

Several years ago I was much struck with a remark, nearly to the above

effect, published by Mr. Waterhouse. I infer also from an observation

made by Professor Owen, with respect to the length of the arms of the

ourang-outang, that he has come to a nearly similar conclusion. It is

hopeless to attempt to convince any one of the truth of this proposition

without giving the long array of facts which I have collected, and which

cannot possibly be here introduced. I can only state my conviction that

it is a rule of high generality. I am aware of several causes of

error, but I hope that I have made due allowance for them. It should

be understood that the rule by no means applies to any part, however

unusually developed, unless it be unusually developed in comparison with

the same part in closely allied species. Thus, the bat's wing is a most

abnormal structure in the class mammalia; but the rule would not here

apply, because there is a whole group of bats having wings; it would

apply only if some one species of bat had its wings developed in some

remarkable manner in comparison with the other species of the same

genus. The rule applies very strongly in the case of secondary sexual

characters, when displayed in any unusual manner. The term, secondary

sexual characters, used by Hunter, applies to characters which are

attached to one sex, but are not directly connected with the act of

reproduction. The rule applies to males and females; but as females more

rarely offer remarkable secondary sexual characters, it applies more

rarely to them. The rule being so plainly applicable in the case of

secondary sexual characters, may be due to the great variability of

these characters, whether or not displayed in any unusual manner--of

which fact I think there can be little doubt. But that our rule is not

confined to secondary sexual characters is clearly shown in the case

of hermaphrodite cirripedes; and I may here add, that I particularly

attended to Mr. Waterhouse's remark, whilst investigating this Order,

and I am fully convinced that the rule almost invariably holds good

with cirripedes. I shall, in my future work, give a list of the more

remarkable cases; I will here only briefly give one, as it illustrates

the rule in its largest application. The opercular valves of sessile

cirripedes (rock barnacles) are, in every sense of the word, very

important structures, and they differ extremely little even in different

genera; but in the several species of one genus, Pyrgoma, these valves

present a marvellous amount of diversification: the homologous valves

in the different species being sometimes wholly unlike in shape; and the

amount of variation in the individuals of several of the species is so

great, that it is no exaggeration to state that the varieties differ

more from each other in the characters of these important valves than do

other species of distinct genera.

As birds within the same country vary in a remarkably small degree, I

have particularly attended to them, and the rule seems to me certainly

to hold good in this class. I cannot make out that it applies to plants,

and this would seriously have shaken my belief in its truth, had not the

great variability in plants made it particularly difficult to compare

their relative degrees of variability.

When we see any part or organ developed in a remarkable degree or manner

in any species, the fair presumption is that it is of high importance to

that species; nevertheless the part in this case is eminently liable to

variation. Why should this be so? On the view that each species has been

independently created, with all its parts as we now see them, I can see

no explanation. But on the view that groups of species have descended

from other species, and have been modified through natural selection, I

think we can obtain some light. In our domestic animals, if any part,

or the whole animal, be neglected and no selection be applied, that part

(for instance, the comb in the Dorking fowl) or the whole breed will

cease to have a nearly uniform character. The breed will then be said

to have degenerated. In rudimentary organs, and in those which have

been but little specialised for any particular purpose, and perhaps in

polymorphic groups, we see a nearly parallel natural case; for in such

cases natural selection either has not or cannot come into full play,

and thus the organisation is left in a fluctuating condition. But what

here more especially concerns us is, that in our domestic animals

those points, which at the present time are undergoing rapid change by

continued selection, are also eminently liable to variation. Look at the

breeds of the pigeon; see what a prodigious amount of difference there

is in the beak of the different tumblers, in the beak and wattle of

the different carriers, in the carriage and tail of our fantails, etc.,

these being the points now mainly attended to by English fanciers. Even

in the sub-breeds, as in the short-faced tumbler, it is notoriously

difficult to breed them nearly to perfection, and frequently individuals

are born which depart widely from the standard. There may be truly

said to be a constant struggle going on between, on the one hand, the

tendency to reversion to a less modified state, as well as an innate

tendency to further variability of all kinds, and, on the other hand,

the power of steady selection to keep the breed true. In the long run

selection gains the day, and we do not expect to fail so far as to breed

a bird as coarse as a common tumbler from a good short-faced strain. But

as long as selection is rapidly going on, there may always be expected

to be much variability in the structure undergoing modification. It

further deserves notice that these variable characters, produced by

man's selection, sometimes become attached, from causes quite unknown

to us, more to one sex than to the other, generally to the male sex, as

with the wattle of carriers and the enlarged crop of pouters.

Now let us turn to nature. When a part has been developed in an

extraordinary manner in any one species, compared with the other species

of the same genus, we may conclude that this part has undergone an

extraordinary amount of modification, since the period when the species

branched off from the common progenitor of the genus. This period will

seldom be remote in any extreme degree, as species very rarely endure

for more than one geological period. An extraordinary amount of

modification implies an unusually large and long-continued amount of

variability, which has continually been accumulated by natural

selection for the benefit of the species. But as the variability of

the extraordinarily-developed part or organ has been so great and

long-continued within a period not excessively remote, we might, as a

general rule, expect still to find more variability in such parts than

in other parts of the organisation, which have remained for a much

longer period nearly constant. And this, I am convinced, is the case.

That the struggle between natural selection on the one hand, and the

tendency to reversion and variability on the other hand, will in the

course of time cease; and that the most abnormally developed organs may

be made constant, I can see no reason to doubt. Hence when an organ,

however abnormal it may be, has been transmitted in approximately the

same condition to many modified descendants, as in the case of the wing

of the bat, it must have existed, according to my theory, for an

immense period in nearly the same state; and thus it comes to be no more

variable than any other structure. It is only in those cases in which

the modification has been comparatively recent and extraordinarily great

that we ought to find the GENERATIVE VARIABILITY, as it may be called,

still present in a high degree. For in this case the variability

will seldom as yet have been fixed by the continued selection of the

individuals varying in the required manner and degree, and by the

continued rejection of those tending to revert to a former and less

modified condition.

The principle included in these remarks may be extended. It is notorious

that specific characters are more variable than generic. To explain by a

simple example what is meant. If some species in a large genus of plants

had blue flowers and some had red, the colour would be only a specific

character, and no one would be surprised at one of the blue species

varying into red, or conversely; but if all the species had blue

flowers, the colour would become a generic character, and its variation

would be a more unusual circumstance. I have chosen this example because

an explanation is not in this case applicable, which most naturalists

would advance, namely, that specific characters are more variable

than generic, because they are taken from parts of less physiological

importance than those commonly used for classing genera. I believe this

explanation is partly, yet only indirectly, true; I shall, however, have

to return to this subject in our chapter on Classification. It would be

almost superfluous to adduce evidence in support of the above statement,

that specific characters are more variable than generic; but I have

repeatedly noticed in works on natural history, that when an author

has remarked with surprise that some IMPORTANT organ or part, which is

generally very constant throughout large groups of species, has DIFFERED

considerably in closely-allied species, that it has, also, been VARIABLE

in the individuals of some of the species. And this fact shows that a

character, which is generally of generic value, when it sinks in value

and becomes only of specific value, often becomes variable, though its

physiological importance may remain the same. Something of the same kind

applies to monstrosities: at least Is. Geoffroy St. Hilaire seems to

entertain no doubt, that the more an organ normally differs in

the different species of the same group, the more subject it is to

individual anomalies.

On the ordinary view of each species having been independently created,

why should that part of the structure, which differs from the same

part in other independently-created species of the same genus, be

more variable than those parts which are closely alike in the several

species? I do not see that any explanation can be given. But on the

view of species being only strongly marked and fixed varieties, we might

surely expect to find them still often continuing to vary in those parts

of their structure which have varied within a moderately recent period,

and which have thus come to differ. Or to state the case in another

manner:--the points in which all the species of a genus resemble each

other, and in which they differ from the species of some other genus,

are called generic characters; and these characters in common I

attribute to inheritance from a common progenitor, for it can rarely

have happened that natural selection will have modified several species,

fitted to more or less widely-different habits, in exactly the same

manner: and as these so-called generic characters have been inherited

from a remote period, since that period when the species first branched

off from their common progenitor, and subsequently have not varied or

come to differ in any degree, or only in a slight degree, it is not

probable that they should vary at the present day. On the other hand,

the points in which species differ from other species of the same genus,

are called specific characters; and as these specific characters have

varied and come to differ within the period of the branching off of the

species from a common progenitor, it is probable that they should still

often be in some degree variable,--at least more variable than those

parts of the organisation which have for a very long period remained

constant.

In connexion with the present subject, I will make only two other

remarks. I think it will be admitted, without my entering on details,

that secondary sexual characters are very variable; I think it also will

be admitted that species of the same group differ from each other more

widely in their secondary sexual characters, than in other parts of

their organisation; compare, for instance, the amount of difference

between the males of gallinaceous birds, in which secondary sexual

characters are strongly displayed, with the amount of difference between

their females; and the truth of this proposition will be granted. The

cause of the original variability of secondary sexual characters is

not manifest; but we can see why these characters should not have been

rendered as constant and uniform as other parts of the organisation; for

secondary sexual characters have been accumulated by sexual selection,

which is less rigid in its action than ordinary selection, as it does

not entail death, but only gives fewer offspring to the less favoured

males. Whatever the cause may be of the variability of secondary sexual

characters, as they are highly variable, sexual selection will have had

a wide scope for action, and may thus readily have succeeded in giving

to the species of the same group a greater amount of difference in their

sexual characters, than in other parts of their structure.

It is a remarkable fact, that the secondary sexual differences between

the two sexes of the same species are generally displayed in the very

same parts of the organisation in which the different species of

the same genus differ from each other. Of this fact I will give in

illustration two instances, the first which happen to stand on my list;

and as the differences in these cases are of a very unusual nature,

the relation can hardly be accidental. The same number of joints in the

tarsi is a character generally common to very large groups of beetles,

but in the Engidae, as Westwood has remarked, the number varies greatly;

and the number likewise differs in the two sexes of the same species:

again in fossorial hymenoptera, the manner of neuration of the wings is

a character of the highest importance, because common to large groups;

but in certain genera the neuration differs in the different species,

and likewise in the two sexes of the same species. This relation has a

clear meaning on my view of the subject: I look at all the species

of the same genus as having as certainly descended from the same

progenitor, as have the two sexes of any one of the species.

Consequently, whatever part of the structure of the common progenitor,

or of its early descendants, became variable; variations of this part

would it is highly probable, be taken advantage of by natural and sexual

selection, in order to fit the several species to their several places

in the economy of nature, and likewise to fit the two sexes of the same

species to each other, or to fit the males and females to different

habits of life, or the males to struggle with other males for the

possession of the females.

Finally, then, I conclude that the greater variability of specific

characters, or those which distinguish species from species, than of

generic characters, or those which the species possess in common;--that

the frequent extreme variability of any part which is developed in a

species in an extraordinary manner in comparison with the same part

in its congeners; and the not great degree of variability in a part,

however extraordinarily it may be developed, if it be common to a

whole group of species;--that the great variability of secondary sexual

characters, and the great amount of difference in these same characters

between closely allied species;--that secondary sexual and ordinary

specific differences are generally displayed in the same parts of the

organisation,--are all principles closely connected together. All being

mainly due to the species of the same group having descended from a

common progenitor, from whom they have inherited much in common,--to

parts which have recently and largely varied being more likely still

to go on varying than parts which have long been inherited and have not

varied,--to natural selection having more or less completely, according

to the lapse of time, overmastered the tendency to reversion and to

further variability,--to sexual selection being less rigid than ordinary

selection,--and to variations in the same parts having been accumulated

by natural and sexual selection, and thus adapted for secondary sexual,

and for ordinary specific purposes.

DISTINCT SPECIES PRESENT ANALOGOUS VARIATIONS; AND A VARIETY OF ONE

SPECIES OFTEN ASSUMES SOME OF THE CHARACTERS OF AN ALLIED SPECIES, OR

REVERTS TO SOME OF THE CHARACTERS OF AN EARLY PROGENITOR.

These propositions will be most readily understood by looking to our

domestic races. The most distinct breeds of pigeons, in countries most

widely apart, present sub-varieties with reversed feathers on the head

and feathers on the feet,--characters not possessed by the aboriginal

rock-pigeon; these then are analogous variations in two or more distinct

races. The frequent presence of fourteen or even sixteen tail-feathers

in the pouter, may be considered as a variation representing the normal

structure of another race, the fantail. I presume that no one will doubt

that all such analogous variations are due to the several races of the

pigeon having inherited from a common parent the same constitution and

tendency to variation, when acted on by similar unknown influences.

In the vegetable kingdom we have a case of analogous variation, in the

enlarged stems, or roots as commonly called, of the Swedish turnip and

Ruta baga, plants which several botanists rank as varieties produced by

cultivation from a common parent: if this be not so, the case will then

be one of analogous variation in two so-called distinct species; and to

these a third may be added, namely, the common turnip. According to

the ordinary view of each species having been independently created, we

should have to attribute this similarity in the enlarged stems of these

three plants, not to the vera causa of community of descent, and a

consequent tendency to vary in a like manner, but to three separate yet

closely related acts of creation.

With pigeons, however, we have another case, namely, the occasional

appearance in all the breeds, of slaty-blue birds with two black bars

on the wings, a white rump, a bar at the end of the tail, with the outer

feathers externally edged near their bases with white. As all these

marks are characteristic of the parent rock-pigeon, I presume that no

one will doubt that this is a case of reversion, and not of a new yet

analogous variation appearing in the several breeds. We may I think

confidently come to this conclusion, because, as we have seen, these

coloured marks are eminently liable to appear in the crossed offspring

of two distinct and differently coloured breeds; and in this case there

is nothing in the external conditions of life to cause the reappearance

of the slaty-blue, with the several marks, beyond the influence of the

mere act of crossing on the laws of inheritance.

No doubt it is a very surprising fact that characters should reappear

after having been lost for many, perhaps for hundreds of generations.

But when a breed has been crossed only once by some other breed, the

offspring occasionally show a tendency to revert in character to the

foreign breed for many generations--some say, for a dozen or even a

score of generations. After twelve generations, the proportion of blood,

to use a common expression, of any one ancestor, is only 1 in 2048; and

yet, as we see, it is generally believed that a tendency to reversion

is retained by this very small proportion of foreign blood. In a breed

which has not been crossed, but in which BOTH parents have lost some

character which their progenitor possessed, the tendency, whether strong

or weak, to reproduce the lost character might be, as was formerly

remarked, for all that we can see to the contrary, transmitted for

almost any number of generations. When a character which has been lost

in a breed, reappears after a great number of generations, the most

probable hypothesis is, not that the offspring suddenly takes after an

ancestor some hundred generations distant, but that in each successive

generation there has been a tendency to reproduce the character in

question, which at last, under unknown favourable conditions, gains an

ascendancy. For instance, it is probable that in each generation of the

barb-pigeon, which produces most rarely a blue and black-barred bird,

there has been a tendency in each generation in the plumage to assume

this colour. This view is hypothetical, but could be supported by some

facts; and I can see no more abstract improbability in a tendency

to produce any character being inherited for an endless number of

generations, than in quite useless or rudimentary organs being, as we

all know them to be, thus inherited. Indeed, we may sometimes observe

a mere tendency to produce a rudiment inherited: for instance, in the

common snapdragon (Antirrhinum) a rudiment of a fifth stamen so often

appears, that this plant must have an inherited tendency to produce it.

As all the species of the same genus are supposed, on my theory, to have

descended from a common parent, it might be expected that they would

occasionally vary in an analogous manner; so that a variety of one

species would resemble in some of its characters another species; this

other species being on my view only a well-marked and permanent variety.

But characters thus gained would probably be of an unimportant nature,

for the presence of all important characters will be governed by natural

selection, in accordance with the diverse habits of the species, and

will not be left to the mutual action of the conditions of life and of

a similar inherited constitution. It might further be expected that the

species of the same genus would occasionally exhibit reversions to lost

ancestral characters. As, however, we never know the exact character

of the common ancestor of a group, we could not distinguish these two

cases: if, for instance, we did not know that the rock-pigeon was not

feather-footed or turn-crowned, we could not have told, whether these

characters in our domestic breeds were reversions or only analogous

variations; but we might have inferred that the blueness was a case of

reversion, from the number of the markings, which are correlated with

the blue tint, and which it does not appear probable would all appear

together from simple variation. More especially we might have inferred

this, from the blue colour and marks so often appearing when distinct

breeds of diverse colours are crossed. Hence, though under nature

it must generally be left doubtful, what cases are reversions to an

anciently existing character, and what are new but analogous variations,

yet we ought, on my theory, sometimes to find the varying offspring of

a species assuming characters (either from reversion or from analogous

variation) which already occur in some other members of the same group.

And this undoubtedly is the case in nature.

A considerable part of the difficulty in recognising a variable species

in our systematic works, is due to its varieties mocking, as it were,

some of the other species of the same genus. A considerable catalogue,

also, could be given of forms intermediate between two other forms,

which themselves must be doubtfully ranked as either varieties or

species; and this shows, unless all these forms be considered as

independently created species, that the one in varying has assumed some

of the characters of the other, so as to produce the intermediate form.

But the best evidence is afforded by parts or organs of an important and

uniform nature occasionally varying so as to acquire, in some degree,

the character of the same part or organ in an allied species. I have

collected a long list of such cases; but here, as before, I lie under

a great disadvantage in not being able to give them. I can only repeat

that such cases certainly do occur, and seem to me very remarkable.

I will, however, give one curious and complex case, not indeed as

affecting any important character, but from occurring in several species

of the same genus, partly under domestication and partly under nature.

It is a case apparently of reversion. The ass not rarely has very

distinct transverse bars on its legs, like those on the legs of a zebra:

it has been asserted that these are plainest in the foal, and from

inquiries which I have made, I believe this to be true. It has also

been asserted that the stripe on each shoulder is sometimes double.

The shoulder stripe is certainly very variable in length and outline. A

white ass, but NOT an albino, has been described without either spinal

or shoulder-stripe; and these stripes are sometimes very obscure, or

actually quite lost, in dark-coloured asses. The koulan of Pallas is

said to have been seen with a double shoulder-stripe. The hemionus has

no shoulder-stripe; but traces of it, as stated by Mr. Blyth and others,

occasionally appear: and I have been informed by Colonel Poole that the

foals of this species are generally striped on the legs, and faintly on

the shoulder. The quagga, though so plainly barred like a zebra over the

body, is without bars on the legs; but Dr. Gray has figured one specimen

with very distinct zebra-like bars on the hocks.

With respect to the horse, I have collected cases in England of the

spinal stripe in horses of the most distinct breeds, and of ALL colours;

transverse bars on the legs are not rare in duns, mouse-duns, and in one

instance in a chestnut: a faint shoulder-stripe may sometimes be seen

in duns, and I have seen a trace in a bay horse. My son made a careful

examination and sketch for me of a dun Belgian cart-horse with a double

stripe on each shoulder and with leg-stripes; and a man, whom I can

implicitly trust, has examined for me a small dun Welch pony with THREE

short parallel stripes on each shoulder.

In the north-west part of India the Kattywar breed of horses is so

generally striped, that, as I hear from Colonel Poole, who examined

the breed for the Indian Government, a horse without stripes is not

considered as purely-bred. The spine is always striped; the legs are

generally barred; and the shoulder-stripe, which is sometimes double

and sometimes treble, is common; the side of the face, moreover, is

sometimes striped. The stripes are plainest in the foal; and sometimes

quite disappear in old horses. Colonel Poole has seen both gray and

bay Kattywar horses striped when first foaled. I have, also, reason to

suspect, from information given me by Mr. W. W. Edwards, that with the

English race-horse the spinal stripe is much commoner in the foal than

in the full-grown animal. Without here entering on further details, I

may state that I have collected cases of leg and shoulder stripes in

horses of very different breeds, in various countries from Britain to

Eastern China; and from Norway in the north to the Malay Archipelago in

the south. In all parts of the world these stripes occur far oftenest

in duns and mouse-duns; by the term dun a large range of colour is

included, from one between brown and black to a close approach to

cream-colour.

I am aware that Colonel Hamilton Smith, who has written on this subject,

believes that the several breeds of the horse have descended from

several aboriginal species--one of which, the dun, was striped; and that

the above-described appearances are all due to ancient crosses with the

dun stock. But I am not at all satisfied with this theory, and should be

loth to apply it to breeds so distinct as the heavy Belgian cart-horse,

Welch ponies, cobs, the lanky Kattywar race, etc., inhabiting the most

distant parts of the world.

Now let us turn to the effects of crossing the several species of the

horse-genus. Rollin asserts, that the common mule from the ass and horse

is particularly apt to have bars on its legs. I once saw a mule with its

legs so much striped that any one at first would have thought that it

must have been the product of a zebra; and Mr. W. C. Martin, in his

excellent treatise on the horse, has given a figure of a similar mule.

In four coloured drawings, which I have seen, of hybrids between the ass

and zebra, the legs were much more plainly barred than the rest of the

body; and in one of them there was a double shoulder-stripe. In Lord

Moreton's famous hybrid from a chestnut mare and male quagga, the

hybrid, and even the pure offspring subsequently produced from the mare

by a black Arabian sire, were much more plainly barred across the

legs than is even the pure quagga. Lastly, and this is another most

remarkable case, a hybrid has been figured by Dr. Gray (and he informs

me that he knows of a second case) from the ass and the hemionus; and

this hybrid, though the ass seldom has stripes on its legs and the

hemionus has none and has not even a shoulder-stripe, nevertheless had

all four legs barred, and had three short shoulder-stripes, like those

on the dun Welch pony, and even had some zebra-like stripes on the sides

of its face. With respect to this last fact, I was so convinced that not

even a stripe of colour appears from what would commonly be called an

accident, that I was led solely from the occurrence of the face-stripes

on this hybrid from the ass and hemionus, to ask Colonel Poole whether

such face-stripes ever occur in the eminently striped Kattywar breed of

horses, and was, as we have seen, answered in the affirmative.

What now are we to say to these several facts? We see several very

distinct species of the horse-genus becoming, by simple variation,

striped on the legs like a zebra, or striped on the shoulders like

an ass. In the horse we see this tendency strong whenever a dun tint

appears--a tint which approaches to that of the general colouring of

the other species of the genus. The appearance of the stripes is not

accompanied by any change of form or by any other new character. We see

this tendency to become striped most strongly displayed in hybrids from

between several of the most distinct species. Now observe the case

of the several breeds of pigeons: they are descended from a pigeon

(including two or three sub-species or geographical races) of a bluish

colour, with certain bars and other marks; and when any breed assumes

by simple variation a bluish tint, these bars and other marks invariably

reappear; but without any other change of form or character. When the

oldest and truest breeds of various colours are crossed, we see a

strong tendency for the blue tint and bars and marks to reappear in the

mongrels. I have stated that the most probable hypothesis to account

for the reappearance of very ancient characters, is--that there is

a TENDENCY in the young of each successive generation to produce the

long-lost character, and that this tendency, from unknown causes,

sometimes prevails. And we have just seen that in several species of the

horse-genus the stripes are either plainer or appear more commonly in

the young than in the old. Call the breeds of pigeons, some of which

have bred true for centuries, species; and how exactly parallel is the

case with that of the species of the horse-genus! For myself, I venture

confidently to look back thousands on thousands of generations, and

I see an animal striped like a zebra, but perhaps otherwise very

differently constructed, the common parent of our domestic horse,

whether or not it be descended from one or more wild stocks, of the ass,

the hemionus, quagga, and zebra.

He who believes that each equine species was independently created,

will, I presume, assert that each species has been created with a

tendency to vary, both under nature and under domestication, in this

particular manner, so as often to become striped like other species of

the genus; and that each has been created with a strong tendency,

when crossed with species inhabiting distant quarters of the world, to

produce hybrids resembling in their stripes, not their own parents, but

other species of the genus. To admit this view is, as it seems to me, to

reject a real for an unreal, or at least for an unknown, cause. It makes

the works of God a mere mockery and deception; I would almost as soon

believe with the old and ignorant cosmogonists, that fossil shells had

never lived, but had been created in stone so as to mock the shells now

living on the sea-shore.

SUMMARY.

Our ignorance of the laws of variation is profound. Not in one case out

of a hundred can we pretend to assign any reason why this or that part

differs, more or less, from the same part in the parents. But whenever

we have the means of instituting a comparison, the same laws appear to

have acted in producing the lesser differences between varieties of the

same species, and the greater differences between species of the same

genus. The external conditions of life, as climate and food, etc.,

seem to have induced some slight modifications. Habit in producing

constitutional differences, and use in strengthening, and disuse in

weakening and diminishing organs, seem to have been more potent in their

effects. Homologous parts tend to vary in the same way, and homologous

parts tend to cohere. Modifications in hard parts and in external parts

sometimes affect softer and internal parts. When one part is largely

developed, perhaps it tends to draw nourishment from the adjoining

parts; and every part of the structure which can be saved without

detriment to the individual, will be saved. Changes of structure at an

early age will generally affect parts subsequently developed; and there

are very many other correlations of growth, the nature of which we are

utterly unable to understand. Multiple parts are variable in number and

in structure, perhaps arising from such parts not having been closely

specialised to any particular function, so that their modifications have

not been closely checked by natural selection. It is probably from

this same cause that organic beings low in the scale of nature are

more variable than those which have their whole organisation more

specialised, and are higher in the scale. Rudimentary organs, from being

useless, will be disregarded by natural selection, and hence probably

are variable. Specific characters--that is, the characters which have

come to differ since the several species of the same genus branched

off from a common parent--are more variable than generic characters, or

those which have long been inherited, and have not differed within

this same period. In these remarks we have referred to special parts or

organs being still variable, because they have recently varied and thus

come to differ; but we have also seen in the second Chapter that the

same principle applies to the whole individual; for in a district where

many species of any genus are found--that is, where there has been much

former variation and differentiation, or where the manufactory of new

specific forms has been actively at work--there, on an average, we now

find most varieties or incipient species. Secondary sexual characters

are highly variable, and such characters differ much in the species of

the same group. Variability in the same parts of the organisation has

generally been taken advantage of in giving secondary sexual differences

to the sexes of the same species, and specific differences to the

several species of the same genus. Any part or organ developed to an

extraordinary size or in an extraordinary manner, in comparison with

the same part or organ in the allied species, must have gone through an

extraordinary amount of modification since the genus arose; and thus we

can understand why it should often still be variable in a much higher

degree than other parts; for variation is a long-continued and slow

process, and natural selection will in such cases not as yet have had

time to overcome the tendency to further variability and to

reversion to a less modified state. But when a species with any

extraordinarily-developed organ has become the parent of many modified

descendants--which on my view must be a very slow process, requiring

a long lapse of time--in this case, natural selection may readily

have succeeded in giving a fixed character to the organ, in however

extraordinary a manner it may be developed. Species inheriting nearly

the same constitution from a common parent and exposed to similar

influences will naturally tend to present analogous variations, and

these same species may occasionally revert to some of the characters of

their ancient progenitors. Although new and important modifications may

not arise from reversion and analogous variation, such modifications

will add to the beautiful and harmonious diversity of nature.

Whatever the cause may be of each slight difference in the offspring

from their parents--and a cause for each must exist--it is the steady

accumulation, through natural selection, of such differences, when

beneficial to the individual, that gives rise to all the more important

modifications of structure, by which the innumerable beings on the face

of this earth are enabled to struggle with each other, and the best

adapted to survive.

6. DIFFICULTIES ON THEORY.

Difficulties on the theory of descent with modification. Transitions.

Absence or rarity of transitional varieties. Transitions in habits of

life. Diversified habits in the same species. Species with habits widely

different from those of their allies. Organs of extreme perfection.

Means of transition. Cases of difficulty. Natura non facit saltum.

Organs of small importance. Organs not in all cases absolutely perfect.

The law of Unity of Type and of the Conditions of Existence embraced by

the theory of Natural Selection.

Long before having arrived at this part of my work, a crowd of

difficulties will have occurred to the reader. Some of them are so grave

that to this day I can never reflect on them without being staggered;

but, to the best of my judgment, the greater number are only apparent,

and those that are real are not, I think, fatal to my theory.

These difficulties and objections may be classed under the following

heads:--

Firstly, why, if species have descended from other species by insensibly

fine gradations, do we not everywhere see innumerable transitional

forms? Why is not all nature in confusion instead of the species being,

as we see them, well defined?

Secondly, is it possible that an animal having, for instance,

the structure and habits of a bat, could have been formed by the

modification of some animal with wholly different habits? Can we

believe that natural selection could produce, on the one hand, organs

of trifling importance, such as the tail of a giraffe, which serves as a

fly-flapper, and, on the other hand, organs of such wonderful structure,

as the eye, of which we hardly as yet fully understand the inimitable

perfection?

Thirdly, can instincts be acquired and modified through natural

selection? What shall we say to so marvellous an instinct as that which

leads the bee to make cells, which have practically anticipated the

discoveries of profound mathematicians?

Fourthly, how can we account for species, when crossed, being sterile

and producing sterile offspring, whereas, when varieties are crossed,

their fertility is unimpaired?

The two first heads shall be here discussed--Instinct and Hybridism in

separate chapters.

ON THE ABSENCE OR RARITY OF TRANSITIONAL VARIETIES.

As natural selection acts solely by the preservation of profitable

modifications, each new form will tend in a fully-stocked country to

take the place of, and finally to exterminate, its own less improved

parent or other less-favoured forms with which it comes into

competition. Thus extinction and natural selection will, as we have

seen, go hand in hand. Hence, if we look at each species as descended

from some other unknown form, both the parent and all the transitional

varieties will generally have been exterminated by the very process of

formation and perfection of the new form.

But, as by this theory innumerable transitional forms must have existed,

why do we not find them embedded in countless numbers in the crust of

the earth? It will be much more convenient to discuss this question in

the chapter on the Imperfection of the geological record; and I will

here only state that I believe the answer mainly lies in the record

being incomparably less perfect than is generally supposed; the

imperfection of the record being chiefly due to organic beings not

inhabiting profound depths of the sea, and to their remains being

embedded and preserved to a future age only in masses of sediment

sufficiently thick and extensive to withstand an enormous amount of

future degradation; and such fossiliferous masses can be accumulated

only where much sediment is deposited on the shallow bed of the sea,

whilst it slowly subsides. These contingencies will concur only rarely,

and after enormously long intervals. Whilst the bed of the sea

is stationary or is rising, or when very little sediment is being

deposited, there will be blanks in our geological history. The crust of

the earth is a vast museum; but the natural collections have been made

only at intervals of time immensely remote.

But it may be urged that when several closely-allied species inhabit

the same territory we surely ought to find at the present time many

transitional forms. Let us take a simple case: in travelling from north

to south over a continent, we generally meet at successive intervals

with closely allied or representative species, evidently filling nearly

the same place in the natural economy of the land. These representative

species often meet and interlock; and as the one becomes rarer and

rarer, the other becomes more and more frequent, till the one replaces

the other. But if we compare these species where they intermingle, they

are generally as absolutely distinct from each other in every detail of

structure as are specimens taken from the metropolis inhabited by each.

By my theory these allied species have descended from a common parent;

and during the process of modification, each has become adapted to

the conditions of life of its own region, and has supplanted and

exterminated its original parent and all the transitional varieties

between its past and present states. Hence we ought not to expect at

the present time to meet with numerous transitional varieties in each

region, though they must have existed there, and may be embedded

there in a fossil condition. But in the intermediate region, having

intermediate conditions of life, why do we not now find closely-linking

intermediate varieties? This difficulty for a long time quite confounded

me. But I think it can be in large part explained.

In the first place we should be extremely cautious in inferring, because

an area is now continuous, that it has been continuous during a long

period. Geology would lead us to believe that almost every continent has

been broken up into islands even during the later tertiary periods;

and in such islands distinct species might have been separately formed

without the possibility of intermediate varieties existing in the

intermediate zones. By changes in the form of the land and of climate,

marine areas now continuous must often have existed within recent times

in a far less continuous and uniform condition than at present. But I

will pass over this way of escaping from the difficulty; for I believe

that many perfectly defined species have been formed on strictly

continuous areas; though I do not doubt that the formerly broken

condition of areas now continuous has played an important part in the

formation of new species, more especially with freely-crossing and

wandering animals.

In looking at species as they are now distributed over a wide area,

we generally find them tolerably numerous over a large territory, then

becoming somewhat abruptly rarer and rarer on the confines, and finally

disappearing. Hence the neutral territory between two representative

species is generally narrow in comparison with the territory proper to

each. We see the same fact in ascending mountains, and sometimes it

is quite remarkable how abruptly, as Alph. De Candolle has observed,

a common alpine species disappears. The same fact has been noticed by

Forbes in sounding the depths of the sea with the dredge. To those who

look at climate and the physical conditions of life as the all-important

elements of distribution, these facts ought to cause surprise, as

climate and height or depth graduate away insensibly. But when we

bear in mind that almost every species, even in its metropolis, would

increase immensely in numbers, were it not for other competing species;

that nearly all either prey on or serve as prey for others; in short,

that each organic being is either directly or indirectly related in

the most important manner to other organic beings, we must see that the

range of the inhabitants of any country by no means exclusively depends

on insensibly changing physical conditions, but in large part on the

presence of other species, on which it depends, or by which it is

destroyed, or with which it comes into competition; and as these species

are already defined objects (however they may have become so), not

blending one into another by insensible gradations, the range of any one

species, depending as it does on the range of others, will tend to be

sharply defined. Moreover, each species on the confines of its range,

where it exists in lessened numbers, will, during fluctuations in the

number of its enemies or of its prey, or in the seasons, be extremely

liable to utter extermination; and thus its geographical range will come

to be still more sharply defined.

If I am right in believing that allied or representative species, when

inhabiting a continuous area, are generally so distributed that each

has a wide range, with a comparatively narrow neutral territory between

them, in which they become rather suddenly rarer and rarer; then, as

varieties do not essentially differ from species, the same rule will

probably apply to both; and if we in imagination adapt a varying species

to a very large area, we shall have to adapt two varieties to two

large areas, and a third variety to a narrow intermediate zone. The

intermediate variety, consequently, will exist in lesser numbers from

inhabiting a narrow and lesser area; and practically, as far as I can

make out, this rule holds good with varieties in a state of nature. I

have met with striking instances of the rule in the case of varieties

intermediate between well-marked varieties in the genus Balanus. And it

would appear from information given me by Mr. Watson, Dr. Asa Gray, and

Mr. Wollaston, that generally when varieties intermediate between two

other forms occur, they are much rarer numerically than the forms which

they connect. Now, if we may trust these facts and inferences, and

therefore conclude that varieties linking two other varieties together

have generally existed in lesser numbers than the forms which they

connect, then, I think, we can understand why intermediate varieties

should not endure for very long periods;--why as a general rule they

should be exterminated and disappear, sooner than the forms which they

originally linked together.

For any form existing in lesser numbers would, as already remarked,

run a greater chance of being exterminated than one existing in large

numbers; and in this particular case the intermediate form would be

eminently liable to the inroads of closely allied forms existing on both

sides of it. But a far more important consideration, as I believe, is

that, during the process of further modification, by which two varieties

are supposed on my theory to be converted and perfected into two

distinct species, the two which exist in larger numbers from inhabiting

larger areas, will have a great advantage over the intermediate variety,

which exists in smaller numbers in a narrow and intermediate zone.

For forms existing in larger numbers will always have a better chance,

within any given period, of presenting further favourable variations for

natural selection to seize on, than will the rarer forms which exist in

lesser numbers. Hence, the more common forms, in the race for life, will

tend to beat and supplant the less common forms, for these will be

more slowly modified and improved. It is the same principle which, as

I believe, accounts for the common species in each country, as shown

in the second chapter, presenting on an average a greater number of

well-marked varieties than do the rarer species. I may illustrate what I

mean by supposing three varieties of sheep to be kept, one adapted to an

extensive mountainous region; a second to a comparatively narrow, hilly

tract; and a third to wide plains at the base; and that the inhabitants

are all trying with equal steadiness and skill to improve their stocks

by selection; the chances in this case will be strongly in favour of the

great holders on the mountains or on the plains improving their breeds

more quickly than the small holders on the intermediate narrow, hilly

tract; and consequently the improved mountain or plain breed will soon

take the place of the less improved hill breed; and thus the two breeds,

which originally existed in greater numbers, will come into close

contact with each other, without the interposition of the supplanted,

intermediate hill-variety.

To sum up, I believe that species come to be tolerably well-defined

objects, and do not at any one period present an inextricable chaos of

varying and intermediate links: firstly, because new varieties are

very slowly formed, for variation is a very slow process, and natural

selection can do nothing until favourable variations chance to occur,

and until a place in the natural polity of the country can be better

filled by some modification of some one or more of its inhabitants.

And such new places will depend on slow changes of climate, or on the

occasional immigration of new inhabitants, and, probably, in a still

more important degree, on some of the old inhabitants becoming slowly

modified, with the new forms thus produced and the old ones acting and

reacting on each other. So that, in any one region and at any one time,

we ought only to see a few species presenting slight modifications of

structure in some degree permanent; and this assuredly we do see.

Secondly, areas now continuous must often have existed within the

recent period in isolated portions, in which many forms, more especially

amongst the classes which unite for each birth and wander much, may have

separately been rendered sufficiently distinct to rank as representative

species. In this case, intermediate varieties between the several

representative species and their common parent, must formerly have

existed in each broken portion of the land, but these links will

have been supplanted and exterminated during the process of natural

selection, so that they will no longer exist in a living state.

Thirdly, when two or more varieties have been formed in different

portions of a strictly continuous area, intermediate varieties will, it

is probable, at first have been formed in the intermediate zones, but

they will generally have had a short duration. For these intermediate

varieties will, from reasons already assigned (namely from what we know

of the actual distribution of closely allied or representative species,

and likewise of acknowledged varieties), exist in the intermediate zones

in lesser numbers than the varieties which they tend to connect. From

this cause alone the intermediate varieties will be liable to accidental

extermination; and during the process of further modification through

natural selection, they will almost certainly be beaten and supplanted

by the forms which they connect; for these from existing in greater

numbers will, in the aggregate, present more variation, and thus be

further improved through natural selection and gain further advantages.

Lastly, looking not to any one time, but to all time, if my theory be

true, numberless intermediate varieties, linking most closely all the

species of the same group together, must assuredly have existed; but the

very process of natural selection constantly tends, as has been so often

remarked, to exterminate the parent forms and the intermediate links.

Consequently evidence of their former existence could be found only

amongst fossil remains, which are preserved, as we shall in a future

chapter attempt to show, in an extremely imperfect and intermittent

record.

ON THE ORIGIN AND TRANSITIONS OF ORGANIC BEINGS WITH PECULIAR HABITS AND

STRUCTURE.

It has been asked by the opponents of such views as I hold, how, for

instance, a land carnivorous animal could have been converted into one

with aquatic habits; for how could the animal in its transitional state

have subsisted? It would be easy to show that within the same group

carnivorous animals exist having every intermediate grade between

truly aquatic and strictly terrestrial habits; and as each exists by a

struggle for life, it is clear that each is well adapted in its habits

to its place in nature. Look at the Mustela vison of North America,

which has webbed feet and which resembles an otter in its fur, short

legs, and form of tail; during summer this animal dives for and preys on

fish, but during the long winter it leaves the frozen waters, and preys

like other polecats on mice and land animals. If a different case had

been taken, and it had been asked how an insectivorous quadruped could

possibly have been converted into a flying bat, the question would have

been far more difficult, and I could have given no answer. Yet I think

such difficulties have very little weight.

Here, as on other occasions, I lie under a heavy disadvantage, for out

of the many striking cases which I have collected, I can give only one

or two instances of transitional habits and structures in closely allied

species of the same genus; and of diversified habits, either constant

or occasional, in the same species. And it seems to me that nothing less

than a long list of such cases is sufficient to lessen the difficulty in

any particular case like that of the bat.

Look at the family of squirrels; here we have the finest gradation from

animals with their tails only slightly flattened, and from others, as

Sir J. Richardson has remarked, with the posterior part of their bodies

rather wide and with the skin on their flanks rather full, to the

so-called flying squirrels; and flying squirrels have their limbs and

even the base of the tail united by a broad expanse of skin, which

serves as a parachute and allows them to glide through the air to

an astonishing distance from tree to tree. We cannot doubt that each

structure is of use to each kind of squirrel in its own country, by

enabling it to escape birds or beasts of prey, or to collect food more

quickly, or, as there is reason to believe, by lessening the danger

from occasional falls. But it does not follow from this fact that the

structure of each squirrel is the best that it is possible to conceive

under all natural conditions. Let the climate and vegetation change,

let other competing rodents or new beasts of prey immigrate, or old ones

become modified, and all analogy would lead us to believe that some at

least of the squirrels would decrease in numbers or become exterminated,

unless they also became modified and improved in structure in a

corresponding manner. Therefore, I can see no difficulty, more

especially under changing conditions of life, in the continued

preservation of individuals with fuller and fuller flank-membranes,

each modification being useful, each being propagated, until by the

accumulated effects of this process of natural selection, a perfect

so-called flying squirrel was produced.

Now look at the Galeopithecus or flying lemur, which formerly was

falsely ranked amongst bats. It has an extremely wide flank-membrane,

stretching from the corners of the jaw to the tail, and including the

limbs and the elongated fingers: the flank membrane is, also, furnished

with an extensor muscle. Although no graduated links of structure,

fitted for gliding through the air, now connect the Galeopithecus with

the other Lemuridae, yet I can see no difficulty in supposing that such

links formerly existed, and that each had been formed by the same steps

as in the case of the less perfectly gliding squirrels; and that each

grade of structure had been useful to its possessor. Nor can I see

any insuperable difficulty in further believing it possible that the

membrane-connected fingers and fore-arm of the Galeopithecus might be

greatly lengthened by natural selection; and this, as far as the organs

of flight are concerned, would convert it into a bat. In bats which have

the wing-membrane extended from the top of the shoulder to the

tail, including the hind-legs, we perhaps see traces of an apparatus

originally constructed for gliding through the air rather than for

flight.

If about a dozen genera of birds had become extinct or were unknown, who

would have ventured to have surmised that birds might have existed

which used their wings solely as flappers, like the logger-headed duck

(Micropterus of Eyton); as fins in the water and front legs on the land,

like the penguin; as sails, like the ostrich; and functionally for no

purpose, like the Apteryx. Yet the structure of each of these birds is

good for it, under the conditions of life to which it is exposed, for

each has to live by a struggle; but it is not necessarily the best

possible under all possible conditions. It must not be inferred from

these remarks that any of the grades of wing-structure here alluded to,

which perhaps may all have resulted from disuse, indicate the natural

steps by which birds have acquired their perfect power of flight; but

they serve, at least, to show what diversified means of transition are

possible.

Seeing that a few members of such water-breathing classes as the

Crustacea and Mollusca are adapted to live on the land, and seeing that

we have flying birds and mammals, flying insects of the most diversified

types, and formerly had flying reptiles, it is conceivable that

flying-fish, which now glide far through the air, slightly rising and

turning by the aid of their fluttering fins, might have been modified

into perfectly winged animals. If this had been effected, who would

have ever imagined that in an early transitional state they had been

inhabitants of the open ocean, and had used their incipient organs of

flight exclusively, as far as we know, to escape being devoured by other

fish?

When we see any structure highly perfected for any particular habit,

as the wings of a bird for flight, we should bear in mind that animals

displaying early transitional grades of the structure will seldom

continue to exist to the present day, for they will have been

supplanted by the very process of perfection through natural selection.

Furthermore, we may conclude that transitional grades between structures

fitted for very different habits of life will rarely have been developed

at an early period in great numbers and under many subordinate forms.

Thus, to return to our imaginary illustration of the flying-fish, it

does not seem probable that fishes capable of true flight would have

been developed under many subordinate forms, for taking prey of many

kinds in many ways, on the land and in the water, until their organs of

flight had come to a high stage of perfection, so as to have given them

a decided advantage over other animals in the battle for life. Hence the

chance of discovering species with transitional grades of structure in

a fossil condition will always be less, from their having existed

in lesser numbers, than in the case of species with fully developed

structures.

I will now give two or three instances of diversified and of changed

habits in the individuals of the same species. When either case occurs,

it would be easy for natural selection to fit the animal, by some

modification of its structure, for its changed habits, or exclusively

for one of its several different habits. But it is difficult to tell,

and immaterial for us, whether habits generally change first and

structure afterwards; or whether slight modifications of structure lead

to changed habits; both probably often change almost simultaneously. Of

cases of changed habits it will suffice merely to allude to that of the

many British insects which now feed on exotic plants, or exclusively on

artificial substances. Of diversified habits innumerable instances

could be given: I have often watched a tyrant flycatcher (Saurophagus

sulphuratus) in South America, hovering over one spot and then

proceeding to another, like a kestrel, and at other times standing

stationary on the margin of water, and then dashing like a kingfisher at

a fish. In our own country the larger titmouse (Parus major) may be seen

climbing branches, almost like a creeper; it often, like a shrike, kills

small birds by blows on the head; and I have many times seen and heard

it hammering the seeds of the yew on a branch, and thus breaking them

like a nuthatch. In North America the black bear was seen by Hearne

swimming for hours with widely open mouth, thus catching, like a whale,

insects in the water. Even in so extreme a case as this, if the supply

of insects were constant, and if better adapted competitors did not

already exist in the country, I can see no difficulty in a race of bears

being rendered, by natural selection, more and more aquatic in their

structure and habits, with larger and larger mouths, till a creature was

produced as monstrous as a whale.

As we sometimes see individuals of a species following habits widely

different from those both of their own species and of the other species

of the same genus, we might expect, on my theory, that such individuals

would occasionally have given rise to new species, having anomalous

habits, and with their structure either slightly or considerably

modified from that of their proper type. And such instances do occur in

nature. Can a more striking instance of adaptation be given than that of

a woodpecker for climbing trees and for seizing insects in the chinks of

the bark? Yet in North America there are woodpeckers which feed largely

on fruit, and others with elongated wings which chase insects on the

wing; and on the plains of La Plata, where not a tree grows, there is a

woodpecker, which in every essential part of its organisation, even in

its colouring, in the harsh tone of its voice, and undulatory flight,

told me plainly of its close blood-relationship to our common species;

yet it is a woodpecker which never climbs a tree!

Petrels are the most aerial and oceanic of birds, yet in the quiet

Sounds of Tierra del Fuego, the Puffinuria berardi, in its general

habits, in its astonishing power of diving, its manner of swimming, and

of flying when unwillingly it takes flight, would be mistaken by any one

for an auk or grebe; nevertheless, it is essentially a petrel, but with

many parts of its organisation profoundly modified. On the other hand,

the acutest observer by examining the dead body of the water-ouzel would

never have suspected its sub-aquatic habits; yet this anomalous

member of the strictly terrestrial thrush family wholly subsists by

diving,--grasping the stones with its feet and using its wings under

water.

He who believes that each being has been created as we now see it, must

occasionally have felt surprise when he has met with an animal having

habits and structure not at all in agreement. What can be plainer than

that the webbed feet of ducks and geese are formed for swimming? yet

there are upland geese with webbed feet which rarely or never go near

the water; and no one except Audubon has seen the frigate-bird, which

has all its four toes webbed, alight on the surface of the sea. On the

other hand, grebes and coots are eminently aquatic, although their toes

are only bordered by membrane. What seems plainer than that the long

toes of grallatores are formed for walking over swamps and floating

plants, yet the water-hen is nearly as aquatic as the coot; and the

landrail nearly as terrestrial as the quail or partridge. In such

cases, and many others could be given, habits have changed without a

corresponding change of structure. The webbed feet of the upland goose

may be said to have become rudimentary in function, though not in

structure. In the frigate-bird, the deeply-scooped membrane between the

toes shows that structure has begun to change.

He who believes in separate and innumerable acts of creation will say,

that in these cases it has pleased the Creator to cause a being of one

type to take the place of one of another type; but this seems to me

only restating the fact in dignified language. He who believes in the

struggle for existence and in the principle of natural selection, will

acknowledge that every organic being is constantly endeavouring to

increase in numbers; and that if any one being vary ever so little,

either in habits or structure, and thus gain an advantage over some

other inhabitant of the country, it will seize on the place of that

inhabitant, however different it may be from its own place. Hence it

will cause him no surprise that there should be geese and frigate-birds

with webbed feet, either living on the dry land or most rarely alighting

on the water; that there should be long-toed corncrakes living in

meadows instead of in swamps; that there should be woodpeckers where not

a tree grows; that there should be diving thrushes, and petrels with the

habits of auks.

ORGANS OF EXTREME PERFECTION AND COMPLICATION.

To suppose that the eye, with all its inimitable contrivances for

adjusting the focus to different distances, for admitting different

amounts of light, and for the correction of spherical and chromatic

aberration, could have been formed by natural selection, seems, I freely

confess, absurd in the highest possible degree. Yet reason tells me,

that if numerous gradations from a perfect and complex eye to one very

imperfect and simple, each grade being useful to its possessor, can be

shown to exist; if further, the eye does vary ever so slightly, and

the variations be inherited, which is certainly the case; and if any

variation or modification in the organ be ever useful to an animal under

changing conditions of life, then the difficulty of believing that a

perfect and complex eye could be formed by natural selection, though

insuperable by our imagination, can hardly be considered real. How a

nerve comes to be sensitive to light, hardly concerns us more than how

life itself first originated; but I may remark that several facts make

me suspect that any sensitive nerve may be rendered sensitive to light,

and likewise to those coarser vibrations of the air which produce sound.

In looking for the gradations by which an organ in any species has been

perfected, we ought to look exclusively to its lineal ancestors; but

this is scarcely ever possible, and we are forced in each case to look

to species of the same group, that is to the collateral descendants

from the same original parent-form, in order to see what gradations are

possible, and for the chance of some gradations having been transmitted

from the earlier stages of descent, in an unaltered or little altered

condition. Amongst existing Vertebrata, we find but a small amount of

gradation in the structure of the eye, and from fossil species we can

learn nothing on this head. In this great class we should probably

have to descend far beneath the lowest known fossiliferous stratum to

discover the earlier stages, by which the eye has been perfected.

In the Articulata we can commence a series with an optic nerve merely

coated with pigment, and without any other mechanism; and from this

low stage, numerous gradations of structure, branching off in two

fundamentally different lines, can be shown to exist, until we reach

a moderately high stage of perfection. In certain crustaceans, for

instance, there is a double cornea, the inner one divided into

facets, within each of which there is a lens-shaped swelling. In other

crustaceans the transparent cones which are coated by pigment, and which

properly act only by excluding lateral pencils of light, are convex at

their upper ends and must act by convergence; and at their lower ends

there seems to be an imperfect vitreous substance. With these facts,

here far too briefly and imperfectly given, which show that there is

much graduated diversity in the eyes of living crustaceans, and bearing

in mind how small the number of living animals is in proportion to those

which have become extinct, I can see no very great difficulty (not more

than in the case of many other structures) in believing that natural

selection has converted the simple apparatus of an optic nerve merely

coated with pigment and invested by transparent membrane, into an

optical instrument as perfect as is possessed by any member of the great

Articulate class.

He who will go thus far, if he find on finishing this treatise that

large bodies of facts, otherwise inexplicable, can be explained by the

theory of descent, ought not to hesitate to go further, and to admit

that a structure even as perfect as the eye of an eagle might be formed

by natural selection, although in this case he does not know any of the

transitional grades. His reason ought to conquer his imagination; though

I have felt the difficulty far too keenly to be surprised at any degree

of hesitation in extending the principle of natural selection to such

startling lengths.

It is scarcely possible to avoid comparing the eye to a telescope.

We know that this instrument has been perfected by the long-continued

efforts of the highest human intellects; and we naturally infer that the

eye has been formed by a somewhat analogous process. But may not this

inference be presumptuous? Have we any right to assume that the Creator

works by intellectual powers like those of man? If we must compare the

eye to an optical instrument, we ought in imagination to take a thick

layer of transparent tissue, with a nerve sensitive to light beneath,

and then suppose every part of this layer to be continually changing

slowly in density, so as to separate into layers of different densities

and thicknesses, placed at different distances from each other, and

with the surfaces of each layer slowly changing in form. Further we

must suppose that there is a power always intently watching each slight

accidental alteration in the transparent layers; and carefully selecting

each alteration which, under varied circumstances, may in any way, or in

any degree, tend to produce a distincter image. We must suppose each new

state of the instrument to be multiplied by the million; and each to

be preserved till a better be produced, and then the old ones to

be destroyed. In living bodies, variation will cause the slight

alterations, generation will multiply them almost infinitely, and

natural selection will pick out with unerring skill each improvement.

Let this process go on for millions on millions of years; and during

each year on millions of individuals of many kinds; and may we not

believe that a living optical instrument might thus be formed as

superior to one of glass, as the works of the Creator are to those of

man?

If it could be demonstrated that any complex organ existed, which

could not possibly have been formed by numerous, successive, slight

modifications, my theory would absolutely break down. But I can find

out no such case. No doubt many organs exist of which we do not know

the transitional grades, more especially if we look to much-isolated

species, round which, according to my theory, there has been much

extinction. Or again, if we look to an organ common to all the members

of a large class, for in this latter case the organ must have been first

formed at an extremely remote period, since which all the many members

of the class have been developed; and in order to discover the early

transitional grades through which the organ has passed, we should have

to look to very ancient ancestral forms, long since become extinct.

We should be extremely cautious in concluding that an organ could not

have been formed by transitional gradations of some kind. Numerous cases

could be given amongst the lower animals of the same organ performing

at the same time wholly distinct functions; thus the alimentary canal

respires, digests, and excretes in the larva of the dragon-fly and in

the fish Cobites. In the Hydra, the animal may be turned inside out, and

the exterior surface will then digest and the stomach respire. In such

cases natural selection might easily specialise, if any advantage were

thus gained, a part or organ, which had performed two functions, for one

function alone, and thus wholly change its nature by insensible steps.

Two distinct organs sometimes perform simultaneously the same function

in the same individual; to give one instance, there are fish with gills

or branchiae that breathe the air dissolved in the water, at the same

time that they breathe free air in their swimbladders, this latter organ

having a ductus pneumaticus for its supply, and being divided by highly

vascular partitions. In these cases, one of the two organs might with

ease be modified and perfected so as to perform all the work by itself,

being aided during the process of modification by the other organ;

and then this other organ might be modified for some other and quite

distinct purpose, or be quite obliterated.

The illustration of the swimbladder in fishes is a good one, because

it shows us clearly the highly important fact that an organ originally

constructed for one purpose, namely flotation, may be converted into one

for a wholly different purpose, namely respiration. The swimbladder has,

also, been worked in as an accessory to the auditory organs of certain

fish, or, for I do not know which view is now generally held, a part

of the auditory apparatus has been worked in as a complement to the

swimbladder. All physiologists admit that the swimbladder is homologous,

or "ideally similar," in position and structure with the lungs of

the higher vertebrate animals: hence there seems to me to be no great

difficulty in believing that natural selection has actually converted a

swimbladder into a lung, or organ used exclusively for respiration.

I can, indeed, hardly doubt that all vertebrate animals having true

lungs have descended by ordinary generation from an ancient prototype,

of which we know nothing, furnished with a floating apparatus or

swimbladder. We can thus, as I infer from Professor Owen's interesting

description of these parts, understand the strange fact that every

particle of food and drink which we swallow has to pass over the

orifice of the trachea, with some risk of falling into the lungs,

notwithstanding the beautiful contrivance by which the glottis

is closed. In the higher Vertebrata the branchiae have wholly

disappeared--the slits on the sides of the neck and the loop-like course

of the arteries still marking in the embryo their former position. But

it is conceivable that the now utterly lost branchiae might have

been gradually worked in by natural selection for some quite distinct

purpose: in the same manner as, on the view entertained by some

naturalists that the branchiae and dorsal scales of Annelids are

homologous with the wings and wing-covers of insects, it is probable

that organs which at a very ancient period served for respiration have

been actually converted into organs of flight.

In considering transitions of organs, it is so important to bear in mind

the probability of conversion from one function to another, that I will

give one more instance. Pedunculated cirripedes have two minute folds of

skin, called by me the ovigerous frena, which serve, through the means

of a sticky secretion, to retain the eggs until they are hatched within

the sack. These cirripedes have no branchiae, the whole surface of the

body and sack, including the small frena, serving for respiration. The

Balanidae or sessile cirripedes, on the other hand, have no ovigerous

frena, the eggs lying loose at the bottom of the sack, in the

well-enclosed shell; but they have large folded branchiae. Now I think

no one will dispute that the ovigerous frena in the one family are

strictly homologous with the branchiae of the other family; indeed, they

graduate into each other. Therefore I do not doubt that little folds of

skin, which originally served as ovigerous frena, but which, likewise,

very slightly aided the act of respiration, have been gradually

converted by natural selection into branchiae, simply through an

increase in their size and the obliteration of their adhesive glands.

If all pedunculated cirripedes had become extinct, and they have already

suffered far more extinction than have sessile cirripedes, who would

ever have imagined that the branchiae in this latter family had

originally existed as organs for preventing the ova from being washed

out of the sack?

Although we must be extremely cautious in concluding that any organ

could not possibly have been produced by successive transitional

gradations, yet, undoubtedly, grave cases of difficulty occur, some of

which will be discussed in my future work.

One of the gravest is that of neuter insects, which are often very

differently constructed from either the males or fertile females; but

this case will be treated of in the next chapter. The electric organs

of fishes offer another case of special difficulty; it is impossible to

conceive by what steps these wondrous organs have been produced; but,

as Owen and others have remarked, their intimate structure closely

resembles that of common muscle; and as it has lately been shown that

Rays have an organ closely analogous to the electric apparatus, and yet

do not, as Matteuchi asserts, discharge any electricity, we must own

that we are far too ignorant to argue that no transition of any kind is

possible.

The electric organs offer another and even more serious difficulty; for

they occur in only about a dozen fishes, of which several are widely

remote in their affinities. Generally when the same organ appears in

several members of the same class, especially if in members having very

different habits of life, we may attribute its presence to inheritance

from a common ancestor; and its absence in some of the members to its

loss through disuse or natural selection. But if the electric organs had

been inherited from one ancient progenitor thus provided, we might have

expected that all electric fishes would have been specially related to

each other. Nor does geology at all lead to the belief that formerly

most fishes had electric organs, which most of their modified

descendants have lost. The presence of luminous organs in a few insects,

belonging to different families and orders, offers a parallel case of

difficulty. Other cases could be given; for instance in plants, the very

curious contrivance of a mass of pollen-grains, borne on a

foot-stalk with a sticky gland at the end, is the same in Orchis and

Asclepias,--genera almost as remote as possible amongst flowering

plants. In all these cases of two very distinct species furnished

with apparently the same anomalous organ, it should be observed that,

although the general appearance and function of the organ may be the

same, yet some fundamental difference can generally be detected. I

am inclined to believe that in nearly the same way as two men have

sometimes independently hit on the very same invention, so natural

selection, working for the good of each being and taking advantage of

analogous variations, has sometimes modified in very nearly the same

manner two parts in two organic beings, which owe but little of their

structure in common to inheritance from the same ancestor.

Although in many cases it is most difficult to conjecture by what

transitions an organ could have arrived at its present state; yet,

considering that the proportion of living and known forms to the extinct

and unknown is very small, I have been astonished how rarely an organ

can be named, towards which no transitional grade is known to lead.

The truth of this remark is indeed shown by that old canon in natural

history of "Natura non facit saltum." We meet with this admission in the

writings of almost every experienced naturalist; or, as Milne Edwards

has well expressed it, nature is prodigal in variety, but niggard in

innovation. Why, on the theory of Creation, should this be so? Why

should all the parts and organs of many independent beings, each

supposed to have been separately created for its proper place in nature,

be so invariably linked together by graduated steps? Why should not

Nature have taken a leap from structure to structure? On the theory of

natural selection, we can clearly understand why she should not; for

natural selection can act only by taking advantage of slight successive

variations; she can never take a leap, but must advance by the shortest

and slowest steps.

ORGANS OF LITTLE APPARENT IMPORTANCE.

As natural selection acts by life and death,--by the preservation of

individuals with any favourable variation, and by the destruction of

those with any unfavourable deviation of structure,--I have sometimes

felt much difficulty in understanding the origin of simple parts, of

which the importance does not seem sufficient to cause the preservation

of successively varying individuals. I have sometimes felt as much

difficulty, though of a very different kind, on this head, as in the

case of an organ as perfect and complex as the eye.

In the first place, we are much too ignorant in regard to the whole

economy of any one organic being, to say what slight modifications would

be of importance or not. In a former chapter I have given instances of

most trifling characters, such as the down on fruit and the colour of

the flesh, which, from determining the attacks of insects or from being

correlated with constitutional differences, might assuredly be acted on

by natural selection. The tail of the giraffe looks like an artificially

constructed fly-flapper; and it seems at first incredible that this

could have been adapted for its present purpose by successive slight

modifications, each better and better, for so trifling an object as

driving away flies; yet we should pause before being too positive even

in this case, for we know that the distribution and existence of cattle

and other animals in South America absolutely depends on their power of

resisting the attacks of insects: so that individuals which could by any

means defend themselves from these small enemies, would be able to range

into new pastures and thus gain a great advantage. It is not that the

larger quadrupeds are actually destroyed (except in some rare cases) by

the flies, but they are incessantly harassed and their strength reduced,

so that they are more subject to disease, or not so well enabled in a

coming dearth to search for food, or to escape from beasts of prey.

Organs now of trifling importance have probably in some cases been of

high importance to an early progenitor, and, after having been slowly

perfected at a former period, have been transmitted in nearly the

same state, although now become of very slight use; and any actually

injurious deviations in their structure will always have been checked by

natural selection. Seeing how important an organ of locomotion the

tail is in most aquatic animals, its general presence and use for many

purposes in so many land animals, which in their lungs or modified

swim-bladders betray their aquatic origin, may perhaps be thus accounted

for. A well-developed tail having been formed in an aquatic animal, it

might subsequently come to be worked in for all sorts of purposes, as

a fly-flapper, an organ of prehension, or as an aid in turning, as with

the dog, though the aid must be slight, for the hare, with hardly any

tail, can double quickly enough.

In the second place, we may sometimes attribute importance to characters

which are really of very little importance, and which have originated

from quite secondary causes, independently of natural selection. We

should remember that climate, food, etc., probably have some little

direct influence on the organisation; that characters reappear from

the law of reversion; that correlation of growth will have had a most

important influence in modifying various structures; and finally,

that sexual selection will often have largely modified the external

characters of animals having a will, to give one male an advantage

in fighting with another or in charming the females. Moreover when a

modification of structure has primarily arisen from the above or

other unknown causes, it may at first have been of no advantage to

the species, but may subsequently have been taken advantage of by the

descendants of the species under new conditions of life and with newly

acquired habits.

To give a few instances to illustrate these latter remarks. If green

woodpeckers alone had existed, and we did not know that there were many

black and pied kinds, I dare say that we should have thought that the

green colour was a beautiful adaptation to hide this tree-frequenting

bird from its enemies; and consequently that it was a character of

importance and might have been acquired through natural selection; as it

is, I have no doubt that the colour is due to some quite distinct cause,

probably to sexual selection. A trailing bamboo in the Malay Archipelago

climbs the loftiest trees by the aid of exquisitely constructed hooks

clustered around the ends of the branches, and this contrivance, no

doubt, is of the highest service to the plant; but as we see nearly

similar hooks on many trees which are not climbers, the hooks on the

bamboo may have arisen from unknown laws of growth, and have been

subsequently taken advantage of by the plant undergoing further

modification and becoming a climber. The naked skin on the head of a

vulture is generally looked at as a direct adaptation for wallowing in

putridity; and so it may be, or it may possibly be due to the direct

action of putrid matter; but we should be very cautious in drawing

any such inference, when we see that the skin on the head of the

clean-feeding male turkey is likewise naked. The sutures in the skulls

of young mammals have been advanced as a beautiful adaptation for aiding

parturition, and no doubt they facilitate, or may be indispensable

for this act; but as sutures occur in the skulls of young birds and

reptiles, which have only to escape from a broken egg, we may infer that

this structure has arisen from the laws of growth, and has been taken

advantage of in the parturition of the higher animals.

We are profoundly ignorant of the causes producing slight and

unimportant variations; and we are immediately made conscious of this by

reflecting on the differences in the breeds of our domesticated animals

in different countries,--more especially in the less civilized countries

where there has been but little artificial selection. Careful observers

are convinced that a damp climate affects the growth of the hair, and

that with the hair the horns are correlated. Mountain breeds always

differ from lowland breeds; and a mountainous country would probably

affect the hind limbs from exercising them more, and possibly even the

form of the pelvis; and then by the law of homologous variation, the

front limbs and even the head would probably be affected. The shape,

also, of the pelvis might affect by pressure the shape of the head of

the young in the womb. The laborious breathing necessary in high regions

would, we have some reason to believe, increase the size of the chest;

and again correlation would come into play. Animals kept by savages in

different countries often have to struggle for their own subsistence,

and would be exposed to a certain extent to natural selection, and

individuals with slightly different constitutions would succeed

best under different climates; and there is reason to believe that

constitution and colour are correlated. A good observer, also, states

that in cattle susceptibility to the attacks of flies is correlated with

colour, as is the liability to be poisoned by certain plants; so that

colour would be thus subjected to the action of natural selection. But

we are far too ignorant to speculate on the relative importance of the

several known and unknown laws of variation; and I have here alluded

to them only to show that, if we are unable to account for the

characteristic differences of our domestic breeds, which nevertheless we

generally admit to have arisen through ordinary generation, we ought

not to lay too much stress on our ignorance of the precise cause of the

slight analogous differences between species. I might have adduced for

this same purpose the differences between the races of man, which are

so strongly marked; I may add that some little light can apparently

be thrown on the origin of these differences, chiefly through sexual

selection of a particular kind, but without here entering on copious

details my reasoning would appear frivolous.

The foregoing remarks lead me to say a few words on the protest lately

made by some naturalists, against the utilitarian doctrine that every

detail of structure has been produced for the good of its possessor.

They believe that very many structures have been created for beauty in

the eyes of man, or for mere variety. This doctrine, if true, would be

absolutely fatal to my theory. Yet I fully admit that many structures

are of no direct use to their possessors. Physical conditions probably

have had some little effect on structure, quite independently of any

good thus gained. Correlation of growth has no doubt played a most

important part, and a useful modification of one part will often have

entailed on other parts diversified changes of no direct use. So again

characters which formerly were useful, or which formerly had arisen from

correlation of growth, or from other unknown cause, may reappear from

the law of reversion, though now of no direct use. The effects of sexual

selection, when displayed in beauty to charm the females, can be called

useful only in rather a forced sense. But by far the most important

consideration is that the chief part of the organisation of every

being is simply due to inheritance; and consequently, though each being

assuredly is well fitted for its place in nature, many structures now

have no direct relation to the habits of life of each species. Thus, we

can hardly believe that the webbed feet of the upland goose or of the

frigate-bird are of special use to these birds; we cannot believe that

the same bones in the arm of the monkey, in the fore leg of the horse,

in the wing of the bat, and in the flipper of the seal, are of special

use to these animals. We may safely attribute these structures to

inheritance. But to the progenitor of the upland goose and of the

frigate-bird, webbed feet no doubt were as useful as they now are to the

most aquatic of existing birds. So we may believe that the progenitor of

the seal had not a flipper, but a foot with five toes fitted for walking

or grasping; and we may further venture to believe that the several

bones in the limbs of the monkey, horse, and bat, which have been

inherited from a common progenitor, were formerly of more special use to

that progenitor, or its progenitors, than they now are to these animals

having such widely diversified habits. Therefore we may infer that

these several bones might have been acquired through natural selection,

subjected formerly, as now, to the several laws of inheritance,

reversion, correlation of growth, etc. Hence every detail of structure

in every living creature (making some little allowance for the direct

action of physical conditions) may be viewed, either as having been of

special use to some ancestral form, or as being now of special use to

the descendants of this form--either directly, or indirectly through the

complex laws of growth.

Natural selection cannot possibly produce any modification in any one

species exclusively for the good of another species; though throughout

nature one species incessantly takes advantage of, and profits by, the

structure of another. But natural selection can and does often produce

structures for the direct injury of other species, as we see in the fang

of the adder, and in the ovipositor of the ichneumon, by which its eggs

are deposited in the living bodies of other insects. If it could be

proved that any part of the structure of any one species had been

formed for the exclusive good of another species, it would annihilate my

theory, for such could not have been produced through natural selection.

Although many statements may be found in works on natural history to

this effect, I cannot find even one which seems to me of any weight. It

is admitted that the rattlesnake has a poison-fang for its own defence

and for the destruction of its prey; but some authors suppose that at

the same time this snake is furnished with a rattle for its own injury,

namely, to warn its prey to escape. I would almost as soon believe that

the cat curls the end of its tail when preparing to spring, in order to

warn the doomed mouse. But I have not space here to enter on this and

other such cases.

Natural selection will never produce in a being anything injurious to

itself, for natural selection acts solely by and for the good of each.

No organ will be formed, as Paley has remarked, for the purpose of

causing pain or for doing an injury to its possessor. If a fair balance

be struck between the good and evil caused by each part, each will be

found on the whole advantageous. After the lapse of time, under changing

conditions of life, if any part comes to be injurious, it will be

modified; or if it be not so, the being will become extinct, as myriads

have become extinct.

Natural selection tends only to make each organic being as perfect as,

or slightly more perfect than, the other inhabitants of the same country

with which it has to struggle for existence. And we see that this is the

degree of perfection attained under nature. The endemic productions of

New Zealand, for instance, are perfect one compared with another; but

they are now rapidly yielding before the advancing legions of plants

and animals introduced from Europe. Natural selection will not produce

absolute perfection, nor do we always meet, as far as we can judge, with

this high standard under nature. The correction for the aberration of

light is said, on high authority, not to be perfect even in that most

perfect organ, the eye. If our reason leads us to admire with enthusiasm

a multitude of inimitable contrivances in nature, this same reason tells

us, though we may easily err on both sides, that some other contrivances

are less perfect. Can we consider the sting of the wasp or of the bee

as perfect, which, when used against many attacking animals, cannot be

withdrawn, owing to the backward serratures, and so inevitably causes

the death of the insect by tearing out its viscera?

If we look at the sting of the bee, as having originally existed in a

remote progenitor as a boring and serrated instrument, like that in so

many members of the same great order, and which has been modified

but not perfected for its present purpose, with the poison originally

adapted to cause galls subsequently intensified, we can perhaps

understand how it is that the use of the sting should so often cause the

insect's own death: for if on the whole the power of stinging be

useful to the community, it will fulfil all the requirements of natural

selection, though it may cause the death of some few members. If we

admire the truly wonderful power of scent by which the males of many

insects find their females, can we admire the production for this

single purpose of thousands of drones, which are utterly useless to the

community for any other end, and which are ultimately slaughtered by

their industrious and sterile sisters? It may be difficult, but we ought

to admire the savage instinctive hatred of the queen-bee, which urges

her instantly to destroy the young queens her daughters as soon as born,

or to perish herself in the combat; for undoubtedly this is for the

good of the community; and maternal love or maternal hatred, though

the latter fortunately is most rare, is all the same to the inexorable

principle of natural selection. If we admire the several ingenious

contrivances, by which the flowers of the orchis and of many other

plants are fertilised through insect agency, can we consider as equally

perfect the elaboration by our fir-trees of dense clouds of pollen, in

order that a few granules may be wafted by a chance breeze on to the

ovules?

SUMMARY OF CHAPTER.

We have in this chapter discussed some of the difficulties and

objections which may be urged against my theory. Many of them are very

grave; but I think that in the discussion light has been thrown on

several facts, which on the theory of independent acts of creation are

utterly obscure. We have seen that species at any one period are not

indefinitely variable, and are not linked together by a multitude of

intermediate gradations, partly because the process of natural selection

will always be very slow, and will act, at any one time, only on a very

few forms; and partly because the very process of natural selection

almost implies the continual supplanting and extinction of preceding

and intermediate gradations. Closely allied species, now living on

a continuous area, must often have been formed when the area was not

continuous, and when the conditions of life did not insensibly graduate

away from one part to another. When two varieties are formed in two

districts of a continuous area, an intermediate variety will often be

formed, fitted for an intermediate zone; but from reasons assigned, the

intermediate variety will usually exist in lesser numbers than the two

forms which it connects; consequently the two latter, during the course

of further modification, from existing in greater numbers, will have a

great advantage over the less numerous intermediate variety, and will

thus generally succeed in supplanting and exterminating it.

We have seen in this chapter how cautious we should be in concluding

that the most different habits of life could not graduate into each

other; that a bat, for instance, could not have been formed by natural

selection from an animal which at first could only glide through the

air.

We have seen that a species may under new conditions of life change its

habits, or have diversified habits, with some habits very unlike those

of its nearest congeners. Hence we can understand, bearing in mind that

each organic being is trying to live wherever it can live, how it has

arisen that there are upland geese with webbed feet, ground woodpeckers,

diving thrushes, and petrels with the habits of auks.

Although the belief that an organ so perfect as the eye could have been

formed by natural selection, is more than enough to stagger any one; yet

in the case of any organ, if we know of a long series of gradations in

complexity, each good for its possessor, then, under changing conditions

of life, there is no logical impossibility in the acquirement of any

conceivable degree of perfection through natural selection. In the cases

in which we know of no intermediate or transitional states, we should

be very cautious in concluding that none could have existed, for the

homologies of many organs and their intermediate states show that

wonderful metamorphoses in function are at least possible. For instance,

a swim-bladder has apparently been converted into an air-breathing lung.

The same organ having performed simultaneously very different functions,

and then having been specialised for one function; and two very distinct

organs having performed at the same time the same function, the one

having been perfected whilst aided by the other, must often have largely

facilitated transitions.

We are far too ignorant, in almost every case, to be enabled to assert

that any part or organ is so unimportant for the welfare of a species,

that modifications in its structure could not have been slowly

accumulated by means of natural selection. But we may confidently

believe that many modifications, wholly due to the laws of growth, and

at first in no way advantageous to a species, have been subsequently

taken advantage of by the still further modified descendants of this

species. We may, also, believe that a part formerly of high importance

has often been retained (as the tail of an aquatic animal by its

terrestrial descendants), though it has become of such small importance

that it could not, in its present state, have been acquired by natural

selection,--a power which acts solely by the preservation of profitable

variations in the struggle for life.

Natural selection will produce nothing in one species for the exclusive

good or injury of another; though it may well produce parts, organs, and

excretions highly useful or even indispensable, or highly injurious to

another species, but in all cases at the same time useful to the owner.

Natural selection in each well-stocked country, must act chiefly through

the competition of the inhabitants one with another, and consequently

will produce perfection, or strength in the battle for life, only

according to the standard of that country. Hence the inhabitants of one

country, generally the smaller one, will often yield, as we see they do

yield, to the inhabitants of another and generally larger country. For

in the larger country there will have existed more individuals, and more

diversified forms, and the competition will have been severer, and

thus the standard of perfection will have been rendered higher. Natural

selection will not necessarily produce absolute perfection; nor, as far

as we can judge by our limited faculties, can absolute perfection be

everywhere found.

On the theory of natural selection we can clearly understand the full

meaning of that old canon in natural history, "Natura non facit saltum."

This canon, if we look only to the present inhabitants of the world, is

not strictly correct, but if we include all those of past times, it must

by my theory be strictly true.

It is generally acknowledged that all organic beings have been formed on

two great laws--Unity of Type, and the Conditions of Existence. By unity

of type is meant that fundamental agreement in structure, which we see

in organic beings of the same class, and which is quite independent of

their habits of life. On my theory, unity of type is explained by unity

of descent. The expression of conditions of existence, so often insisted

on by the illustrious Cuvier, is fully embraced by the principle of

natural selection. For natural selection acts by either now adapting the

varying parts of each being to its organic and inorganic conditions of

life; or by having adapted them during long-past periods of time: the

adaptations being aided in some cases by use and disuse, being slightly

affected by the direct action of the external conditions of life, and

being in all cases subjected to the several laws of growth. Hence, in

fact, the law of the Conditions of Existence is the higher law; as it

includes, through the inheritance of former adaptations, that of Unity

of Type.

7. INSTINCT.

Instincts comparable with habits, but different in their origin.

Instincts graduated. Aphides and ants. Instincts variable. Domestic

instincts, their origin. Natural instincts of the cuckoo, ostrich, and

parasitic bees. Slave-making ants. Hive-bee, its cell-making instinct.

Difficulties on the theory of the Natural Selection of instincts. Neuter

or sterile insects. Summary.

The subject of instinct might have been worked into the previous

chapters; but I have thought that it would be more convenient to treat

the subject separately, especially as so wonderful an instinct as that

of the hive-bee making its cells will probably have occurred to many

readers, as a difficulty sufficient to overthrow my whole theory. I must

premise, that I have nothing to do with the origin of the primary mental

powers, any more than I have with that of life itself. We are concerned

only with the diversities of instinct and of the other mental qualities

of animals within the same class.

I will not attempt any definition of instinct. It would be easy to show

that several distinct mental actions are commonly embraced by this term;

but every one understands what is meant, when it is said that instinct

impels the cuckoo to migrate and to lay her eggs in other birds' nests.

An action, which we ourselves should require experience to enable us to

perform, when performed by an animal, more especially by a very young

one, without any experience, and when performed by many individuals in

the same way, without their knowing for what purpose it is performed,

is usually said to be instinctive. But I could show that none of these

characters of instinct are universal. A little dose, as Pierre Huber

expresses it, of judgment or reason, often comes into play, even in

animals very low in the scale of nature.

Frederick Cuvier and several of the older metaphysicians have compared

instinct with habit. This comparison gives, I think, a remarkably

accurate notion of the frame of mind under which an instinctive action

is performed, but not of its origin. How unconsciously many habitual

actions are performed, indeed not rarely in direct opposition to our

conscious will! yet they may be modified by the will or reason. Habits

easily become associated with other habits, and with certain periods

of time and states of the body. When once acquired, they often remain

constant throughout life. Several other points of resemblance between

instincts and habits could be pointed out. As in repeating a well-known

song, so in instincts, one action follows another by a sort of rhythm;

if a person be interrupted in a song, or in repeating anything by rote,

he is generally forced to go back to recover the habitual train of

thought: so P. Huber found it was with a caterpillar, which makes a very

complicated hammock; for if he took a caterpillar which had completed

its hammock up to, say, the sixth stage of construction, and put it into

a hammock completed up only to the third stage, the caterpillar simply

re-performed the fourth, fifth, and sixth stages of construction.

If, however, a caterpillar were taken out of a hammock made up, for

instance, to the third stage, and were put into one finished up to the

sixth stage, so that much of its work was already done for it, far from

feeling the benefit of this, it was much embarrassed, and, in order to

complete its hammock, seemed forced to start from the third stage, where

it had left off, and thus tried to complete the already finished work.

If we suppose any habitual action to become inherited--and I think

it can be shown that this does sometimes happen--then the resemblance

between what originally was a habit and an instinct becomes so close as

not to be distinguished. If Mozart, instead of playing the pianoforte at

three years old with wonderfully little practice, had played a tune

with no practice at all, he might truly be said to have done so

instinctively. But it would be the most serious error to suppose that

the greater number of instincts have been acquired by habit in

one generation, and then transmitted by inheritance to succeeding

generations. It can be clearly shown that the most wonderful instincts

with which we are acquainted, namely, those of the hive-bee and of many

ants, could not possibly have been thus acquired.

It will be universally admitted that instincts are as important as

corporeal structure for the welfare of each species, under its present

conditions of life. Under changed conditions of life, it is at least

possible that slight modifications of instinct might be profitable to a

species; and if it can be shown that instincts do vary ever so little,

then I can see no difficulty in natural selection preserving and

continually accumulating variations of instinct to any extent that may

be profitable. It is thus, as I believe, that all the most complex

and wonderful instincts have originated. As modifications of corporeal

structure arise from, and are increased by, use or habit, and are

diminished or lost by disuse, so I do not doubt it has been with

instincts. But I believe that the effects of habit are of quite

subordinate importance to the effects of the natural selection of what

may be called accidental variations of instincts;--that is of variations

produced by the same unknown causes which produce slight deviations of

bodily structure.

No complex instinct can possibly be produced through natural selection,

except by the slow and gradual accumulation of numerous, slight, yet

profitable, variations. Hence, as in the case of corporeal structures,

we ought to find in nature, not the actual transitional gradations by

which each complex instinct has been acquired--for these could be found

only in the lineal ancestors of each species--but we ought to find in

the collateral lines of descent some evidence of such gradations; or

we ought at least to be able to show that gradations of some kind are

possible; and this we certainly can do. I have been surprised to find,

making allowance for the instincts of animals having been but little

observed except in Europe and North America, and for no instinct being

known amongst extinct species, how very generally gradations, leading to

the most complex instincts, can be discovered. The canon of "Natura non

facit saltum" applies with almost equal force to instincts as to bodily

organs. Changes of instinct may sometimes be facilitated by the same

species having different instincts at different periods of life, or

at different seasons of the year, or when placed under different

circumstances, etc.; in which case either one or the other instinct

might be preserved by natural selection. And such instances of diversity

of instinct in the same species can be shown to occur in nature.

Again as in the case of corporeal structure, and conformably with my

theory, the instinct of each species is good for itself, but has never,

as far as we can judge, been produced for the exclusive good of others.

One of the strongest instances of an animal apparently performing an

action for the sole good of another, with which I am acquainted, is that

of aphides voluntarily yielding their sweet excretion to ants: that they

do so voluntarily, the following facts show. I removed all the ants from

a group of about a dozen aphides on a dock-plant, and prevented their

attendance during several hours. After this interval, I felt sure that

the aphides would want to excrete. I watched them for some time through

a lens, but not one excreted; I then tickled and stroked them with a

hair in the same manner, as well as I could, as the ants do with their

antennae; but not one excreted. Afterwards I allowed an ant to visit

them, and it immediately seemed, by its eager way of running about, to

be well aware what a rich flock it had discovered; it then began to play

with its antennae on the abdomen first of one aphis and then of another;

and each aphis, as soon as it felt the antennae, immediately lifted up

its abdomen and excreted a limpid drop of sweet juice, which was eagerly

devoured by the ant. Even the quite young aphides behaved in this

manner, showing that the action was instinctive, and not the result of

experience. But as the excretion is extremely viscid, it is probably a

convenience to the aphides to have it removed; and therefore probably

the aphides do not instinctively excrete for the sole good of the ants.

Although I do not believe that any animal in the world performs an

action for the exclusive good of another of a distinct species, yet

each species tries to take advantage of the instincts of others, as each

takes advantage of the weaker bodily structure of others. So again, in

some few cases, certain instincts cannot be considered as absolutely

perfect; but as details on this and other such points are not

indispensable, they may be here passed over.

As some degree of variation in instincts under a state of nature, and

the inheritance of such variations, are indispensable for the action of

natural selection, as many instances as possible ought to have been here

given; but want of space prevents me. I can only assert, that instincts

certainly do vary--for instance, the migratory instinct, both in extent

and direction, and in its total loss. So it is with the nests of birds,

which vary partly in dependence on the situations chosen, and on the

nature and temperature of the country inhabited, but often from causes

wholly unknown to us: Audubon has given several remarkable cases of

differences in nests of the same species in the northern and southern

United States. Fear of any particular enemy is certainly an instinctive

quality, as may be seen in nestling birds, though it is strengthened by

experience, and by the sight of fear of the same enemy in other animals.

But fear of man is slowly acquired, as I have elsewhere shown, by

various animals inhabiting desert islands; and we may see an instance

of this, even in England, in the greater wildness of all our large birds

than of our small birds; for the large birds have been most persecuted

by man. We may safely attribute the greater wildness of our large birds

to this cause; for in uninhabited islands large birds are not more

fearful than small; and the magpie, so wary in England, is tame in

Norway, as is the hooded crow in Egypt.

That the general disposition of individuals of the same species, born in

a state of nature, is extremely diversified, can be shown by a multitude

of facts. Several cases also, could be given, of occasional and strange

habits in certain species, which might, if advantageous to the species,

give rise, through natural selection, to quite new instincts. But I am

well aware that these general statements, without facts given in detail,

can produce but a feeble effect on the reader's mind. I can only repeat

my assurance, that I do not speak without good evidence.

The possibility, or even probability, of inherited variations

of instinct in a state of nature will be strengthened by briefly

considering a few cases under domestication. We shall thus also be

enabled to see the respective parts which habit and the selection of

so-called accidental variations have played in modifying the mental

qualities of our domestic animals. A number of curious and authentic

instances could be given of the inheritance of all shades of disposition

and tastes, and likewise of the oddest tricks, associated with certain

frames of mind or periods of time. But let us look to the familiar case

of the several breeds of dogs: it cannot be doubted that young pointers

(I have myself seen a striking instance) will sometimes point and even

back other dogs the very first time that they are taken out; retrieving

is certainly in some degree inherited by retrievers; and a tendency to

run round, instead of at, a flock of sheep, by shepherd-dogs. I cannot

see that these actions, performed without experience by the young,

and in nearly the same manner by each individual, performed with eager

delight by each breed, and without the end being known,--for the young

pointer can no more know that he points to aid his master, than

the white butterfly knows why she lays her eggs on the leaf of the

cabbage,--I cannot see that these actions differ essentially from true

instincts. If we were to see one kind of wolf, when young and without

any training, as soon as it scented its prey, stand motionless like a

statue, and then slowly crawl forward with a peculiar gait; and another

kind of wolf rushing round, instead of at, a herd of deer, and driving

them to a distant point, we should assuredly call these actions

instinctive. Domestic instincts, as they may be called, are certainly

far less fixed or invariable than natural instincts; but they have been

acted on by far less rigorous selection, and have been transmitted for

an incomparably shorter period, under less fixed conditions of life.

How strongly these domestic instincts, habits, and dispositions are

inherited, and how curiously they become mingled, is well shown when

different breeds of dogs are crossed. Thus it is known that a cross with

a bull-dog has affected for many generations the courage and obstinacy

of greyhounds; and a cross with a greyhound has given to a whole family

of shepherd-dogs a tendency to hunt hares. These domestic instincts,

when thus tested by crossing, resemble natural instincts, which in a

like manner become curiously blended together, and for a long period

exhibit traces of the instincts of either parent: for example, Le Roy

describes a dog, whose great-grandfather was a wolf, and this dog

showed a trace of its wild parentage only in one way, by not coming in a

straight line to his master when called.

Domestic instincts are sometimes spoken of as actions which have become

inherited solely from long-continued and compulsory habit, but this,

I think, is not true. No one would ever have thought of teaching, or

probably could have taught, the tumbler-pigeon to tumble,--an action

which, as I have witnessed, is performed by young birds, that have

never seen a pigeon tumble. We may believe that some one pigeon showed

a slight tendency to this strange habit, and that the long-continued

selection of the best individuals in successive generations made

tumblers what they now are; and near Glasgow there are house-tumblers,

as I hear from Mr. Brent, which cannot fly eighteen inches high without

going head over heels. It may be doubted whether any one would have

thought of training a dog to point, had not some one dog naturally shown

a tendency in this line; and this is known occasionally to happen, as I

once saw in a pure terrier. When the first tendency was once displayed,

methodical selection and the inherited effects of compulsory training in

each successive generation would soon complete the work; and unconscious

selection is still at work, as each man tries to procure, without

intending to improve the breed, dogs which will stand and hunt best.

On the other hand, habit alone in some cases has sufficed; no animal is

more difficult to tame than the young of the wild rabbit; scarcely any

animal is tamer than the young of the tame rabbit; but I do not suppose

that domestic rabbits have ever been selected for tameness; and I

presume that we must attribute the whole of the inherited change from

extreme wildness to extreme tameness, simply to habit and long-continued

close confinement.

Natural instincts are lost under domestication: a remarkable instance of

this is seen in those breeds of fowls which very rarely or never become

"broody," that is, never wish to sit on their eggs. Familiarity alone

prevents our seeing how universally and largely the minds of our

domestic animals have been modified by domestication. It is scarcely

possible to doubt that the love of man has become instinctive in the

dog. All wolves, foxes, jackals, and species of the cat genus, when

kept tame, are most eager to attack poultry, sheep, and pigs; and this

tendency has been found incurable in dogs which have been brought home

as puppies from countries, such as Tierra del Fuego and Australia, where

the savages do not keep these domestic animals. How rarely, on the other

hand, do our civilised dogs, even when quite young, require to be taught

not to attack poultry, sheep, and pigs! No doubt they occasionally

do make an attack, and are then beaten; and if not cured, they are

destroyed; so that habit, with some degree of selection, has probably

concurred in civilising by inheritance our dogs. On the other hand,

young chickens have lost, wholly by habit, that fear of the dog and cat

which no doubt was originally instinctive in them, in the same way as it

is so plainly instinctive in young pheasants, though reared under a hen.

It is not that chickens have lost all fear, but fear only of dogs and

cats, for if the hen gives the danger-chuckle, they will run (more

especially young turkeys) from under her, and conceal themselves in

the surrounding grass or thickets; and this is evidently done for the

instinctive purpose of allowing, as we see in wild ground-birds, their

mother to fly away. But this instinct retained by our chickens has

become useless under domestication, for the mother-hen has almost lost

by disuse the power of flight.

Hence, we may conclude, that domestic instincts have been acquired and

natural instincts have been lost partly by habit, and partly by man

selecting and accumulating during successive generations, peculiar

mental habits and actions, which at first appeared from what we must in

our ignorance call an accident. In some cases compulsory habit alone

has sufficed to produce such inherited mental changes; in other cases

compulsory habit has done nothing, and all has been the result of

selection, pursued both methodically and unconsciously; but in most

cases, probably, habit and selection have acted together.

We shall, perhaps, best understand how instincts in a state of nature

have become modified by selection, by considering a few cases. I will

select only three, out of the several which I shall have to discuss in

my future work,--namely, the instinct which leads the cuckoo to lay her

eggs in other birds' nests; the slave-making instinct of certain ants;

and the comb-making power of the hive-bee: these two latter instincts

have generally, and most justly, been ranked by naturalists as the most

wonderful of all known instincts.

It is now commonly admitted that the more immediate and final cause

of the cuckoo's instinct is, that she lays her eggs, not daily, but at

intervals of two or three days; so that, if she were to make her own

nest and sit on her own eggs, those first laid would have to be left

for some time unincubated, or there would be eggs and young birds of

different ages in the same nest. If this were the case, the process of

laying and hatching might be inconveniently long, more especially as she

has to migrate at a very early period; and the first hatched young would

probably have to be fed by the male alone. But the American cuckoo is

in this predicament; for she makes her own nest and has eggs and young

successively hatched, all at the same time. It has been asserted that

the American cuckoo occasionally lays her eggs in other birds' nests;

but I hear on the high authority of Dr. Brewer, that this is a mistake.

Nevertheless, I could give several instances of various birds which have

been known occasionally to lay their eggs in other birds' nests. Now let

us suppose that the ancient progenitor of our European cuckoo had the

habits of the American cuckoo; but that occasionally she laid an egg in

another bird's nest. If the old bird profited by this occasional habit,

or if the young were made more vigorous by advantage having been taken

of the mistaken maternal instinct of another bird, than by their own

mother's care, encumbered as she can hardly fail to be by having eggs

and young of different ages at the same time; then the old birds or the

fostered young would gain an advantage. And analogy would lead me

to believe, that the young thus reared would be apt to follow by

inheritance the occasional and aberrant habit of their mother, and in

their turn would be apt to lay their eggs in other birds' nests, and

thus be successful in rearing their young. By a continued process of

this nature, I believe that the strange instinct of our cuckoo could be,

and has been, generated. I may add that, according to Dr. Gray and

to some other observers, the European cuckoo has not utterly lost all

maternal love and care for her own offspring.

The occasional habit of birds laying their eggs in other birds' nests,

either of the same or of a distinct species, is not very uncommon with

the Gallinaceae; and this perhaps explains the origin of a singular

instinct in the allied group of ostriches. For several hen ostriches,

at least in the case of the American species, unite and lay first a

few eggs in one nest and then in another; and these are hatched by the

males. This instinct may probably be accounted for by the fact of the

hens laying a large number of eggs; but, as in the case of the cuckoo,

at intervals of two or three days. This instinct, however, of the

American ostrich has not as yet been perfected; for a surprising number

of eggs lie strewed over the plains, so that in one day's hunting I

picked up no less than twenty lost and wasted eggs.

Many bees are parasitic, and always lay their eggs in the nests of bees

of other kinds. This case is more remarkable than that of the cuckoo;

for these bees have not only their instincts but their structure

modified in accordance with their parasitic habits; for they do not

possess the pollen-collecting apparatus which would be necessary if

they had to store food for their own young. Some species, likewise, of

Sphegidae (wasp-like insects) are parasitic on other species; and M.

Fabre has lately shown good reason for believing that although the

Tachytes nigra generally makes its own burrow and stores it with

paralysed prey for its own larvae to feed on, yet that when this insect

finds a burrow already made and stored by another sphex, it takes

advantage of the prize, and becomes for the occasion parasitic. In this

case, as with the supposed case of the cuckoo, I can see no difficulty

in natural selection making an occasional habit permanent, if of

advantage to the species, and if the insect whose nest and stored food

are thus feloniously appropriated, be not thus exterminated.

SLAVE-MAKING INSTINCT.

This remarkable instinct was first discovered in the Formica (Polyerges)

rufescens by Pierre Huber, a better observer even than his celebrated

father. This ant is absolutely dependent on its slaves; without their

aid, the species would certainly become extinct in a single year. The

males and fertile females do no work. The workers or sterile females,

though most energetic and courageous in capturing slaves, do no other

work. They are incapable of making their own nests, or of feeding their

own larvae. When the old nest is found inconvenient, and they have to

migrate, it is the slaves which determine the migration, and actually

carry their masters in their jaws. So utterly helpless are the masters,

that when Huber shut up thirty of them without a slave, but with plenty

of the food which they like best, and with their larvae and pupae to

stimulate them to work, they did nothing; they could not even feed

themselves, and many perished of hunger. Huber then introduced a single

slave (F. fusca), and she instantly set to work, fed and saved the

survivors; made some cells and tended the larvae, and put all to rights.

What can be more extraordinary than these well-ascertained facts? If we

had not known of any other slave-making ant, it would have been

hopeless to have speculated how so wonderful an instinct could have been

perfected.

Formica sanguinea was likewise first discovered by P. Huber to be

a slave-making ant. This species is found in the southern parts of

England, and its habits have been attended to by Mr. F. Smith, of the

British Museum, to whom I am much indebted for information on this and

other subjects. Although fully trusting to the statements of Huber and

Mr. Smith, I tried to approach the subject in a sceptical frame of

mind, as any one may well be excused for doubting the truth of so

extraordinary and odious an instinct as that of making slaves. Hence

I will give the observations which I have myself made, in some little

detail. I opened fourteen nests of F. sanguinea, and found a few slaves

in all. Males and fertile females of the slave-species are found only in

their own proper communities, and have never been observed in the nests

of F. sanguinea. The slaves are black and not above half the size of

their red masters, so that the contrast in their appearance is very

great. When the nest is slightly disturbed, the slaves occasionally come

out, and like their masters are much agitated and defend the nest: when

the nest is much disturbed and the larvae and pupae are exposed, the

slaves work energetically with their masters in carrying them away to a

place of safety. Hence, it is clear, that the slaves feel quite at home.

During the months of June and July, on three successive years, I have

watched for many hours several nests in Surrey and Sussex, and never

saw a slave either leave or enter a nest. As, during these months,

the slaves are very few in number, I thought that they might behave

differently when more numerous; but Mr. Smith informs me that he has

watched the nests at various hours during May, June and August, both in

Surrey and Hampshire, and has never seen the slaves, though present

in large numbers in August, either leave or enter the nest. Hence he

considers them as strictly household slaves. The masters, on the other

hand, may be constantly seen bringing in materials for the nest, and

food of all kinds. During the present year, however, in the month of

July, I came across a community with an unusually large stock of slaves,

and I observed a few slaves mingled with their masters leaving the nest,

and marching along the same road to a tall Scotch-fir-tree, twenty-five

yards distant, which they ascended together, probably in search of

aphides or cocci. According to Huber, who had ample opportunities

for observation, in Switzerland the slaves habitually work with their

masters in making the nest, and they alone open and close the doors in

the morning and evening; and, as Huber expressly states, their principal

office is to search for aphides. This difference in the usual habits of

the masters and slaves in the two countries, probably depends merely

on the slaves being captured in greater numbers in Switzerland than in

England.

One day I fortunately chanced to witness a migration from one nest to

another, and it was a most interesting spectacle to behold the masters

carefully carrying, as Huber has described, their slaves in their jaws.

Another day my attention was struck by about a score of the slave-makers

haunting the same spot, and evidently not in search of food; they

approached and were vigorously repulsed by an independent community of

the slave species (F. fusca); sometimes as many as three of these

ants clinging to the legs of the slave-making F. sanguinea. The latter

ruthlessly killed their small opponents, and carried their dead

bodies as food to their nest, twenty-nine yards distant; but they were

prevented from getting any pupae to rear as slaves. I then dug up a

small parcel of the pupae of F. fusca from another nest, and put them

down on a bare spot near the place of combat; they were eagerly seized,

and carried off by the tyrants, who perhaps fancied that, after all,

they had been victorious in their late combat.

At the same time I laid on the same place a small parcel of the pupae of

another species, F. flava, with a few of these little yellow ants still

clinging to the fragments of the nest. This species is sometimes, though

rarely, made into slaves, as has been described by Mr. Smith.

Although so small a species, it is very courageous, and I have seen it

ferociously attack other ants. In one instance I found to my surprise

an independent community of F. flava under a stone beneath a nest of the

slave-making F. sanguinea; and when I had accidentally disturbed both

nests, the little ants attacked their big neighbours with surprising

courage. Now I was curious to ascertain whether F. sanguinea could

distinguish the pupae of F. fusca, which they habitually make into

slaves, from those of the little and furious F. flava, which they rarely

capture, and it was evident that they did at once distinguish them:

for we have seen that they eagerly and instantly seized the pupae of F.

fusca, whereas they were much terrified when they came across the pupae,

or even the earth from the nest of F. flava, and quickly ran away; but

in about a quarter of an hour, shortly after all the little yellow ants

had crawled away, they took heart and carried off the pupae.

One evening I visited another community of F. sanguinea, and found a

number of these ants entering their nest, carrying the dead bodies of F.

fusca (showing that it was not a migration) and numerous pupae. I traced

the returning file burthened with booty, for about forty yards, to

a very thick clump of heath, whence I saw the last individual of F.

sanguinea emerge, carrying a pupa; but I was not able to find the

desolated nest in the thick heath. The nest, however, must have been

close at hand, for two or three individuals of F. fusca were rushing

about in the greatest agitation, and one was perched motionless with its

own pupa in its mouth on the top of a spray of heath over its ravaged

home.

Such are the facts, though they did not need confirmation by me, in

regard to the wonderful instinct of making slaves. Let it be observed

what a contrast the instinctive habits of F. sanguinea present with

those of the F. rufescens. The latter does not build its own nest, does

not determine its own migrations, does not collect food for itself or

its young, and cannot even feed itself: it is absolutely dependent on

its numerous slaves. Formica sanguinea, on the other hand, possesses

much fewer slaves, and in the early part of the summer extremely few.

The masters determine when and where a new nest shall be formed, and

when they migrate, the masters carry the slaves. Both in Switzerland and

England the slaves seem to have the exclusive care of the larvae, and

the masters alone go on slave-making expeditions. In Switzerland the

slaves and masters work together, making and bringing materials for the

nest: both, but chiefly the slaves, tend, and milk as it may be called,

their aphides; and thus both collect food for the community. In England

the masters alone usually leave the nest to collect building materials

and food for themselves, their slaves and larvae. So that the masters in

this country receive much less service from their slaves than they do in

Switzerland.

By what steps the instinct of F. sanguinea originated I will not pretend

to conjecture. But as ants, which are not slave-makers, will, as I have

seen, carry off pupae of other species, if scattered near their nests,

it is possible that pupae originally stored as food might become

developed; and the ants thus unintentionally reared would then follow

their proper instincts, and do what work they could. If their presence

proved useful to the species which had seized them--if it were more

advantageous to this species to capture workers than to procreate

them--the habit of collecting pupae originally for food might by natural

selection be strengthened and rendered permanent for the very different

purpose of raising slaves. When the instinct was once acquired, if

carried out to a much less extent even than in our British F. sanguinea,

which, as we have seen, is less aided by its slaves than the same

species in Switzerland, I can see no difficulty in natural selection

increasing and modifying the instinct--always supposing each

modification to be of use to the species--until an ant was formed as

abjectly dependent on its slaves as is the Formica rufescens.

CELL-MAKING INSTINCT OF THE HIVE-BEE.

I will not here enter on minute details on this subject, but will merely

give an outline of the conclusions at which I have arrived. He must be

a dull man who can examine the exquisite structure of a comb, so

beautifully adapted to its end, without enthusiastic admiration. We

hear from mathematicians that bees have practically solved a recondite

problem, and have made their cells of the proper shape to hold the

greatest possible amount of honey, with the least possible consumption

of precious wax in their construction. It has been remarked that a

skilful workman, with fitting tools and measures, would find it

very difficult to make cells of wax of the true form, though this is

perfectly effected by a crowd of bees working in a dark hive. Grant

whatever instincts you please, and it seems at first quite inconceivable

how they can make all the necessary angles and planes, or even perceive

when they are correctly made. But the difficulty is not nearly so great

as it at first appears: all this beautiful work can be shown, I think,

to follow from a few very simple instincts.

I was led to investigate this subject by Mr. Waterhouse, who has shown

that the form of the cell stands in close relation to the presence of

adjoining cells; and the following view may, perhaps, be considered only

as a modification of his theory. Let us look to the great principle of

gradation, and see whether Nature does not reveal to us her method of

work. At one end of a short series we have humble-bees, which use their

old cocoons to hold honey, sometimes adding to them short tubes of wax,

and likewise making separate and very irregular rounded cells of wax. At

the other end of the series we have the cells of the hive-bee, placed in

a double layer: each cell, as is well known, is an hexagonal prism, with

the basal edges of its six sides bevelled so as to join on to a pyramid,

formed of three rhombs. These rhombs have certain angles, and the three

which form the pyramidal base of a single cell on one side of the comb,

enter into the composition of the bases of three adjoining cells on the

opposite side. In the series between the extreme perfection of the cells

of the hive-bee and the simplicity of those of the humble-bee, we have

the cells of the Mexican Melipona domestica, carefully described

and figured by Pierre Huber. The Melipona itself is intermediate in

structure between the hive and humble bee, but more nearly related to

the latter: it forms a nearly regular waxen comb of cylindrical cells,

in which the young are hatched, and, in addition, some large cells of

wax for holding honey. These latter cells are nearly spherical and of

nearly equal sizes, and are aggregated into an irregular mass. But the

important point to notice, is that these cells are always made at that

degree of nearness to each other, that they would have intersected or

broken into each other, if the spheres had been completed; but this is

never permitted, the bees building perfectly flat walls of wax between

the spheres which thus tend to intersect. Hence each cell consists of

an outer spherical portion and of two, three, or more perfectly flat

surfaces, according as the cell adjoins two, three or more other cells.

When one cell comes into contact with three other cells, which, from

the spheres being nearly of the same size, is very frequently and

necessarily the case, the three flat surfaces are united into a pyramid;

and this pyramid, as Huber has remarked, is manifestly a gross imitation

of the three-sided pyramidal basis of the cell of the hive-bee. As in

the cells of the hive-bee, so here, the three plane surfaces in any one

cell necessarily enter into the construction of three adjoining cells.

It is obvious that the Melipona saves wax by this manner of building;

for the flat walls between the adjoining cells are not double, but are

of the same thickness as the outer spherical portions, and yet each flat

portion forms a part of two cells.

Reflecting on this case, it occurred to me that if the Melipona had made

its spheres at some given distance from each other, and had made them of

equal sizes and had arranged them symmetrically in a double layer, the

resulting structure would probably have been as perfect as the comb of

the hive-bee. Accordingly I wrote to Professor Miller, of Cambridge,

and this geometer has kindly read over the following statement, drawn up

from his information, and tells me that it is strictly correct:--

If a number of equal spheres be described with their centres placed in

two parallel layers; with the centre of each sphere at the distance of

radius x the square root of 2 or radius x 1.41421 (or at some lesser

distance), from the centres of the six surrounding spheres in the

same layer; and at the same distance from the centres of the adjoining

spheres in the other and parallel layer; then, if planes of intersection

between the several spheres in both layers be formed, there will result

a double layer of hexagonal prisms united together by pyramidal bases

formed of three rhombs; and the rhombs and the sides of the hexagonal

prisms will have every angle identically the same with the best

measurements which have been made of the cells of the hive-bee.

Hence we may safely conclude that if we could slightly modify the

instincts already possessed by the Melipona, and in themselves not very

wonderful, this bee would make a structure as wonderfully perfect as

that of the hive-bee. We must suppose the Melipona to make her cells

truly spherical, and of equal sizes; and this would not be very

surprising, seeing that she already does so to a certain extent, and

seeing what perfectly cylindrical burrows in wood many insects can

make, apparently by turning round on a fixed point. We must suppose the

Melipona to arrange her cells in level layers, as she already does her

cylindrical cells; and we must further suppose, and this is the greatest

difficulty, that she can somehow judge accurately at what distance to

stand from her fellow-labourers when several are making their spheres;

but she is already so far enabled to judge of distance, that she always

describes her spheres so as to intersect largely; and then she unites

the points of intersection by perfectly flat surfaces. We have further

to suppose, but this is no difficulty, that after hexagonal prisms have

been formed by the intersection of adjoining spheres in the same layer,

she can prolong the hexagon to any length requisite to hold the stock of

honey; in the same way as the rude humble-bee adds cylinders of wax

to the circular mouths of her old cocoons. By such modifications of

instincts in themselves not very wonderful,--hardly more wonderful than

those which guide a bird to make its nest,--I believe that the hive-bee

has acquired, through natural selection, her inimitable architectural

powers.

But this theory can be tested by experiment. Following the example of

Mr. Tegetmeier, I separated two combs, and put between them a long,

thick, square strip of wax: the bees instantly began to excavate minute

circular pits in it; and as they deepened these little pits, they made

them wider and wider until they were converted into shallow basins,

appearing to the eye perfectly true or parts of a sphere, and of about

the diameter of a cell. It was most interesting to me to observe that

wherever several bees had begun to excavate these basins near together,

they had begun their work at such a distance from each other, that by

the time the basins had acquired the above stated width (i.e. about the

width of an ordinary cell), and were in depth about one sixth of the

diameter of the sphere of which they formed a part, the rims of the

basins intersected or broke into each other. As soon as this occurred,

the bees ceased to excavate, and began to build up flat walls of wax

on the lines of intersection between the basins, so that each hexagonal

prism was built upon the festooned edge of a smooth basin, instead of on

the straight edges of a three-sided pyramid as in the case of ordinary

cells.

I then put into the hive, instead of a thick, square piece of wax, a

thin and narrow, knife-edged ridge, coloured with vermilion. The bees

instantly began on both sides to excavate little basins near to each

other, in the same way as before; but the ridge of wax was so thin, that

the bottoms of the basins, if they had been excavated to the same depth

as in the former experiment, would have broken into each other from the

opposite sides. The bees, however, did not suffer this to happen, and

they stopped their excavations in due time; so that the basins, as soon

as they had been a little deepened, came to have flat bottoms; and these

flat bottoms, formed by thin little plates of the vermilion wax having

been left ungnawed, were situated, as far as the eye could judge,

exactly along the planes of imaginary intersection between the basins on

the opposite sides of the ridge of wax. In parts, only little bits, in

other parts, large portions of a rhombic plate had been left between the

opposed basins, but the work, from the unnatural state of things, had

not been neatly performed. The bees must have worked at very nearly the

same rate on the opposite sides of the ridge of vermilion wax, as they

circularly gnawed away and deepened the basins on both sides, in order

to have succeeded in thus leaving flat plates between the basins, by

stopping work along the intermediate planes or planes of intersection.

Considering how flexible thin wax is, I do not see that there is any

difficulty in the bees, whilst at work on the two sides of a strip

of wax, perceiving when they have gnawed the wax away to the proper

thinness, and then stopping their work. In ordinary combs it has

appeared to me that the bees do not always succeed in working at exactly

the same rate from the opposite sides; for I have noticed half-completed

rhombs at the base of a just-commenced cell, which were slightly concave

on one side, where I suppose that the bees had excavated too quickly,

and convex on the opposed side, where the bees had worked less quickly.

In one well-marked instance, I put the comb back into the hive, and

allowed the bees to go on working for a short time, and again examined

the cell, and I found that the rhombic plate had been completed, and had

become PERFECTLY FLAT: it was absolutely impossible, from the extreme

thinness of the little rhombic plate, that they could have effected this

by gnawing away the convex side; and I suspect that the bees in such

cases stand in the opposed cells and push and bend the ductile and warm

wax (which as I have tried is easily done) into its proper intermediate

plane, and thus flatten it.

From the experiment of the ridge of vermilion wax, we can clearly see

that if the bees were to build for themselves a thin wall of wax, they

could make their cells of the proper shape, by standing at the proper

distance from each other, by excavating at the same rate, and by

endeavouring to make equal spherical hollows, but never allowing the

spheres to break into each other. Now bees, as may be clearly seen by

examining the edge of a growing comb, do make a rough, circumferential

wall or rim all round the comb; and they gnaw into this from the

opposite sides, always working circularly as they deepen each cell. They

do not make the whole three-sided pyramidal base of any one cell at the

same time, but only the one rhombic plate which stands on the extreme

growing margin, or the two plates, as the case may be; and they never

complete the upper edges of the rhombic plates, until the hexagonal

walls are commenced. Some of these statements differ from those made by

the justly celebrated elder Huber, but I am convinced of their accuracy;

and if I had space, I could show that they are conformable with my

theory.

Huber's statement that the very first cell is excavated out of a little

parallel-sided wall of wax, is not, as far as I have seen, strictly

correct; the first commencement having always been a little hood of wax;

but I will not here enter on these details. We see how important a part

excavation plays in the construction of the cells; but it would be a

great error to suppose that the bees cannot build up a rough wall of wax

in the proper position--that is, along the plane of intersection between

two adjoining spheres. I have several specimens showing clearly that

they can do this. Even in the rude circumferential rim or wall of wax

round a growing comb, flexures may sometimes be observed, corresponding

in position to the planes of the rhombic basal plates of future cells.

But the rough wall of wax has in every case to be finished off, by being

largely gnawed away on both sides. The manner in which the bees build is

curious; they always make the first rough wall from ten to twenty times

thicker than the excessively thin finished wall of the cell, which will

ultimately be left. We shall understand how they work, by supposing

masons first to pile up a broad ridge of cement, and then to begin

cutting it away equally on both sides near the ground, till a smooth,

very thin wall is left in the middle; the masons always piling up the

cut-away cement, and adding fresh cement, on the summit of the ridge. We

shall thus have a thin wall steadily growing upward; but always crowned

by a gigantic coping. From all the cells, both those just commenced and

those completed, being thus crowned by a strong coping of wax, the

bees can cluster and crawl over the comb without injuring the delicate

hexagonal walls, which are only about one four-hundredth of an inch in

thickness; the plates of the pyramidal basis being about twice as thick.

By this singular manner of building, strength is continually given to

the comb, with the utmost ultimate economy of wax.

It seems at first to add to the difficulty of understanding how the

cells are made, that a multitude of bees all work together; one bee

after working a short time at one cell going to another, so that, as

Huber has stated, a score of individuals work even at the commencement

of the first cell. I was able practically to show this fact, by covering

the edges of the hexagonal walls of a single cell, or the extreme margin

of the circumferential rim of a growing comb, with an extremely thin

layer of melted vermilion wax; and I invariably found that the colour

was most delicately diffused by the bees--as delicately as a painter

could have done with his brush--by atoms of the coloured wax having been

taken from the spot on which it had been placed, and worked into the

growing edges of the cells all round. The work of construction seems

to be a sort of balance struck between many bees, all instinctively

standing at the same relative distance from each other, all trying to

sweep equal spheres, and then building up, or leaving ungnawed, the

planes of intersection between these spheres. It was really curious to

note in cases of difficulty, as when two pieces of comb met at an angle,

how often the bees would entirely pull down and rebuild in different

ways the same cell, sometimes recurring to a shape which they had at

first rejected.

When bees have a place on which they can stand in their proper positions

for working,--for instance, on a slip of wood, placed directly under the

middle of a comb growing downwards so that the comb has to be built over

one face of the slip--in this case the bees can lay the foundations

of one wall of a new hexagon, in its strictly proper place, projecting

beyond the other completed cells. It suffices that the bees should be

enabled to stand at their proper relative distances from each other

and from the walls of the last completed cells, and then, by striking

imaginary spheres, they can build up a wall intermediate between two

adjoining spheres; but, as far as I have seen, they never gnaw away and

finish off the angles of a cell till a large part both of that cell and

of the adjoining cells has been built. This capacity in bees of laying

down under certain circumstances a rough wall in its proper place

between two just-commenced cells, is important, as it bears on a fact,

which seems at first quite subversive of the foregoing theory; namely,

that the cells on the extreme margin of wasp-combs are sometimes

strictly hexagonal; but I have not space here to enter on this subject.

Nor does there seem to me any great difficulty in a single insect (as

in the case of a queen-wasp) making hexagonal cells, if she work

alternately on the inside and outside of two or three cells commenced at

the same time, always standing at the proper relative distance from

the parts of the cells just begun, sweeping spheres or cylinders, and

building up intermediate planes. It is even conceivable that an insect

might, by fixing on a point at which to commence a cell, and then moving

outside, first to one point, and then to five other points, at the

proper relative distances from the central point and from each other,

strike the planes of intersection, and so make an isolated hexagon: but

I am not aware that any such case has been observed; nor would any good

be derived from a single hexagon being built, as in its construction

more materials would be required than for a cylinder.

As natural selection acts only by the accumulation of slight

modifications of structure or instinct, each profitable to the

individual under its conditions of life, it may reasonably be asked, how

a long and graduated succession of modified architectural instincts,

all tending towards the present perfect plan of construction, could

have profited the progenitors of the hive-bee? I think the answer is

not difficult: it is known that bees are often hard pressed to get

sufficient nectar; and I am informed by Mr. Tegetmeier that it has been

experimentally found that no less than from twelve to fifteen pounds of

dry sugar are consumed by a hive of bees for the secretion of each pound

of wax; so that a prodigious quantity of fluid nectar must be collected

and consumed by the bees in a hive for the secretion of the wax

necessary for the construction of their combs. Moreover, many bees have

to remain idle for many days during the process of secretion. A large

store of honey is indispensable to support a large stock of bees during

the winter; and the security of the hive is known mainly to depend on a

large number of bees being supported. Hence the saving of wax by largely

saving honey must be a most important element of success in any family

of bees. Of course the success of any species of bee may be dependent

on the number of its parasites or other enemies, or on quite distinct

causes, and so be altogether independent of the quantity of honey which

the bees could collect. But let us suppose that this latter circumstance

determined, as it probably often does determine, the numbers of a

humble-bee which could exist in a country; and let us further suppose

that the community lived throughout the winter, and consequently

required a store of honey: there can in this case be no doubt that it

would be an advantage to our humble-bee, if a slight modification of

her instinct led her to make her waxen cells near together, so as to

intersect a little; for a wall in common even to two adjoining cells,

would save some little wax. Hence it would continually be more and more

advantageous to our humble-bee, if she were to make her cells more and

more regular, nearer together, and aggregated into a mass, like the

cells of the Melipona; for in this case a large part of the bounding

surface of each cell would serve to bound other cells, and much wax

would be saved. Again, from the same cause, it would be advantageous to

the Melipona, if she were to make her cells closer together, and more

regular in every way than at present; for then, as we have seen, the

spherical surfaces would wholly disappear, and would all be replaced by

plane surfaces; and the Melipona would make a comb as perfect as that of

the hive-bee. Beyond this stage of perfection in architecture, natural

selection could not lead; for the comb of the hive-bee, as far as we can

see, is absolutely perfect in economising wax.

Thus, as I believe, the most wonderful of all known instincts, that

of the hive-bee, can be explained by natural selection having taken

advantage of numerous, successive, slight modifications of simpler

instincts; natural selection having by slow degrees, more and more

perfectly, led the bees to sweep equal spheres at a given distance from

each other in a double layer, and to build up and excavate the wax along

the planes of intersection. The bees, of course, no more knowing that

they swept their spheres at one particular distance from each other,

than they know what are the several angles of the hexagonal prisms and

of the basal rhombic plates. The motive power of the process of natural

selection having been economy of wax; that individual swarm which wasted

least honey in the secretion of wax, having succeeded best, and having

transmitted by inheritance its newly acquired economical instinct to new

swarms, which in their turn will have had the best chance of succeeding

in the struggle for existence.

No doubt many instincts of very difficult explanation could be opposed

to the theory of natural selection,--cases, in which we cannot see

how an instinct could possibly have originated; cases, in which no

intermediate gradations are known to exist; cases of instinct of

apparently such trifling importance, that they could hardly have been

acted on by natural selection; cases of instincts almost identically the

same in animals so remote in the scale of nature, that we cannot account

for their similarity by inheritance from a common parent, and must

therefore believe that they have been acquired by independent acts of

natural selection. I will not here enter on these several cases, but

will confine myself to one special difficulty, which at first appeared

to me insuperable, and actually fatal to my whole theory. I allude to

the neuters or sterile females in insect-communities: for these neuters

often differ widely in instinct and in structure from both the males

and fertile females, and yet, from being sterile, they cannot propagate

their kind.

The subject well deserves to be discussed at great length, but I will

here take only a single case, that of working or sterile ants. How the

workers have been rendered sterile is a difficulty; but not much greater

than that of any other striking modification of structure; for it can

be shown that some insects and other articulate animals in a state of

nature occasionally become sterile; and if such insects had been social,

and it had been profitable to the community that a number should have

been annually born capable of work, but incapable of procreation, I

can see no very great difficulty in this being effected by natural

selection. But I must pass over this preliminary difficulty. The great

difficulty lies in the working ants differing widely from both the males

and the fertile females in structure, as in the shape of the thorax and

in being destitute of wings and sometimes of eyes, and in instinct. As

far as instinct alone is concerned, the prodigious difference in this

respect between the workers and the perfect females, would have been

far better exemplified by the hive-bee. If a working ant or other

neuter insect had been an animal in the ordinary state, I should have

unhesitatingly assumed that all its characters had been slowly acquired

through natural selection; namely, by an individual having been born

with some slight profitable modification of structure, this being

inherited by its offspring, which again varied and were again selected,

and so onwards. But with the working ant we have an insect differing

greatly from its parents, yet absolutely sterile; so that it could never

have transmitted successively acquired modifications of structure or

instinct to its progeny. It may well be asked how is it possible to

reconcile this case with the theory of natural selection?

First, let it be remembered that we have innumerable instances, both in

our domestic productions and in those in a state of nature, of all sorts

of differences of structure which have become correlated to certain

ages, and to either sex. We have differences correlated not only to

one sex, but to that short period alone when the reproductive system is

active, as in the nuptial plumage of many birds, and in the hooked jaws

of the male salmon. We have even slight differences in the horns of

different breeds of cattle in relation to an artificially imperfect

state of the male sex; for oxen of certain breeds have longer horns than

in other breeds, in comparison with the horns of the bulls or cows of

these same breeds. Hence I can see no real difficulty in any character

having become correlated with the sterile condition of certain members

of insect-communities: the difficulty lies in understanding how such

correlated modifications of structure could have been slowly accumulated

by natural selection.

This difficulty, though appearing insuperable, is lessened, or, as I

believe, disappears, when it is remembered that selection may be applied

to the family, as well as to the individual, and may thus gain the

desired end. Thus, a well-flavoured vegetable is cooked, and the

individual is destroyed; but the horticulturist sows seeds of the same

stock, and confidently expects to get nearly the same variety; breeders

of cattle wish the flesh and fat to be well marbled together; the animal

has been slaughtered, but the breeder goes with confidence to the same

family. I have such faith in the powers of selection, that I do not

doubt that a breed of cattle, always yielding oxen with extraordinarily

long horns, could be slowly formed by carefully watching which

individual bulls and cows, when matched, produced oxen with the longest

horns; and yet no one ox could ever have propagated its kind. Thus

I believe it has been with social insects: a slight modification of

structure, or instinct, correlated with the sterile condition of certain

members of the community, has been advantageous to the community:

consequently the fertile males and females of the same community

flourished, and transmitted to their fertile offspring a tendency to

produce sterile members having the same modification. And I believe

that this process has been repeated, until that prodigious amount of

difference between the fertile and sterile females of the same species

has been produced, which we see in many social insects.

But we have not as yet touched on the climax of the difficulty; namely,

the fact that the neuters of several ants differ, not only from the

fertile females and males, but from each other, sometimes to an almost

incredible degree, and are thus divided into two or even three castes.

The castes, moreover, do not generally graduate into each other, but are

perfectly well defined; being as distinct from each other, as are any

two species of the same genus, or rather as any two genera of the same

family. Thus in Eciton, there are working and soldier neuters, with jaws

and instincts extraordinarily different: in Cryptocerus, the workers of

one caste alone carry a wonderful sort of shield on their heads, the use

of which is quite unknown: in the Mexican Myrmecocystus, the workers of

one caste never leave the nest; they are fed by the workers of another

caste, and they have an enormously developed abdomen which secretes a

sort of honey, supplying the place of that excreted by the aphides, or

the domestic cattle as they may be called, which our European ants guard

or imprison.

It will indeed be thought that I have an overweening confidence in the

principle of natural selection, when I do not admit that such wonderful

and well-established facts at once annihilate my theory. In the simpler

case of neuter insects all of one caste or of the same kind, which have

been rendered by natural selection, as I believe to be quite possible,

different from the fertile males and females,--in this case, we may

safely conclude from the analogy of ordinary variations, that each

successive, slight, profitable modification did not probably at first

appear in all the individual neuters in the same nest, but in a few

alone; and that by the long-continued selection of the fertile parents

which produced most neuters with the profitable modification, all the

neuters ultimately came to have the desired character. On this view we

ought occasionally to find neuter-insects of the same species, in the

same nest, presenting gradations of structure; and this we do find,

even often, considering how few neuter-insects out of Europe have been

carefully examined. Mr. F. Smith has shown how surprisingly the neuters

of several British ants differ from each other in size and sometimes

in colour; and that the extreme forms can sometimes be perfectly linked

together by individuals taken out of the same nest: I have myself

compared perfect gradations of this kind. It often happens that the

larger or the smaller sized workers are the most numerous; or that both

large and small are numerous, with those of an intermediate size scanty

in numbers. Formica flava has larger and smaller workers, with some of

intermediate size; and, in this species, as Mr. F. Smith has observed,

the larger workers have simple eyes (ocelli), which though small can

be plainly distinguished, whereas the smaller workers have their ocelli

rudimentary. Having carefully dissected several specimens of these

workers, I can affirm that the eyes are far more rudimentary in the

smaller workers than can be accounted for merely by their proportionally

lesser size; and I fully believe, though I dare not assert so

positively, that the workers of intermediate size have their ocelli in

an exactly intermediate condition. So that we here have two bodies of

sterile workers in the same nest, differing not only in size, but

in their organs of vision, yet connected by some few members in an

intermediate condition. I may digress by adding, that if the smaller

workers had been the most useful to the community, and those males and

females had been continually selected, which produced more and more

of the smaller workers, until all the workers had come to be in this

condition; we should then have had a species of ant with neuters very

nearly in the same condition with those of Myrmica. For the workers of

Myrmica have not even rudiments of ocelli, though the male and female

ants of this genus have well-developed ocelli.

I may give one other case: so confidently did I expect to find

gradations in important points of structure between the different castes

of neuters in the same species, that I gladly availed myself of Mr. F.

Smith's offer of numerous specimens from the same nest of the driver

ant (Anomma) of West Africa. The reader will perhaps best appreciate

the amount of difference in these workers, by my giving not the actual

measurements, but a strictly accurate illustration: the difference was

the same as if we were to see a set of workmen building a house of whom

many were five feet four inches high, and many sixteen feet high; but

we must suppose that the larger workmen had heads four instead of three

times as big as those of the smaller men, and jaws nearly five times

as big. The jaws, moreover, of the working ants of the several sizes

differed wonderfully in shape, and in the form and number of the teeth.

But the important fact for us is, that though the workers can be grouped

into castes of different sizes, yet they graduate insensibly into each

other, as does the widely-different structure of their jaws. I speak

confidently on this latter point, as Mr. Lubbock made drawings for

me with the camera lucida of the jaws which I had dissected from the

workers of the several sizes.

With these facts before me, I believe that natural selection, by acting

on the fertile parents, could form a species which should regularly

produce neuters, either all of large size with one form of jaw, or all

of small size with jaws having a widely different structure; or lastly,

and this is our climax of difficulty, one set of workers of one size and

structure, and simultaneously another set of workers of a different size

and structure;--a graduated series having been first formed, as in the

case of the driver ant, and then the extreme forms, from being the most

useful to the community, having been produced in greater and greater

numbers through the natural selection of the parents which generated

them; until none with an intermediate structure were produced.

Thus, as I believe, the wonderful fact of two distinctly defined castes

of sterile workers existing in the same nest, both widely different from

each other and from their parents, has originated. We can see how useful

their production may have been to a social community of insects, on the

same principle that the division of labour is useful to civilised man.

As ants work by inherited instincts and by inherited tools or weapons,

and not by acquired knowledge and manufactured instruments, a perfect

division of labour could be effected with them only by the workers being

sterile; for had they been fertile, they would have intercrossed, and

their instincts and structure would have become blended. And nature

has, as I believe, effected this admirable division of labour in the

communities of ants, by the means of natural selection. But I am bound

to confess, that, with all my faith in this principle, I should never

have anticipated that natural selection could have been efficient in so

high a degree, had not the case of these neuter insects convinced me

of the fact. I have, therefore, discussed this case, at some little

but wholly insufficient length, in order to show the power of natural

selection, and likewise because this is by far the most serious special

difficulty, which my theory has encountered. The case, also, is very

interesting, as it proves that with animals, as with plants, any amount

of modification in structure can be effected by the accumulation of

numerous, slight, and as we must call them accidental, variations, which

are in any manner profitable, without exercise or habit having come into

play. For no amount of exercise, or habit, or volition, in the utterly

sterile members of a community could possibly have affected the

structure or instincts of the fertile members, which alone leave

descendants. I am surprised that no one has advanced this demonstrative

case of neuter insects, against the well-known doctrine of Lamarck.

SUMMARY.

I have endeavoured briefly in this chapter to show that the mental

qualities of our domestic animals vary, and that the variations are

inherited. Still more briefly I have attempted to show that instincts

vary slightly in a state of nature. No one will dispute that instincts

are of the highest importance to each animal. Therefore I can see no

difficulty, under changing conditions of life, in natural selection

accumulating slight modifications of instinct to any extent, in any

useful direction. In some cases habit or use and disuse have probably

come into play. I do not pretend that the facts given in this chapter

strengthen in any great degree my theory; but none of the cases of

difficulty, to the best of my judgment, annihilate it. On the other

hand, the fact that instincts are not always absolutely perfect and

are liable to mistakes;--that no instinct has been produced for the

exclusive good of other animals, but that each animal takes advantage of

the instincts of others;--that the canon in natural history, of "natura

non facit saltum" is applicable to instincts as well as to corporeal

structure, and is plainly explicable on the foregoing views, but is

otherwise inexplicable,--all tend to corroborate the theory of natural

selection.

This theory is, also, strengthened by some few other facts in regard

to instincts; as by that common case of closely allied, but certainly

distinct, species, when inhabiting distant parts of the world and living

under considerably different conditions of life, yet often retaining

nearly the same instincts. For instance, we can understand on the

principle of inheritance, how it is that the thrush of South America

lines its nest with mud, in the same peculiar manner as does our British

thrush: how it is that the male wrens (Troglodytes) of North America,

build "cock-nests," to roost in, like the males of our distinct

Kitty-wrens,--a habit wholly unlike that of any other known bird.

Finally, it may not be a logical deduction, but to my imagination it

is far more satisfactory to look at such instincts as the young cuckoo

ejecting its foster-brothers,--ants making slaves,--the larvae of

ichneumonidae feeding within the live bodies of caterpillars,--not as

specially endowed or created instincts, but as small consequences of one

general law, leading to the advancement of all organic beings, namely,

multiply, vary, let the strongest live and the weakest die.

8. HYBRIDISM.

Distinction between the sterility of first crosses and of hybrids.

Sterility various in degree, not universal, affected by close

interbreeding, removed by domestication. Laws governing the sterility

of hybrids. Sterility not a special endowment, but incidental on other

differences. Causes of the sterility of first crosses and of hybrids.

Parallelism between the effects of changed conditions of life and

crossing. Fertility of varieties when crossed and of their mongrel

offspring not universal. Hybrids and mongrels compared independently of

their fertility. Summary.

The view generally entertained by naturalists is that species, when

intercrossed, have been specially endowed with the quality of sterility,

in order to prevent the confusion of all organic forms. This view

certainly seems at first probable, for species within the same country

could hardly have kept distinct had they been capable of crossing

freely. The importance of the fact that hybrids are very generally

sterile, has, I think, been much underrated by some late writers. On the

theory of natural selection the case is especially important, inasmuch

as the sterility of hybrids could not possibly be of any advantage

to them, and therefore could not have been acquired by the continued

preservation of successive profitable degrees of sterility. I hope,

however, to be able to show that sterility is not a specially acquired

or endowed quality, but is incidental on other acquired differences.

In treating this subject, two classes of facts, to a large extent

fundamentally different, have generally been confounded together;

namely, the sterility of two species when first crossed, and the

sterility of the hybrids produced from them.

Pure species have of course their organs of reproduction in a perfect

condition, yet when intercrossed they produce either few or no

offspring. Hybrids, on the other hand, have their reproductive organs

functionally impotent, as may be clearly seen in the state of the male

element in both plants and animals; though the organs themselves are

perfect in structure, as far as the microscope reveals. In the first

case the two sexual elements which go to form the embryo are perfect; in

the second case they are either not at all developed, or are imperfectly

developed. This distinction is important, when the cause of the

sterility, which is common to the two cases, has to be considered. The

distinction has probably been slurred over, owing to the sterility in

both cases being looked on as a special endowment, beyond the province

of our reasoning powers.

The fertility of varieties, that is of the forms known or believed to

have descended from common parents, when intercrossed, and likewise

the fertility of their mongrel offspring, is, on my theory, of equal

importance with the sterility of species; for it seems to make a broad

and clear distinction between varieties and species.

First, for the sterility of species when crossed and of their hybrid

offspring. It is impossible to study the several memoirs and works of

those two conscientious and admirable observers, Kolreuter and Gartner,

who almost devoted their lives to this subject, without being deeply

impressed with the high generality of some degree of sterility.

Kolreuter makes the rule universal; but then he cuts the knot, for in

ten cases in which he found two forms, considered by most authors as

distinct species, quite fertile together, he unhesitatingly ranks them

as varieties. Gartner, also, makes the rule equally universal; and he

disputes the entire fertility of Kolreuter's ten cases. But in these and

in many other cases, Gartner is obliged carefully to count the seeds, in

order to show that there is any degree of sterility. He always compares

the maximum number of seeds produced by two species when crossed and by

their hybrid offspring, with the average number produced by both pure

parent-species in a state of nature. But a serious cause of error seems

to me to be here introduced: a plant to be hybridised must be castrated,

and, what is often more important, must be secluded in order to prevent

pollen being brought to it by insects from other plants. Nearly all the

plants experimentised on by Gartner were potted, and apparently were

kept in a chamber in his house. That these processes are often injurious

to the fertility of a plant cannot be doubted; for Gartner gives in

his table about a score of cases of plants which he castrated, and

artificially fertilised with their own pollen, and (excluding all cases

such as the Leguminosae, in which there is an acknowledged difficulty

in the manipulation) half of these twenty plants had their fertility

in some degree impaired. Moreover, as Gartner during several years

repeatedly crossed the primrose and cowslip, which we have such good

reason to believe to be varieties, and only once or twice succeeded in

getting fertile seed; as he found the common red and blue pimpernels

(Anagallis arvensis and coerulea), which the best botanists rank as

varieties, absolutely sterile together; and as he came to the same

conclusion in several other analogous cases; it seems to me that we

may well be permitted to doubt whether many other species are really so

sterile, when intercrossed, as Gartner believes.

It is certain, on the one hand, that the sterility of various species

when crossed is so different in degree and graduates away so insensibly,

and, on the other hand, that the fertility of pure species is so easily

affected by various circumstances, that for all practical purposes it is

most difficult to say where perfect fertility ends and sterility begins.

I think no better evidence of this can be required than that the two

most experienced observers who have ever lived, namely, Kolreuter and

Gartner, should have arrived at diametrically opposite conclusions

in regard to the very same species. It is also most instructive to

compare--but I have not space here to enter on details--the evidence

advanced by our best botanists on the question whether certain doubtful

forms should be ranked as species or varieties, with the evidence from

fertility adduced by different hybridisers, or by the same author,

from experiments made during different years. It can thus be shown that

neither sterility nor fertility affords any clear distinction between

species and varieties; but that the evidence from this source graduates

away, and is doubtful in the same degree as is the evidence derived from

other constitutional and structural differences.

In regard to the sterility of hybrids in successive generations; though

Gartner was enabled to rear some hybrids, carefully guarding them from a

cross with either pure parent, for six or seven, and in one case for

ten generations, yet he asserts positively that their fertility never

increased, but generally greatly decreased. I do not doubt that this is

usually the case, and that the fertility often suddenly decreases in

the first few generations. Nevertheless I believe that in all these

experiments the fertility has been diminished by an independent cause,

namely, from close interbreeding. I have collected so large a body of

facts, showing that close interbreeding lessens fertility, and, on

the other hand, that an occasional cross with a distinct individual or

variety increases fertility, that I cannot doubt the correctness of this

almost universal belief amongst breeders. Hybrids are seldom raised by

experimentalists in great numbers; and as the parent-species, or other

allied hybrids, generally grow in the same garden, the visits of insects

must be carefully prevented during the flowering season: hence hybrids

will generally be fertilised during each generation by their own

individual pollen; and I am convinced that this would be injurious

to their fertility, already lessened by their hybrid origin. I am

strengthened in this conviction by a remarkable statement repeatedly

made by Gartner, namely, that if even the less fertile hybrids be

artificially fertilised with hybrid pollen of the same kind, their

fertility, notwithstanding the frequent ill effects of manipulation,

sometimes decidedly increases, and goes on increasing. Now, in

artificial fertilisation pollen is as often taken by chance (as I know

from my own experience) from the anthers of another flower, as from the

anthers of the flower itself which is to be fertilised; so that a cross

between two flowers, though probably on the same plant, would be thus

effected. Moreover, whenever complicated experiments are in progress,

so careful an observer as Gartner would have castrated his hybrids, and

this would have insured in each generation a cross with the pollen from

a distinct flower, either from the same plant or from another plant of

the same hybrid nature. And thus, the strange fact of the increase

of fertility in the successive generations of ARTIFICIALLY FERTILISED

hybrids may, I believe, be accounted for by close interbreeding having

been avoided.

Now let us turn to the results arrived at by the third most experienced

hybridiser, namely, the Honourable and Reverend W. Herbert. He is as

emphatic in his conclusion that some hybrids are perfectly fertile--as

fertile as the pure parent-species--as are Kolreuter and Gartner that

some degree of sterility between distinct species is a universal law

of nature. He experimentised on some of the very same species as did

Gartner. The difference in their results may, I think, be in part

accounted for by Herbert's great horticultural skill, and by his having

hothouses at his command. Of his many important statements I will here

give only a single one as an example, namely, that "every ovule in a pod

of Crinum capense fertilised by C. revolutum produced a plant, which

(he says) I never saw to occur in a case of its natural fecundation." So

that we here have perfect, or even more than commonly perfect, fertility

in a first cross between two distinct species.

This case of the Crinum leads me to refer to a most singular fact,

namely, that there are individual plants, as with certain species of

Lobelia, and with all the species of the genus Hippeastrum, which can

be far more easily fertilised by the pollen of another and distinct

species, than by their own pollen. For these plants have been found to

yield seed to the pollen of a distinct species, though quite sterile

with their own pollen, notwithstanding that their own pollen was found

to be perfectly good, for it fertilised distinct species. So that

certain individual plants and all the individuals of certain species

can actually be hybridised much more readily than they can be

self-fertilised! For instance, a bulb of Hippeastrum aulicum produced

four flowers; three were fertilised by Herbert with their own pollen,

and the fourth was subsequently fertilised by the pollen of a compound

hybrid descended from three other and distinct species: the result was

that "the ovaries of the three first flowers soon ceased to grow, and

after a few days perished entirely, whereas the pod impregnated by

the pollen of the hybrid made vigorous growth and rapid progress to

maturity, and bore good seed, which vegetated freely." In a letter to

me, in 1839, Mr. Herbert told me that he had then tried the experiment

during five years, and he continued to try it during several subsequent

years, and always with the same result. This result has, also, been

confirmed by other observers in the case of Hippeastrum with its

sub-genera, and in the case of some other genera, as Lobelia, Passiflora

and Verbascum. Although the plants in these experiments appeared

perfectly healthy, and although both the ovules and pollen of the same

flower were perfectly good with respect to other species, yet as they

were functionally imperfect in their mutual self-action, we must infer

that the plants were in an unnatural state. Nevertheless these facts

show on what slight and mysterious causes the lesser or greater

fertility of species when crossed, in comparison with the same species

when self-fertilised, sometimes depends.

The practical experiments of horticulturists, though not made with

scientific precision, deserve some notice. It is notorious in how

complicated a manner the species of Pelargonium, Fuchsia, Calceolaria,

Petunia, Rhododendron, etc., have been crossed, yet many of these

hybrids seed freely. For instance, Herbert asserts that a hybrid from

Calceolaria integrifolia and plantaginea, species most widely dissimilar

in general habit, "reproduced itself as perfectly as if it had been a

natural species from the mountains of Chile." I have taken some pains

to ascertain the degree of fertility of some of the complex crosses of

Rhododendrons, and I am assured that many of them are perfectly fertile.

Mr. C. Noble, for instance, informs me that he raises stocks for

grafting from a hybrid between Rhododendron Ponticum and Catawbiense,

and that this hybrid "seeds as freely as it is possible to imagine." Had

hybrids, when fairly treated, gone on decreasing in fertility in each

successive generation, as Gartner believes to be the case, the fact

would have been notorious to nurserymen. Horticulturists raise large

beds of the same hybrids, and such alone are fairly treated, for by

insect agency the several individuals of the same hybrid variety are

allowed to freely cross with each other, and the injurious influence

of close interbreeding is thus prevented. Any one may readily convince

himself of the efficiency of insect-agency by examining the flowers of

the more sterile kinds of hybrid rhododendrons, which produce no pollen,

for he will find on their stigmas plenty of pollen brought from other

flowers.

In regard to animals, much fewer experiments have been carefully tried

than with plants. If our systematic arrangements can be trusted, that

is if the genera of animals are as distinct from each other, as are the

genera of plants, then we may infer that animals more widely separated

in the scale of nature can be more easily crossed than in the case of

plants; but the hybrids themselves are, I think, more sterile. I doubt

whether any case of a perfectly fertile hybrid animal can be considered

as thoroughly well authenticated. It should, however, be borne in

mind that, owing to few animals breeding freely under confinement, few

experiments have been fairly tried: for instance, the canary-bird has

been crossed with nine other finches, but as not one of these nine

species breeds freely in confinement, we have no right to expect that

the first crosses between them and the canary, or that their hybrids,

should be perfectly fertile. Again, with respect to the fertility in

successive generations of the more fertile hybrid animals, I hardly know

of an instance in which two families of the same hybrid have been raised

at the same time from different parents, so as to avoid the ill effects

of close interbreeding. On the contrary, brothers and sisters have

usually been crossed in each successive generation, in opposition to the

constantly repeated admonition of every breeder. And in this case, it is

not at all surprising that the inherent sterility in the hybrids should

have gone on increasing. If we were to act thus, and pair brothers and

sisters in the case of any pure animal, which from any cause had the

least tendency to sterility, the breed would assuredly be lost in a very

few generations.

Although I do not know of any thoroughly well-authenticated cases of

perfectly fertile hybrid animals, I have some reason to believe that

the hybrids from Cervulus vaginalis and Reevesii, and from Phasianus

colchicus with P. torquatus and with P. versicolor are perfectly

fertile. The hybrids from the common and Chinese geese (A. cygnoides),

species which are so different that they are generally ranked in

distinct genera, have often bred in this country with either pure

parent, and in one single instance they have bred inter se. This was

effected by Mr. Eyton, who raised two hybrids from the same parents but

from different hatches; and from these two birds he raised no less than

eight hybrids (grandchildren of the pure geese) from one nest. In India,

however, these cross-bred geese must be far more fertile; for I am

assured by two eminently capable judges, namely Mr. Blyth and Capt.

Hutton, that whole flocks of these crossed geese are kept in various

parts of the country; and as they are kept for profit, where neither

pure parent-species exists, they must certainly be highly fertile.

A doctrine which originated with Pallas, has been largely accepted

by modern naturalists; namely, that most of our domestic animals have

descended from two or more aboriginal species, since commingled by

intercrossing. On this view, the aboriginal species must either at first

have produced quite fertile hybrids, or the hybrids must have become in

subsequent generations quite fertile under domestication. This latter

alternative seems to me the most probable, and I am inclined to believe

in its truth, although it rests on no direct evidence. I believe, for

instance, that our dogs have descended from several wild stocks; yet,

with perhaps the exception of certain indigenous domestic dogs of South

America, all are quite fertile together; and analogy makes me greatly

doubt, whether the several aboriginal species would at first have freely

bred together and have produced quite fertile hybrids. So again there

is reason to believe that our European and the humped Indian cattle are

quite fertile together; but from facts communicated to me by Mr. Blyth,

I think they must be considered as distinct species. On this view of

the origin of many of our domestic animals, we must either give up the

belief of the almost universal sterility of distinct species of

animals when crossed; or we must look at sterility, not as an indelible

characteristic, but as one capable of being removed by domestication.

Finally, looking to all the ascertained facts on the intercrossing of

plants and animals, it may be concluded that some degree of sterility,

both in first crosses and in hybrids, is an extremely general result;

but that it cannot, under our present state of knowledge, be considered

as absolutely universal.

LAWS GOVERNING THE STERILITY OF FIRST CROSSES AND OF HYBRIDS.

We will now consider a little more in detail the circumstances and

rules governing the sterility of first crosses and of hybrids. Our chief

object will be to see whether or not the rules indicate that species

have specially been endowed with this quality, in order to prevent their

crossing and blending together in utter confusion. The following rules

and conclusions are chiefly drawn up from Gartner's admirable work on

the hybridisation of plants. I have taken much pains to ascertain how

far the rules apply to animals, and considering how scanty our knowledge

is in regard to hybrid animals, I have been surprised to find how

generally the same rules apply to both kingdoms.

It has been already remarked, that the degree of fertility, both of

first crosses and of hybrids, graduates from zero to perfect fertility.

It is surprising in how many curious ways this gradation can be shown to

exist; but only the barest outline of the facts can here be given. When

pollen from a plant of one family is placed on the stigma of a plant of

a distinct family, it exerts no more influence than so much inorganic

dust. From this absolute zero of fertility, the pollen of different

species of the same genus applied to the stigma of some one species,

yields a perfect gradation in the number of seeds produced, up to nearly

complete or even quite complete fertility; and, as we have seen, in

certain abnormal cases, even to an excess of fertility, beyond that

which the plant's own pollen will produce. So in hybrids themselves,

there are some which never have produced, and probably never would

produce, even with the pollen of either pure parent, a single fertile

seed: but in some of these cases a first trace of fertility may be

detected, by the pollen of one of the pure parent-species causing the

flower of the hybrid to wither earlier than it otherwise would have

done; and the early withering of the flower is well known to be a sign

of incipient fertilisation. From this extreme degree of sterility we

have self-fertilised hybrids producing a greater and greater number of

seeds up to perfect fertility.

Hybrids from two species which are very difficult to cross, and which

rarely produce any offspring, are generally very sterile; but the

parallelism between the difficulty of making a first cross, and the

sterility of the hybrids thus produced--two classes of facts which are

generally confounded together--is by no means strict. There are many

cases, in which two pure species can be united with unusual facility,

and produce numerous hybrid-offspring, yet these hybrids are remarkably

sterile. On the other hand, there are species which can be crossed

very rarely, or with extreme difficulty, but the hybrids, when at last

produced, are very fertile. Even within the limits of the same genus,

for instance in Dianthus, these two opposite cases occur.

The fertility, both of first crosses and of hybrids, is more easily

affected by unfavourable conditions, than is the fertility of pure

species. But the degree of fertility is likewise innately variable; for

it is not always the same when the same two species are crossed under

the same circumstances, but depends in part upon the constitution of the

individuals which happen to have been chosen for the experiment. So it

is with hybrids, for their degree of fertility is often found to differ

greatly in the several individuals raised from seed out of the same

capsule and exposed to exactly the same conditions.

By the term systematic affinity is meant, the resemblance between

species in structure and in constitution, more especially in the

structure of parts which are of high physiological importance and which

differ little in the allied species. Now the fertility of first crosses

between species, and of the hybrids produced from them, is largely

governed by their systematic affinity. This is clearly shown by hybrids

never having been raised between species ranked by systematists in

distinct families; and on the other hand, by very closely allied

species generally uniting with facility. But the correspondence between

systematic affinity and the facility of crossing is by no means strict.

A multitude of cases could be given of very closely allied species which

will not unite, or only with extreme difficulty; and on the other hand

of very distinct species which unite with the utmost facility. In

the same family there may be a genus, as Dianthus, in which very many

species can most readily be crossed; and another genus, as Silene,

in which the most persevering efforts have failed to produce between

extremely close species a single hybrid. Even within the limits of the

same genus, we meet with this same difference; for instance, the many

species of Nicotiana have been more largely crossed than the species of

almost any other genus; but Gartner found that N. acuminata, which is

not a particularly distinct species, obstinately failed to fertilise, or

to be fertilised by, no less than eight other species of Nicotiana. Very

many analogous facts could be given.

No one has been able to point out what kind, or what amount, of

difference in any recognisable character is sufficient to prevent two

species crossing. It can be shown that plants most widely different in

habit and general appearance, and having strongly marked differences in

every part of the flower, even in the pollen, in the fruit, and in the

cotyledons, can be crossed. Annual and perennial plants, deciduous and

evergreen trees, plants inhabiting different stations and fitted for

extremely different climates, can often be crossed with ease.

By a reciprocal cross between two species, I mean the case, for

instance, of a stallion-horse being first crossed with a female-ass, and

then a male-ass with a mare: these two species may then be said to have

been reciprocally crossed. There is often the widest possible difference

in the facility of making reciprocal crosses. Such cases are highly

important, for they prove that the capacity in any two species to cross

is often completely independent of their systematic affinity, or of any

recognisable difference in their whole organisation. On the other hand,

these cases clearly show that the capacity for crossing is connected

with constitutional differences imperceptible by us, and confined to the

reproductive system. This difference in the result of reciprocal crosses

between the same two species was long ago observed by Kolreuter. To give

an instance: Mirabilis jalappa can easily be fertilised by the pollen of

M. longiflora, and the hybrids thus produced are sufficiently fertile;

but Kolreuter tried more than two hundred times, during eight following

years, to fertilise reciprocally M. longiflora with the pollen of M.

jalappa, and utterly failed. Several other equally striking cases could

be given. Thuret has observed the same fact with certain sea-weeds

or Fuci. Gartner, moreover, found that this difference of facility in

making reciprocal crosses is extremely common in a lesser degree. He has

observed it even between forms so closely related (as Matthiola annua

and glabra) that many botanists rank them only as varieties. It is also

a remarkable fact, that hybrids raised from reciprocal crosses, though

of course compounded of the very same two species, the one species

having first been used as the father and then as the mother, generally

differ in fertility in a small, and occasionally in a high degree.

Several other singular rules could be given from Gartner: for instance,

some species have a remarkable power of crossing with other species;

other species of the same genus have a remarkable power of impressing

their likeness on their hybrid offspring; but these two powers do not at

all necessarily go together. There are certain hybrids which instead

of having, as is usual, an intermediate character between their two

parents, always closely resemble one of them; and such hybrids, though

externally so like one of their pure parent-species, are with rare

exceptions extremely sterile. So again amongst hybrids which are

usually intermediate in structure between their parents, exceptional and

abnormal individuals sometimes are born, which closely resemble one of

their pure parents; and these hybrids are almost always utterly sterile,

even when the other hybrids raised from seed from the same capsule have

a considerable degree of fertility. These facts show how completely

fertility in the hybrid is independent of its external resemblance to

either pure parent.

Considering the several rules now given, which govern the fertility

of first crosses and of hybrids, we see that when forms, which must be

considered as good and distinct species, are united, their fertility

graduates from zero to perfect fertility, or even to fertility under

certain conditions in excess. That their fertility, besides being

eminently susceptible to favourable and unfavourable conditions, is

innately variable. That it is by no means always the same in degree in

the first cross and in the hybrids produced from this cross. That the

fertility of hybrids is not related to the degree in which they resemble

in external appearance either parent. And lastly, that the facility of

making a first cross between any two species is not always governed by

their systematic affinity or degree of resemblance to each other. This

latter statement is clearly proved by reciprocal crosses between the

same two species, for according as the one species or the other is used

as the father or the mother, there is generally some difference,

and occasionally the widest possible difference, in the facility of

effecting an union. The hybrids, moreover, produced from reciprocal

crosses often differ in fertility.

Now do these complex and singular rules indicate that species have been

endowed with sterility simply to prevent their becoming confounded

in nature? I think not. For why should the sterility be so extremely

different in degree, when various species are crossed, all of which

we must suppose it would be equally important to keep from blending

together? Why should the degree of sterility be innately variable in

the individuals of the same species? Why should some species cross with

facility, and yet produce very sterile hybrids; and other species cross

with extreme difficulty, and yet produce fairly fertile hybrids?

Why should there often be so great a difference in the result of a

reciprocal cross between the same two species? Why, it may even be

asked, has the production of hybrids been permitted? to grant to species

the special power of producing hybrids, and then to stop their further

propagation by different degrees of sterility, not strictly related to

the facility of the first union between their parents, seems to be a

strange arrangement.

The foregoing rules and facts, on the other hand, appear to me clearly

to indicate that the sterility both of first crosses and of hybrids is

simply incidental or dependent on unknown differences, chiefly in the

reproductive systems, of the species which are crossed. The differences

being of so peculiar and limited a nature, that, in reciprocal crosses

between two species the male sexual element of the one will often freely

act on the female sexual element of the other, but not in a reversed

direction. It will be advisable to explain a little more fully by an

example what I mean by sterility being incidental on other differences,

and not a specially endowed quality. As the capacity of one plant to be

grafted or budded on another is so entirely unimportant for its welfare

in a state of nature, I presume that no one will suppose that this

capacity is a SPECIALLY endowed quality, but will admit that it is

incidental on differences in the laws of growth of the two plants. We

can sometimes see the reason why one tree will not take on another, from

differences in their rate of growth, in the hardness of their wood, in

the period of the flow or nature of their sap, etc.; but in a multitude

of cases we can assign no reason whatever. Great diversity in the size

of two plants, one being woody and the other herbaceous, one being

evergreen and the other deciduous, and adaptation to widely different

climates, does not always prevent the two grafting together. As in

hybridisation, so with grafting, the capacity is limited by systematic

affinity, for no one has been able to graft trees together belonging to

quite distinct families; and, on the other hand, closely allied species,

and varieties of the same species, can usually, but not invariably,

be grafted with ease. But this capacity, as in hybridisation, is by no

means absolutely governed by systematic affinity. Although many distinct

genera within the same family have been grafted together, in other cases

species of the same genus will not take on each other. The pear can be

grafted far more readily on the quince, which is ranked as a distinct

genus, than on the apple, which is a member of the same genus. Even

different varieties of the pear take with different degrees of facility

on the quince; so do different varieties of the apricot and peach on

certain varieties of the plum.

As Gartner found that there was sometimes an innate difference in

different INDIVIDUALS of the same two species in crossing; so Sagaret

believes this to be the case with different individuals of the same

two species in being grafted together. As in reciprocal crosses, the

facility of effecting an union is often very far from equal, so it

sometimes is in grafting; the common gooseberry, for instance, cannot

be grafted on the currant, whereas the currant will take, though with

difficulty, on the gooseberry.

We have seen that the sterility of hybrids, which have their

reproductive organs in an imperfect condition, is a very different

case from the difficulty of uniting two pure species, which have their

reproductive organs perfect; yet these two distinct cases run to a

certain extent parallel. Something analogous occurs in grafting; for

Thouin found that three species of Robinia, which seeded freely on

their own roots, and which could be grafted with no great difficulty on

another species, when thus grafted were rendered barren. On the other

hand, certain species of Sorbus, when grafted on other species, yielded

twice as much fruit as when on their own roots. We are reminded by this

latter fact of the extraordinary case of Hippeastrum, Lobelia, etc.,

which seeded much more freely when fertilised with the pollen of

distinct species, than when self-fertilised with their own pollen.

We thus see, that although there is a clear and fundamental difference

between the mere adhesion of grafted stocks, and the union of the male

and female elements in the act of reproduction, yet that there is a

rude degree of parallelism in the results of grafting and of crossing

distinct species. And as we must look at the curious and complex laws

governing the facility with which trees can be grafted on each other

as incidental on unknown differences in their vegetative systems, so I

believe that the still more complex laws governing the facility of

first crosses, are incidental on unknown differences, chiefly in their

reproductive systems. These differences, in both cases, follow to a

certain extent, as might have been expected, systematic affinity, by

which every kind of resemblance and dissimilarity between organic

beings is attempted to be expressed. The facts by no means seem to me

to indicate that the greater or lesser difficulty of either grafting or

crossing together various species has been a special endowment;

although in the case of crossing, the difficulty is as important for the

endurance and stability of specific forms, as in the case of grafting it

is unimportant for their welfare.

CAUSES OF THE STERILITY OF FIRST CROSSES AND OF HYBRIDS.

We may now look a little closer at the probable causes of the sterility

of first crosses and of hybrids. These two cases are fundamentally

different, for, as just remarked, in the union of two pure species the

male and female sexual elements are perfect, whereas in hybrids they are

imperfect. Even in first crosses, the greater or lesser difficulty in

effecting a union apparently depends on several distinct causes. There

must sometimes be a physical impossibility in the male element reaching

the ovule, as would be the case with a plant having a pistil too long

for the pollen-tubes to reach the ovarium. It has also been observed

that when pollen of one species is placed on the stigma of a distantly

allied species, though the pollen-tubes protrude, they do not penetrate

the stigmatic surface. Again, the male element may reach the female

element, but be incapable of causing an embryo to be developed, as seems

to have been the case with some of Thuret's experiments on Fuci. No

explanation can be given of these facts, any more than why certain trees

cannot be grafted on others. Lastly, an embryo may be developed, and

then perish at an early period. This latter alternative has not been

sufficiently attended to; but I believe, from observations communicated

to me by Mr. Hewitt, who has had great experience in hybridising

gallinaceous birds, that the early death of the embryo is a very

frequent cause of sterility in first crosses. I was at first very

unwilling to believe in this view; as hybrids, when once born, are

generally healthy and long-lived, as we see in the case of the common

mule. Hybrids, however, are differently circumstanced before and after

birth: when born and living in a country where their two parents can

live, they are generally placed under suitable conditions of life. But

a hybrid partakes of only half of the nature and constitution of its

mother, and therefore before birth, as long as it is nourished within

its mother's womb or within the egg or seed produced by the mother, it

may be exposed to conditions in some degree unsuitable, and consequently

be liable to perish at an early period; more especially as all very

young beings seem eminently sensitive to injurious or unnatural

conditions of life.

In regard to the sterility of hybrids, in which the sexual elements are

imperfectly developed, the case is very different. I have more than once

alluded to a large body of facts, which I have collected, showing that

when animals and plants are removed from their natural conditions,

they are extremely liable to have their reproductive systems seriously

affected. This, in fact, is the great bar to the domestication of

animals. Between the sterility thus superinduced and that of hybrids,

there are many points of similarity. In both cases the sterility is

independent of general health, and is often accompanied by excess of

size or great luxuriance. In both cases, the sterility occurs in various

degrees; in both, the male element is the most liable to be affected;

but sometimes the female more than the male. In both, the tendency

goes to a certain extent with systematic affinity, for whole groups

of animals and plants are rendered impotent by the same unnatural

conditions; and whole groups of species tend to produce sterile hybrids.

On the other hand, one species in a group will sometimes resist great

changes of conditions with unimpaired fertility; and certain species in

a group will produce unusually fertile hybrids. No one can tell, till he

tries, whether any particular animal will breed under confinement or any

plant seed freely under culture; nor can he tell, till he tries, whether

any two species of a genus will produce more or less sterile hybrids.

Lastly, when organic beings are placed during several generations under

conditions not natural to them, they are extremely liable to vary,

which is due, as I believe, to their reproductive systems having been

specially affected, though in a lesser degree than when sterility

ensues. So it is with hybrids, for hybrids in successive generations are

eminently liable to vary, as every experimentalist has observed.

Thus we see that when organic beings are placed under new and unnatural

conditions, and when hybrids are produced by the unnatural crossing of

two species, the reproductive system, independently of the general state

of health, is affected by sterility in a very similar manner. In the

one case, the conditions of life have been disturbed, though often in so

slight a degree as to be inappreciable by us; in the other case, or

that of hybrids, the external conditions have remained the same, but

the organisation has been disturbed by two different structures and

constitutions having been blended into one. For it is scarcely possible

that two organisations should be compounded into one, without some

disturbance occurring in the development, or periodical action, or

mutual relation of the different parts and organs one to another, or to

the conditions of life. When hybrids are able to breed inter se, they

transmit to their offspring from generation to generation the same

compounded organisation, and hence we need not be surprised that their

sterility, though in some degree variable, rarely diminishes.

It must, however, be confessed that we cannot understand, excepting

on vague hypotheses, several facts with respect to the sterility of

hybrids; for instance, the unequal fertility of hybrids produced from

reciprocal crosses; or the increased sterility in those hybrids which

occasionally and exceptionally resemble closely either pure parent. Nor

do I pretend that the foregoing remarks go to the root of the matter:

no explanation is offered why an organism, when placed under unnatural

conditions, is rendered sterile. All that I have attempted to show,

is that in two cases, in some respects allied, sterility is the common

result,--in the one case from the conditions of life having been

disturbed, in the other case from the organisation having been disturbed

by two organisations having been compounded into one.

It may seem fanciful, but I suspect that a similar parallelism extends

to an allied yet very different class of facts. It is an old and almost

universal belief, founded, I think, on a considerable body of evidence,

that slight changes in the conditions of life are beneficial to all

living things. We see this acted on by farmers and gardeners in their

frequent exchanges of seed, tubers, etc., from one soil or climate to

another, and back again. During the convalescence of animals, we plainly

see that great benefit is derived from almost any change in the

habits of life. Again, both with plants and animals, there is abundant

evidence, that a cross between very distinct individuals of the same

species, that is between members of different strains or sub-breeds,

gives vigour and fertility to the offspring. I believe, indeed, from

the facts alluded to in our fourth chapter, that a certain amount of

crossing is indispensable even with hermaphrodites; and that close

interbreeding continued during several generations between the nearest

relations, especially if these be kept under the same conditions of

life, always induces weakness and sterility in the progeny.

Hence it seems that, on the one hand, slight changes in the conditions

of life benefit all organic beings, and on the other hand, that slight

crosses, that is crosses between the males and females of the same

species which have varied and become slightly different, give vigour and

fertility to the offspring. But we have seen that greater changes, or

changes of a particular nature, often render organic beings in some

degree sterile; and that greater crosses, that is crosses between males

and females which have become widely or specifically different, produce

hybrids which are generally sterile in some degree. I cannot persuade

myself that this parallelism is an accident or an illusion. Both series

of facts seem to be connected together by some common but unknown bond,

which is essentially related to the principle of life.

FERTILITY OF VARIETIES WHEN CROSSED, AND OF THEIR MONGREL OFFSPRING.

It may be urged, as a most forcible argument, that there must be some

essential distinction between species and varieties, and that there

must be some error in all the foregoing remarks, inasmuch as varieties,

however much they may differ from each other in external appearance,

cross with perfect facility, and yield perfectly fertile offspring. I

fully admit that this is almost invariably the case. But if we look to

varieties produced under nature, we are immediately involved in hopeless

difficulties; for if two hitherto reputed varieties be found in any

degree sterile together, they are at once ranked by most naturalists

as species. For instance, the blue and red pimpernel, the primrose

and cowslip, which are considered by many of our best botanists as

varieties, are said by Gartner not to be quite fertile when crossed, and

he consequently ranks them as undoubted species. If we thus argue in

a circle, the fertility of all varieties produced under nature will

assuredly have to be granted.

If we turn to varieties, produced, or supposed to have been produced,

under domestication, we are still involved in doubt. For when it is

stated, for instance, that the German Spitz dog unites more easily

than other dogs with foxes, or that certain South American indigenous

domestic dogs do not readily cross with European dogs, the explanation

which will occur to everyone, and probably the true one, is that

these dogs have descended from several aboriginally distinct species.

Nevertheless the perfect fertility of so many domestic varieties,

differing widely from each other in appearance, for instance of the

pigeon or of the cabbage, is a remarkable fact; more especially when we

reflect how many species there are, which, though resembling each

other most closely, are utterly sterile when intercrossed. Several

considerations, however, render the fertility of domestic varieties less

remarkable than at first appears. It can, in the first place, be clearly

shown that mere external dissimilarity between two species does not

determine their greater or lesser degree of sterility when crossed; and

we may apply the same rule to domestic varieties. In the second place,

some eminent naturalists believe that a long course of domestication

tends to eliminate sterility in the successive generations of hybrids,

which were at first only slightly sterile; and if this be so, we surely

ought not to expect to find sterility both appearing and disappearing

under nearly the same conditions of life. Lastly, and this seems to me

by far the most important consideration, new races of animals and plants

are produced under domestication by man's methodical and unconscious

power of selection, for his own use and pleasure: he neither wishes to

select, nor could select, slight differences in the reproductive system,

or other constitutional differences correlated with the reproductive

system. He supplies his several varieties with the same food; treats

them in nearly the same manner, and does not wish to alter their general

habits of life. Nature acts uniformly and slowly during vast periods

of time on the whole organisation, in any way which may be for each

creature's own good; and thus she may, either directly, or more probably

indirectly, through correlation, modify the reproductive system in the

several descendants from any one species. Seeing this difference in the

process of selection, as carried on by man and nature, we need not be

surprised at some difference in the result.

I have as yet spoken as if the varieties of the same species were

invariably fertile when intercrossed. But it seems to me impossible to

resist the evidence of the existence of a certain amount of sterility in

the few following cases, which I will briefly abstract. The evidence

is at least as good as that from which we believe in the sterility of

a multitude of species. The evidence is, also, derived from hostile

witnesses, who in all other cases consider fertility and sterility as

safe criterions of specific distinction. Gartner kept during several

years a dwarf kind of maize with yellow seeds, and a tall variety with

red seeds, growing near each other in his garden; and although these

plants have separated sexes, they never naturally crossed. He then

fertilised thirteen flowers of the one with the pollen of the other; but

only a single head produced any seed, and this one head produced only

five grains. Manipulation in this case could not have been injurious, as

the plants have separated sexes. No one, I believe, has suspected that

these varieties of maize are distinct species; and it is important to

notice that the hybrid plants thus raised were themselves PERFECTLY

fertile; so that even Gartner did not venture to consider the two

varieties as specifically distinct.

Girou de Buzareingues crossed three varieties of gourd, which like

the maize has separated sexes, and he asserts that their mutual

fertilisation is by so much the less easy as their differences are

greater. How far these experiments may be trusted, I know not; but the

forms experimentised on, are ranked by Sagaret, who mainly founds his

classification by the test of infertility, as varieties.

The following case is far more remarkable, and seems at first quite

incredible; but it is the result of an astonishing number of experiments

made during many years on nine species of Verbascum, by so good an

observer and so hostile a witness, as Gartner: namely, that yellow

and white varieties of the same species of Verbascum when intercrossed

produce less seed, than do either coloured varieties when fertilised

with pollen from their own coloured flowers. Moreover, he asserts that

when yellow and white varieties of one species are crossed with yellow

and white varieties of a DISTINCT species, more seed is produced by the

crosses between the same coloured flowers, than between those which are

differently coloured. Yet these varieties of Verbascum present no other

difference besides the mere colour of the flower; and one variety can

sometimes be raised from the seed of the other.

From observations which I have made on certain varieties of hollyhock, I

am inclined to suspect that they present analogous facts.

Kolreuter, whose accuracy has been confirmed by every subsequent

observer, has proved the remarkable fact, that one variety of the common

tobacco is more fertile, when crossed with a widely distinct species,

than are the other varieties. He experimentised on five forms, which are

commonly reputed to be varieties, and which he tested by the severest

trial, namely, by reciprocal crosses, and he found their mongrel

offspring perfectly fertile. But one of these five varieties, when used

either as father or mother, and crossed with the Nicotiana glutinosa,

always yielded hybrids not so sterile as those which were produced

from the four other varieties when crossed with N. glutinosa. Hence the

reproductive system of this one variety must have been in some manner

and in some degree modified.

From these facts; from the great difficulty of ascertaining the

infertility of varieties in a state of nature, for a supposed variety if

infertile in any degree would generally be ranked as species; from

man selecting only external characters in the production of the most

distinct domestic varieties, and from not wishing or being able to

produce recondite and functional differences in the reproductive system;

from these several considerations and facts, I do not think that the

very general fertility of varieties can be proved to be of universal

occurrence, or to form a fundamental distinction between varieties

and species. The general fertility of varieties does not seem to me

sufficient to overthrow the view which I have taken with respect to

the very general, but not invariable, sterility of first crosses and of

hybrids, namely, that it is not a special endowment, but is incidental

on slowly acquired modifications, more especially in the reproductive

systems of the forms which are crossed.

HYBRIDS AND MONGRELS COMPARED, INDEPENDENTLY OF THEIR FERTILITY.

Independently of the question of fertility, the offspring of species

when crossed and of varieties when crossed may be compared in several

other respects. Gartner, whose strong wish was to draw a marked line of

distinction between species and varieties, could find very few and,

as it seems to me, quite unimportant differences between the so-called

hybrid offspring of species, and the so-called mongrel offspring of

varieties. And, on the other hand, they agree most closely in very many

important respects.

I shall here discuss this subject with extreme brevity. The most

important distinction is, that in the first generation mongrels are

more variable than hybrids; but Gartner admits that hybrids from

species which have long been cultivated are often variable in the first

generation; and I have myself seen striking instances of this fact.

Gartner further admits that hybrids between very closely allied species

are more variable than those from very distinct species; and this shows

that the difference in the degree of variability graduates away.

When mongrels and the more fertile hybrids are propagated for several

generations an extreme amount of variability in their offspring

is notorious; but some few cases both of hybrids and mongrels long

retaining uniformity of character could be given. The variability,

however, in the successive generations of mongrels is, perhaps, greater

than in hybrids.

This greater variability of mongrels than of hybrids does not seem to me

at all surprising. For the parents of mongrels are varieties, and mostly

domestic varieties (very few experiments having been tried on natural

varieties), and this implies in most cases that there has been recent

variability; and therefore we might expect that such variability would

often continue and be super-added to that arising from the mere act of

crossing. The slight degree of variability in hybrids from the first

cross or in the first generation, in contrast with their extreme

variability in the succeeding generations, is a curious fact and

deserves attention. For it bears on and corroborates the view which I

have taken on the cause of ordinary variability; namely, that it is due

to the reproductive system being eminently sensitive to any change in

the conditions of life, being thus often rendered either impotent or at

least incapable of its proper function of producing offspring identical

with the parent-form. Now hybrids in the first generation are descended

from species (excluding those long cultivated) which have not had their

reproductive systems in any way affected, and they are not variable; but

hybrids themselves have their reproductive systems seriously affected,

and their descendants are highly variable.

But to return to our comparison of mongrels and hybrids: Gartner

states that mongrels are more liable than hybrids to revert to either

parent-form; but this, if it be true, is certainly only a difference in

degree. Gartner further insists that when any two species, although

most closely allied to each other, are crossed with a third species,

the hybrids are widely different from each other; whereas if two very

distinct varieties of one species are crossed with another species, the

hybrids do not differ much. But this conclusion, as far as I can make

out, is founded on a single experiment; and seems directly opposed to

the results of several experiments made by Kolreuter.

These alone are the unimportant differences, which Gartner is able to

point out, between hybrid and mongrel plants. On the other hand, the

resemblance in mongrels and in hybrids to their respective parents,

more especially in hybrids produced from nearly related species, follows

according to Gartner the same laws. When two species are crossed,

one has sometimes a prepotent power of impressing its likeness on the

hybrid; and so I believe it to be with varieties of plants. With animals

one variety certainly often has this prepotent power over another

variety. Hybrid plants produced from a reciprocal cross, generally

resemble each other closely; and so it is with mongrels from a

reciprocal cross. Both hybrids and mongrels can be reduced to either

pure parent-form, by repeated crosses in successive generations with

either parent.

These several remarks are apparently applicable to animals; but the

subject is here excessively complicated, partly owing to the existence

of secondary sexual characters; but more especially owing to prepotency

in transmitting likeness running more strongly in one sex than in the

other, both when one species is crossed with another, and when one

variety is crossed with another variety. For instance, I think those

authors are right, who maintain that the ass has a prepotent power over

the horse, so that both the mule and the hinny more resemble the ass

than the horse; but that the prepotency runs more strongly in the

male-ass than in the female, so that the mule, which is the offspring of

the male-ass and mare, is more like an ass, than is the hinny, which is

the offspring of the female-ass and stallion.

Much stress has been laid by some authors on the supposed fact, that

mongrel animals alone are born closely like one of their parents; but

it can be shown that this does sometimes occur with hybrids; yet I grant

much less frequently with hybrids than with mongrels. Looking to the

cases which I have collected of cross-bred animals closely resembling

one parent, the resemblances seem chiefly confined to characters almost

monstrous in their nature, and which have suddenly appeared--such as

albinism, melanism, deficiency of tail or horns, or additional fingers

and toes; and do not relate to characters which have been slowly

acquired by selection. Consequently, sudden reversions to the perfect

character of either parent would be more likely to occur with mongrels,

which are descended from varieties often suddenly produced and

semi-monstrous in character, than with hybrids, which are descended from

species slowly and naturally produced. On the whole I entirely agree

with Dr. Prosper Lucas, who, after arranging an enormous body of facts

with respect to animals, comes to the conclusion, that the laws of

resemblance of the child to its parents are the same, whether the two

parents differ much or little from each other, namely in the union

of individuals of the same variety, or of different varieties, or of

distinct species.

Laying aside the question of fertility and sterility, in all other

respects there seems to be a general and close similarity in the

offspring of crossed species, and of crossed varieties. If we look at

species as having been specially created, and at varieties as having

been produced by secondary laws, this similarity would be an astonishing

fact. But it harmonises perfectly with the view that there is no

essential distinction between species and varieties.

SUMMARY OF CHAPTER.

First crosses between forms sufficiently distinct to be ranked as

species, and their hybrids, are very generally, but not universally,

sterile. The sterility is of all degrees, and is often so slight that

the two most careful experimentalists who have ever lived, have come to

diametrically opposite conclusions in ranking forms by this test. The

sterility is innately variable in individuals of the same species, and

is eminently susceptible of favourable and unfavourable conditions. The

degree of sterility does not strictly follow systematic affinity, but is

governed by several curious and complex laws. It is generally different,

and sometimes widely different, in reciprocal crosses between the same

two species. It is not always equal in degree in a first cross and in

the hybrid produced from this cross.

In the same manner as in grafting trees, the capacity of one species

or variety to take on another, is incidental on generally unknown

differences in their vegetative systems, so in crossing, the greater

or less facility of one species to unite with another, is incidental

on unknown differences in their reproductive systems. There is no more

reason to think that species have been specially endowed with various

degrees of sterility to prevent them crossing and blending in nature,

than to think that trees have been specially endowed with various and

somewhat analogous degrees of difficulty in being grafted together in

order to prevent them becoming inarched in our forests.

The sterility of first crosses between pure species, which have their

reproductive systems perfect, seems to depend on several circumstances;

in some cases largely on the early death of the embryo. The sterility of

hybrids, which have their reproductive systems imperfect, and which

have had this system and their whole organisation disturbed by being

compounded of two distinct species, seems closely allied to that

sterility which so frequently affects pure species, when their natural

conditions of life have been disturbed. This view is supported by a

parallelism of another kind;--namely, that the crossing of forms only

slightly different is favourable to the vigour and fertility of their

offspring; and that slight changes in the conditions of life are

apparently favourable to the vigour and fertility of all organic beings.

It is not surprising that the degree of difficulty in uniting two

species, and the degree of sterility of their hybrid-offspring should

generally correspond, though due to distinct causes; for both depend

on the amount of difference of some kind between the species which are

crossed. Nor is it surprising that the facility of effecting a first

cross, the fertility of the hybrids produced, and the capacity of being

grafted together--though this latter capacity evidently depends on

widely different circumstances--should all run, to a certain extent,

parallel with the systematic affinity of the forms which are subjected

to experiment; for systematic affinity attempts to express all kinds of

resemblance between all species.

First crosses between forms known to be varieties, or sufficiently alike

to be considered as varieties, and their mongrel offspring, are very

generally, but not quite universally, fertile. Nor is this nearly

general and perfect fertility surprising, when we remember how liable we

are to argue in a circle with respect to varieties in a state of nature;

and when we remember that the greater number of varieties have

been produced under domestication by the selection of mere external

differences, and not of differences in the reproductive system. In

all other respects, excluding fertility, there is a close general

resemblance between hybrids and mongrels. Finally, then, the facts

briefly given in this chapter do not seem to me opposed to, but even

rather to support the view, that there is no fundamental distinction

between species and varieties.

9. ON THE IMPERFECTION OF THE GEOLOGICAL RECORD.

On the absence of intermediate varieties at the present day. On the

nature of extinct intermediate varieties; on their number. On the

vast lapse of time, as inferred from the rate of deposition and of

denudation. On the poorness of our palaeontological collections. On the

intermittence of geological formations. On the absence of intermediate

varieties in any one formation. On the sudden appearance of groups of

species. On their sudden appearance in the lowest known fossiliferous

strata.

In the sixth chapter I enumerated the chief objections which might be

justly urged against the views maintained in this volume. Most of them

have now been discussed. One, namely the distinctness of specific forms,

and their not being blended together by innumerable transitional links,

is a very obvious difficulty. I assigned reasons why such links do not

commonly occur at the present day, under the circumstances apparently

most favourable for their presence, namely on an extensive and

continuous area with graduated physical conditions. I endeavoured to

show, that the life of each species depends in a more important manner

on the presence of other already defined organic forms, than on climate;

and, therefore, that the really governing conditions of life do not

graduate away quite insensibly like heat or moisture. I endeavoured,

also, to show that intermediate varieties, from existing in lesser

numbers than the forms which they connect, will generally be beaten

out and exterminated during the course of further modification and

improvement. The main cause, however, of innumerable intermediate links

not now occurring everywhere throughout nature depends on the very

process of natural selection, through which new varieties continually

take the places of and exterminate their parent-forms. But just in

proportion as this process of extermination has acted on an enormous

scale, so must the number of intermediate varieties, which have

formerly existed on the earth, be truly enormous. Why then is not every

geological formation and every stratum full of such intermediate links?

Geology assuredly does not reveal any such finely graduated organic

chain; and this, perhaps, is the most obvious and gravest objection

which can be urged against my theory. The explanation lies, as I

believe, in the extreme imperfection of the geological record.

In the first place it should always be borne in mind what sort of

intermediate forms must, on my theory, have formerly existed. I have

found it difficult, when looking at any two species, to avoid picturing

to myself, forms DIRECTLY intermediate between them. But this is a

wholly false view; we should always look for forms intermediate between

each species and a common but unknown progenitor; and the progenitor

will generally have differed in some respects from all its modified

descendants. To give a simple illustration: the fantail and pouter

pigeons have both descended from the rock-pigeon; if we possessed all

the intermediate varieties which have ever existed, we should have an

extremely close series between both and the rock-pigeon; but we should

have no varieties directly intermediate between the fantail and pouter;

none, for instance, combining a tail somewhat expanded with a crop

somewhat enlarged, the characteristic features of these two breeds.

These two breeds, moreover, have become so much modified, that if we had

no historical or indirect evidence regarding their origin, it would not

have been possible to have determined from a mere comparison of their

structure with that of the rock-pigeon, whether they had descended from

this species or from some other allied species, such as C. oenas.

So with natural species, if we look to forms very distinct, for instance

to the horse and tapir, we have no reason to suppose that links ever

existed directly intermediate between them, but between each and an

unknown common parent. The common parent will have had in its whole

organisation much general resemblance to the tapir and to the horse; but

in some points of structure may have differed considerably from both,

even perhaps more than they differ from each other. Hence in all such

cases, we should be unable to recognise the parent-form of any two or

more species, even if we closely compared the structure of the parent

with that of its modified descendants, unless at the same time we had a

nearly perfect chain of the intermediate links.

It is just possible by my theory, that one of two living forms might

have descended from the other; for instance, a horse from a tapir; and

in this case DIRECT intermediate links will have existed between them.

But such a case would imply that one form had remained for a very long

period unaltered, whilst its descendants had undergone a vast amount of

change; and the principle of competition between organism and organism,

between child and parent, will render this a very rare event; for in all

cases the new and improved forms of life will tend to supplant the old

and unimproved forms.

By the theory of natural selection all living species have been

connected with the parent-species of each genus, by differences not

greater than we see between the varieties of the same species at the

present day; and these parent-species, now generally extinct, have in

their turn been similarly connected with more ancient species; and so on

backwards, always converging to the common ancestor of each great class.

So that the number of intermediate and transitional links, between all

living and extinct species, must have been inconceivably great. But

assuredly, if this theory be true, such have lived upon this earth.

ON THE LAPSE OF TIME.

Independently of our not finding fossil remains of such infinitely

numerous connecting links, it may be objected, that time will not have

sufficed for so great an amount of organic change, all changes having

been effected very slowly through natural selection. It is hardly

possible for me even to recall to the reader, who may not be a practical

geologist, the facts leading the mind feebly to comprehend the lapse of

time. He who can read Sir Charles Lyell's grand work on the Principles

of Geology, which the future historian will recognise as having produced

a revolution in natural science, yet does not admit how incomprehensibly

vast have been the past periods of time, may at once close this volume.

Not that it suffices to study the Principles of Geology, or to read

special treatises by different observers on separate formations, and to

mark how each author attempts to give an inadequate idea of the duration

of each formation or even each stratum. A man must for years examine for

himself great piles of superimposed strata, and watch the sea at work

grinding down old rocks and making fresh sediment, before he can hope to

comprehend anything of the lapse of time, the monuments of which we see

around us.

It is good to wander along lines of sea-coast, when formed of moderately

hard rocks, and mark the process of degradation. The tides in most cases

reach the cliffs only for a short time twice a day, and the waves eat

into them only when they are charged with sand or pebbles; for there

is reason to believe that pure water can effect little or nothing in

wearing away rock. At last the base of the cliff is undermined, huge

fragments fall down, and these remaining fixed, have to be worn away,

atom by atom, until reduced in size they can be rolled about by the

waves, and then are more quickly ground into pebbles, sand, or mud.

But how often do we see along the bases of retreating cliffs rounded

boulders, all thickly clothed by marine productions, showing how little

they are abraded and how seldom they are rolled about! Moreover, if

we follow for a few miles any line of rocky cliff, which is undergoing

degradation, we find that it is only here and there, along a short

length or round a promontory, that the cliffs are at the present time

suffering. The appearance of the surface and the vegetation show that

elsewhere years have elapsed since the waters washed their base.

He who most closely studies the action of the sea on our shores, will,

I believe, be most deeply impressed with the slowness with which rocky

coasts are worn away. The observations on this head by Hugh Miller,

and by that excellent observer Mr. Smith of Jordan Hill, are most

impressive. With the mind thus impressed, let any one examine beds of

conglomerate many thousand feet in thickness, which, though probably

formed at a quicker rate than many other deposits, yet, from being

formed of worn and rounded pebbles, each of which bears the stamp of

time, are good to show how slowly the mass has been accumulated. Let

him remember Lyell's profound remark, that the thickness and extent of

sedimentary formations are the result and measure of the degradation

which the earth's crust has elsewhere suffered. And what an amount of

degradation is implied by the sedimentary deposits of many countries!

Professor Ramsay has given me the maximum thickness, in most cases from

actual measurement, in a few cases from estimate, of each formation in

different parts of Great Britain; and this is the result:--

Feet

Palaeozoic strata (not including igneous beds)..57,154.

Secondary strata................................13,190.

Tertiary strata..................................2,240.

--making altogether 72,584 feet; that is, very nearly thirteen and

three-quarters British miles. Some of these formations, which are

represented in England by thin beds, are thousands of feet in thickness

on the Continent. Moreover, between each successive formation, we have,

in the opinion of most geologists, enormously long blank periods.

So that the lofty pile of sedimentary rocks in Britain, gives but an

inadequate idea of the time which has elapsed during their accumulation;

yet what time this must have consumed! Good observers have estimated

that sediment is deposited by the great Mississippi river at the rate

of only 600 feet in a hundred thousand years. This estimate may be quite

erroneous; yet, considering over what wide spaces very fine sediment is

transported by the currents of the sea, the process of accumulation in

any one area must be extremely slow.

But the amount of denudation which the strata have in many places

suffered, independently of the rate of accumulation of the degraded

matter, probably offers the best evidence of the lapse of time. I

remember having been much struck with the evidence of denudation, when

viewing volcanic islands, which have been worn by the waves and pared

all round into perpendicular cliffs of one or two thousand feet in

height; for the gentle slope of the lava-streams, due to their formerly

liquid state, showed at a glance how far the hard, rocky beds had once

extended into the open ocean. The same story is still more plainly told

by faults,--those great cracks along which the strata have been upheaved

on one side, or thrown down on the other, to the height or depth of

thousands of feet; for since the crust cracked, the surface of the land

has been so completely planed down by the action of the sea, that no

trace of these vast dislocations is externally visible.

The Craven fault, for instance, extends for upwards of 30 miles, and

along this line the vertical displacement of the strata has varied

from 600 to 3000 feet. Professor Ramsay has published an account of

a downthrow in Anglesea of 2300 feet; and he informs me that he fully

believes there is one in Merionethshire of 12,000 feet; yet in these

cases there is nothing on the surface to show such prodigious movements;

the pile of rocks on the one or other side having been smoothly swept

away. The consideration of these facts impresses my mind almost in

the same manner as does the vain endeavour to grapple with the idea of

eternity.

I am tempted to give one other case, the well-known one of the

denudation of the Weald. Though it must be admitted that the denudation

of the Weald has been a mere trifle, in comparison with that which has

removed masses of our palaeozoic strata, in parts ten thousand feet

in thickness, as shown in Professor Ramsay's masterly memoir on this

subject. Yet it is an admirable lesson to stand on the North Downs and

to look at the distant South Downs; for, remembering that at no great

distance to the west the northern and southern escarpments meet and

close, one can safely picture to oneself the great dome of rocks which

must have covered up the Weald within so limited a period as since the

latter part of the Chalk formation. The distance from the northern to

the southern Downs is about 22 miles, and the thickness of the several

formations is on an average about 1100 feet, as I am informed by

Professor Ramsay. But if, as some geologists suppose, a range of

older rocks underlies the Weald, on the flanks of which the overlying

sedimentary deposits might have accumulated in thinner masses than

elsewhere, the above estimate would be erroneous; but this source of

doubt probably would not greatly affect the estimate as applied to the

western extremity of the district. If, then, we knew the rate at which

the sea commonly wears away a line of cliff of any given height, we

could measure the time requisite to have denuded the Weald. This, of

course, cannot be done; but we may, in order to form some crude notion

on the subject, assume that the sea would eat into cliffs 500 feet in

height at the rate of one inch in a century. This will at first appear

much too small an allowance; but it is the same as if we were to assume

a cliff one yard in height to be eaten back along a whole line of

coast at the rate of one yard in nearly every twenty-two years. I

doubt whether any rock, even as soft as chalk, would yield at this rate

excepting on the most exposed coasts; though no doubt the degradation

of a lofty cliff would be more rapid from the breakage of the fallen

fragments. On the other hand, I do not believe that any line of coast,

ten or twenty miles in length, ever suffers degradation at the same time

along its whole indented length; and we must remember that almost all

strata contain harder layers or nodules, which from long resisting

attrition form a breakwater at the base. Hence, under ordinary

circumstances, I conclude that for a cliff 500 feet in height, a

denudation of one inch per century for the whole length would be an

ample allowance. At this rate, on the above data, the denudation of the

Weald must have required 306,662,400 years; or say three hundred million

years.

The action of fresh water on the gently inclined Wealden district, when

upraised, could hardly have been great, but it would somewhat reduce the

above estimate. On the other hand, during oscillations of level, which

we know this area has undergone, the surface may have existed for

millions of years as land, and thus have escaped the action of the

sea: when deeply submerged for perhaps equally long periods, it would,

likewise, have escaped the action of the coast-waves. So that in all

probability a far longer period than 300 million years has elapsed since

the latter part of the Secondary period.

I have made these few remarks because it is highly important for us to

gain some notion, however imperfect, of the lapse of years. During each

of these years, over the whole world, the land and the water has

been peopled by hosts of living forms. What an infinite number of

generations, which the mind cannot grasp, must have succeeded each other

in the long roll of years! Now turn to our richest geological museums,

and what a paltry display we behold!

ON THE POORNESS OF OUR PALAEONTOLOGICAL COLLECTIONS.

That our palaeontological collections are very imperfect, is admitted by

every one. The remark of that admirable palaeontologist, the late Edward

Forbes, should not be forgotten, namely, that numbers of our fossil

species are known and named from single and often broken specimens, or

from a few specimens collected on some one spot. Only a small portion

of the surface of the earth has been geologically explored, and no part

with sufficient care, as the important discoveries made every year in

Europe prove. No organism wholly soft can be preserved. Shells and

bones will decay and disappear when left on the bottom of the sea, where

sediment is not accumulating. I believe we are continually taking a

most erroneous view, when we tacitly admit to ourselves that sediment

is being deposited over nearly the whole bed of the sea, at a rate

sufficiently quick to embed and preserve fossil remains. Throughout an

enormously large proportion of the ocean, the bright blue tint of the

water bespeaks its purity. The many cases on record of a formation

conformably covered, after an enormous interval of time, by another

and later formation, without the underlying bed having suffered in the

interval any wear and tear, seem explicable only on the view of the

bottom of the sea not rarely lying for ages in an unaltered condition.

The remains which do become embedded, if in sand or gravel, will when

the beds are upraised generally be dissolved by the percolation of

rain-water. I suspect that but few of the very many animals which live

on the beach between high and low watermark are preserved. For instance,

the several species of the Chthamalinae (a sub-family of sessile

cirripedes) coat the rocks all over the world in infinite numbers: they

are all strictly littoral, with the exception of a single Mediterranean

species, which inhabits deep water and has been found fossil in Sicily,

whereas not one other species has hitherto been found in any tertiary

formation: yet it is now known that the genus Chthamalus existed

during the chalk period. The molluscan genus Chiton offers a partially

analogous case.

With respect to the terrestrial productions which lived during the

Secondary and Palaeozoic periods, it is superfluous to state that our

evidence from fossil remains is fragmentary in an extreme degree. For

instance, not a land shell is known belonging to either of these

vast periods, with one exception discovered by Sir C. Lyell in the

carboniferous strata of North America. In regard to mammiferous remains,

a single glance at the historical table published in the Supplement to

Lyell's Manual, will bring home the truth, how accidental and rare is

their preservation, far better than pages of detail. Nor is their rarity

surprising, when we remember how large a proportion of the bones of

tertiary mammals have been discovered either in caves or in lacustrine

deposits; and that not a cave or true lacustrine bed is known belonging

to the age of our secondary or palaeozoic formations.

But the imperfection in the geological record mainly results from

another and more important cause than any of the foregoing; namely, from

the several formations being separated from each other by wide intervals

of time. When we see the formations tabulated in written works, or when

we follow them in nature, it is difficult to avoid believing that

they are closely consecutive. But we know, for instance, from Sir R.

Murchison's great work on Russia, what wide gaps there are in that

country between the superimposed formations; so it is in North America,

and in many other parts of the world. The most skilful geologist, if

his attention had been exclusively confined to these large territories,

would never have suspected that during the periods which were blank and

barren in his own country, great piles of sediment, charged with new and

peculiar forms of life, had elsewhere been accumulated. And if in each

separate territory, hardly any idea can be formed of the length of time

which has elapsed between the consecutive formations, we may infer that

this could nowhere be ascertained. The frequent and great changes in the

mineralogical composition of consecutive formations, generally implying

great changes in the geography of the surrounding lands, whence the

sediment has been derived, accords with the belief of vast intervals of

time having elapsed between each formation.

But we can, I think, see why the geological formations of each region

are almost invariably intermittent; that is, have not followed each

other in close sequence. Scarcely any fact struck me more when examining

many hundred miles of the South American coasts, which have been

upraised several hundred feet within the recent period, than the absence

of any recent deposits sufficiently extensive to last for even a short

geological period. Along the whole west coast, which is inhabited by a

peculiar marine fauna, tertiary beds are so scantily developed, that no

record of several successive and peculiar marine faunas will probably be

preserved to a distant age. A little reflection will explain why along

the rising coast of the western side of South America, no extensive

formations with recent or tertiary remains can anywhere be found, though

the supply of sediment must for ages have been great, from the enormous

degradation of the coast-rocks and from muddy streams entering the

sea. The explanation, no doubt, is, that the littoral and sub-littoral

deposits are continually worn away, as soon as they are brought up by

the slow and gradual rising of the land within the grinding action of

the coast-waves.

We may, I think, safely conclude that sediment must be accumulated in

extremely thick, solid, or extensive masses, in order to withstand the

incessant action of the waves, when first upraised and during subsequent

oscillations of level. Such thick and extensive accumulations of

sediment may be formed in two ways; either, in profound depths of the

sea, in which case, judging from the researches of E. Forbes, we may

conclude that the bottom will be inhabited by extremely few animals, and

the mass when upraised will give a most imperfect record of the forms

of life which then existed; or, sediment may be accumulated to any

thickness and extent over a shallow bottom, if it continue slowly to

subside. In this latter case, as long as the rate of subsidence and

supply of sediment nearly balance each other, the sea will remain

shallow and favourable for life, and thus a fossiliferous formation

thick enough, when upraised, to resist any amount of degradation, may be

formed.

I am convinced that all our ancient formations, which are rich in

fossils, have thus been formed during subsidence. Since publishing my

views on this subject in 1845, I have watched the progress of Geology,

and have been surprised to note how author after author, in treating

of this or that great formation, has come to the conclusion that it was

accumulated during subsidence. I may add, that the only ancient tertiary

formation on the west coast of South America, which has been bulky

enough to resist such degradation as it has as yet suffered, but which

will hardly last to a distant geological age, was certainly deposited

during a downward oscillation of level, and thus gained considerable

thickness.

All geological facts tell us plainly that each area has undergone

numerous slow oscillations of level, and apparently these oscillations

have affected wide spaces. Consequently formations rich in fossils and

sufficiently thick and extensive to resist subsequent degradation, may

have been formed over wide spaces during periods of subsidence, but only

where the supply of sediment was sufficient to keep the sea shallow and

to embed and preserve the remains before they had time to decay. On the

other hand, as long as the bed of the sea remained stationary, THICK

deposits could not have been accumulated in the shallow parts, which are

the most favourable to life. Still less could this have happened during

the alternate periods of elevation; or, to speak more accurately, the

beds which were then accumulated will have been destroyed by being

upraised and brought within the limits of the coast-action.

Thus the geological record will almost necessarily be rendered

intermittent. I feel much confidence in the truth of these views, for

they are in strict accordance with the general principles inculcated

by Sir C. Lyell; and E. Forbes independently arrived at a similar

conclusion.

One remark is here worth a passing notice. During periods of elevation

the area of the land and of the adjoining shoal parts of the sea will

be increased, and new stations will often be formed;--all circumstances

most favourable, as previously explained, for the formation of new

varieties and species; but during such periods there will generally be

a blank in the geological record. On the other hand, during subsidence,

the inhabited area and number of inhabitants will decrease (excepting

the productions on the shores of a continent when first broken up into

an archipelago), and consequently during subsidence, though there will

be much extinction, fewer new varieties or species will be formed; and

it is during these very periods of subsidence, that our great deposits

rich in fossils have been accumulated. Nature may almost be said to have

guarded against the frequent discovery of her transitional or linking

forms.

From the foregoing considerations it cannot be doubted that the

geological record, viewed as a whole, is extremely imperfect; but if we

confine our attention to any one formation, it becomes more difficult

to understand, why we do not therein find closely graduated varieties

between the allied species which lived at its commencement and at its

close. Some cases are on record of the same species presenting distinct

varieties in the upper and lower parts of the same formation, but, as

they are rare, they may be here passed over. Although each formation has

indisputably required a vast number of years for its deposition, I can

see several reasons why each should not include a graduated series

of links between the species which then lived; but I can by no

means pretend to assign due proportional weight to the following

considerations.

Although each formation may mark a very long lapse of years, each

perhaps is short compared with the period requisite to change one

species into another. I am aware that two palaeontologists, whose

opinions are worthy of much deference, namely Bronn and Woodward, have

concluded that the average duration of each formation is twice or thrice

as long as the average duration of specific forms. But insuperable

difficulties, as it seems to me, prevent us coming to any just

conclusion on this head. When we see a species first appearing in the

middle of any formation, it would be rash in the extreme to infer that

it had not elsewhere previously existed. So again when we find a species

disappearing before the uppermost layers have been deposited, it would

be equally rash to suppose that it then became wholly extinct. We forget

how small the area of Europe is compared with the rest of the world;

nor have the several stages of the same formation throughout Europe been

correlated with perfect accuracy.

With marine animals of all kinds, we may safely infer a large amount of

migration during climatal and other changes; and when we see a species

first appearing in any formation, the probability is that it only then

first immigrated into that area. It is well known, for instance, that

several species appeared somewhat earlier in the palaeozoic beds of

North America than in those of Europe; time having apparently been

required for their migration from the American to the European seas. In

examining the latest deposits of various quarters of the world, it has

everywhere been noted, that some few still existing species are common

in the deposit, but have become extinct in the immediately surrounding

sea; or, conversely, that some are now abundant in the neighbouring sea,

but are rare or absent in this particular deposit. It is an excellent

lesson to reflect on the ascertained amount of migration of the

inhabitants of Europe during the Glacial period, which forms only a part

of one whole geological period; and likewise to reflect on the great

changes of level, on the inordinately great change of climate, on the

prodigious lapse of time, all included within this same glacial period.

Yet it may be doubted whether in any quarter of the world, sedimentary

deposits, INCLUDING FOSSIL REMAINS, have gone on accumulating within

the same area during the whole of this period. It is not, for instance,

probable that sediment was deposited during the whole of the glacial

period near the mouth of the Mississippi, within that limit of depth at

which marine animals can flourish; for we know what vast geographical

changes occurred in other parts of America during this space of time.

When such beds as were deposited in shallow water near the mouth of

the Mississippi during some part of the glacial period shall have been

upraised, organic remains will probably first appear and disappear at

different levels, owing to the migration of species and to geographical

changes. And in the distant future, a geologist examining these beds,

might be tempted to conclude that the average duration of life of the

embedded fossils had been less than that of the glacial period, instead

of having been really far greater, that is extending from before the

glacial epoch to the present day.

In order to get a perfect gradation between two forms in the upper

and lower parts of the same formation, the deposit must have gone on

accumulating for a very long period, in order to have given sufficient

time for the slow process of variation; hence the deposit will generally

have to be a very thick one; and the species undergoing modification

will have had to live on the same area throughout this whole time.

But we have seen that a thick fossiliferous formation can only be

accumulated during a period of subsidence; and to keep the depth

approximately the same, which is necessary in order to enable the same

species to live on the same space, the supply of sediment must nearly

have counterbalanced the amount of subsidence. But this same movement

of subsidence will often tend to sink the area whence the sediment

is derived, and thus diminish the supply whilst the downward movement

continues. In fact, this nearly exact balancing between the supply of

sediment and the amount of subsidence is probably a rare contingency;

for it has been observed by more than one palaeontologist, that very

thick deposits are usually barren of organic remains, except near their

upper or lower limits.

It would seem that each separate formation, like the whole pile of

formations in any country, has generally been intermittent in its

accumulation. When we see, as is so often the case, a formation composed

of beds of different mineralogical composition, we may reasonably

suspect that the process of deposition has been much interrupted, as

a change in the currents of the sea and a supply of sediment of a

different nature will generally have been due to geographical changes

requiring much time. Nor will the closest inspection of a formation give

any idea of the time which its deposition has consumed. Many instances

could be given of beds only a few feet in thickness, representing

formations, elsewhere thousands of feet in thickness, and which must

have required an enormous period for their accumulation; yet no one

ignorant of this fact would have suspected the vast lapse of time

represented by the thinner formation. Many cases could be given of the

lower beds of a formation having been upraised, denuded, submerged, and

then re-covered by the upper beds of the same formation,--facts,

showing what wide, yet easily overlooked, intervals have occurred in

its accumulation. In other cases we have the plainest evidence in great

fossilised trees, still standing upright as they grew, of many long

intervals of time and changes of level during the process of deposition,

which would never even have been suspected, had not the trees chanced to

have been preserved: thus, Messrs. Lyell and Dawson found carboniferous

beds 1400 feet thick in Nova Scotia, with ancient root-bearing strata,

one above the other, at no less than sixty-eight different levels.

Hence, when the same species occur at the bottom, middle, and top of a

formation, the probability is that they have not lived on the same

spot during the whole period of deposition, but have disappeared and

reappeared, perhaps many times, during the same geological period.

So that if such species were to undergo a considerable amount of

modification during any one geological period, a section would not

probably include all the fine intermediate gradations which must on

my theory have existed between them, but abrupt, though perhaps very

slight, changes of form.

It is all-important to remember that naturalists have no golden rule

by which to distinguish species and varieties; they grant some little

variability to each species, but when they meet with a somewhat greater

amount of difference between any two forms, they rank both as species,

unless they are enabled to connect them together by close intermediate

gradations. And this from the reasons just assigned we can seldom hope

to effect in any one geological section. Supposing B and C to be two

species, and a third, A, to be found in an underlying bed; even if A

were strictly intermediate between B and C, it would simply be ranked as

a third and distinct species, unless at the same time it could be

most closely connected with either one or both forms by intermediate

varieties. Nor should it be forgotten, as before explained, that A

might be the actual progenitor of B and C, and yet might not at all

necessarily be strictly intermediate between them in all points of

structure. So that we might obtain the parent-species and its several

modified descendants from the lower and upper beds of a formation,

and unless we obtained numerous transitional gradations, we should not

recognise their relationship, and should consequently be compelled to

rank them all as distinct species.

It is notorious on what excessively slight differences many

palaeontologists have founded their species; and they do this the more

readily if the specimens come from different sub-stages of the same

formation. Some experienced conchologists are now sinking many of the

very fine species of D'Orbigny and others into the rank of varieties;

and on this view we do find the kind of evidence of change which on my

theory we ought to find. Moreover, if we look to rather wider intervals,

namely, to distinct but consecutive stages of the same great formation,

we find that the embedded fossils, though almost universally ranked as

specifically different, yet are far more closely allied to each other

than are the species found in more widely separated formations; but to

this subject I shall have to return in the following chapter.

One other consideration is worth notice: with animals and plants that

can propagate rapidly and are not highly locomotive, there is reason to

suspect, as we have formerly seen, that their varieties are generally

at first local; and that such local varieties do not spread widely and

supplant their parent-forms until they have been modified and perfected

in some considerable degree. According to this view, the chance of

discovering in a formation in any one country all the early stages of

transition between any two forms, is small, for the successive changes

are supposed to have been local or confined to some one spot. Most

marine animals have a wide range; and we have seen that with plants it

is those which have the widest range, that oftenest present varieties;

so that with shells and other marine animals, it is probably those

which have had the widest range, far exceeding the limits of the known

geological formations of Europe, which have oftenest given rise, first

to local varieties and ultimately to new species; and this again would

greatly lessen the chance of our being able to trace the stages of

transition in any one geological formation.

It should not be forgotten, that at the present day, with perfect

specimens for examination, two forms can seldom be connected by

intermediate varieties and thus proved to be the same species, until

many specimens have been collected from many places; and in the case

of fossil species this could rarely be effected by palaeontologists. We

shall, perhaps, best perceive the improbability of our being enabled to

connect species by numerous, fine, intermediate, fossil links, by asking

ourselves whether, for instance, geologists at some future period will

be able to prove, that our different breeds of cattle, sheep, horses,

and dogs have descended from a single stock or from several aboriginal

stocks; or, again, whether certain sea-shells inhabiting the shores

of North America, which are ranked by some conchologists as distinct

species from their European representatives, and by other conchologists

as only varieties, are really varieties or are, as it is called,

specifically distinct. This could be effected only by the future

geologist discovering in a fossil state numerous intermediate

gradations; and such success seems to me improbable in the highest

degree.

Geological research, though it has added numerous species to existing

and extinct genera, and has made the intervals between some few groups

less wide than they otherwise would have been, yet has done scarcely

anything in breaking down the distinction between species, by connecting

them together by numerous, fine, intermediate varieties; and this not

having been effected, is probably the gravest and most obvious of all

the many objections which may be urged against my views. Hence it will

be worth while to sum up the foregoing remarks, under an imaginary

illustration. The Malay Archipelago is of about the size of Europe from

the North Cape to the Mediterranean, and from Britain to Russia; and

therefore equals all the geological formations which have been examined

with any accuracy, excepting those of the United States of America. I

fully agree with Mr. Godwin-Austen, that the present condition of the

Malay Archipelago, with its numerous large islands separated by wide and

shallow seas, probably represents the former state of Europe, when most

of our formations were accumulating. The Malay Archipelago is one of

the richest regions of the whole world in organic beings; yet if all

the species were to be collected which have ever lived there, how

imperfectly would they represent the natural history of the world!

But we have every reason to believe that the terrestrial productions of

the archipelago would be preserved in an excessively imperfect manner in

the formations which we suppose to be there accumulating. I suspect that

not many of the strictly littoral animals, or of those which lived on

naked submarine rocks, would be embedded; and those embedded in gravel

or sand, would not endure to a distant epoch. Wherever sediment did not

accumulate on the bed of the sea, or where it did not accumulate at a

sufficient rate to protect organic bodies from decay, no remains could

be preserved.

In our archipelago, I believe that fossiliferous formations could be

formed of sufficient thickness to last to an age, as distant in futurity

as the secondary formations lie in the past, only during periods of

subsidence. These periods of subsidence would be separated from each

other by enormous intervals, during which the area would be either

stationary or rising; whilst rising, each fossiliferous formation

would be destroyed, almost as soon as accumulated, by the incessant

coast-action, as we now see on the shores of South America. During the

periods of subsidence there would probably be much extinction of life;

during the periods of elevation, there would be much variation, but the

geological record would then be least perfect.

It may be doubted whether the duration of any one great period of

subsidence over the whole or part of the archipelago, together with

a contemporaneous accumulation of sediment, would EXCEED the average

duration of the same specific forms; and these contingencies are

indispensable for the preservation of all the transitional gradations

between any two or more species. If such gradations were not fully

preserved, transitional varieties would merely appear as so many

distinct species. It is, also, probable that each great period of

subsidence would be interrupted by oscillations of level, and that

slight climatal changes would intervene during such lengthy periods; and

in these cases the inhabitants of the archipelago would have to migrate,

and no closely consecutive record of their modifications could be

preserved in any one formation.

Very many of the marine inhabitants of the archipelago now range

thousands of miles beyond its confines; and analogy leads me to believe

that it would be chiefly these far-ranging species which would oftenest

produce new varieties; and the varieties would at first generally

be local or confined to one place, but if possessed of any decided

advantage, or when further modified and improved, they would slowly

spread and supplant their parent-forms. When such varieties returned to

their ancient homes, as they would differ from their former state, in

a nearly uniform, though perhaps extremely slight degree, they would,

according to the principles followed by many palaeontologists, be ranked

as new and distinct species.

If then, there be some degree of truth in these remarks, we have no

right to expect to find in our geological formations, an infinite number

of those fine transitional forms, which on my theory assuredly have

connected all the past and present species of the same group into one

long and branching chain of life. We ought only to look for a few links,

some more closely, some more distantly related to each other; and these

links, let them be ever so close, if found in different stages of the

same formation, would, by most palaeontologists, be ranked as distinct

species. But I do not pretend that I should ever have suspected how poor

a record of the mutations of life, the best preserved geological section

presented, had not the difficulty of our not discovering innumerable

transitional links between the species which appeared at the

commencement and close of each formation, pressed so hardly on my

theory.

ON THE SUDDEN APPEARANCE OF WHOLE GROUPS OF ALLIED SPECIES.

The abrupt manner in which whole groups of species suddenly appear in

certain formations, has been urged by several palaeontologists,

for instance, by Agassiz, Pictet, and by none more forcibly than

by Professor Sedgwick, as a fatal objection to the belief in the

transmutation of species. If numerous species, belonging to the same

genera or families, have really started into life all at once, the fact

would be fatal to the theory of descent with slow modification through

natural selection. For the development of a group of forms, all of which

have descended from some one progenitor, must have been an extremely

slow process; and the progenitors must have lived long ages before their

modified descendants. But we continually over-rate the perfection of the

geological record, and falsely infer, because certain genera or families

have not been found beneath a certain stage, that they did not exist

before that stage. We continually forget how large the world is,

compared with the area over which our geological formations have been

carefully examined; we forget that groups of species may elsewhere have

long existed and have slowly multiplied before they invaded the ancient

archipelagoes of Europe and of the United States. We do not make due

allowance for the enormous intervals of time, which have probably

elapsed between our consecutive formations,--longer perhaps in some

cases than the time required for the accumulation of each formation.

These intervals will have given time for the multiplication of species

from some one or some few parent-forms; and in the succeeding formation

such species will appear as if suddenly created.

I may here recall a remark formerly made, namely that it might require

a long succession of ages to adapt an organism to some new and peculiar

line of life, for instance to fly through the air; but that when this

had been effected, and a few species had thus acquired a great advantage

over other organisms, a comparatively short time would be necessary to

produce many divergent forms, which would be able to spread rapidly and

widely throughout the world.

I will now give a few examples to illustrate these remarks; and to show

how liable we are to error in supposing that whole groups of species

have suddenly been produced. I may recall the well-known fact that in

geological treatises, published not many years ago, the great class

of mammals was always spoken of as having abruptly come in at the

commencement of the tertiary series. And now one of the richest known

accumulations of fossil mammals belongs to the middle of the secondary

series; and one true mammal has been discovered in the new red sandstone

at nearly the commencement of this great series. Cuvier used to urge

that no monkey occurred in any tertiary stratum; but now extinct species

have been discovered in India, South America, and in Europe even as far

back as the eocene stage. The most striking case, however, is that of

the Whale family; as these animals have huge bones, are marine, and

range over the world, the fact of not a single bone of a whale having

been discovered in any secondary formation, seemed fully to justify the

belief that this great and distinct order had been suddenly produced

in the interval between the latest secondary and earliest tertiary

formation. But now we may read in the Supplement to Lyell's 'Manual,'

published in 1858, clear evidence of the existence of whales in the

upper greensand, some time before the close of the secondary period.

I may give another instance, which from having passed under my own eyes

has much struck me. In a memoir on Fossil Sessile Cirripedes, I have

stated that, from the number of existing and extinct tertiary species;

from the extraordinary abundance of the individuals of many species

all over the world, from the Arctic regions to the equator, inhabiting

various zones of depths from the upper tidal limits to 50 fathoms;

from the perfect manner in which specimens are preserved in the oldest

tertiary beds; from the ease with which even a fragment of a valve can

be recognised; from all these circumstances, I inferred that had sessile

cirripedes existed during the secondary periods, they would certainly

have been preserved and discovered; and as not one species had been

discovered in beds of this age, I concluded that this great group had

been suddenly developed at the commencement of the tertiary series. This

was a sore trouble to me, adding as I thought one more instance of the

abrupt appearance of a great group of species. But my work had hardly

been published, when a skilful palaeontologist, M. Bosquet, sent me a

drawing of a perfect specimen of an unmistakeable sessile cirripede,

which he had himself extracted from the chalk of Belgium. And, as if

to make the case as striking as possible, this sessile cirripede was a

Chthamalus, a very common, large, and ubiquitous genus, of which not one

specimen has as yet been found even in any tertiary stratum. Hence we

now positively know that sessile cirripedes existed during the secondary

period; and these cirripedes might have been the progenitors of our many

tertiary and existing species.

The case most frequently insisted on by palaeontologists of the

apparently sudden appearance of a whole group of species, is that of the

teleostean fishes, low down in the Chalk period. This group includes the

large majority of existing species. Lately, Professor Pictet has carried

their existence one sub-stage further back; and some palaeontologists

believe that certain much older fishes, of which the affinities are as

yet imperfectly known, are really teleostean. Assuming, however, that

the whole of them did appear, as Agassiz believes, at the commencement

of the chalk formation, the fact would certainly be highly remarkable;

but I cannot see that it would be an insuperable difficulty on my

theory, unless it could likewise be shown that the species of this group

appeared suddenly and simultaneously throughout the world at this same

period. It is almost superfluous to remark that hardly any fossil-fish

are known from south of the equator; and by running through Pictet's

Palaeontology it will be seen that very few species are known from

several formations in Europe. Some few families of fish now have a

confined range; the teleostean fish might formerly have had a similarly

confined range, and after having been largely developed in some one sea,

might have spread widely. Nor have we any right to suppose that the seas

of the world have always been so freely open from south to north as

they are at present. Even at this day, if the Malay Archipelago were

converted into land, the tropical parts of the Indian Ocean would form

a large and perfectly enclosed basin, in which any great group of marine

animals might be multiplied; and here they would remain confined, until

some of the species became adapted to a cooler climate, and were enabled

to double the southern capes of Africa or Australia, and thus reach

other and distant seas.

From these and similar considerations, but chiefly from our ignorance

of the geology of other countries beyond the confines of Europe and the

United States; and from the revolution in our palaeontological ideas

on many points, which the discoveries of even the last dozen years have

effected, it seems to me to be about as rash in us to dogmatize on the

succession of organic beings throughout the world, as it would be for

a naturalist to land for five minutes on some one barren point in

Australia, and then to discuss the number and range of its productions.

ON THE SUDDEN APPEARANCE OF GROUPS OF ALLIED SPECIES IN THE LOWEST KNOWN

FOSSILIFEROUS STRATA.

There is another and allied difficulty, which is much graver. I allude

to the manner in which numbers of species of the same group, suddenly

appear in the lowest known fossiliferous rocks. Most of the arguments

which have convinced me that all the existing species of the same group

have descended from one progenitor, apply with nearly equal force to

the earliest known species. For instance, I cannot doubt that all the

Silurian trilobites have descended from some one crustacean, which must

have lived long before the Silurian age, and which probably differed

greatly from any known animal. Some of the most ancient Silurian

animals, as the Nautilus, Lingula, etc., do not differ much from living

species; and it cannot on my theory be supposed, that these old species

were the progenitors of all the species of the orders to which they

belong, for they do not present characters in any degree intermediate

between them. If, moreover, they had been the progenitors of these

orders, they would almost certainly have been long ago supplanted and

exterminated by their numerous and improved descendants.

Consequently, if my theory be true, it is indisputable that before the

lowest Silurian stratum was deposited, long periods elapsed, as long as,

or probably far longer than, the whole interval from the Silurian age to

the present day; and that during these vast, yet quite unknown, periods

of time, the world swarmed with living creatures.

To the question why we do not find records of these vast primordial

periods, I can give no satisfactory answer. Several of the most eminent

geologists, with Sir R. Murchison at their head, are convinced that we

see in the organic remains of the lowest Silurian stratum the dawn of

life on this planet. Other highly competent judges, as Lyell and the

late E. Forbes, dispute this conclusion. We should not forget that only

a small portion of the world is known with accuracy. M. Barrande has

lately added another and lower stage to the Silurian system, abounding

with new and peculiar species. Traces of life have been detected in the

Longmynd beds beneath Barrande's so-called primordial zone. The presence

of phosphatic nodules and bituminous matter in some of the lowest azoic

rocks, probably indicates the former existence of life at these periods.

But the difficulty of understanding the absence of vast piles of

fossiliferous strata, which on my theory no doubt were somewhere

accumulated before the Silurian epoch, is very great. If these most

ancient beds had been wholly worn away by denudation, or obliterated

by metamorphic action, we ought to find only small remnants of the

formations next succeeding them in age, and these ought to be very

generally in a metamorphosed condition. But the descriptions which we

now possess of the Silurian deposits over immense territories in

Russia and in North America, do not support the view, that the older a

formation is, the more it has suffered the extremity of denudation and

metamorphism.

The case at present must remain inexplicable; and may be truly urged as

a valid argument against the views here entertained. To show that it

may hereafter receive some explanation, I will give the following

hypothesis. From the nature of the organic remains, which do not appear

to have inhabited profound depths, in the several formations of Europe

and of the United States; and from the amount of sediment, miles in

thickness, of which the formations are composed, we may infer that from

first to last large islands or tracts of land, whence the sediment was

derived, occurred in the neighbourhood of the existing continents of

Europe and North America. But we do not know what was the state of

things in the intervals between the successive formations; whether

Europe and the United States during these intervals existed as dry

land, or as a submarine surface near land, on which sediment was not

deposited, or again as the bed of an open and unfathomable sea.

Looking to the existing oceans, which are thrice as extensive as the

land, we see them studded with many islands; but not one oceanic island

is as yet known to afford even a remnant of any palaeozoic or secondary

formation. Hence we may perhaps infer, that during the palaeozoic and

secondary periods, neither continents nor continental islands existed

where our oceans now extend; for had they existed there, palaeozoic and

secondary formations would in all probability have been accumulated from

sediment derived from their wear and tear; and would have been at least

partially upheaved by the oscillations of level, which we may fairly

conclude must have intervened during these enormously long periods. If

then we may infer anything from these facts, we may infer that where

our oceans now extend, oceans have extended from the remotest period of

which we have any record; and on the other hand, that where continents

now exist, large tracts of land have existed, subjected no doubt to

great oscillations of level, since the earliest silurian period. The

coloured map appended to my volume on Coral Reefs, led me to conclude

that the great oceans are still mainly areas of subsidence, the great

archipelagoes still areas of oscillations of level, and the continents

areas of elevation. But have we any right to assume that things have

thus remained from eternity? Our continents seem to have been formed

by a preponderance, during many oscillations of level, of the force of

elevation; but may not the areas of preponderant movement have changed

in the lapse of ages? At a period immeasurably antecedent to the

silurian epoch, continents may have existed where oceans are now spread

out; and clear and open oceans may have existed where our continents now

stand. Nor should we be justified in assuming that if, for instance, the

bed of the Pacific Ocean were now converted into a continent, we should

there find formations older than the silurian strata, supposing such to

have been formerly deposited; for it might well happen that strata which

had subsided some miles nearer to the centre of the earth, and which

had been pressed on by an enormous weight of superincumbent water, might

have undergone far more metamorphic action than strata which have always

remained nearer to the surface. The immense areas in some parts of the

world, for instance in South America, of bare metamorphic rocks, which

must have been heated under great pressure, have always seemed to me to

require some special explanation; and we may perhaps believe that we see

in these large areas, the many formations long anterior to the silurian

epoch in a completely metamorphosed condition.

The several difficulties here discussed, namely our not finding in the

successive formations infinitely numerous transitional links between the

many species which now exist or have existed; the sudden manner in which

whole groups of species appear in our European formations; the almost

entire absence, as at present known, of fossiliferous formations beneath

the Silurian strata, are all undoubtedly of the gravest nature. We

see this in the plainest manner by the fact that all the most eminent

palaeontologists, namely Cuvier, Owen, Agassiz, Barrande, Falconer,

E. Forbes, etc., and all our greatest geologists, as Lyell, Murchison,

Sedgwick, etc., have unanimously, often vehemently, maintained the

immutability of species. But I have reason to believe that one great

authority, Sir Charles Lyell, from further reflexion entertains grave

doubts on this subject. I feel how rash it is to differ from these great

authorities, to whom, with others, we owe all our knowledge. Those who

think the natural geological record in any degree perfect, and who do

not attach much weight to the facts and arguments of other kinds given

in this volume, will undoubtedly at once reject my theory. For my part,

following out Lyell's metaphor, I look at the natural geological record,

as a history of the world imperfectly kept, and written in a changing

dialect; of this history we possess the last volume alone, relating only

to two or three countries. Of this volume, only here and there a short

chapter has been preserved; and of each page, only here and there a few

lines. Each word of the slowly-changing language, in which the

history is supposed to be written, being more or less different in

the interrupted succession of chapters, may represent the apparently

abruptly changed forms of life, entombed in our consecutive, but widely

separated formations. On this view, the difficulties above discussed are

greatly diminished, or even disappear.

10. ON THE GEOLOGICAL SUCCESSION OF ORGANIC BEINGS.

On the slow and successive appearance of new species. On their different

rates of change. Species once lost do not reappear. Groups of species

follow the same general rules in their appearance and disappearance as

do single species. On Extinction. On simultaneous changes in the forms

of life throughout the world. On the affinities of extinct species to

each other and to living species. On the state of development of ancient

forms. On the succession of the same types within the same areas.

Summary of preceding and present chapters.

Let us now see whether the several facts and rules relating to the

geological succession of organic beings, better accord with the common

view of the immutability of species, or with that of their slow and

gradual modification, through descent and natural selection.

New species have appeared very slowly, one after another, both on the

land and in the waters. Lyell has shown that it is hardly possible to

resist the evidence on this head in the case of the several tertiary

stages; and every year tends to fill up the blanks between them, and to

make the percentage system of lost and new forms more gradual. In

some of the most recent beds, though undoubtedly of high antiquity if

measured by years, only one or two species are lost forms, and only one

or two are new forms, having here appeared for the first time, either

locally, or, as far as we know, on the face of the earth. If we may

trust the observations of Philippi in Sicily, the successive changes in

the marine inhabitants of that island have been many and most gradual.

The secondary formations are more broken; but, as Bronn has remarked,

neither the appearance nor disappearance of their many now extinct

species has been simultaneous in each separate formation.

Species of different genera and classes have not changed at the same

rate, or in the same degree. In the oldest tertiary beds a few living

shells may still be found in the midst of a multitude of extinct forms.

Falconer has given a striking instance of a similar fact, in an existing

crocodile associated with many strange and lost mammals and reptiles in

the sub-Himalayan deposits. The Silurian Lingula differs but little from

the living species of this genus; whereas most of the other Silurian

Molluscs and all the Crustaceans have changed greatly. The productions

of the land seem to change at a quicker rate than those of the sea, of

which a striking instance has lately been observed in Switzerland. There

is some reason to believe that organisms, considered high in the scale

of nature, change more quickly than those that are low: though there

are exceptions to this rule. The amount of organic change, as Pictet

has remarked, does not strictly correspond with the succession of our

geological formations; so that between each two consecutive formations,

the forms of life have seldom changed in exactly the same degree. Yet if

we compare any but the most closely related formations, all the species

will be found to have undergone some change. When a species has once

disappeared from the face of the earth, we have reason to believe

that the same identical form never reappears. The strongest apparent

exception to this latter rule, is that of the so-called "colonies" of M.

Barrande, which intrude for a period in the midst of an older formation,

and then allow the pre-existing fauna to reappear; but Lyell's

explanation, namely, that it is a case of temporary migration from a

distinct geographical province, seems to me satisfactory.

These several facts accord well with my theory. I believe in no fixed

law of development, causing all the inhabitants of a country to change

abruptly, or simultaneously, or to an equal degree. The process of

modification must be extremely slow. The variability of each species

is quite independent of that of all others. Whether such variability be

taken advantage of by natural selection, and whether the variations be

accumulated to a greater or lesser amount, thus causing a greater or

lesser amount of modification in the varying species, depends on many

complex contingencies,--on the variability being of a beneficial nature,

on the power of intercrossing, on the rate of breeding, on the slowly

changing physical conditions of the country, and more especially on the

nature of the other inhabitants with which the varying species comes

into competition. Hence it is by no means surprising that one species

should retain the same identical form much longer than others; or,

if changing, that it should change less. We see the same fact in

geographical distribution; for instance, in the land-shells and

coleopterous insects of Madeira having come to differ considerably from

their nearest allies on the continent of Europe, whereas the marine

shells and birds have remained unaltered. We can perhaps understand

the apparently quicker rate of change in terrestrial and in more highly

organised productions compared with marine and lower productions, by

the more complex relations of the higher beings to their organic and

inorganic conditions of life, as explained in a former chapter. When

many of the inhabitants of a country have become modified and improved,

we can understand, on the principle of competition, and on that of the

many all-important relations of organism to organism, that any form

which does not become in some degree modified and improved, will be

liable to be exterminated. Hence we can see why all the species in the

same region do at last, if we look to wide enough intervals of time,

become modified; for those which do not change will become extinct.

In members of the same class the average amount of change, during long

and equal periods of time, may, perhaps, be nearly the same; but as the

accumulation of long-enduring fossiliferous formations depends on great

masses of sediment having been deposited on areas whilst subsiding,

our formations have been almost necessarily accumulated at wide and

irregularly intermittent intervals; consequently the amount of organic

change exhibited by the fossils embedded in consecutive formations

is not equal. Each formation, on this view, does not mark a new and

complete act of creation, but only an occasional scene, taken almost at

hazard, in a slowly changing drama.

We can clearly understand why a species when once lost should never

reappear, even if the very same conditions of life, organic and

inorganic, should recur. For though the offspring of one species might

be adapted (and no doubt this has occurred in innumerable instances) to

fill the exact place of another species in the economy of nature, and

thus supplant it; yet the two forms--the old and the new--would not be

identically the same; for both would almost certainly inherit different

characters from their distinct progenitors. For instance, it is just

possible, if our fantail-pigeons were all destroyed, that fanciers, by

striving during long ages for the same object, might make a new breed

hardly distinguishable from our present fantail; but if the parent

rock-pigeon were also destroyed, and in nature we have every reason

to believe that the parent-form will generally be supplanted and

exterminated by its improved offspring, it is quite incredible that a

fantail, identical with the existing breed, could be raised from any

other species of pigeon, or even from the other well-established races

of the domestic pigeon, for the newly-formed fantail would be almost

sure to inherit from its new progenitor some slight characteristic

differences.

Groups of species, that is, genera and families, follow the same general

rules in their appearance and disappearance as do single species,

changing more or less quickly, and in a greater or lesser degree. A

group does not reappear after it has once disappeared; or its existence,

as long as it lasts, is continuous. I am aware that there are some

apparent exceptions to this rule, but the exceptions are surprisingly

few, so few, that E. Forbes, Pictet, and Woodward (though all strongly

opposed to such views as I maintain) admit its truth; and the rule

strictly accords with my theory. For as all the species of the same

group have descended from some one species, it is clear that as long as

any species of the group have appeared in the long succession of ages,

so long must its members have continuously existed, in order to have

generated either new and modified or the same old and unmodified forms.

Species of the genus Lingula, for instance, must have continuously

existed by an unbroken succession of generations, from the lowest

Silurian stratum to the present day.

We have seen in the last chapter that the species of a group sometimes

falsely appear to have come in abruptly; and I have attempted to give

an explanation of this fact, which if true would have been fatal to my

views. But such cases are certainly exceptional; the general rule being

a gradual increase in number, till the group reaches its maximum, and

then, sooner or later, it gradually decreases. If the number of

the species of a genus, or the number of the genera of a family, be

represented by a vertical line of varying thickness, crossing the

successive geological formations in which the species are found, the

line will sometimes falsely appear to begin at its lower end, not in a

sharp point, but abruptly; it then gradually thickens upwards, sometimes

keeping for a space of equal thickness, and ultimately thins out in the

upper beds, marking the decrease and final extinction of the species.

This gradual increase in number of the species of a group is strictly

conformable with my theory; as the species of the same genus, and the

genera of the same family, can increase only slowly and progressively;

for the process of modification and the production of a number of allied

forms must be slow and gradual,--one species giving rise first to two

or three varieties, these being slowly converted into species, which in

their turn produce by equally slow steps other species, and so on, like

the branching of a great tree from a single stem, till the group becomes

large.

ON EXTINCTION.

We have as yet spoken only incidentally of the disappearance of species

and of groups of species. On the theory of natural selection the

extinction of old forms and the production of new and improved forms are

intimately connected together. The old notion of all the inhabitants of

the earth having been swept away at successive periods by catastrophes,

is very generally given up, even by those geologists, as Elie de

Beaumont, Murchison, Barrande, etc., whose general views would naturally

lead them to this conclusion. On the contrary, we have every reason to

believe, from the study of the tertiary formations, that species and

groups of species gradually disappear, one after another, first from one

spot, then from another, and finally from the world. Both single species

and whole groups of species last for very unequal periods; some groups,

as we have seen, having endured from the earliest known dawn of life

to the present day; some having disappeared before the close of the

palaeozoic period. No fixed law seems to determine the length of time

during which any single species or any single genus endures. There is

reason to believe that the complete extinction of the species of a group

is generally a slower process than their production: if the appearance

and disappearance of a group of species be represented, as before, by

a vertical line of varying thickness, the line is found to taper more

gradually at its upper end, which marks the progress of extermination,

than at its lower end, which marks the first appearance and increase

in numbers of the species. In some cases, however, the extermination

of whole groups of beings, as of ammonites towards the close of the

secondary period, has been wonderfully sudden.

The whole subject of the extinction of species has been involved in the

most gratuitous mystery. Some authors have even supposed that as the

individual has a definite length of life, so have species a definite

duration. No one I think can have marvelled more at the extinction of

species, than I have done. When I found in La Plata the tooth of a horse

embedded with the remains of Mastodon, Megatherium, Toxodon, and other

extinct monsters, which all co-existed with still living shells at a

very late geological period, I was filled with astonishment; for seeing

that the horse, since its introduction by the Spaniards into South

America, has run wild over the whole country and has increased in

numbers at an unparalleled rate, I asked myself what could so recently

have exterminated the former horse under conditions of life apparently

so favourable. But how utterly groundless was my astonishment! Professor

Owen soon perceived that the tooth, though so like that of the existing

horse, belonged to an extinct species. Had this horse been still

living, but in some degree rare, no naturalist would have felt the least

surprise at its rarity; for rarity is the attribute of a vast number of

species of all classes, in all countries. If we ask ourselves why this

or that species is rare, we answer that something is unfavourable in its

conditions of life; but what that something is, we can hardly ever tell.

On the supposition of the fossil horse still existing as a rare species,

we might have felt certain from the analogy of all other mammals,

even of the slow-breeding elephant, and from the history of the

naturalisation of the domestic horse in South America, that under more

favourable conditions it would in a very few years have stocked the

whole continent. But we could not have told what the unfavourable

conditions were which checked its increase, whether some one or several

contingencies, and at what period of the horse's life, and in what

degree, they severally acted. If the conditions had gone on, however

slowly, becoming less and less favourable, we assuredly should not have

perceived the fact, yet the fossil horse would certainly have become

rarer and rarer, and finally extinct;--its place being seized on by some

more successful competitor.

It is most difficult always to remember that the increase of every

living being is constantly being checked by unperceived injurious

agencies; and that these same unperceived agencies are amply sufficient

to cause rarity, and finally extinction. We see in many cases in the

more recent tertiary formations, that rarity precedes extinction; and we

know that this has been the progress of events with those animals which

have been exterminated, either locally or wholly, through man's agency.

I may repeat what I published in 1845, namely, that to admit that

species generally become rare before they become extinct--to feel no

surprise at the rarity of a species, and yet to marvel greatly when

it ceases to exist, is much the same as to admit that sickness in the

individual is the forerunner of death--to feel no surprise at sickness,

but when the sick man dies, to wonder and to suspect that he died by

some unknown deed of violence.

The theory of natural selection is grounded on the belief that each new

variety, and ultimately each new species, is produced and maintained by

having some advantage over those with which it comes into competition;

and the consequent extinction of less-favoured forms almost inevitably

follows. It is the same with our domestic productions: when a new and

slightly improved variety has been raised, it at first supplants the

less improved varieties in the same neighbourhood; when much improved it

is transported far and near, like our short-horn cattle, and takes the

place of other breeds in other countries. Thus the appearance of new

forms and the disappearance of old forms, both natural and artificial,

are bound together. In certain flourishing groups, the number of new

specific forms which have been produced within a given time is probably

greater than that of the old forms which have been exterminated; but we

know that the number of species has not gone on indefinitely increasing,

at least during the later geological periods, so that looking to later

times we may believe that the production of new forms has caused the

extinction of about the same number of old forms.

The competition will generally be most severe, as formerly explained

and illustrated by examples, between the forms which are most like each

other in all respects. Hence the improved and modified descendants of

a species will generally cause the extermination of the parent-species;

and if many new forms have been developed from any one species, the

nearest allies of that species, i.e. the species of the same genus, will

be the most liable to extermination. Thus, as I believe, a number of

new species descended from one species, that is a new genus, comes to

supplant an old genus, belonging to the same family. But it must often

have happened that a new species belonging to some one group will have

seized on the place occupied by a species belonging to a distinct group,

and thus caused its extermination; and if many allied forms be developed

from the successful intruder, many will have to yield their places; and

it will generally be allied forms, which will suffer from some inherited

inferiority in common. But whether it be species belonging to the same

or to a distinct class, which yield their places to other species which

have been modified and improved, a few of the sufferers may often long

be preserved, from being fitted to some peculiar line of life, or from

inhabiting some distant and isolated station, where they have escaped

severe competition. For instance, a single species of Trigonia, a great

genus of shells in the secondary formations, survives in the Australian

seas; and a few members of the great and almost extinct group of Ganoid

fishes still inhabit our fresh waters. Therefore the utter extinction

of a group is generally, as we have seen, a slower process than its

production.

With respect to the apparently sudden extermination of whole families

or orders, as of Trilobites at the close of the palaeozoic period and

of Ammonites at the close of the secondary period, we must remember what

has been already said on the probable wide intervals of time between our

consecutive formations; and in these intervals there may have been much

slow extermination. Moreover, when by sudden immigration or by unusually

rapid development, many species of a new group have taken possession

of a new area, they will have exterminated in a correspondingly rapid

manner many of the old inhabitants; and the forms which thus yield

their places will commonly be allied, for they will partake of some

inferiority in common.

Thus, as it seems to me, the manner in which single species and whole

groups of species become extinct, accords well with the theory of

natural selection. We need not marvel at extinction; if we must

marvel, let it be at our presumption in imagining for a moment that we

understand the many complex contingencies, on which the existence of

each species depends. If we forget for an instant, that each species

tends to increase inordinately, and that some check is always in action,

yet seldom perceived by us, the whole economy of nature will be utterly

obscured. Whenever we can precisely say why this species is more

abundant in individuals than that; why this species and not another

can be naturalised in a given country; then, and not till then, we may

justly feel surprise why we cannot account for the extinction of this

particular species or group of species.

ON THE FORMS OF LIFE CHANGING ALMOST SIMULTANEOUSLY THROUGHOUT THE

WORLD.

Scarcely any palaeontological discovery is more striking than the fact,

that the forms of life change almost simultaneously throughout the

world. Thus our European Chalk formation can be recognised in many

distant parts of the world, under the most different climates, where not

a fragment of the mineral chalk itself can be found; namely, in North

America, in equatorial South America, in Tierra del Fuego, at the

Cape of Good Hope, and in the peninsula of India. For at these distant

points, the organic remains in certain beds present an unmistakeable

degree of resemblance to those of the Chalk. It is not that the same

species are met with; for in some cases not one species is identically

the same, but they belong to the same families, genera, and sections

of genera, and sometimes are similarly characterised in such trifling

points as mere superficial sculpture. Moreover other forms, which are

not found in the Chalk of Europe, but which occur in the formations

either above or below, are similarly absent at these distant points of

the world. In the several successive palaeozoic formations of Russia,

Western Europe and North America, a similar parallelism in the forms of

life has been observed by several authors: so it is, according to Lyell,

with the several European and North American tertiary deposits. Even

if the few fossil species which are common to the Old and New Worlds be

kept wholly out of view, the general parallelism in the successive forms

of life, in the stages of the widely separated palaeozoic and tertiary

periods, would still be manifest, and the several formations could be

easily correlated.

These observations, however, relate to the marine inhabitants of distant

parts of the world: we have not sufficient data to judge whether the

productions of the land and of fresh water change at distant points in

the same parallel manner. We may doubt whether they have thus changed:

if the Megatherium, Mylodon, Macrauchenia, and Toxodon had been brought

to Europe from La Plata, without any information in regard to their

geological position, no one would have suspected that they had coexisted

with still living sea-shells; but as these anomalous monsters coexisted

with the Mastodon and Horse, it might at least have been inferred that

they had lived during one of the latter tertiary stages.

When the marine forms of life are spoken of as having changed

simultaneously throughout the world, it must not be supposed that this

expression relates to the same thousandth or hundred-thousandth year, or

even that it has a very strict geological sense; for if all the marine

animals which live at the present day in Europe, and all those that

lived in Europe during the pleistocene period (an enormously remote

period as measured by years, including the whole glacial epoch), were to

be compared with those now living in South America or in Australia, the

most skilful naturalist would hardly be able to say whether the existing

or the pleistocene inhabitants of Europe resembled most closely those of

the southern hemisphere. So, again, several highly competent observers

believe that the existing productions of the United States are more

closely related to those which lived in Europe during certain later

tertiary stages, than to those which now live here; and if this be so,

it is evident that fossiliferous beds deposited at the present day on

the shores of North America would hereafter be liable to be classed with

somewhat older European beds. Nevertheless, looking to a remotely future

epoch, there can, I think, be little doubt that all the more modern

MARINE formations, namely, the upper pliocene, the pleistocene and

strictly modern beds, of Europe, North and South America, and Australia,

from containing fossil remains in some degree allied, and from not

including those forms which are only found in the older underlying

deposits, would be correctly ranked as simultaneous in a geological

sense.

The fact of the forms of life changing simultaneously, in the above

large sense, at distant parts of the world, has greatly struck those

admirable observers, MM. de Verneuil and d'Archiac. After referring

to the parallelism of the palaeozoic forms of life in various parts

of Europe, they add, "If struck by this strange sequence, we turn our

attention to North America, and there discover a series of analogous

phenomena, it will appear certain that all these modifications of

species, their extinction, and the introduction of new ones, cannot be

owing to mere changes in marine currents or other causes more or less

local and temporary, but depend on general laws which govern the whole

animal kingdom." M. Barrande has made forcible remarks to precisely the

same effect. It is, indeed, quite futile to look to changes of currents,

climate, or other physical conditions, as the cause of these great

mutations in the forms of life throughout the world, under the most

different climates. We must, as Barrande has remarked, look to some

special law. We shall see this more clearly when we treat of the present

distribution of organic beings, and find how slight is the relation

between the physical conditions of various countries, and the nature of

their inhabitants.

This great fact of the parallel succession of the forms of life

throughout the world, is explicable on the theory of natural selection.

New species are formed by new varieties arising, which have some

advantage over older forms; and those forms, which are already dominant,

or have some advantage over the other forms in their own country, would

naturally oftenest give rise to new varieties or incipient species; for

these latter must be victorious in a still higher degree in order to be

preserved and to survive. We have distinct evidence on this head, in

the plants which are dominant, that is, which are commonest in their own

homes, and are most widely diffused, having produced the greatest number

of new varieties. It is also natural that the dominant, varying, and

far-spreading species, which already have invaded to a certain extent

the territories of other species, should be those which would have

the best chance of spreading still further, and of giving rise in new

countries to new varieties and species. The process of diffusion

may often be very slow, being dependent on climatal and geographical

changes, or on strange accidents, but in the long run the dominant

forms will generally succeed in spreading. The diffusion would, it

is probable, be slower with the terrestrial inhabitants of distinct

continents than with the marine inhabitants of the continuous sea. We

might therefore expect to find, as we apparently do find, a less strict

degree of parallel succession in the productions of the land than of the

sea.

Dominant species spreading from any region might encounter still more

dominant species, and then their triumphant course, or even their

existence, would cease. We know not at all precisely what are all the

conditions most favourable for the multiplication of new and dominant

species; but we can, I think, clearly see that a number of individuals,

from giving a better chance of the appearance of favourable variations,

and that severe competition with many already existing forms, would

be highly favourable, as would be the power of spreading into new

territories. A certain amount of isolation, recurring at long intervals

of time, would probably be also favourable, as before explained. One

quarter of the world may have been most favourable for the production

of new and dominant species on the land, and another for those in the

waters of the sea. If two great regions had been for a long period

favourably circumstanced in an equal degree, whenever their inhabitants

met, the battle would be prolonged and severe; and some from one

birthplace and some from the other might be victorious. But in the

course of time, the forms dominant in the highest degree, wherever

produced, would tend everywhere to prevail. As they prevailed, they

would cause the extinction of other and inferior forms; and as these

inferior forms would be allied in groups by inheritance, whole groups

would tend slowly to disappear; though here and there a single member

might long be enabled to survive.

Thus, as it seems to me, the parallel, and, taken in a large sense,

simultaneous, succession of the same forms of life throughout the world,

accords well with the principle of new species having been formed by

dominant species spreading widely and varying; the new species thus

produced being themselves dominant owing to inheritance, and to having

already had some advantage over their parents or over other species;

these again spreading, varying, and producing new species. The forms

which are beaten and which yield their places to the new and victorious

forms, will generally be allied in groups, from inheriting some

inferiority in common; and therefore as new and improved groups spread

throughout the world, old groups will disappear from the world; and the

succession of forms in both ways will everywhere tend to correspond.

There is one other remark connected with this subject worth making. I

have given my reasons for believing that all our greater fossiliferous

formations were deposited during periods of subsidence; and that blank

intervals of vast duration occurred during the periods when the bed of

the sea was either stationary or rising, and likewise when sediment was

not thrown down quickly enough to embed and preserve organic remains.

During these long and blank intervals I suppose that the inhabitants

of each region underwent a considerable amount of modification and

extinction, and that there was much migration from other parts of the

world. As we have reason to believe that large areas are affected by the

same movement, it is probable that strictly contemporaneous formations

have often been accumulated over very wide spaces in the same quarter

of the world; but we are far from having any right to conclude that this

has invariably been the case, and that large areas have invariably been

affected by the same movements. When two formations have been deposited

in two regions during nearly, but not exactly the same period, we should

find in both, from the causes explained in the foregoing paragraphs, the

same general succession in the forms of life; but the species would not

exactly correspond; for there will have been a little more time in

the one region than in the other for modification, extinction, and

immigration.

I suspect that cases of this nature have occurred in Europe. Mr.

Prestwich, in his admirable Memoirs on the eocene deposits of England

and France, is able to draw a close general parallelism between the

successive stages in the two countries; but when he compares certain

stages in England with those in France, although he finds in both a

curious accordance in the numbers of the species belonging to the same

genera, yet the species themselves differ in a manner very difficult

to account for, considering the proximity of the two areas,--unless,

indeed, it be assumed that an isthmus separated two seas inhabited

by distinct, but contemporaneous, faunas. Lyell has made similar

observations on some of the later tertiary formations. Barrande, also,

shows that there is a striking general parallelism in the successive

Silurian deposits of Bohemia and Scandinavia; nevertheless he finds

a surprising amount of difference in the species. If the several

formations in these regions have not been deposited during the same

exact periods,--a formation in one region often corresponding with a

blank interval in the other,--and if in both regions the species

have gone on slowly changing during the accumulation of the several

formations and during the long intervals of time between them; in this

case, the several formations in the two regions could be arranged in

the same order, in accordance with the general succession of the form

of life, and the order would falsely appear to be strictly parallel;

nevertheless the species would not all be the same in the apparently

corresponding stages in the two regions.

ON THE AFFINITIES OF EXTINCT SPECIES TO EACH OTHER, AND TO LIVING FORMS.

Let us now look to the mutual affinities of extinct and living species.

They all fall into one grand natural system; and this fact is at once

explained on the principle of descent. The more ancient any form is, the

more, as a general rule, it differs from living forms. But, as Buckland

long ago remarked, all fossils can be classed either in still existing

groups, or between them. That the extinct forms of life help to fill up

the wide intervals between existing genera, families, and orders, cannot

be disputed. For if we confine our attention either to the living or

to the extinct alone, the series is far less perfect than if we combine

both into one general system. With respect to the Vertebrata, whole

pages could be filled with striking illustrations from our great

palaeontologist, Owen, showing how extinct animals fall in between

existing groups. Cuvier ranked the Ruminants and Pachyderms, as the two

most distinct orders of mammals; but Owen has discovered so many fossil

links, that he has had to alter the whole classification of these two

orders; and has placed certain pachyderms in the same sub-order with

ruminants: for example, he dissolves by fine gradations the apparently

wide difference between the pig and the camel. In regard to the

Invertebrata, Barrande, and a higher authority could not be named,

asserts that he is every day taught that palaeozoic animals, though

belonging to the same orders, families, or genera with those living at

the present day, were not at this early epoch limited in such distinct

groups as they now are.

Some writers have objected to any extinct species or group of species

being considered as intermediate between living species or groups. If by

this term it is meant that an extinct form is directly intermediate in

all its characters between two living forms, the objection is probably

valid. But I apprehend that in a perfectly natural classification many

fossil species would have to stand between living species, and some

extinct genera between living genera, even between genera belonging to

distinct families. The most common case, especially with respect to very

distinct groups, such as fish and reptiles, seems to be, that supposing

them to be distinguished at the present day from each other by a

dozen characters, the ancient members of the same two groups would be

distinguished by a somewhat lesser number of characters, so that the two

groups, though formerly quite distinct, at that period made some small

approach to each other.

It is a common belief that the more ancient a form is, by so much the

more it tends to connect by some of its characters groups now widely

separated from each other. This remark no doubt must be restricted

to those groups which have undergone much change in the course of

geological ages; and it would be difficult to prove the truth of

the proposition, for every now and then even a living animal, as the

Lepidosiren, is discovered having affinities directed towards very

distinct groups. Yet if we compare the older Reptiles and Batrachians,

the older Fish, the older Cephalopods, and the eocene Mammals, with the

more recent members of the same classes, we must admit that there is

some truth in the remark.

Let us see how far these several facts and inferences accord with the

theory of descent with modification. As the subject is somewhat complex,

I must request the reader to turn to the diagram in the fourth chapter.

We may suppose that the numbered letters represent genera, and the

dotted lines diverging from them the species in each genus. The diagram

is much too simple, too few genera and too few species being given,

but this is unimportant for us. The horizontal lines may represent

successive geological formations, and all the forms beneath the

uppermost line may be considered as extinct. The three existing genera,

a14, q14, p14, will form a small family; b14 and f14 a closely allied

family or sub-family; and o14, e14, m14, a third family. These three

families, together with the many extinct genera on the several lines of

descent diverging from the parent-form A, will form an order; for all

will have inherited something in common from their ancient and common

progenitor. On the principle of the continued tendency to divergence

of character, which was formerly illustrated by this diagram, the more

recent any form is, the more it will generally differ from its ancient

progenitor. Hence we can understand the rule that the most ancient

fossils differ most from existing forms. We must not, however, assume

that divergence of character is a necessary contingency; it depends

solely on the descendants from a species being thus enabled to seize

on many and different places in the economy of nature. Therefore it is

quite possible, as we have seen in the case of some Silurian forms,

that a species might go on being slightly modified in relation to its

slightly altered conditions of life, and yet retain throughout a vast

period the same general characteristics. This is represented in the

diagram by the letter F14.

All the many forms, extinct and recent, descended from A, make, as

before remarked, one order; and this order, from the continued effects

of extinction and divergence of character, has become divided into

several sub-families and families, some of which are supposed to have

perished at different periods, and some to have endured to the present

day.

By looking at the diagram we can see that if many of the extinct forms,

supposed to be embedded in the successive formations, were discovered

at several points low down in the series, the three existing families on

the uppermost line would be rendered less distinct from each other. If,

for instance, the genera a1, a5, a10, f8, m3, m6, m9 were disinterred,

these three families would be so closely linked together that they

probably would have to be united into one great family, in nearly the

same manner as has occurred with ruminants and pachyderms. Yet he who

objected to call the extinct genera, which thus linked the living

genera of three families together, intermediate in character, would be

justified, as they are intermediate, not directly, but only by a long

and circuitous course through many widely different forms. If many

extinct forms were to be discovered above one of the middle horizontal

lines or geological formations--for instance, above Number VI.--but

none from beneath this line, then only the two families on the left

hand (namely, a14, etc., and b14, etc.) would have to be united into

one family; and the two other families (namely, a14 to f14 now including

five genera, and o14 to m14) would yet remain distinct. These two

families, however, would be less distinct from each other than they were

before the discovery of the fossils. If, for instance, we suppose the

existing genera of the two families to differ from each other by a dozen

characters, in this case the genera, at the early period marked VI.,

would differ by a lesser number of characters; for at this early

stage of descent they have not diverged in character from the common

progenitor of the order, nearly so much as they subsequently diverged.

Thus it comes that ancient and extinct genera are often in some slight

degree intermediate in character between their modified descendants, or

between their collateral relations.

In nature the case will be far more complicated than is represented in

the diagram; for the groups will have been more numerous, they will

have endured for extremely unequal lengths of time, and will have been

modified in various degrees. As we possess only the last volume of the

geological record, and that in a very broken condition, we have no right

to expect, except in very rare cases, to fill up wide intervals in the

natural system, and thus unite distinct families or orders. All that we

have a right to expect, is that those groups, which have within known

geological periods undergone much modification, should in the older

formations make some slight approach to each other; so that the older

members should differ less from each other in some of their characters

than do the existing members of the same groups; and this by the

concurrent evidence of our best palaeontologists seems frequently to be

the case.

Thus, on the theory of descent with modification, the main facts with

respect to the mutual affinities of the extinct forms of life to each

other and to living forms, seem to me explained in a satisfactory

manner. And they are wholly inexplicable on any other view.

On this same theory, it is evident that the fauna of any great period

in the earth's history will be intermediate in general character between

that which preceded and that which succeeded it. Thus, the species

which lived at the sixth great stage of descent in the diagram are the

modified offspring of those which lived at the fifth stage, and are the

parents of those which became still more modified at the seventh stage;

hence they could hardly fail to be nearly intermediate in character

between the forms of life above and below. We must, however, allow for

the entire extinction of some preceding forms, and for the coming in of

quite new forms by immigration, and for a large amount of modification,

during the long and blank intervals between the successive formations.

Subject to these allowances, the fauna of each geological period

undoubtedly is intermediate in character, between the preceding and

succeeding faunas. I need give only one instance, namely, the manner

in which the fossils of the Devonian system, when this system was first

discovered, were at once recognised by palaeontologists as intermediate

in character between those of the overlying carboniferous, and

underlying Silurian system. But each fauna is not necessarily exactly

intermediate, as unequal intervals of time have elapsed between

consecutive formations.

It is no real objection to the truth of the statement, that the fauna of

each period as a whole is nearly intermediate in character between the

preceding and succeeding faunas, that certain genera offer exceptions

to the rule. For instance, mastodons and elephants, when arranged by Dr.

Falconer in two series, first according to their mutual affinities

and then according to their periods of existence, do not accord in

arrangement. The species extreme in character are not the oldest, or

the most recent; nor are those which are intermediate in character,

intermediate in age. But supposing for an instant, in this and other

such cases, that the record of the first appearance and disappearance

of the species was perfect, we have no reason to believe that forms

successively produced necessarily endure for corresponding lengths of

time: a very ancient form might occasionally last much longer than

a form elsewhere subsequently produced, especially in the case of

terrestrial productions inhabiting separated districts. To compare small

things with great: if the principal living and extinct races of the

domestic pigeon were arranged as well as they could be in serial

affinity, this arrangement would not closely accord with the order

in time of their production, and still less with the order of their

disappearance; for the parent rock-pigeon now lives; and many varieties

between the rock-pigeon and the carrier have become extinct; and

carriers which are extreme in the important character of length of beak

originated earlier than short-beaked tumblers, which are at the opposite

end of the series in this same respect.

Closely connected with the statement, that the organic remains from an

intermediate formation are in some degree intermediate in character,

is the fact, insisted on by all palaeontologists, that fossils from two

consecutive formations are far more closely related to each other, than

are the fossils from two remote formations. Pictet gives as a well-known

instance, the general resemblance of the organic remains from the

several stages of the chalk formation, though the species are distinct

in each stage. This fact alone, from its generality, seems to have

shaken Professor Pictet in his firm belief in the immutability of

species. He who is acquainted with the distribution of existing species

over the globe, will not attempt to account for the close resemblance of

the distinct species in closely consecutive formations, by the physical

conditions of the ancient areas having remained nearly the same. Let it

be remembered that the forms of life, at least those inhabiting the sea,

have changed almost simultaneously throughout the world, and therefore

under the most different climates and conditions. Consider the

prodigious vicissitudes of climate during the pleistocene period, which

includes the whole glacial period, and note how little the specific

forms of the inhabitants of the sea have been affected.

On the theory of descent, the full meaning of the fact of fossil remains

from closely consecutive formations, though ranked as distinct species,

being closely related, is obvious. As the accumulation of each formation

has often been interrupted, and as long blank intervals have intervened

between successive formations, we ought not to expect to find, as I

attempted to show in the last chapter, in any one or two formations all

the intermediate varieties between the species which appeared at the

commencement and close of these periods; but we ought to find after

intervals, very long as measured by years, but only moderately long

as measured geologically, closely allied forms, or, as they have been

called by some authors, representative species; and these we assuredly

do find. We find, in short, such evidence of the slow and scarcely

sensible mutation of specific forms, as we have a just right to expect

to find.

ON THE STATE OF DEVELOPMENT OF ANCIENT FORMS.

There has been much discussion whether recent forms are more highly

developed than ancient. I will not here enter on this subject, for

naturalists have not as yet defined to each other's satisfaction what is

meant by high and low forms. But in one particular sense the more recent

forms must, on my theory, be higher than the more ancient; for each new

species is formed by having had some advantage in the struggle for life

over other and preceding forms. If under a nearly similar climate, the

eocene inhabitants of one quarter of the world were put into competition

with the existing inhabitants of the same or some other quarter, the

eocene fauna or flora would certainly be beaten and exterminated;

as would a secondary fauna by an eocene, and a palaeozoic fauna by a

secondary fauna. I do not doubt that this process of improvement has

affected in a marked and sensible manner the organisation of the more

recent and victorious forms of life, in comparison with the ancient and

beaten forms; but I can see no way of testing this sort of progress.

Crustaceans, for instance, not the highest in their own class, may have

beaten the highest molluscs. From the extraordinary manner in which

European productions have recently spread over New Zealand, and have

seized on places which must have been previously occupied, we may

believe, if all the animals and plants of Great Britain were set free

in New Zealand, that in the course of time a multitude of British forms

would become thoroughly naturalized there, and would exterminate many

of the natives. On the other hand, from what we see now occurring in New

Zealand, and from hardly a single inhabitant of the southern hemisphere

having become wild in any part of Europe, we may doubt, if all the

productions of New Zealand were set free in Great Britain, whether any

considerable number would be enabled to seize on places now occupied by

our native plants and animals. Under this point of view, the productions

of Great Britain may be said to be higher than those of New Zealand. Yet

the most skilful naturalist from an examination of the species of the

two countries could not have foreseen this result.

Agassiz insists that ancient animals resemble to a certain extent the

embryos of recent animals of the same classes; or that the geological

succession of extinct forms is in some degree parallel to the

embryological development of recent forms. I must follow Pictet and

Huxley in thinking that the truth of this doctrine is very far from

proved. Yet I fully expect to see it hereafter confirmed, at least in

regard to subordinate groups, which have branched off from each other

within comparatively recent times. For this doctrine of Agassiz accords

well with the theory of natural selection. In a future chapter I

shall attempt to show that the adult differs from its embryo, owing

to variations supervening at a not early age, and being inherited at

a corresponding age. This process, whilst it leaves the embryo almost

unaltered, continually adds, in the course of successive generations,

more and more difference to the adult.

Thus the embryo comes to be left as a sort of picture, preserved by

nature, of the ancient and less modified condition of each animal. This

view may be true, and yet it may never be capable of full proof. Seeing,

for instance, that the oldest known mammals, reptiles, and fish strictly

belong to their own proper classes, though some of these old forms are

in a slight degree less distinct from each other than are the typical

members of the same groups at the present day, it would be vain to look

for animals having the common embryological character of the Vertebrata,

until beds far beneath the lowest Silurian strata are discovered--a

discovery of which the chance is very small.

ON THE SUCCESSION OF THE SAME TYPES WITHIN THE SAME AREAS, DURING THE

LATER TERTIARY PERIODS.

Mr. Clift many years ago showed that the fossil mammals from the

Australian caves were closely allied to the living marsupials of that

continent. In South America, a similar relationship is manifest, even

to an uneducated eye, in the gigantic pieces of armour like those of the

armadillo, found in several parts of La Plata; and Professor Owen has

shown in the most striking manner that most of the fossil mammals,

buried there in such numbers, are related to South American types. This

relationship is even more clearly seen in the wonderful collection of

fossil bones made by MM. Lund and Clausen in the caves of Brazil. I was

so much impressed with these facts that I strongly insisted, in 1839

and 1845, on this "law of the succession of types,"--on "this wonderful

relationship in the same continent between the dead and the living."

Professor Owen has subsequently extended the same generalisation to

the mammals of the Old World. We see the same law in this author's

restorations of the extinct and gigantic birds of New Zealand. We see

it also in the birds of the caves of Brazil. Mr. Woodward has shown that

the same law holds good with sea-shells, but from the wide distribution

of most genera of molluscs, it is not well displayed by them. Other

cases could be added, as the relation between the extinct and

living land-shells of Madeira; and between the extinct and living

brackish-water shells of the Aralo-Caspian Sea.

Now what does this remarkable law of the succession of the same types

within the same areas mean? He would be a bold man, who after comparing

the present climate of Australia and of parts of South America under the

same latitude, would attempt to account, on the one hand, by dissimilar

physical conditions for the dissimilarity of the inhabitants of these

two continents, and, on the other hand, by similarity of conditions,

for the uniformity of the same types in each during the later tertiary

periods. Nor can it be pretended that it is an immutable law that

marsupials should have been chiefly or solely produced in Australia; or

that Edentata and other American types should have been solely produced

in South America. For we know that Europe in ancient times was peopled

by numerous marsupials; and I have shown in the publications above

alluded to, that in America the law of distribution of terrestrial

mammals was formerly different from what it now is. North America

formerly partook strongly of the present character of the southern

half of the continent; and the southern half was formerly more closely

allied, than it is at present, to the northern half. In a similar manner

we know from Falconer and Cautley's discoveries, that northern India was

formerly more closely related in its mammals to Africa than it is at

the present time. Analogous facts could be given in relation to the

distribution of marine animals.

On the theory of descent with modification, the great law of the long

enduring, but not immutable, succession of the same types within the

same areas, is at once explained; for the inhabitants of each quarter of

the world will obviously tend to leave in that quarter, during the next

succeeding period of time, closely allied though in some degree modified

descendants. If the inhabitants of one continent formerly differed

greatly from those of another continent, so will their modified

descendants still differ in nearly the same manner and degree. But

after very long intervals of time and after great geographical changes,

permitting much inter-migration, the feebler will yield to the more

dominant forms, and there will be nothing immutable in the laws of past

and present distribution.

It may be asked in ridicule, whether I suppose that the megatherium and

other allied huge monsters have left behind them in South America the

sloth, armadillo, and anteater, as their degenerate descendants. This

cannot for an instant be admitted. These huge animals have become wholly

extinct, and have left no progeny. But in the caves of Brazil, there

are many extinct species which are closely allied in size and in other

characters to the species still living in South America; and some of

these fossils may be the actual progenitors of living species. It must

not be forgotten that, on my theory, all the species of the same genus

have descended from some one species; so that if six genera, each having

eight species, be found in one geological formation, and in the next

succeeding formation there be six other allied or representative genera

with the same number of species, then we may conclude that only one

species of each of the six older genera has left modified descendants,

constituting the six new genera. The other seven species of the old

genera have all died out and have left no progeny. Or, which would

probably be a far commoner case, two or three species of two or three

alone of the six older genera will have been the parents of the six new

genera; the other old species and the other whole genera having

become utterly extinct. In failing orders, with the genera and species

decreasing in numbers, as apparently is the case of the Edentata of

South America, still fewer genera and species will have left modified

blood-descendants.

SUMMARY OF THE PRECEDING AND PRESENT CHAPTERS.

I have attempted to show that the geological record is extremely

imperfect; that only a small portion of the globe has been geologically

explored with care; that only certain classes of organic beings have

been largely preserved in a fossil state; that the number both of

specimens and of species, preserved in our museums, is absolutely as

nothing compared with the incalculable number of generations which

must have passed away even during a single formation; that, owing

to subsidence being necessary for the accumulation of fossiliferous

deposits thick enough to resist future degradation, enormous intervals

of time have elapsed between the successive formations; that there has

probably been more extinction during the periods of subsidence, and more

variation during the periods of elevation, and during the latter the

record will have been least perfectly kept; that each single formation

has not been continuously deposited; that the duration of each formation

is, perhaps, short compared with the average duration of specific forms;

that migration has played an important part in the first appearance of

new forms in any one area and formation; that widely ranging species

are those which have varied most, and have oftenest given rise to new

species; and that varieties have at first often been local. All these

causes taken conjointly, must have tended to make the geological record

extremely imperfect, and will to a large extent explain why we do not

find interminable varieties, connecting together all the extinct and

existing forms of life by the finest graduated steps.

He who rejects these views on the nature of the geological record, will

rightly reject my whole theory. For he may ask in vain where are the

numberless transitional links which must formerly have connected the

closely allied or representative species, found in the several stages of

the same great formation. He may disbelieve in the enormous intervals

of time which have elapsed between our consecutive formations; he may

overlook how important a part migration must have played, when the

formations of any one great region alone, as that of Europe, are

considered; he may urge the apparent, but often falsely apparent, sudden

coming in of whole groups of species. He may ask where are the remains

of those infinitely numerous organisms which must have existed long

before the first bed of the Silurian system was deposited: I can answer

this latter question only hypothetically, by saying that as far as we

can see, where our oceans now extend they have for an enormous period

extended, and where our oscillating continents now stand they have stood

ever since the Silurian epoch; but that long before that period, the

world may have presented a wholly different aspect; and that the older

continents, formed of formations older than any known to us, may now all

be in a metamorphosed condition, or may lie buried under the ocean.

Passing from these difficulties, all the other great leading facts in

palaeontology seem to me simply to follow on the theory of descent with

modification through natural selection. We can thus understand how it

is that new species come in slowly and successively; how species of

different classes do not necessarily change together, or at the same

rate, or in the same degree; yet in the long run that all undergo

modification to some extent. The extinction of old forms is the almost

inevitable consequence of the production of new forms. We can understand

why when a species has once disappeared it never reappears. Groups of

species increase in numbers slowly, and endure for unequal periods of

time; for the process of modification is necessarily slow, and depends

on many complex contingencies. The dominant species of the larger

dominant groups tend to leave many modified descendants, and thus new

sub-groups and groups are formed. As these are formed, the species of

the less vigorous groups, from their inferiority inherited from a common

progenitor, tend to become extinct together, and to leave no modified

offspring on the face of the earth. But the utter extinction of a whole

group of species may often be a very slow process, from the survival of

a few descendants, lingering in protected and isolated situations. When

a group has once wholly disappeared, it does not reappear; for the link

of generation has been broken.

We can understand how the spreading of the dominant forms of life, which

are those that oftenest vary, will in the long run tend to people the

world with allied, but modified, descendants; and these will generally

succeed in taking the places of those groups of species which are their

inferiors in the struggle for existence. Hence, after long intervals

of time, the productions of the world will appear to have changed

simultaneously.

We can understand how it is that all the forms of life, ancient and

recent, make together one grand system; for all are connected by

generation. We can understand, from the continued tendency to divergence

of character, why the more ancient a form is, the more it generally

differs from those now living. Why ancient and extinct forms often tend

to fill up gaps between existing forms, sometimes blending two groups

previously classed as distinct into one; but more commonly only bringing

them a little closer together. The more ancient a form is, the more

often, apparently, it displays characters in some degree intermediate

between groups now distinct; for the more ancient a form is, the more

nearly it will be related to, and consequently resemble, the common

progenitor of groups, since become widely divergent. Extinct forms

are seldom directly intermediate between existing forms; but are

intermediate only by a long and circuitous course through many extinct

and very different forms. We can clearly see why the organic remains of

closely consecutive formations are more closely allied to each other,

than are those of remote formations; for the forms are more closely

linked together by generation: we can clearly see why the remains of an

intermediate formation are intermediate in character.

The inhabitants of each successive period in the world's history have

beaten their predecessors in the race for life, and are, in so far,

higher in the scale of nature; and this may account for that vague yet

ill-defined sentiment, felt by many palaeontologists, that organisation

on the whole has progressed. If it should hereafter be proved that

ancient animals resemble to a certain extent the embryos of more recent

animals of the same class, the fact will be intelligible. The succession

of the same types of structure within the same areas during the later

geological periods ceases to be mysterious, and is simply explained by

inheritance.

If then the geological record be as imperfect as I believe it to be, and

it may at least be asserted that the record cannot be proved to be much

more perfect, the main objections to the theory of natural selection are

greatly diminished or disappear. On the other hand, all the chief laws

of palaeontology plainly proclaim, as it seems to me, that species have

been produced by ordinary generation: old forms having been supplanted

by new and improved forms of life, produced by the laws of variation

still acting round us, and preserved by Natural Selection.

11. GEOGRAPHICAL DISTRIBUTION.

Present distribution cannot be accounted for by differences in physical

conditions. Importance of barriers. Affinity of the productions of the

same continent. Centres of creation. Means of dispersal, by changes of

climate and of the level of the land, and by occasional means. Dispersal

during the Glacial period co-extensive with the world.

In considering the distribution of organic beings over the face of

the globe, the first great fact which strikes us is, that neither the

similarity nor the dissimilarity of the inhabitants of various regions

can be accounted for by their climatal and other physical conditions. Of

late, almost every author who has studied the subject has come to this

conclusion. The case of America alone would almost suffice to prove its

truth: for if we exclude the northern parts where the circumpolar land

is almost continuous, all authors agree that one of the most fundamental

divisions in geographical distribution is that between the New and Old

Worlds; yet if we travel over the vast American continent, from the

central parts of the United States to its extreme southern point, we

meet with the most diversified conditions; the most humid districts,

arid deserts, lofty mountains, grassy plains, forests, marshes, lakes,

and great rivers, under almost every temperature. There is hardly a

climate or condition in the Old World which cannot be paralleled in the

New--at least as closely as the same species generally require; for it

is a most rare case to find a group of organisms confined to any small

spot, having conditions peculiar in only a slight degree; for instance,

small areas in the Old World could be pointed out hotter than any in

the New World, yet these are not inhabited by a peculiar fauna or flora.

Notwithstanding this parallelism in the conditions of the Old and New

Worlds, how widely different are their living productions!

In the southern hemisphere, if we compare large tracts of land in

Australia, South Africa, and western South America, between latitudes

25 deg and 35 deg, we shall find parts extremely similar in all their

conditions, yet it would not be possible to point out three faunas and

floras more utterly dissimilar. Or again we may compare the productions

of South America south of lat. 35 deg with those north of 25 deg, which

consequently inhabit a considerably different climate, and they will be

found incomparably more closely related to each other, than they are to

the productions of Australia or Africa under nearly the same climate.

Analogous facts could be given with respect to the inhabitants of the

sea.

A second great fact which strikes us in our general review is, that

barriers of any kind, or obstacles to free migration, are related in a

close and important manner to the differences between the productions of

various regions. We see this in the great difference of nearly all the

terrestrial productions of the New and Old Worlds, excepting in the

northern parts, where the land almost joins, and where, under a slightly

different climate, there might have been free migration for the northern

temperate forms, as there now is for the strictly arctic productions.

We see the same fact in the great difference between the inhabitants of

Australia, Africa, and South America under the same latitude: for these

countries are almost as much isolated from each other as is possible. On

each continent, also, we see the same fact; for on the opposite sides

of lofty and continuous mountain-ranges, and of great deserts, and

sometimes even of large rivers, we find different productions; though as

mountain chains, deserts, etc., are not as impassable, or likely to have

endured so long as the oceans separating continents, the differences are

very inferior in degree to those characteristic of distinct continents.

Turning to the sea, we find the same law. No two marine faunas are more

distinct, with hardly a fish, shell, or crab in common, than those of

the eastern and western shores of South and Central America; yet these

great faunas are separated only by the narrow, but impassable, isthmus

of Panama. Westward of the shores of America, a wide space of open ocean

extends, with not an island as a halting-place for emigrants; here we

have a barrier of another kind, and as soon as this is passed we meet

in the eastern islands of the Pacific, with another and totally

distinct fauna. So that here three marine faunas range far northward

and southward, in parallel lines not far from each other, under

corresponding climates; but from being separated from each other

by impassable barriers, either of land or open sea, they are wholly

distinct. On the other hand, proceeding still further westward from the

eastern islands of the tropical parts of the Pacific, we encounter no

impassable barriers, and we have innumerable islands as halting-places,

until after travelling over a hemisphere we come to the shores of

Africa; and over this vast space we meet with no well-defined and

distinct marine faunas. Although hardly one shell, crab or fish is

common to the above-named three approximate faunas of Eastern and

Western America and the eastern Pacific islands, yet many fish range

from the Pacific into the Indian Ocean, and many shells are common to

the eastern islands of the Pacific and the eastern shores of Africa, on

almost exactly opposite meridians of longitude.

A third great fact, partly included in the foregoing statements, is the

affinity of the productions of the same continent or sea, though the

species themselves are distinct at different points and stations. It is

a law of the widest generality, and every continent offers innumerable

instances. Nevertheless the naturalist in travelling, for instance,

from north to south never fails to be struck by the manner in which

successive groups of beings, specifically distinct, yet clearly related,

replace each other. He hears from closely allied, yet distinct kinds of

birds, notes nearly similar, and sees their nests similarly constructed,

but not quite alike, with eggs coloured in nearly the same manner. The

plains near the Straits of Magellan are inhabited by one species of

Rhea (American ostrich), and northward the plains of La Plata by another

species of the same genus; and not by a true ostrich or emeu, like those

found in Africa and Australia under the same latitude. On these same

plains of La Plata, we see the agouti and bizcacha, animals having

nearly the same habits as our hares and rabbits and belonging to the

same order of Rodents, but they plainly display an American type of

structure. We ascend the lofty peaks of the Cordillera and we find an

alpine species of bizcacha; we look to the waters, and we do not find

the beaver or musk-rat, but the coypu and capybara, rodents of the

American type. Innumerable other instances could be given. If we look

to the islands off the American shore, however much they may differ in

geological structure, the inhabitants, though they may be all peculiar

species, are essentially American. We may look back to past ages, as

shown in the last chapter, and we find American types then prevalent on

the American continent and in the American seas. We see in these facts

some deep organic bond, prevailing throughout space and time, over

the same areas of land and water, and independent of their physical

conditions. The naturalist must feel little curiosity, who is not led to

inquire what this bond is.

This bond, on my theory, is simply inheritance, that cause which alone,

as far as we positively know, produces organisms quite like, or, as we

see in the case of varieties nearly like each other. The dissimilarity

of the inhabitants of different regions may be attributed to

modification through natural selection, and in a quite subordinate

degree to the direct influence of different physical conditions.

The degree of dissimilarity will depend on the migration of the more

dominant forms of life from one region into another having been effected

with more or less ease, at periods more or less remote;--on the nature

and number of the former immigrants;--and on their action and reaction,

in their mutual struggles for life;--the relation of organism to

organism being, as I have already often remarked, the most important of

all relations. Thus the high importance of barriers comes into play by

checking migration; as does time for the slow process of modification

through natural selection. Widely-ranging species, abounding in

individuals, which have already triumphed over many competitors in their

own widely-extended homes will have the best chance of seizing on new

places, when they spread into new countries. In their new homes they

will be exposed to new conditions, and will frequently undergo further

modification and improvement; and thus they will become still further

victorious, and will produce groups of modified descendants. On this

principle of inheritance with modification, we can understand how it is

that sections of genera, whole genera, and even families are confined to

the same areas, as is so commonly and notoriously the case.

I believe, as was remarked in the last chapter, in no law of necessary

development. As the variability of each species is an independent

property, and will be taken advantage of by natural selection, only so

far as it profits the individual in its complex struggle for life,

so the degree of modification in different species will be no uniform

quantity. If, for instance, a number of species, which stand in direct

competition with each other, migrate in a body into a new and afterwards

isolated country, they will be little liable to modification; for

neither migration nor isolation in themselves can do anything. These

principles come into play only by bringing organisms into new relations

with each other, and in a lesser degree with the surrounding physical

conditions. As we have seen in the last chapter that some forms have

retained nearly the same character from an enormously remote geological

period, so certain species have migrated over vast spaces, and have not

become greatly modified.

On these views, it is obvious, that the several species of the same

genus, though inhabiting the most distant quarters of the world, must

originally have proceeded from the same source, as they have descended

from the same progenitor. In the case of those species, which have

undergone during whole geological periods but little modification, there

is not much difficulty in believing that they may have migrated from the

same region; for during the vast geographical and climatal changes which

will have supervened since ancient times, almost any amount of migration

is possible. But in many other cases, in which we have reason to believe

that the species of a genus have been produced within comparatively

recent times, there is great difficulty on this head. It is also obvious

that the individuals of the same species, though now inhabiting distant

and isolated regions, must have proceeded from one spot, where their

parents were first produced: for, as explained in the last chapter, it

is incredible that individuals identically the same should ever have

been produced through natural selection from parents specifically

distinct.

We are thus brought to the question which has been largely discussed by

naturalists, namely, whether species have been created at one or more

points of the earth's surface. Undoubtedly there are very many cases of

extreme difficulty, in understanding how the same species could possibly

have migrated from some one point to the several distant and isolated

points, where now found. Nevertheless the simplicity of the view that

each species was first produced within a single region captivates the

mind. He who rejects it, rejects the vera causa of ordinary generation

with subsequent migration, and calls in the agency of a miracle. It is

universally admitted, that in most cases the area inhabited by a species

is continuous; and when a plant or animal inhabits two points so distant

from each other, or with an interval of such a nature, that the space

could not be easily passed over by migration, the fact is given as

something remarkable and exceptional. The capacity of migrating across

the sea is more distinctly limited in terrestrial mammals, than perhaps

in any other organic beings; and, accordingly, we find no inexplicable

cases of the same mammal inhabiting distant points of the world. No

geologist will feel any difficulty in such cases as Great Britain having

been formerly united to Europe, and consequently possessing the same

quadrupeds. But if the same species can be produced at two separate

points, why do we not find a single mammal common to Europe and

Australia or South America? The conditions of life are nearly the

same, so that a multitude of European animals and plants have become

naturalised in America and Australia; and some of the aboriginal plants

are identically the same at these distant points of the northern and

southern hemispheres? The answer, as I believe, is, that mammals have

not been able to migrate, whereas some plants, from their varied means

of dispersal, have migrated across the vast and broken interspace. The

great and striking influence which barriers of every kind have had on

distribution, is intelligible only on the view that the great majority

of species have been produced on one side alone, and have not been able

to migrate to the other side. Some few families, many sub-families,

very many genera, and a still greater number of sections of genera

are confined to a single region; and it has been observed by several

naturalists, that the most natural genera, or those genera in which the

species are most closely related to each other, are generally local,

or confined to one area. What a strange anomaly it would be, if, when

coming one step lower in the series, to the individuals of the same

species, a directly opposite rule prevailed; and species were not local,

but had been produced in two or more distinct areas!

Hence it seems to me, as it has to many other naturalists, that the

view of each species having been produced in one area alone, and having

subsequently migrated from that area as far as its powers of migration

and subsistence under past and present conditions permitted, is the most

probable. Undoubtedly many cases occur, in which we cannot explain how

the same species could have passed from one point to the other. But the

geographical and climatal changes, which have certainly occurred within

recent geological times, must have interrupted or rendered discontinuous

the formerly continuous range of many species. So that we are reduced to

consider whether the exceptions to continuity of range are so numerous

and of so grave a nature, that we ought to give up the belief, rendered

probable by general considerations, that each species has been produced

within one area, and has migrated thence as far as it could. It would

be hopelessly tedious to discuss all the exceptional cases of the same

species, now living at distant and separated points; nor do I for a

moment pretend that any explanation could be offered of many such cases.

But after some preliminary remarks, I will discuss a few of the most

striking classes of facts; namely, the existence of the same species

on the summits of distant mountain-ranges, and at distant points in the

arctic and antarctic regions; and secondly (in the following chapter),

the wide distribution of freshwater productions; and thirdly, the

occurrence of the same terrestrial species on islands and on the

mainland, though separated by hundreds of miles of open sea. If the

existence of the same species at distant and isolated points of the

earth's surface, can in many instances be explained on the view of each

species having migrated from a single birthplace; then, considering our

ignorance with respect to former climatal and geographical changes and

various occasional means of transport, the belief that this has been the

universal law, seems to me incomparably the safest.

In discussing this subject, we shall be enabled at the same time to

consider a point equally important for us, namely, whether the several

distinct species of a genus, which on my theory have all descended from

a common progenitor, can have migrated (undergoing modification

during some part of their migration) from the area inhabited by their

progenitor. If it can be shown to be almost invariably the case, that

a region, of which most of its inhabitants are closely related to,

or belong to the same genera with the species of a second region, has

probably received at some former period immigrants from this other

region, my theory will be strengthened; for we can clearly understand,

on the principle of modification, why the inhabitants of a region should

be related to those of another region, whence it has been stocked. A

volcanic island, for instance, upheaved and formed at the distance of a

few hundreds of miles from a continent, would probably receive from it

in the course of time a few colonists, and their descendants, though

modified, would still be plainly related by inheritance to the

inhabitants of the continent. Cases of this nature are common, and are,

as we shall hereafter more fully see, inexplicable on the theory of

independent creation. This view of the relation of species in one region

to those in another, does not differ much (by substituting the word

variety for species) from that lately advanced in an ingenious paper by

Mr. Wallace, in which he concludes, that "every species has come into

existence coincident both in space and time with a pre-existing

closely allied species." And I now know from correspondence, that this

coincidence he attributes to generation with modification.

The previous remarks on "single and multiple centres of creation" do

not directly bear on another allied question,--namely whether all the

individuals of the same species have descended from a single pair, or

single hermaphrodite, or whether, as some authors suppose, from many

individuals simultaneously created. With those organic beings which

never intercross (if such exist), the species, on my theory, must have

descended from a succession of improved varieties, which will never have

blended with other individuals or varieties, but will have supplanted

each other; so that, at each successive stage of modification and

improvement, all the individuals of each variety will have descended

from a single parent. But in the majority of cases, namely, with

all organisms which habitually unite for each birth, or which often

intercross, I believe that during the slow process of modification

the individuals of the species will have been kept nearly uniform by

intercrossing; so that many individuals will have gone on simultaneously

changing, and the whole amount of modification will not have been due,

at each stage, to descent from a single parent. To illustrate what I

mean: our English racehorses differ slightly from the horses of every

other breed; but they do not owe their difference and superiority to

descent from any single pair, but to continued care in selecting and

training many individuals during many generations.

Before discussing the three classes of facts, which I have selected as

presenting the greatest amount of difficulty on the theory of "single

centres of creation," I must say a few words on the means of dispersal.

MEANS OF DISPERSAL.

Sir C. Lyell and other authors have ably treated this subject. I can

give here only the briefest abstract of the more important facts. Change

of climate must have had a powerful influence on migration: a region

when its climate was different may have been a high road for migration,

but now be impassable; I shall, however, presently have to discuss this

branch of the subject in some detail. Changes of level in the land must

also have been highly influential: a narrow isthmus now separates two

marine faunas; submerge it, or let it formerly have been submerged, and

the two faunas will now blend or may formerly have blended: where the

sea now extends, land may at a former period have connected islands or

possibly even continents together, and thus have allowed terrestrial

productions to pass from one to the other. No geologist will dispute

that great mutations of level have occurred within the period of

existing organisms. Edward Forbes insisted that all the islands in the

Atlantic must recently have been connected with Europe or Africa, and

Europe likewise with America. Other authors have thus hypothetically

bridged over every ocean, and have united almost every island to some

mainland. If indeed the arguments used by Forbes are to be trusted,

it must be admitted that scarcely a single island exists which has not

recently been united to some continent. This view cuts the Gordian knot

of the dispersal of the same species to the most distant points, and

removes many a difficulty: but to the best of my judgment we are not

authorized in admitting such enormous geographical changes within

the period of existing species. It seems to me that we have abundant

evidence of great oscillations of level in our continents; but not of

such vast changes in their position and extension, as to have united

them within the recent period to each other and to the several

intervening oceanic islands. I freely admit the former existence of many

islands, now buried beneath the sea, which may have served as halting

places for plants and for many animals during their migration. In the

coral-producing oceans such sunken islands are now marked, as I believe,

by rings of coral or atolls standing over them. Whenever it is fully

admitted, as I believe it will some day be, that each species has

proceeded from a single birthplace, and when in the course of time we

know something definite about the means of distribution, we shall be

enabled to speculate with security on the former extension of the land.

But I do not believe that it will ever be proved that within the recent

period continents which are now quite separate, have been continuously,

or almost continuously, united with each other, and with the many

existing oceanic islands. Several facts in distribution,--such as the

great difference in the marine faunas on the opposite sides of almost

every continent,--the close relation of the tertiary inhabitants of

several lands and even seas to their present inhabitants,--a certain

degree of relation (as we shall hereafter see) between the distribution

of mammals and the depth of the sea,--these and other such facts seem to

me opposed to the admission of such prodigious geographical revolutions

within the recent period, as are necessitated on the view advanced

by Forbes and admitted by his many followers. The nature and relative

proportions of the inhabitants of oceanic islands likewise seem to me

opposed to the belief of their former continuity with continents. Nor

does their almost universally volcanic composition favour the admission

that they are the wrecks of sunken continents;--if they had originally

existed as mountain-ranges on the land, some at least of the islands

would have been formed, like other mountain-summits, of granite,

metamorphic schists, old fossiliferous or other such rocks, instead of

consisting of mere piles of volcanic matter.

I must now say a few words on what are called accidental means, but

which more properly might be called occasional means of distribution.

I shall here confine myself to plants. In botanical works, this or

that plant is stated to be ill adapted for wide dissemination; but for

transport across the sea, the greater or less facilities may be said to

be almost wholly unknown. Until I tried, with Mr. Berkeley's aid, a

few experiments, it was not even known how far seeds could resist the

injurious action of sea-water. To my surprise I found that out of 87

kinds, 64 germinated after an immersion of 28 days, and a few survived

an immersion of 137 days. For convenience sake I chiefly tried small

seeds, without the capsule or fruit; and as all of these sank in a few

days, they could not be floated across wide spaces of the sea, whether

or not they were injured by the salt-water. Afterwards I tried some

larger fruits, capsules, etc., and some of these floated for a long

time. It is well known what a difference there is in the buoyancy of

green and seasoned timber; and it occurred to me that floods might wash

down plants or branches, and that these might be dried on the banks, and

then by a fresh rise in the stream be washed into the sea. Hence I was

led to dry stems and branches of 94 plants with ripe fruit, and to place

them on sea water. The majority sank quickly, but some which whilst

green floated for a very short time, when dried floated much longer; for

instance, ripe hazel-nuts sank immediately, but when dried, they floated

for 90 days and afterwards when planted they germinated; an asparagus

plant with ripe berries floated for 23 days, when dried it floated

for 85 days, and the seeds afterwards germinated: the ripe seeds of

Helosciadium sank in two days, when dried they floated for above 90

days, and afterwards germinated. Altogether out of the 94 dried plants,

18 floated for above 28 days, and some of the 18 floated for a very much

longer period. So that as 64/87 seeds germinated after an immersion

of 28 days; and as 18/94 plants with ripe fruit (but not all the same

species as in the foregoing experiment) floated, after being dried, for

above 28 days, as far as we may infer anything from these scanty facts,

we may conclude that the seeds of 14/100 plants of any country might be

floated by sea-currents during 28 days, and would retain their power

of germination. In Johnston's Physical Atlas, the average rate of the

several Atlantic currents is 33 miles per diem (some currents running

at the rate of 60 miles per diem); on this average, the seeds of 14/100

plants belonging to one country might be floated across 924 miles of sea

to another country; and when stranded, if blown to a favourable spot by

an inland gale, they would germinate.

Subsequently to my experiments, M. Martens tried similar ones, but in a

much better manner, for he placed the seeds in a box in the actual sea,

so that they were alternately wet and exposed to the air like really

floating plants. He tried 98 seeds, mostly different from mine; but he

chose many large fruits and likewise seeds from plants which live

near the sea; and this would have favoured the average length of

their flotation and of their resistance to the injurious action of the

salt-water. On the other hand he did not previously dry the plants or

branches with the fruit; and this, as we have seen, would have caused

some of them to have floated much longer. The result was that 18/98 of

his seeds floated for 42 days, and were then capable of germination. But

I do not doubt that plants exposed to the waves would float for a less

time than those protected from violent movement as in our experiments.

Therefore it would perhaps be safer to assume that the seeds of about

10/100 plants of a flora, after having been dried, could be floated

across a space of sea 900 miles in width, and would then germinate.

The fact of the larger fruits often floating longer than the small,

is interesting; as plants with large seeds or fruit could hardly be

transported by any other means; and Alph. de Candolle has shown that

such plants generally have restricted ranges.

But seeds may be occasionally transported in another manner. Drift

timber is thrown up on most islands, even on those in the midst of the

widest oceans; and the natives of the coral-islands in the Pacific,

procure stones for their tools, solely from the roots of drifted trees,

these stones being a valuable royal tax. I find on examination, that

when irregularly shaped stones are embedded in the roots of trees, small

parcels of earth are very frequently enclosed in their interstices and

behind them,--so perfectly that not a particle could be washed away in

the longest transport: out of one small portion of earth thus COMPLETELY

enclosed by wood in an oak about 50 years old, three dicotyledonous

plants germinated: I am certain of the accuracy of this observation.

Again, I can show that the carcasses of birds, when floating on the sea,

sometimes escape being immediately devoured; and seeds of many kinds

in the crops of floating birds long retain their vitality: peas and

vetches, for instance, are killed by even a few days' immersion in

sea-water; but some taken out of the crop of a pigeon, which had

floated on artificial salt-water for 30 days, to my surprise nearly all

germinated.

Living birds can hardly fail to be highly effective agents in the

transportation of seeds. I could give many facts showing how frequently

birds of many kinds are blown by gales to vast distances across the

ocean. We may I think safely assume that under such circumstances their

rate of flight would often be 35 miles an hour; and some authors have

given a far higher estimate. I have never seen an instance of nutritious

seeds passing through the intestines of a bird; but hard seeds of fruit

will pass uninjured through even the digestive organs of a turkey. In

the course of two months, I picked up in my garden 12 kinds of seeds,

out of the excrement of small birds, and these seemed perfect, and

some of them, which I tried, germinated. But the following fact is more

important: the crops of birds do not secrete gastric juice, and do not

in the least injure, as I know by trial, the germination of seeds;

now after a bird has found and devoured a large supply of food, it is

positively asserted that all the grains do not pass into the gizzard for

12 or even 18 hours. A bird in this interval might easily be blown to

the distance of 500 miles, and hawks are known to look out for tired

birds, and the contents of their torn crops might thus readily get

scattered. Mr. Brent informs me that a friend of his had to give up

flying carrier-pigeons from France to England, as the hawks on the

English coast destroyed so many on their arrival. Some hawks and owls

bolt their prey whole, and after an interval of from twelve to twenty

hours, disgorge pellets, which, as I know from experiments made in the

Zoological Gardens, include seeds capable of germination. Some seeds of

the oat, wheat, millet, canary, hemp, clover, and beet germinated after

having been from twelve to twenty-one hours in the stomachs of different

birds of prey; and two seeds of beet grew after having been thus

retained for two days and fourteen hours. Freshwater fish, I find, eat

seeds of many land and water plants: fish are frequently devoured by

birds, and thus the seeds might be transported from place to place. I

forced many kinds of seeds into the stomachs of dead fish, and then gave

their bodies to fishing-eagles, storks, and pelicans; these birds after

an interval of many hours, either rejected the seeds in pellets or

passed them in their excrement; and several of these seeds retained

their power of germination. Certain seeds, however, were always killed

by this process.

Although the beaks and feet of birds are generally quite clean, I can

show that earth sometimes adheres to them: in one instance I removed

twenty-two grains of dry argillaceous earth from one foot of a

partridge, and in this earth there was a pebble quite as large as the

seed of a vetch. Thus seeds might occasionally be transported to great

distances; for many facts could be given showing that soil almost

everywhere is charged with seeds. Reflect for a moment on the millions

of quails which annually cross the Mediterranean; and can we doubt that

the earth adhering to their feet would sometimes include a few minute

seeds? But I shall presently have to recur to this subject.

As icebergs are known to be sometimes loaded with earth and stones, and

have even carried brushwood, bones, and the nest of a land-bird, I can

hardly doubt that they must occasionally have transported seeds from

one part to another of the arctic and antarctic regions, as suggested by

Lyell; and during the Glacial period from one part of the now temperate

regions to another. In the Azores, from the large number of the species

of plants common to Europe, in comparison with the plants of other

oceanic islands nearer to the mainland, and (as remarked by Mr. H. C.

Watson) from the somewhat northern character of the flora in comparison

with the latitude, I suspected that these islands had been partly

stocked by ice-borne seeds, during the Glacial epoch. At my request Sir

C. Lyell wrote to M. Hartung to inquire whether he had observed erratic

boulders on these islands, and he answered that he had found large

fragments of granite and other rocks, which do not occur in the

archipelago. Hence we may safely infer that icebergs formerly landed

their rocky burthens on the shores of these mid-ocean islands, and it

is at least possible that they may have brought thither the seeds of

northern plants.

Considering that the several above means of transport, and that several

other means, which without doubt remain to be discovered, have been in

action year after year, for centuries and tens of thousands of years,

it would I think be a marvellous fact if many plants had not thus

become widely transported. These means of transport are sometimes called

accidental, but this is not strictly correct: the currents of the sea

are not accidental, nor is the direction of prevalent gales of wind.

It should be observed that scarcely any means of transport would carry

seeds for very great distances; for seeds do not retain their vitality

when exposed for a great length of time to the action of seawater; nor

could they be long carried in the crops or intestines of birds. These

means, however, would suffice for occasional transport across tracts of

sea some hundred miles in breadth, or from island to island, or from a

continent to a neighbouring island, but not from one distant continent

to another. The floras of distant continents would not by such means

become mingled in any great degree; but would remain as distinct as we

now see them to be. The currents, from their course, would never bring

seeds from North America to Britain, though they might and do bring

seeds from the West Indies to our western shores, where, if not killed

by so long an immersion in salt-water, they could not endure our

climate. Almost every year, one or two land-birds are blown across

the whole Atlantic Ocean, from North America to the western shores of

Ireland and England; but seeds could be transported by these wanderers

only by one means, namely, in dirt sticking to their feet, which is in

itself a rare accident. Even in this case, how small would the chance

be of a seed falling on favourable soil, and coming to maturity! But it

would be a great error to argue that because a well-stocked island,

like Great Britain, has not, as far as is known (and it would be very

difficult to prove this), received within the last few centuries,

through occasional means of transport, immigrants from Europe or any

other continent, that a poorly-stocked island, though standing more

remote from the mainland, would not receive colonists by similar means.

I do not doubt that out of twenty seeds or animals transported to an

island, even if far less well-stocked than Britain, scarcely more than

one would be so well fitted to its new home, as to become naturalised.

But this, as it seems to me, is no valid argument against what would

be effected by occasional means of transport, during the long lapse of

geological time, whilst an island was being upheaved and formed, and

before it had become fully stocked with inhabitants. On almost bare

land, with few or no destructive insects or birds living there, nearly

every seed, which chanced to arrive, would be sure to germinate and

survive.

DISPERSAL DURING THE GLACIAL PERIOD.

The identity of many plants and animals, on mountain-summits, separated

from each other by hundreds of miles of lowlands, where the Alpine

species could not possibly exist, is one of the most striking cases

known of the same species living at distant points, without the apparent

possibility of their having migrated from one to the other. It is indeed

a remarkable fact to see so many of the same plants living on the snowy

regions of the Alps or Pyrenees, and in the extreme northern parts of

Europe; but it is far more remarkable, that the plants on the White

Mountains, in the United States of America, are all the same with those

of Labrador, and nearly all the same, as we hear from Asa Gray, with

those on the loftiest mountains of Europe. Even as long ago as 1747,

such facts led Gmelin to conclude that the same species must have been

independently created at several distinct points; and we might have

remained in this same belief, had not Agassiz and others called vivid

attention to the Glacial period, which, as we shall immediately see,

affords a simple explanation of these facts. We have evidence of almost

every conceivable kind, organic and inorganic, that within a very recent

geological period, central Europe and North America suffered under an

Arctic climate. The ruins of a house burnt by fire do not tell their

tale more plainly, than do the mountains of Scotland and Wales, with

their scored flanks, polished surfaces, and perched boulders, of the icy

streams with which their valleys were lately filled. So greatly has the

climate of Europe changed, that in Northern Italy, gigantic moraines,

left by old glaciers, are now clothed by the vine and maize. Throughout

a large part of the United States, erratic boulders, and rocks scored by

drifted icebergs and coast-ice, plainly reveal a former cold period.

The former influence of the glacial climate on the distribution of the

inhabitants of Europe, as explained with remarkable clearness by Edward

Forbes, is substantially as follows. But we shall follow the changes

more readily, by supposing a new glacial period to come slowly on, and

then pass away, as formerly occurred. As the cold came on, and as each

more southern zone became fitted for arctic beings and ill-fitted for

their former more temperate inhabitants, the latter would be supplanted

and arctic productions would take their places. The inhabitants of the

more temperate regions would at the same time travel southward, unless

they were stopped by barriers, in which case they would perish. The

mountains would become covered with snow and ice, and their former

Alpine inhabitants would descend to the plains. By the time that the

cold had reached its maximum, we should have a uniform arctic fauna and

flora, covering the central parts of Europe, as far south as the Alps

and Pyrenees, and even stretching into Spain. The now temperate regions

of the United States would likewise be covered by arctic plants and

animals, and these would be nearly the same with those of Europe; for

the present circumpolar inhabitants, which we suppose to have everywhere

travelled southward, are remarkably uniform round the world. We may

suppose that the Glacial period came on a little earlier or later in

North America than in Europe, so will the southern migration there have

been a little earlier or later; but this will make no difference in the

final result.

As the warmth returned, the arctic forms would retreat northward,

closely followed up in their retreat by the productions of the more

temperate regions. And as the snow melted from the bases of the

mountains, the arctic forms would seize on the cleared and thawed

ground, always ascending higher and higher, as the warmth increased,

whilst their brethren were pursuing their northern journey. Hence, when

the warmth had fully returned, the same arctic species, which had lately

lived in a body together on the lowlands of the Old and New Worlds,

would be left isolated on distant mountain-summits (having been

exterminated on all lesser heights) and in the arctic regions of both

hemispheres.

Thus we can understand the identity of many plants at points so

immensely remote as on the mountains of the United States and of Europe.

We can thus also understand the fact that the Alpine plants of each

mountain-range are more especially related to the arctic forms living

due north or nearly due north of them: for the migration as the cold

came on, and the re-migration on the returning warmth, will generally

have been due south and north. The Alpine plants, for example, of

Scotland, as remarked by Mr. H. C. Watson, and those of the Pyrenees, as

remarked by Ramond, are more especially allied to the plants of northern

Scandinavia; those of the United States to Labrador; those of the

mountains of Siberia to the arctic regions of that country. These views,

grounded as they are on the perfectly well-ascertained occurrence of a

former Glacial period, seem to me to explain in so satisfactory a manner

the present distribution of the Alpine and Arctic productions of Europe

and America, that when in other regions we find the same species on

distant mountain-summits, we may almost conclude without other evidence,

that a colder climate permitted their former migration across the low

intervening tracts, since become too warm for their existence.

If the climate, since the Glacial period, has ever been in any degree

warmer than at present (as some geologists in the United States believe

to have been the case, chiefly from the distribution of the fossil

Gnathodon), then the arctic and temperate productions will at a very

late period have marched a little further north, and subsequently have

retreated to their present homes; but I have met with no satisfactory

evidence with respect to this intercalated slightly warmer period, since

the Glacial period.

The arctic forms, during their long southern migration and re-migration

northward, will have been exposed to nearly the same climate, and, as

is especially to be noticed, they will have kept in a body together;

consequently their mutual relations will not have been much disturbed,

and, in accordance with the principles inculcated in this volume, they

will not have been liable to much modification. But with our Alpine

productions, left isolated from the moment of the returning warmth,

first at the bases and ultimately on the summits of the mountains, the

case will have been somewhat different; for it is not likely that all

the same arctic species will have been left on mountain ranges distant

from each other, and have survived there ever since; they will, also, in

all probability have become mingled with ancient Alpine species, which

must have existed on the mountains before the commencement of the

Glacial epoch, and which during its coldest period will have been

temporarily driven down to the plains; they will, also, have been

exposed to somewhat different climatal influences. Their mutual

relations will thus have been in some degree disturbed; consequently

they will have been liable to modification; and this we find has been

the case; for if we compare the present Alpine plants and animals of the

several great European mountain-ranges, though very many of the species

are identically the same, some present varieties, some are ranked

as doubtful forms, and some few are distinct yet closely allied or

representative species.

In illustrating what, as I believe, actually took place during

the Glacial period, I assumed that at its commencement the arctic

productions were as uniform round the polar regions as they are at the

present day. But the foregoing remarks on distribution apply not only

to strictly arctic forms, but also to many sub-arctic and to some few

northern temperate forms, for some of these are the same on the lower

mountains and on the plains of North America and Europe; and it may be

reasonably asked how I account for the necessary degree of uniformity

of the sub-arctic and northern temperate forms round the world, at the

commencement of the Glacial period. At the present day, the sub-arctic

and northern temperate productions of the Old and New Worlds are

separated from each other by the Atlantic Ocean and by the extreme

northern part of the Pacific. During the Glacial period, when the

inhabitants of the Old and New Worlds lived further southwards than at

present, they must have been still more completely separated by wider

spaces of ocean. I believe the above difficulty may be surmounted by

looking to still earlier changes of climate of an opposite nature.

We have good reason to believe that during the newer Pliocene period,

before the Glacial epoch, and whilst the majority of the inhabitants of

the world were specifically the same as now, the climate was warmer than

at the present day. Hence we may suppose that the organisms now living

under the climate of latitude 60 deg, during the Pliocene period lived

further north under the Polar Circle, in latitude 66 deg-67 deg; and

that the strictly arctic productions then lived on the broken land still

nearer to the pole. Now if we look at a globe, we shall see that under

the Polar Circle there is almost continuous land from western Europe,

through Siberia, to eastern America. And to this continuity of the

circumpolar land, and to the consequent freedom for intermigration

under a more favourable climate, I attribute the necessary amount of

uniformity in the sub-arctic and northern temperate productions of the

Old and New Worlds, at a period anterior to the Glacial epoch.

Believing, from reasons before alluded to, that our continents have

long remained in nearly the same relative position, though subjected

to large, but partial oscillations of level, I am strongly inclined to

extend the above view, and to infer that during some earlier and still

warmer period, such as the older Pliocene period, a large number of

the same plants and animals inhabited the almost continuous circumpolar

land; and that these plants and animals, both in the Old and New Worlds,

began slowly to migrate southwards as the climate became less warm, long

before the commencement of the Glacial period. We now see, as I believe,

their descendants, mostly in a modified condition, in the central parts

of Europe and the United States. On this view we can understand the

relationship, with very little identity, between the productions of

North America and Europe,--a relationship which is most remarkable,

considering the distance of the two areas, and their separation by the

Atlantic Ocean. We can further understand the singular fact remarked on

by several observers, that the productions of Europe and America during

the later tertiary stages were more closely related to each other

than they are at the present time; for during these warmer periods

the northern parts of the Old and New Worlds will have been almost

continuously united by land, serving as a bridge, since rendered

impassable by cold, for the inter-migration of their inhabitants.

During the slowly decreasing warmth of the Pliocene period, as soon as

the species in common, which inhabited the New and Old Worlds, migrated

south of the Polar Circle, they must have been completely cut off from

each other. This separation, as far as the more temperate productions

are concerned, took place long ages ago. And as the plants and animals

migrated southward, they will have become mingled in the one great

region with the native American productions, and have had to compete

with them; and in the other great region, with those of the Old

World. Consequently we have here everything favourable for much

modification,--for far more modification than with the Alpine

productions, left isolated, within a much more recent period, on the

several mountain-ranges and on the arctic lands of the two Worlds. Hence

it has come, that when we compare the now living productions of the

temperate regions of the New and Old Worlds, we find very few identical

species (though Asa Gray has lately shown that more plants are identical

than was formerly supposed), but we find in every great class many

forms, which some naturalists rank as geographical races, and others as

distinct species; and a host of closely allied or representative forms

which are ranked by all naturalists as specifically distinct.

As on the land, so in the waters of the sea, a slow southern migration

of a marine fauna, which during the Pliocene or even a somewhat earlier

period, was nearly uniform along the continuous shores of the Polar

Circle, will account, on the theory of modification, for many closely

allied forms now living in areas completely sundered. Thus, I think, we

can understand the presence of many existing and tertiary representative

forms on the eastern and western shores of temperate North America;

and the still more striking case of many closely allied crustaceans

(as described in Dana's admirable work), of some fish and other marine

animals, in the Mediterranean and in the seas of Japan,--areas now

separated by a continent and by nearly a hemisphere of equatorial ocean.

These cases of relationship, without identity, of the inhabitants of

seas now disjoined, and likewise of the past and present inhabitants of

the temperate lands of North America and Europe, are inexplicable on the

theory of creation. We cannot say that they have been created alike, in

correspondence with the nearly similar physical conditions of the areas;

for if we compare, for instance, certain parts of South America with

the southern continents of the Old World, we see countries closely

corresponding in all their physical conditions, but with their

inhabitants utterly dissimilar.

But we must return to our more immediate subject, the Glacial period.

I am convinced that Forbes's view may be largely extended. In Europe we

have the plainest evidence of the cold period, from the western shores

of Britain to the Oural range, and southward to the Pyrenees. We may

infer, from the frozen mammals and nature of the mountain vegetation,

that Siberia was similarly affected. Along the Himalaya, at points 900

miles apart, glaciers have left the marks of their former low descent;

and in Sikkim, Dr. Hooker saw maize growing on gigantic ancient

moraines. South of the equator, we have some direct evidence of former

glacial action in New Zealand; and the same plants, found on widely

separated mountains in this island, tell the same story. If one account

which has been published can be trusted, we have direct evidence of

glacial action in the south-eastern corner of Australia.

Looking to America; in the northern half, ice-borne fragments of rock

have been observed on the eastern side as far south as lat. 36 deg-37

deg, and on the shores of the Pacific, where the climate is now so

different, as far south as lat. 46 deg; erratic boulders have, also,

been noticed on the Rocky Mountains. In the Cordillera of Equatorial

South America, glaciers once extended far below their present level.

In central Chile I was astonished at the structure of a vast mound of

detritus, about 800 feet in height, crossing a valley of the Andes; and

this I now feel convinced was a gigantic moraine, left far below any

existing glacier. Further south on both sides of the continent, from

lat. 41 deg to the southernmost extremity, we have the clearest evidence

of former glacial action, in huge boulders transported far from their

parent source.

We do not know that the Glacial epoch was strictly simultaneous at these

several far distant points on opposite sides of the world. But we have

good evidence in almost every case, that the epoch was included within

the latest geological period. We have, also, excellent evidence, that it

endured for an enormous time, as measured by years, at each point. The

cold may have come on, or have ceased, earlier at one point of the globe

than at another, but seeing that it endured for long at each, and that

it was contemporaneous in a geological sense, it seems to me probable

that it was, during a part at least of the period, actually simultaneous

throughout the world. Without some distinct evidence to the contrary, we

may at least admit as probable that the glacial action was simultaneous

on the eastern and western sides of North America, in the Cordillera

under the equator and under the warmer temperate zones, and on both

sides of the southern extremity of the continent. If this be admitted,

it is difficult to avoid believing that the temperature of the whole

world was at this period simultaneously cooler. But it would suffice for

my purpose, if the temperature was at the same time lower along certain

broad belts of longitude.

On this view of the whole world, or at least of broad longitudinal

belts, having been simultaneously colder from pole to pole, much light

can be thrown on the present distribution of identical and allied

species. In America, Dr. Hooker has shown that between forty and fifty

of the flowering plants of Tierra del Fuego, forming no inconsiderable

part of its scanty flora, are common to Europe, enormously remote as

these two points are; and there are many closely allied species. On

the lofty mountains of equatorial America a host of peculiar species

belonging to European genera occur. On the highest mountains of Brazil,

some few European genera were found by Gardner, which do not exist in

the wide intervening hot countries. So on the Silla of Caraccas

the illustrious Humboldt long ago found species belonging to genera

characteristic of the Cordillera. On the mountains of Abyssinia, several

European forms and some few representatives of the peculiar flora of the

Cape of Good Hope occur. At the Cape of Good Hope a very few European

species, believed not to have been introduced by man, and on the

mountains, some few representative European forms are found, which

have not been discovered in the intertropical parts of Africa. On the

Himalaya, and on the isolated mountain-ranges of the peninsula of India,

on the heights of Ceylon, and on the volcanic cones of Java, many plants

occur, either identically the same or representing each other, and

at the same time representing plants of Europe, not found in the

intervening hot lowlands. A list of the genera collected on the loftier

peaks of Java raises a picture of a collection made on a hill in Europe!

Still more striking is the fact that southern Australian forms are

clearly represented by plants growing on the summits of the mountains

of Borneo. Some of these Australian forms, as I hear from Dr. Hooker,

extend along the heights of the peninsula of Malacca, and are thinly

scattered, on the one hand over India and on the other as far north as

Japan.

On the southern mountains of Australia, Dr. F. Muller has discovered

several European species; other species, not introduced by man, occur

on the lowlands; and a long list can be given, as I am informed by

Dr. Hooker, of European genera, found in Australia, but not in the

intermediate torrid regions. In the admirable 'Introduction to the Flora

of New Zealand,' by Dr. Hooker, analogous and striking facts are

given in regard to the plants of that large island. Hence we see that

throughout the world, the plants growing on the more lofty mountains,

and on the temperate lowlands of the northern and southern hemispheres,

are sometimes identically the same; but they are much oftener

specifically distinct, though related to each other in a most remarkable

manner.

This brief abstract applies to plants alone: some strictly analogous

facts could be given on the distribution of terrestrial animals. In

marine productions, similar cases occur; as an example, I may quote a

remark by the highest authority, Professor Dana, that "it is certainly a

wonderful fact that New Zealand should have a closer resemblance in its

crustacea to Great Britain, its antipode, than to any other part of

the world." Sir J. Richardson, also, speaks of the reappearance on the

shores of New Zealand, Tasmania, etc., of northern forms of fish. Dr.

Hooker informs me that twenty-five species of Algae are common to New

Zealand and to Europe, but have not been found in the intermediate

tropical seas.

It should be observed that the northern species and forms found in the

southern parts of the southern hemisphere, and on the mountain-ranges

of the intertropical regions, are not arctic, but belong to the northern

temperate zones. As Mr. H. C. Watson has recently remarked, "In receding

from polar towards equatorial latitudes, the Alpine or mountain floras

really become less and less arctic." Many of the forms living on

the mountains of the warmer regions of the earth and in the southern

hemisphere are of doubtful value, being ranked by some naturalists as

specifically distinct, by others as varieties; but some are certainly

identical, and many, though closely related to northern forms, must be

ranked as distinct species.

Now let us see what light can be thrown on the foregoing facts, on the

belief, supported as it is by a large body of geological evidence, that

the whole world, or a large part of it, was during the Glacial period

simultaneously much colder than at present. The Glacial period, as

measured by years, must have been very long; and when we remember over

what vast spaces some naturalised plants and animals have spread within

a few centuries, this period will have been ample for any amount of

migration. As the cold came slowly on, all the tropical plants and other

productions will have retreated from both sides towards the equator,

followed in the rear by the temperate productions, and these by the

arctic; but with the latter we are not now concerned. The tropical

plants probably suffered much extinction; how much no one can say;

perhaps formerly the tropics supported as many species as we see at the

present day crowded together at the Cape of Good Hope, and in parts of

temperate Australia. As we know that many tropical plants and animals

can withstand a considerable amount of cold, many might have escaped

extermination during a moderate fall of temperature, more especially by

escaping into the warmest spots. But the great fact to bear in mind is,

that all tropical productions will have suffered to a certain extent. On

the other hand, the temperate productions, after migrating nearer to

the equator, though they will have been placed under somewhat new

conditions, will have suffered less. And it is certain that many

temperate plants, if protected from the inroads of competitors, can

withstand a much warmer climate than their own. Hence, it seems to

me possible, bearing in mind that the tropical productions were in

a suffering state and could not have presented a firm front against

intruders, that a certain number of the more vigorous and dominant

temperate forms might have penetrated the native ranks and have reached

or even crossed the equator. The invasion would, of course, have been

greatly favoured by high land, and perhaps by a dry climate; for Dr.

Falconer informs me that it is the damp with the heat of the tropics

which is so destructive to perennial plants from a temperate climate. On

the other hand, the most humid and hottest districts will have afforded

an asylum to the tropical natives. The mountain-ranges north-west of the

Himalaya, and the long line of the Cordillera, seem to have afforded two

great lines of invasion: and it is a striking fact, lately communicated

to me by Dr. Hooker, that all the flowering plants, about forty-six in

number, common to Tierra del Fuego and to Europe still exist in North

America, which must have lain on the line of march. But I do not doubt

that some temperate productions entered and crossed even the LOWLANDS of

the tropics at the period when the cold was most intense,--when arctic

forms had migrated some twenty-five degrees of latitude from their

native country and covered the land at the foot of the Pyrenees. At this

period of extreme cold, I believe that the climate under the equator at

the level of the sea was about the same with that now felt there at the

height of six or seven thousand feet. During this the coldest period, I

suppose that large spaces of the tropical lowlands were clothed with a

mingled tropical and temperate vegetation, like that now growing with

strange luxuriance at the base of the Himalaya, as graphically described

by Hooker.

Thus, as I believe, a considerable number of plants, a few terrestrial

animals, and some marine productions, migrated during the Glacial period

from the northern and southern temperate zones into the intertropical

regions, and some even crossed the equator. As the warmth returned,

these temperate forms would naturally ascend the higher mountains, being

exterminated on the lowlands; those which had not reached the equator,

would re-migrate northward or southward towards their former homes; but

the forms, chiefly northern, which had crossed the equator, would travel

still further from their homes into the more temperate latitudes of the

opposite hemisphere. Although we have reason to believe from geological

evidence that the whole body of arctic shells underwent scarcely any

modification during their long southern migration and re-migration

northward, the case may have been wholly different with those intruding

forms which settled themselves on the intertropical mountains, and in

the southern hemisphere. These being surrounded by strangers will have

had to compete with many new forms of life; and it is probable that

selected modifications in their structure, habits, and constitutions

will have profited them. Thus many of these wanderers, though still

plainly related by inheritance to their brethren of the northern or

southern hemispheres, now exist in their new homes as well-marked

varieties or as distinct species.

It is a remarkable fact, strongly insisted on by Hooker in regard to

America, and by Alph. de Candolle in regard to Australia, that many

more identical plants and allied forms have apparently migrated from the

north to the south, than in a reversed direction. We see, however, a

few southern vegetable forms on the mountains of Borneo and Abyssinia.

I suspect that this preponderant migration from north to south is due

to the greater extent of land in the north, and to the northern forms

having existed in their own homes in greater numbers, and having

consequently been advanced through natural selection and competition

to a higher stage of perfection or dominating power, than the southern

forms. And thus, when they became commingled during the Glacial period,

the northern forms were enabled to beat the less powerful southern

forms. Just in the same manner as we see at the present day, that very

many European productions cover the ground in La Plata, and in a lesser

degree in Australia, and have to a certain extent beaten the natives;

whereas extremely few southern forms have become naturalised in any part

of Europe, though hides, wool, and other objects likely to carry seeds

have been largely imported into Europe during the last two or three

centuries from La Plata, and during the last thirty or forty years

from Australia. Something of the same kind must have occurred on the

intertropical mountains: no doubt before the Glacial period they were

stocked with endemic Alpine forms; but these have almost everywhere

largely yielded to the more dominant forms, generated in the larger

areas and more efficient workshops of the north. In many islands the

native productions are nearly equalled or even outnumbered by the

naturalised; and if the natives have not been actually exterminated,

their numbers have been greatly reduced, and this is the first stage

towards extinction. A mountain is an island on the land; and the

intertropical mountains before the Glacial period must have been

completely isolated; and I believe that the productions of these islands

on the land yielded to those produced within the larger areas of the

north, just in the same way as the productions of real islands have

everywhere lately yielded to continental forms, naturalised by man's

agency.

I am far from supposing that all difficulties are removed on the view

here given in regard to the range and affinities of the allied species

which live in the northern and southern temperate zones and on the

mountains of the intertropical regions. Very many difficulties remain

to be solved. I do not pretend to indicate the exact lines and means

of migration, or the reason why certain species and not others have

migrated; why certain species have been modified and have given rise to

new groups of forms, and others have remained unaltered. We cannot hope

to explain such facts, until we can say why one species and not another

becomes naturalised by man's agency in a foreign land; why one ranges

twice or thrice as far, and is twice or thrice as common, as another

species within their own homes.

I have said that many difficulties remain to be solved: some of the

most remarkable are stated with admirable clearness by Dr. Hooker in

his botanical works on the antarctic regions. These cannot be here

discussed. I will only say that as far as regards the occurrence of

identical species at points so enormously remote as Kerguelen Land, New

Zealand, and Fuegia, I believe that towards the close of the Glacial

period, icebergs, as suggested by Lyell, have been largely concerned in

their dispersal. But the existence of several quite distinct species,

belonging to genera exclusively confined to the south, at these and

other distant points of the southern hemisphere, is, on my theory of

descent with modification, a far more remarkable case of difficulty. For

some of these species are so distinct, that we cannot suppose that there

has been time since the commencement of the Glacial period for their

migration, and for their subsequent modification to the necessary

degree. The facts seem to me to indicate that peculiar and very distinct

species have migrated in radiating lines from some common centre; and I

am inclined to look in the southern, as in the northern hemisphere, to a

former and warmer period, before the commencement of the Glacial period,

when the antarctic lands, now covered with ice, supported a highly

peculiar and isolated flora. I suspect that before this flora was

exterminated by the Glacial epoch, a few forms were widely dispersed

to various points of the southern hemisphere by occasional means of

transport, and by the aid, as halting-places, of existing and now sunken

islands, and perhaps at the commencement of the Glacial period, by

icebergs. By these means, as I believe, the southern shores of America,

Australia, New Zealand have become slightly tinted by the same peculiar

forms of vegetable life.

Sir C. Lyell in a striking passage has speculated, in language almost

identical with mine, on the effects of great alternations of climate on

geographical distribution. I believe that the world has recently felt

one of his great cycles of change; and that on this view, combined with

modification through natural selection, a multitude of facts in the

present distribution both of the same and of allied forms of life can be

explained. The living waters may be said to have flowed during one short

period from the north and from the south, and to have crossed at the

equator; but to have flowed with greater force from the north so as

to have freely inundated the south. As the tide leaves its drift in

horizontal lines, though rising higher on the shores where the tide

rises highest, so have the living waters left their living drift on our

mountain-summits, in a line gently rising from the arctic lowlands to

a great height under the equator. The various beings thus left stranded

may be compared with savage races of man, driven up and surviving in the

mountain-fastnesses of almost every land, which serve as a record,

full of interest to us, of the former inhabitants of the surrounding

lowlands.

12. GEOGRAPHICAL DISTRIBUTION--continued.

Distribution of fresh-water productions. On the inhabitants of oceanic

islands. Absence of Batrachians and of terrestrial Mammals. On the

relation of the inhabitants of islands to those of the nearest mainland.

On colonisation from the nearest source with subsequent modification.

Summary of the last and present chapters.

As lakes and river-systems are separated from each other by barriers of

land, it might have been thought that fresh-water productions would not

have ranged widely within the same country, and as the sea is apparently

a still more impassable barrier, that they never would have extended to

distant countries. But the case is exactly the reverse. Not only have

many fresh-water species, belonging to quite different classes, an

enormous range, but allied species prevail in a remarkable manner

throughout the world. I well remember, when first collecting in the

fresh waters of Brazil, feeling much surprise at the similarity of

the fresh-water insects, shells, etc., and at the dissimilarity of the

surrounding terrestrial beings, compared with those of Britain.

But this power in fresh-water productions of ranging widely, though so

unexpected, can, I think, in most cases be explained by their having

become fitted, in a manner highly useful to them, for short and frequent

migrations from pond to pond, or from stream to stream; and liability

to wide dispersal would follow from this capacity as an almost necessary

consequence. We can here consider only a few cases. In regard to fish, I

believe that the same species never occur in the fresh waters of distant

continents. But on the same continent the species often range widely and

almost capriciously; for two river-systems will have some fish in common

and some different. A few facts seem to favour the possibility of their

occasional transport by accidental means; like that of the live fish

not rarely dropped by whirlwinds in India, and the vitality of their

ova when removed from the water. But I am inclined to attribute the

dispersal of fresh-water fish mainly to slight changes within the recent

period in the level of the land, having caused rivers to flow into each

other. Instances, also, could be given of this having occurred during

floods, without any change of level. We have evidence in the loess of

the Rhine of considerable changes of level in the land within a very

recent geological period, and when the surface was peopled by existing

land and fresh-water shells. The wide difference of the fish on opposite

sides of continuous mountain-ranges, which from an early period must

have parted river-systems and completely prevented their inosculation,

seems to lead to this same conclusion. With respect to allied

fresh-water fish occurring at very distant points of the world, no doubt

there are many cases which cannot at present be explained: but some

fresh-water fish belong to very ancient forms, and in such cases

there will have been ample time for great geographical changes, and

consequently time and means for much migration. In the second place,

salt-water fish can with care be slowly accustomed to live in fresh

water; and, according to Valenciennes, there is hardly a single group of

fishes confined exclusively to fresh water, so that we may imagine that

a marine member of a fresh-water group might travel far along the shores

of the sea, and subsequently become modified and adapted to the fresh

waters of a distant land.

Some species of fresh-water shells have a very wide range, and allied

species, which, on my theory, are descended from a common parent and

must have proceeded from a single source, prevail throughout the world.

Their distribution at first perplexed me much, as their ova are not

likely to be transported by birds, and they are immediately killed

by sea water, as are the adults. I could not even understand how some

naturalised species have rapidly spread throughout the same country. But

two facts, which I have observed--and no doubt many others remain to be

observed--throw some light on this subject. When a duck suddenly emerges

from a pond covered with duck-weed, I have twice seen these little

plants adhering to its back; and it has happened to me, in removing

a little duck-weed from one aquarium to another, that I have quite

unintentionally stocked the one with fresh-water shells from the other.

But another agency is perhaps more effectual: I suspended a duck's feet,

which might represent those of a bird sleeping in a natural pond, in

an aquarium, where many ova of fresh-water shells were hatching; and

I found that numbers of the extremely minute and just hatched shells

crawled on the feet, and clung to them so firmly that when taken out

of the water they could not be jarred off, though at a somewhat more

advanced age they would voluntarily drop off. These just hatched

molluscs, though aquatic in their nature, survived on the duck's feet,

in damp air, from twelve to twenty hours; and in this length of time a

duck or heron might fly at least six or seven hundred miles, and would

be sure to alight on a pool or rivulet, if blown across sea to an

oceanic island or to any other distant point. Sir Charles Lyell also

informs me that a Dyticus has been caught with an Ancylus (a fresh-water

shell like a limpet) firmly adhering to it; and a water-beetle of

the same family, a Colymbetes, once flew on board the 'Beagle,' when

forty-five miles distant from the nearest land: how much farther it

might have flown with a favouring gale no one can tell.

With respect to plants, it has long been known what enormous ranges many

fresh-water and even marsh-species have, both over continents and to the

most remote oceanic islands. This is strikingly shown, as remarked by

Alph. de Candolle, in large groups of terrestrial plants, which have

only a very few aquatic members; for these latter seem immediately to

acquire, as if in consequence, a very wide range. I think favourable

means of dispersal explain this fact. I have before mentioned that earth

occasionally, though rarely, adheres in some quantity to the feet and

beaks of birds. Wading birds, which frequent the muddy edges of ponds,

if suddenly flushed, would be the most likely to have muddy feet.

Birds of this order I can show are the greatest wanderers, and are

occasionally found on the most remote and barren islands in the open

ocean; they would not be likely to alight on the surface of the sea, so

that the dirt would not be washed off their feet; when making land,

they would be sure to fly to their natural fresh-water haunts. I do not

believe that botanists are aware how charged the mud of ponds is with

seeds: I have tried several little experiments, but will here give only

the most striking case: I took in February three table-spoonfuls of

mud from three different points, beneath water, on the edge of a little

pond; this mud when dry weighed only 6 3/4 ounces; I kept it covered

up in my study for six months, pulling up and counting each plant as it

grew; the plants were of many kinds, and were altogether 537 in number;

and yet the viscid mud was all contained in a breakfast cup! Considering

these facts, I think it would be an inexplicable circumstance if

water-birds did not transport the seeds of fresh-water plants to vast

distances, and if consequently the range of these plants was not very

great. The same agency may have come into play with the eggs of some of

the smaller fresh-water animals.

Other and unknown agencies probably have also played a part. I have

stated that fresh-water fish eat some kinds of seeds, though they reject

many other kinds after having swallowed them; even small fish swallow

seeds of moderate size, as of the yellow water-lily and Potamogeton.

Herons and other birds, century after century, have gone on daily

devouring fish; they then take flight and go to other waters, or are

blown across the sea; and we have seen that seeds retain their power

of germination, when rejected in pellets or in excrement, many hours

afterwards. When I saw the great size of the seeds of that fine

water-lily, the Nelumbium, and remembered Alph. de Candolle's remarks

on this plant, I thought that its distribution must remain quite

inexplicable; but Audubon states that he found the seeds of the great

southern water-lily (probably, according to Dr. Hooker, the Nelumbium

luteum) in a heron's stomach; although I do not know the fact, yet

analogy makes me believe that a heron flying to another pond and getting

a hearty meal of fish, would probably reject from its stomach a pellet

containing the seeds of the Nelumbium undigested; or the seeds might be

dropped by the bird whilst feeding its young, in the same way as fish

are known sometimes to be dropped.

In considering these several means of distribution, it should be

remembered that when a pond or stream is first formed, for instance,

on a rising islet, it will be unoccupied; and a single seed or egg

will have a good chance of succeeding. Although there will always be a

struggle for life between the individuals of the species, however

few, already occupying any pond, yet as the number of kinds is small,

compared with those on the land, the competition will probably be less

severe between aquatic than between terrestrial species; consequently

an intruder from the waters of a foreign country, would have a better

chance of seizing on a place, than in the case of terrestrial colonists.

We should, also, remember that some, perhaps many, fresh-water

productions are low in the scale of nature, and that we have reason to

believe that such low beings change or become modified less quickly

than the high; and this will give longer time than the average for

the migration of the same aquatic species. We should not forget the

probability of many species having formerly ranged as continuously as

fresh-water productions ever can range, over immense areas, and having

subsequently become extinct in intermediate regions. But the wide

distribution of fresh-water plants and of the lower animals, whether

retaining the same identical form or in some degree modified, I believe

mainly depends on the wide dispersal of their seeds and eggs by animals,

more especially by fresh-water birds, which have large powers of flight,

and naturally travel from one to another and often distant piece of

water. Nature, like a careful gardener, thus takes her seeds from a bed

of a particular nature, and drops them in another equally well fitted

for them.

ON THE INHABITANTS OF OCEANIC ISLANDS.

We now come to the last of the three classes of facts, which I have

selected as presenting the greatest amount of difficulty, on the view

that all the individuals both of the same and of allied species have

descended from a single parent; and therefore have all proceeded from a

common birthplace, notwithstanding that in the course of time they have

come to inhabit distant points of the globe. I have already stated that

I cannot honestly admit Forbes's view on continental extensions, which,

if legitimately followed out, would lead to the belief that within the

recent period all existing islands have been nearly or quite joined to

some continent. This view would remove many difficulties, but it would

not, I think, explain all the facts in regard to insular productions. In

the following remarks I shall not confine myself to the mere question of

dispersal; but shall consider some other facts, which bear on the

truth of the two theories of independent creation and of descent with

modification.

The species of all kinds which inhabit oceanic islands are few in number

compared with those on equal continental areas: Alph. de Candolle admits

this for plants, and Wollaston for insects. If we look to the large

size and varied stations of New Zealand, extending over 780 miles of

latitude, and compare its flowering plants, only 750 in number, with

those on an equal area at the Cape of Good Hope or in Australia,

we must, I think, admit that something quite independently of any

difference in physical conditions has caused so great a difference in

number. Even the uniform county of Cambridge has 847 plants, and the

little island of Anglesea 764, but a few ferns and a few introduced

plants are included in these numbers, and the comparison in some other

respects is not quite fair. We have evidence that the barren island of

Ascension aboriginally possessed under half-a-dozen flowering plants;

yet many have become naturalised on it, as they have on New Zealand and

on every other oceanic island which can be named. In St. Helena there is

reason to believe that the naturalised plants and animals have nearly or

quite exterminated many native productions. He who admits the doctrine

of the creation of each separate species, will have to admit, that a

sufficient number of the best adapted plants and animals have not been

created on oceanic islands; for man has unintentionally stocked them

from various sources far more fully and perfectly than has nature.

Although in oceanic islands the number of kinds of inhabitants is

scanty, the proportion of endemic species (i.e. those found nowhere else

in the world) is often extremely large. If we compare, for instance, the

number of the endemic land-shells in Madeira, or of the endemic birds in

the Galapagos Archipelago, with the number found on any continent, and

then compare the area of the islands with that of the continent, we

shall see that this is true. This fact might have been expected on my

theory, for, as already explained, species occasionally arriving after

long intervals in a new and isolated district, and having to compete

with new associates, will be eminently liable to modification, and

will often produce groups of modified descendants. But it by no means

follows, that, because in an island nearly all the species of one class

are peculiar, those of another class, or of another section of the same

class, are peculiar; and this difference seems to depend on the species

which do not become modified having immigrated with facility and in a

body, so that their mutual relations have not been much disturbed. Thus

in the Galapagos Islands nearly every land-bird, but only two out of the

eleven marine birds, are peculiar; and it is obvious that marine birds

could arrive at these islands more easily than land-birds. Bermuda, on

the other hand, which lies at about the same distance from North America

as the Galapagos Islands do from South America, and which has a very

peculiar soil, does not possess one endemic land bird; and we know from

Mr. J. M. Jones's admirable account of Bermuda, that very many North

American birds, during their great annual migrations, visit either

periodically or occasionally this island. Madeira does not possess one

peculiar bird, and many European and African birds are almost every year

blown there, as I am informed by Mr. E. V. Harcourt. So that these two

islands of Bermuda and Madeira have been stocked by birds, which for

long ages have struggled together in their former homes, and have become

mutually adapted to each other; and when settled in their new homes,

each kind will have been kept by the others to their proper places and

habits, and will consequently have been little liable to modification.

Madeira, again, is inhabited by a wonderful number of peculiar

land-shells, whereas not one species of sea-shell is confined to its

shores: now, though we do not know how seashells are dispersed, yet

we can see that their eggs or larvae, perhaps attached to seaweed or

floating timber, or to the feet of wading-birds, might be transported

far more easily than land-shells, across three or four hundred miles of

open sea. The different orders of insects in Madeira apparently present

analogous facts.

Oceanic islands are sometimes deficient in certain classes, and

their places are apparently occupied by the other inhabitants; in the

Galapagos Islands reptiles, and in New Zealand gigantic wingless birds,

take the place of mammals. In the plants of the Galapagos Islands, Dr.

Hooker has shown that the proportional numbers of the different

orders are very different from what they are elsewhere. Such cases are

generally accounted for by the physical conditions of the islands;

but this explanation seems to me not a little doubtful. Facility of

immigration, I believe, has been at least as important as the nature of

the conditions.

Many remarkable little facts could be given with respect to the

inhabitants of remote islands. For instance, in certain islands not

tenanted by mammals, some of the endemic plants have beautifully hooked

seeds; yet few relations are more striking than the adaptation of hooked

seeds for transportal by the wool and fur of quadrupeds. This

case presents no difficulty on my view, for a hooked seed might be

transported to an island by some other means; and the plant then

becoming slightly modified, but still retaining its hooked seeds,

would form an endemic species, having as useless an appendage as any

rudimentary organ,--for instance, as the shrivelled wings under the

soldered elytra of many insular beetles. Again, islands often possess

trees or bushes belonging to orders which elsewhere include only

herbaceous species; now trees, as Alph. de Candolle has shown, generally

have, whatever the cause may be, confined ranges. Hence trees would be

little likely to reach distant oceanic islands; and an herbaceous plant,

though it would have no chance of successfully competing in stature

with a fully developed tree, when established on an island and having to

compete with herbaceous plants alone, might readily gain an advantage

by growing taller and taller and overtopping the other plants. If so,

natural selection would often tend to add to the stature of herbaceous

plants when growing on an island, to whatever order they belonged, and

thus convert them first into bushes and ultimately into trees.

With respect to the absence of whole orders on oceanic islands, Bory St.

Vincent long ago remarked that Batrachians (frogs, toads, newts) have

never been found on any of the many islands with which the great oceans

are studded. I have taken pains to verify this assertion, and I have

found it strictly true. I have, however, been assured that a frog exists

on the mountains of the great island of New Zealand; but I suspect that

this exception (if the information be correct) may be explained through

glacial agency. This general absence of frogs, toads, and newts on

so many oceanic islands cannot be accounted for by their physical

conditions; indeed it seems that islands are peculiarly well fitted for

these animals; for frogs have been introduced into Madeira, the Azores,

and Mauritius, and have multiplied so as to become a nuisance. But as

these animals and their spawn are known to be immediately killed by

sea-water, on my view we can see that there would be great difficulty in

their transportal across the sea, and therefore why they do not exist on

any oceanic island. But why, on the theory of creation, they should not

have been created there, it would be very difficult to explain.

Mammals offer another and similar case. I have carefully searched the

oldest voyages, but have not finished my search; as yet I have not found

a single instance, free from doubt, of a terrestrial mammal (excluding

domesticated animals kept by the natives) inhabiting an island situated

above 300 miles from a continent or great continental island; and

many islands situated at a much less distance are equally barren. The

Falkland Islands, which are inhabited by a wolf-like fox, come nearest

to an exception; but this group cannot be considered as oceanic, as it

lies on a bank connected with the mainland; moreover, icebergs formerly

brought boulders to its western shores, and they may have formerly

transported foxes, as so frequently now happens in the arctic regions.

Yet it cannot be said that small islands will not support small mammals,

for they occur in many parts of the world on very small islands, if

close to a continent; and hardly an island can be named on which our

smaller quadrupeds have not become naturalised and greatly multiplied.

It cannot be said, on the ordinary view of creation, that there has

not been time for the creation of mammals; many volcanic islands are

sufficiently ancient, as shown by the stupendous degradation which they

have suffered and by their tertiary strata: there has also been time

for the production of endemic species belonging to other classes; and on

continents it is thought that mammals appear and disappear at a quicker

rate than other and lower animals. Though terrestrial mammals do not

occur on oceanic islands, aerial mammals do occur on almost every

island. New Zealand possesses two bats found nowhere else in the world:

Norfolk Island, the Viti Archipelago, the Bonin Islands, the Caroline

and Marianne Archipelagoes, and Mauritius, all possess their peculiar

bats. Why, it may be asked, has the supposed creative force produced

bats and no other mammals on remote islands? On my view this question

can easily be answered; for no terrestrial mammal can be transported

across a wide space of sea, but bats can fly across. Bats have been seen

wandering by day far over the Atlantic Ocean; and two North American

species either regularly or occasionally visit Bermuda, at the distance

of 600 miles from the mainland. I hear from Mr. Tomes, who has specially

studied this family, that many of the same species have enormous ranges,

and are found on continents and on far distant islands. Hence we have

only to suppose that such wandering species have been modified through

natural selection in their new homes in relation to their new position,

and we can understand the presence of endemic bats on islands, with the

absence of all terrestrial mammals.

Besides the absence of terrestrial mammals in relation to the remoteness

of islands from continents, there is also a relation, to a certain

extent independent of distance, between the depth of the sea separating

an island from the neighbouring mainland, and the presence in both of

the same mammiferous species or of allied species in a more or less

modified condition. Mr. Windsor Earl has made some striking observations

on this head in regard to the great Malay Archipelago, which is

traversed near Celebes by a space of deep ocean; and this space

separates two widely distinct mammalian faunas. On either side the

islands are situated on moderately deep submarine banks, and they are

inhabited by closely allied or identical quadrupeds. No doubt some few

anomalies occur in this great archipelago, and there is much difficulty

in forming a judgment in some cases owing to the probable naturalisation

of certain mammals through man's agency; but we shall soon have much

light thrown on the natural history of this archipelago by the admirable

zeal and researches of Mr. Wallace. I have not as yet had time to follow

up this subject in all other quarters of the world; but as far as I have

gone, the relation generally holds good. We see Britain separated by a

shallow channel from Europe, and the mammals are the same on both

sides; we meet with analogous facts on many islands separated by similar

channels from Australia. The West Indian Islands stand on a deeply

submerged bank, nearly 1000 fathoms in depth, and here we find American

forms, but the species and even the genera are distinct. As the amount

of modification in all cases depends to a certain degree on the lapse

of time, and as during changes of level it is obvious that islands

separated by shallow channels are more likely to have been continuously

united within a recent period to the mainland than islands separated

by deeper channels, we can understand the frequent relation between the

depth of the sea and the degree of affinity of the mammalian inhabitants

of islands with those of a neighbouring continent,--an inexplicable

relation on the view of independent acts of creation.

All the foregoing remarks on the inhabitants of oceanic

islands,--namely, the scarcity of kinds--the richness in endemic forms

in particular classes or sections of classes,--the absence of whole

groups, as of batrachians, and of terrestrial mammals notwithstanding

the presence of aerial bats,--the singular proportions of certain

orders of plants,--herbaceous forms having been developed into trees,

etc.,--seem to me to accord better with the view of occasional means of

transport having been largely efficient in the long course of time, than

with the view of all our oceanic islands having been formerly connected

by continuous land with the nearest continent; for on this latter

view the migration would probably have been more complete; and if

modification be admitted, all the forms of life would have been more

equally modified, in accordance with the paramount importance of the

relation of organism to organism.

I do not deny that there are many and grave difficulties in

understanding how several of the inhabitants of the more remote islands,

whether still retaining the same specific form or modified since their

arrival, could have reached their present homes. But the probability of

many islands having existed as halting-places, of which not a wreck now

remains, must not be overlooked. I will here give a single instance of

one of the cases of difficulty. Almost all oceanic islands, even the

most isolated and smallest, are inhabited by land-shells, generally by

endemic species, but sometimes by species found elsewhere. Dr. Aug. A.

Gould has given several interesting cases in regard to the land-shells

of the islands of the Pacific. Now it is notorious that land-shells are

very easily killed by salt; their eggs, at least such as I have tried,

sink in sea-water and are killed by it. Yet there must be, on my view,

some unknown, but highly efficient means for their transportal. Would

the just-hatched young occasionally crawl on and adhere to the feet of

birds roosting on the ground, and thus get transported? It occurred to

me that land-shells, when hybernating and having a membranous diaphragm

over the mouth of the shell, might be floated in chinks of drifted

timber across moderately wide arms of the sea. And I found that several

species did in this state withstand uninjured an immersion in sea-water

during seven days: one of these shells was the Helix pomatia, and after

it had again hybernated I put it in sea-water for twenty days, and it

perfectly recovered. As this species has a thick calcareous operculum,

I removed it, and when it had formed a new membranous one, I immersed it

for fourteen days in sea-water, and it recovered and crawled away:

but more experiments are wanted on this head. The most striking and

important fact for us in regard to the inhabitants of islands, is their

affinity to those of the nearest mainland, without being actually the

same species. Numerous instances could be given of this fact. I will

give only one, that of the Galapagos Archipelago, situated under the

equator, between 500 and 600 miles from the shores of South America.

Here almost every product of the land and water bears the unmistakeable

stamp of the American continent. There are twenty-six land birds,

and twenty-five of these are ranked by Mr. Gould as distinct species,

supposed to have been created here; yet the close affinity of most of

these birds to American species in every character, in their habits,

gestures, and tones of voice, was manifest. So it is with the other

animals, and with nearly all the plants, as shown by Dr. Hooker in

his admirable memoir on the Flora of this archipelago. The naturalist,

looking at the inhabitants of these volcanic islands in the Pacific,

distant several hundred miles from the continent, yet feels that he is

standing on American land. Why should this be so? why should the species

which are supposed to have been created in the Galapagos Archipelago,

and nowhere else, bear so plain a stamp of affinity to those created in

America? There is nothing in the conditions of life, in the geological

nature of the islands, in their height or climate, or in the proportions

in which the several classes are associated together, which resembles

closely the conditions of the South American coast: in fact there is

a considerable dissimilarity in all these respects. On the other hand,

there is a considerable degree of resemblance in the volcanic nature

of the soil, in climate, height, and size of the islands, between

the Galapagos and Cape de Verde Archipelagos: but what an entire and

absolute difference in their inhabitants! The inhabitants of the Cape

de Verde Islands are related to those of Africa, like those of the

Galapagos to America. I believe this grand fact can receive no sort of

explanation on the ordinary view of independent creation; whereas on the

view here maintained, it is obvious that the Galapagos Islands would be

likely to receive colonists, whether by occasional means of transport or

by formerly continuous land, from America; and the Cape de Verde

Islands from Africa; and that such colonists would be liable to

modification;--the principle of inheritance still betraying their

original birthplace.

Many analogous facts could be given: indeed it is an almost universal

rule that the endemic productions of islands are related to those of the

nearest continent, or of other near islands. The exceptions are few, and

most of them can be explained. Thus the plants of Kerguelen Land, though

standing nearer to Africa than to America, are related, and that very

closely, as we know from Dr. Hooker's account, to those of America: but

on the view that this island has been mainly stocked by seeds brought

with earth and stones on icebergs, drifted by the prevailing currents,

this anomaly disappears. New Zealand in its endemic plants is much more

closely related to Australia, the nearest mainland, than to any other

region: and this is what might have been expected; but it is also

plainly related to South America, which, although the next nearest

continent, is so enormously remote, that the fact becomes an anomaly.

But this difficulty almost disappears on the view that both New Zealand,

South America, and other southern lands were long ago partially stocked

from a nearly intermediate though distant point, namely from the

antarctic islands, when they were clothed with vegetation, before the

commencement of the Glacial period. The affinity, which, though

feeble, I am assured by Dr. Hooker is real, between the flora of the

south-western corner of Australia and of the Cape of Good Hope, is a far

more remarkable case, and is at present inexplicable: but this affinity

is confined to the plants, and will, I do not doubt, be some day

explained.

The law which causes the inhabitants of an archipelago, though

specifically distinct, to be closely allied to those of the nearest

continent, we sometimes see displayed on a small scale, yet in a most

interesting manner, within the limits of the same archipelago. Thus the

several islands of the Galapagos Archipelago are tenanted, as I have

elsewhere shown, in a quite marvellous manner, by very closely related

species; so that the inhabitants of each separate island, though mostly

distinct, are related in an incomparably closer degree to each other

than to the inhabitants of any other part of the world. And this is just

what might have been expected on my view, for the islands are situated

so near each other that they would almost certainly receive

immigrants from the same original source, or from each other. But this

dissimilarity between the endemic inhabitants of the islands may be

used as an argument against my views; for it may be asked, how has it

happened in the several islands situated within sight of each other,

having the same geological nature, the same height, climate, etc., that

many of the immigrants should have been differently modified, though

only in a small degree. This long appeared to me a great difficulty: but

it arises in chief part from the deeply-seated error of considering

the physical conditions of a country as the most important for its

inhabitants; whereas it cannot, I think, be disputed that the nature of

the other inhabitants, with which each has to compete, is at least as

important, and generally a far more important element of success. Now

if we look to those inhabitants of the Galapagos Archipelago which are

found in other parts of the world (laying on one side for the moment

the endemic species, which cannot be here fairly included, as we are

considering how they have come to be modified since their arrival), we

find a considerable amount of difference in the several islands. This

difference might indeed have been expected on the view of the islands

having been stocked by occasional means of transport--a seed, for

instance, of one plant having been brought to one island, and that of

another plant to another island. Hence when in former times an immigrant

settled on any one or more of the islands, or when it subsequently

spread from one island to another, it would undoubtedly be exposed to

different conditions of life in the different islands, for it would

have to compete with different sets of organisms: a plant, for instance,

would find the best-fitted ground more perfectly occupied by distinct

plants in one island than in another, and it would be exposed to the

attacks of somewhat different enemies. If then it varied, natural

selection would probably favour different varieties in the different

islands. Some species, however, might spread and yet retain the same

character throughout the group, just as we see on continents some

species spreading widely and remaining the same.

The really surprising fact in this case of the Galapagos Archipelago,

and in a lesser degree in some analogous instances, is that the new

species formed in the separate islands have not quickly spread to the

other islands. But the islands, though in sight of each other, are

separated by deep arms of the sea, in most cases wider than the British

Channel, and there is no reason to suppose that they have at any former

period been continuously united. The currents of the sea are rapid and

sweep across the archipelago, and gales of wind are extraordinarily

rare; so that the islands are far more effectually separated from each

other than they appear to be on a map. Nevertheless a good many species,

both those found in other parts of the world and those confined to the

archipelago, are common to the several islands, and we may infer from

certain facts that these have probably spread from some one island

to the others. But we often take, I think, an erroneous view of the

probability of closely allied species invading each other's territory,

when put into free intercommunication. Undoubtedly if one species has

any advantage whatever over another, it will in a very brief time wholly

or in part supplant it; but if both are equally well fitted for their

own places in nature, both probably will hold their own places and keep

separate for almost any length of time. Being familiar with the fact

that many species, naturalised through man's agency, have spread with

astonishing rapidity over new countries, we are apt to infer that most

species would thus spread; but we should remember that the forms which

become naturalised in new countries are not generally closely allied to

the aboriginal inhabitants, but are very distinct species, belonging in

a large proportion of cases, as shown by Alph. de Candolle, to distinct

genera. In the Galapagos Archipelago, many even of the birds, though

so well adapted for flying from island to island, are distinct on each;

thus there are three closely-allied species of mocking-thrush, each

confined to its own island. Now let us suppose the mocking-thrush

of Chatham Island to be blown to Charles Island, which has its own

mocking-thrush: why should it succeed in establishing itself there?

We may safely infer that Charles Island is well stocked with its own

species, for annually more eggs are laid there than can possibly be

reared; and we may infer that the mocking-thrush peculiar to Charles

Island is at least as well fitted for its home as is the species

peculiar to Chatham Island. Sir C. Lyell and Mr. Wollaston have

communicated to me a remarkable fact bearing on this subject; namely,

that Madeira and the adjoining islet of Porto Santo possess many

distinct but representative land-shells, some of which live in

crevices of stone; and although large quantities of stone are annually

transported from Porto Santo to Madeira, yet this latter island has not

become colonised by the Porto Santo species: nevertheless both islands

have been colonised by some European land-shells, which no doubt had

some advantage over the indigenous species. From these considerations

I think we need not greatly marvel at the endemic and representative

species, which inhabit the several islands of the Galapagos Archipelago,

not having universally spread from island to island. In many other

instances, as in the several districts of the same continent,

pre-occupation has probably played an important part in checking the

commingling of species under the same conditions of life. Thus, the

south-east and south-west corners of Australia have nearly the same

physical conditions, and are united by continuous land, yet they are

inhabited by a vast number of distinct mammals, birds, and plants.

The principle which determines the general character of the fauna

and flora of oceanic islands, namely, that the inhabitants, when not

identically the same, yet are plainly related to the inhabitants of

that region whence colonists could most readily have been derived,--the

colonists having been subsequently modified and better fitted to their

new homes,--is of the widest application throughout nature. We see

this on every mountain, in every lake and marsh. For Alpine species,

excepting in so far as the same forms, chiefly of plants, have spread

widely throughout the world during the recent Glacial epoch, are related

to those of the surrounding lowlands;--thus we have in South America,

Alpine humming-birds, Alpine rodents, Alpine plants, etc., all of

strictly American forms, and it is obvious that a mountain, as it became

slowly upheaved, would naturally be colonised from the surrounding

lowlands. So it is with the inhabitants of lakes and marshes, excepting

in so far as great facility of transport has given the same general

forms to the whole world. We see this same principle in the blind

animals inhabiting the caves of America and of Europe. Other analogous

facts could be given. And it will, I believe, be universally found to

be true, that wherever in two regions, let them be ever so distant, many

closely allied or representative species occur, there will likewise be

found some identical species, showing, in accordance with the foregoing

view, that at some former period there has been intercommunication or

migration between the two regions. And wherever many closely-allied

species occur, there will be found many forms which some naturalists

rank as distinct species, and some as varieties; these doubtful forms

showing us the steps in the process of modification.

This relation between the power and extent of migration of a species,

either at the present time or at some former period under different

physical conditions, and the existence at remote points of the world of

other species allied to it, is shown in another and more general way.

Mr. Gould remarked to me long ago, that in those genera of birds which

range over the world, many of the species have very wide ranges. I

can hardly doubt that this rule is generally true, though it would be

difficult to prove it. Amongst mammals, we see it strikingly displayed

in Bats, and in a lesser degree in the Felidae and Canidae. We see it,

if we compare the distribution of butterflies and beetles. So it is with

most fresh-water productions, in which so many genera range over the

world, and many individual species have enormous ranges. It is not meant

that in world-ranging genera all the species have a wide range, or even

that they have on an AVERAGE a wide range; but only that some of the

species range very widely; for the facility with which widely-ranging

species vary and give rise to new forms will largely determine their

average range. For instance, two varieties of the same species inhabit

America and Europe, and the species thus has an immense range; but, if

the variation had been a little greater, the two varieties would have

been ranked as distinct species, and the common range would have been

greatly reduced. Still less is it meant, that a species which apparently

has the capacity of crossing barriers and ranging widely, as in the case

of certain powerfully-winged birds, will necessarily range widely; for

we should never forget that to range widely implies not only the power

of crossing barriers, but the more important power of being victorious

in distant lands in the struggle for life with foreign associates. But

on the view of all the species of a genus having descended from a single

parent, though now distributed to the most remote points of the world,

we ought to find, and I believe as a general rule we do find, that some

at least of the species range very widely; for it is necessary that the

unmodified parent should range widely, undergoing modification during

its diffusion, and should place itself under diverse conditions

favourable for the conversion of its offspring, firstly into new

varieties and ultimately into new species.

In considering the wide distribution of certain genera, we should bear

in mind that some are extremely ancient, and must have branched off from

a common parent at a remote epoch; so that in such cases there will

have been ample time for great climatal and geographical changes and for

accidents of transport; and consequently for the migration of some of

the species into all quarters of the world, where they may have become

slightly modified in relation to their new conditions. There is, also,

some reason to believe from geological evidence that organisms low in

the scale within each great class, generally change at a slower rate

than the higher forms; and consequently the lower forms will have had a

better chance of ranging widely and of still retaining the same specific

character. This fact, together with the seeds and eggs of many low forms

being very minute and better fitted for distant transportation, probably

accounts for a law which has long been observed, and which has lately

been admirably discussed by Alph. de Candolle in regard to plants,

namely, that the lower any group of organisms is, the more widely it is

apt to range.

The relations just discussed,--namely, low and slowly-changing

organisms ranging more widely than the high,--some of the species of

widely-ranging genera themselves ranging widely,--such facts, as alpine,

lacustrine, and marsh productions being related (with the exceptions

before specified) to those on the surrounding low lands and dry lands,

though these stations are so different--the very close relation of the

distinct species which inhabit the islets of the same archipelago,--and

especially the striking relation of the inhabitants of each whole

archipelago or island to those of the nearest mainland,--are, I think,

utterly inexplicable on the ordinary view of the independent creation

of each species, but are explicable on the view of colonisation from the

nearest and readiest source, together with the subsequent modification

and better adaptation of the colonists to their new homes.

SUMMARY OF LAST AND PRESENT CHAPTERS.

In these chapters I have endeavoured to show, that if we make due

allowance for our ignorance of the full effects of all the changes of

climate and of the level of the land, which have certainly occurred

within the recent period, and of other similar changes which may have

occurred within the same period; if we remember how profoundly ignorant

we are with respect to the many and curious means of occasional

transport,--a subject which has hardly ever been properly experimentised

on; if we bear in mind how often a species may have ranged continuously

over a wide area, and then have become extinct in the intermediate

tracts, I think the difficulties in believing that all the individuals

of the same species, wherever located, have descended from the same

parents, are not insuperable. And we are led to this conclusion, which

has been arrived at by many naturalists under the designation of single

centres of creation, by some general considerations, more especially

from the importance of barriers and from the analogical distribution of

sub-genera, genera, and families.

With respect to the distinct species of the same genus, which on my

theory must have spread from one parent-source; if we make the same

allowances as before for our ignorance, and remember that some forms of

life change most slowly, enormous periods of time being thus granted for

their migration, I do not think that the difficulties are insuperable;

though they often are in this case, and in that of the individuals of

the same species, extremely grave.

As exemplifying the effects of climatal changes on distribution, I have

attempted to show how important has been the influence of the modern

Glacial period, which I am fully convinced simultaneously affected

the whole world, or at least great meridional belts. As showing how

diversified are the means of occasional transport, I have discussed at

some little length the means of dispersal of fresh-water productions.

If the difficulties be not insuperable in admitting that in the long

course of time the individuals of the same species, and likewise of

allied species, have proceeded from some one source; then I think all

the grand leading facts of geographical distribution are explicable on

the theory of migration (generally of the more dominant forms of life),

together with subsequent modification and the multiplication of new

forms. We can thus understand the high importance of barriers, whether

of land or water, which separate our several zoological and botanical

provinces. We can thus understand the localisation of sub-genera,

genera, and families; and how it is that under different latitudes, for

instance in South America, the inhabitants of the plains and mountains,

of the forests, marshes, and deserts, are in so mysterious a manner

linked together by affinity, and are likewise linked to the extinct

beings which formerly inhabited the same continent. Bearing in mind

that the mutual relations of organism to organism are of the highest

importance, we can see why two areas having nearly the same physical

conditions should often be inhabited by very different forms of

life; for according to the length of time which has elapsed since

new inhabitants entered one region; according to the nature of the

communication which allowed certain forms and not others to enter,

either in greater or lesser numbers; according or not, as those which

entered happened to come in more or less direct competition with each

other and with the aborigines; and according as the immigrants were

capable of varying more or less rapidly, there would ensue in different

regions, independently of their physical conditions, infinitely

diversified conditions of life,--there would be an almost endless amount

of organic action and reaction,--and we should find, as we do find,

some groups of beings greatly, and some only slightly modified,--some

developed in great force, some existing in scanty numbers--in the

different great geographical provinces of the world.

On these same principles, we can understand, as I have endeavoured to

show, why oceanic islands should have few inhabitants, but of these a

great number should be endemic or peculiar; and why, in relation to the

means of migration, one group of beings, even within the same class,

should have all its species endemic, and another group should have all

its species common to other quarters of the world. We can see why whole

groups of organisms, as batrachians and terrestrial mammals, should be

absent from oceanic islands, whilst the most isolated islands possess

their own peculiar species of aerial mammals or bats. We can see why

there should be some relation between the presence of mammals, in a more

or less modified condition, and the depth of the sea between an island

and the mainland. We can clearly see why all the inhabitants of an

archipelago, though specifically distinct on the several islets, should

be closely related to each other, and likewise be related, but less

closely, to those of the nearest continent or other source whence

immigrants were probably derived. We can see why in two areas, however

distant from each other, there should be a correlation, in the presence

of identical species, of varieties, of doubtful species, and of distinct

but representative species.

As the late Edward Forbes often insisted, there is a striking

parallelism in the laws of life throughout time and space: the laws

governing the succession of forms in past times being nearly the same

with those governing at the present time the differences in different

areas. We see this in many facts. The endurance of each species and

group of species is continuous in time; for the exceptions to the rule

are so few, that they may fairly be attributed to our not having as

yet discovered in an intermediate deposit the forms which are therein

absent, but which occur above and below: so in space, it certainly is

the general rule that the area inhabited by a single species, or by a

group of species, is continuous; and the exceptions, which are not rare,

may, as I have attempted to show, be accounted for by migration at

some former period under different conditions or by occasional means of

transport, and by the species having become extinct in the intermediate

tracts. Both in time and space, species and groups of species have their

points of maximum development. Groups of species, belonging either to a

certain period of time, or to a certain area, are often characterised by

trifling characters in common, as of sculpture or colour. In looking

to the long succession of ages, as in now looking to distant provinces

throughout the world, we find that some organisms differ little, whilst

others belonging to a different class, or to a different order, or even

only to a different family of the same order, differ greatly. In both

time and space the lower members of each class generally change less

than the higher; but there are in both cases marked exceptions to the

rule. On my theory these several relations throughout time and space

are intelligible; for whether we look to the forms of life which have

changed during successive ages within the same quarter of the world, or

to those which have changed after having migrated into distant quarters,

in both cases the forms within each class have been connected by the

same bond of ordinary generation; and the more nearly any two forms are

related in blood, the nearer they will generally stand to each other in

time and space; in both cases the laws of variation have been the same,

and modifications have been accumulated by the same power of natural

selection.

13. MUTUAL AFFINITIES OF ORGANIC BEINGS: MORPHOLOGY:

EMBRYOLOGY: RUDIMENTARY ORGANS.

CLASSIFICATION, groups subordinate to groups. Natural system. Rules and

difficulties in classification, explained on the theory of descent

with modification. Classification of varieties. Descent always used in

classification. Analogical or adaptive characters. Affinities, general,

complex and radiating. Extinction separates and defines groups.

MORPHOLOGY, between members of the same class, between parts of the same

individual. EMBRYOLOGY, laws of, explained by variations not supervening

at an early age, and being inherited at a corresponding age. RUDIMENTARY

ORGANS; their origin explained. Summary.

From the first dawn of life, all organic beings are found to resemble

each other in descending degrees, so that they can be classed in groups

under groups. This classification is evidently not arbitrary like the

grouping of the stars in constellations. The existence of groups would

have been of simple signification, if one group had been exclusively

fitted to inhabit the land, and another the water; one to feed on flesh,

another on vegetable matter, and so on; but the case is widely different

in nature; for it is notorious how commonly members of even the same

subgroup have different habits. In our second and fourth chapters, on

Variation and on Natural Selection, I have attempted to show that it is

the widely ranging, the much diffused and common, that is the dominant

species belonging to the larger genera, which vary most. The varieties,

or incipient species, thus produced ultimately become converted, as I

believe, into new and distinct species; and these, on the principle

of inheritance, tend to produce other new and dominant species.

Consequently the groups which are now large, and which generally include

many dominant species, tend to go on increasing indefinitely in size.

I further attempted to show that from the varying descendants of each

species trying to occupy as many and as different places as possible in

the economy of nature, there is a constant tendency in their characters

to diverge. This conclusion was supported by looking at the great

diversity of the forms of life which, in any small area, come into the

closest competition, and by looking to certain facts in naturalisation.

I attempted also to show that there is a constant tendency in the forms

which are increasing in number and diverging in character, to supplant

and exterminate the less divergent, the less improved, and preceding

forms. I request the reader to turn to the diagram illustrating the

action, as formerly explained, of these several principles; and he

will see that the inevitable result is that the modified descendants

proceeding from one progenitor become broken up into groups subordinate

to groups. In the diagram each letter on the uppermost line may

represent a genus including several species; and all the genera on this

line form together one class, for all have descended from one ancient

but unseen parent, and, consequently, have inherited something in

common. But the three genera on the left hand have, on this same

principle, much in common, and form a sub-family, distinct from that

including the next two genera on the right hand, which diverged from a

common parent at the fifth stage of descent. These five genera have also

much, though less, in common; and they form a family distinct from

that including the three genera still further to the right hand, which

diverged at a still earlier period. And all these genera, descended from

(A), form an order distinct from the genera descended from (I). So that

we here have many species descended from a single progenitor grouped

into genera; and the genera are included in, or subordinate to,

sub-families, families, and orders, all united into one class. Thus, the

grand fact in natural history of the subordination of group under group,

which, from its familiarity, does not always sufficiently strike us, is

in my judgment fully explained.

Naturalists try to arrange the species, genera, and families in each

class, on what is called the Natural System. But what is meant by

this system? Some authors look at it merely as a scheme for arranging

together those living objects which are most alike, and for separating

those which are most unlike; or as an artificial means for enunciating,

as briefly as possible, general propositions,--that is, by one sentence

to give the characters common, for instance, to all mammals, by another

those common to all carnivora, by another those common to the dog-genus,

and then by adding a single sentence, a full description is given

of each kind of dog. The ingenuity and utility of this system are

indisputable. But many naturalists think that something more is meant

by the Natural System; they believe that it reveals the plan of the

Creator; but unless it be specified whether order in time or space,

or what else is meant by the plan of the Creator, it seems to me that

nothing is thus added to our knowledge. Such expressions as that

famous one of Linnaeus, and which we often meet with in a more or less

concealed form, that the characters do not make the genus, but that

the genus gives the characters, seem to imply that something more is

included in our classification, than mere resemblance. I believe that

something more is included; and that propinquity of descent,--the only

known cause of the similarity of organic beings,--is the bond, hidden as

it is by various degrees of modification, which is partially revealed to

us by our classifications.

Let us now consider the rules followed in classification, and the

difficulties which are encountered on the view that classification

either gives some unknown plan of creation, or is simply a scheme for

enunciating general propositions and of placing together the forms most

like each other. It might have been thought (and was in ancient times

thought) that those parts of the structure which determined the habits

of life, and the general place of each being in the economy of nature,

would be of very high importance in classification. Nothing can be more

false. No one regards the external similarity of a mouse to a shrew, of

a dugong to a whale, of a whale to a fish, as of any importance. These

resemblances, though so intimately connected with the whole life of the

being, are ranked as merely "adaptive or analogical characters;" but to

the consideration of these resemblances we shall have to recur. It

may even be given as a general rule, that the less any part of the

organisation is concerned with special habits, the more important it

becomes for classification. As an instance: Owen, in speaking of the

dugong, says, "The generative organs being those which are most remotely

related to the habits and food of an animal, I have always regarded as

affording very clear indications of its true affinities. We are least

likely in the modifications of these organs to mistake a merely adaptive

for an essential character." So with plants, how remarkable it is that

the organs of vegetation, on which their whole life depends, are of

little signification, excepting in the first main divisions; whereas the

organs of reproduction, with their product the seed, are of paramount

importance!

We must not, therefore, in classifying, trust to resemblances in parts

of the organisation, however important they may be for the welfare of

the being in relation to the outer world. Perhaps from this cause it has

partly arisen, that almost all naturalists lay the greatest stress on

resemblances in organs of high vital or physiological importance. No

doubt this view of the classificatory importance of organs which

are important is generally, but by no means always, true. But their

importance for classification, I believe, depends on their greater

constancy throughout large groups of species; and this constancy depends

on such organs having generally been subjected to less change in the

adaptation of the species to their conditions of life. That the

mere physiological importance of an organ does not determine its

classificatory value, is almost shown by the one fact, that in allied

groups, in which the same organ, as we have every reason to suppose, has

nearly the same physiological value, its classificatory value is widely

different. No naturalist can have worked at any group without being

struck with this fact; and it has been most fully acknowledged in the

writings of almost every author. It will suffice to quote the highest

authority, Robert Brown, who in speaking of certain organs in the

Proteaceae, says their generic importance, "like that of all their

parts, not only in this but, as I apprehend, in every natural family,

is very unequal, and in some cases seems to be entirely lost." Again in

another work he says, the genera of the Connaraceae "differ in having

one or more ovaria, in the existence or absence of albumen, in the

imbricate or valvular aestivation. Any one of these characters singly

is frequently of more than generic importance, though here even when

all taken together they appear insufficient to separate Cnestis from

Connarus." To give an example amongst insects, in one great division

of the Hymenoptera, the antennae, as Westwood has remarked, are most

constant in structure; in another division they differ much, and the

differences are of quite subordinate value in classification; yet no one

probably will say that the antennae in these two divisions of the same

order are of unequal physiological importance. Any number of instances

could be given of the varying importance for classification of the same

important organ within the same group of beings.

Again, no one will say that rudimentary or atrophied organs are of high

physiological or vital importance; yet, undoubtedly, organs in this

condition are often of high value in classification. No one will dispute

that the rudimentary teeth in the upper jaws of young ruminants,

and certain rudimentary bones of the leg, are highly serviceable in

exhibiting the close affinity between Ruminants and Pachyderms. Robert

Brown has strongly insisted on the fact that the rudimentary florets are

of the highest importance in the classification of the Grasses.

Numerous instances could be given of characters derived from parts which

must be considered of very trifling physiological importance, but which

are universally admitted as highly serviceable in the definition of

whole groups. For instance, whether or not there is an open passage from

the nostrils to the mouth, the only character, according to Owen, which

absolutely distinguishes fishes and reptiles--the inflection of the

angle of the jaws in Marsupials--the manner in which the wings of

insects are folded--mere colour in certain Algae--mere pubescence on

parts of the flower in grasses--the nature of the dermal covering, as

hair or feathers, in the Vertebrata. If the Ornithorhynchus had been

covered with feathers instead of hair, this external and trifling

character would, I think, have been considered by naturalists as

important an aid in determining the degree of affinity of this strange

creature to birds and reptiles, as an approach in structure in any one

internal and important organ.

The importance, for classification, of trifling characters, mainly

depends on their being correlated with several other characters of more

or less importance. The value indeed of an aggregate of characters is

very evident in natural history. Hence, as has often been remarked, a

species may depart from its allies in several characters, both of high

physiological importance and of almost universal prevalence, and yet

leave us in no doubt where it should be ranked. Hence, also, it has been

found, that a classification founded on any single character,

however important that may be, has always failed; for no part of the

organisation is universally constant. The importance of an aggregate of

characters, even when none are important, alone explains, I think, that

saying of Linnaeus, that the characters do not give the genus, but

the genus gives the characters; for this saying seems founded on an

appreciation of many trifling points of resemblance, too slight to be

defined. Certain plants, belonging to the Malpighiaceae, bear perfect

and degraded flowers; in the latter, as A. de Jussieu has remarked, "the

greater number of the characters proper to the species, to the genus,

to the family, to the class, disappear, and thus laugh at our

classification." But when Aspicarpa produced in France, during several

years, only degraded flowers, departing so wonderfully in a number

of the most important points of structure from the proper type of the

order, yet M. Richard sagaciously saw, as Jussieu observes, that this

genus should still be retained amongst the Malpighiaceae. This case

seems to me well to illustrate the spirit with which our classifications

are sometimes necessarily founded.

Practically when naturalists are at work, they do not trouble themselves

about the physiological value of the characters which they use in

defining a group, or in allocating any particular species. If they find

a character nearly uniform, and common to a great number of forms, and

not common to others, they use it as one of high value; if common to

some lesser number, they use it as of subordinate value. This principle

has been broadly confessed by some naturalists to be the true one; and

by none more clearly than by that excellent botanist, Aug. St. Hilaire.

If certain characters are always found correlated with others, though

no apparent bond of connexion can be discovered between them, especial

value is set on them. As in most groups of animals, important organs,

such as those for propelling the blood, or for aerating it, or those for

propagating the race, are found nearly uniform, they are considered as

highly serviceable in classification; but in some groups of animals all

these, the most important vital organs, are found to offer characters of

quite subordinate value.

We can see why characters derived from the embryo should be of equal

importance with those derived from the adult, for our classifications of

course include all ages of each species. But it is by no means obvious,

on the ordinary view, why the structure of the embryo should be more

important for this purpose than that of the adult, which alone plays its

full part in the economy of nature. Yet it has been strongly urged

by those great naturalists, Milne Edwards and Agassiz, that embryonic

characters are the most important of any in the classification of

animals; and this doctrine has very generally been admitted as true.

The same fact holds good with flowering plants, of which the two main

divisions have been founded on characters derived from the embryo,--on

the number and position of the embryonic leaves or cotyledons, and on

the mode of development of the plumule and radicle. In our discussion

on embryology, we shall see why such characters are so valuable, on the

view of classification tacitly including the idea of descent.

Our classifications are often plainly influenced by chains of

affinities. Nothing can be easier than to define a number of characters

common to all birds; but in the case of crustaceans, such definition has

hitherto been found impossible. There are crustaceans at the opposite

ends of the series, which have hardly a character in common; yet the

species at both ends, from being plainly allied to others, and these to

others, and so onwards, can be recognised as unequivocally belonging to

this, and to no other class of the Articulata.

Geographical distribution has often been used, though perhaps not quite

logically, in classification, more especially in very large groups of

closely allied forms. Temminck insists on the utility or even necessity

of this practice in certain groups of birds; and it has been followed by

several entomologists and botanists.

Finally, with respect to the comparative value of the various groups of

species, such as orders, sub-orders, families, sub-families, and genera,

they seem to be, at least at present, almost arbitrary. Several of the

best botanists, such as Mr. Bentham and others, have strongly insisted

on their arbitrary value. Instances could be given amongst plants and

insects, of a group of forms, first ranked by practised naturalists as

only a genus, and then raised to the rank of a sub-family or family; and

this has been done, not because further research has detected important

structural differences, at first overlooked, but because numerous

allied species, with slightly different grades of difference, have been

subsequently discovered.

All the foregoing rules and aids and difficulties in classification

are explained, if I do not greatly deceive myself, on the view that

the natural system is founded on descent with modification; that the

characters which naturalists consider as showing true affinity between

any two or more species, are those which have been inherited from a

common parent, and, in so far, all true classification is genealogical;

that community of descent is the hidden bond which naturalists have been

unconsciously seeking, and not some unknown plan of creation, or the

enunciation of general propositions, and the mere putting together and

separating objects more or less alike.

But I must explain my meaning more fully. I believe that the ARRANGEMENT

of the groups within each class, in due subordination and relation to

the other groups, must be strictly genealogical in order to be natural;

but that the AMOUNT of difference in the several branches or groups,

though allied in the same degree in blood to their common progenitor,

may differ greatly, being due to the different degrees of modification

which they have undergone; and this is expressed by the forms being

ranked under different genera, families, sections, or orders. The reader

will best understand what is meant, if he will take the trouble of

referring to the diagram in the fourth chapter. We will suppose the

letters A to L to represent allied genera, which lived during the

Silurian epoch, and these have descended from a species which existed at

an unknown anterior period. Species of three of these genera (A, F, and

I) have transmitted modified descendants to the present day, represented

by the fifteen genera (a14 to z14) on the uppermost horizontal line. Now

all these modified descendants from a single species, are represented as

related in blood or descent to the same degree; they may metaphorically

be called cousins to the same millionth degree; yet they differ widely

and in different degrees from each other. The forms descended from A,

now broken up into two or three families, constitute a distinct order

from those descended from I, also broken up into two families. Nor can

the existing species, descended from A, be ranked in the same genus with

the parent A; or those from I, with the parent I. But the existing genus

F14 may be supposed to have been but slightly modified; and it will

then rank with the parent-genus F; just as some few still living organic

beings belong to Silurian genera. So that the amount or value of the

differences between organic beings all related to each other in the same

degree in blood, has come to be widely different. Nevertheless their

genealogical ARRANGEMENT remains strictly true, not only at the present

time, but at each successive period of descent. All the modified

descendants from A will have inherited something in common from their

common parent, as will all the descendants from I; so will it be with

each subordinate branch of descendants, at each successive period. If,

however, we choose to suppose that any of the descendants of A or of

I have been so much modified as to have more or less completely lost

traces of their parentage, in this case, their places in a natural

classification will have been more or less completely lost,--as

sometimes seems to have occurred with existing organisms. All the

descendants of the genus F, along its whole line of descent, are

supposed to have been but little modified, and they yet form a single

genus. But this genus, though much isolated, will still occupy its

proper intermediate position; for F originally was intermediate in

character between A and I, and the several genera descended from these

two genera will have inherited to a certain extent their characters.

This natural arrangement is shown, as far as is possible on paper, in

the diagram, but in much too simple a manner. If a branching diagram had

not been used, and only the names of the groups had been written in a

linear series, it would have been still less possible to have given a

natural arrangement; and it is notoriously not possible to represent in

a series, on a flat surface, the affinities which we discover in nature

amongst the beings of the same group. Thus, on the view which I hold,

the natural system is genealogical in its arrangement, like a pedigree;

but the degrees of modification which the different groups have

undergone, have to be expressed by ranking them under different

so-called genera, sub-families, families, sections, orders, and classes.

It may be worth while to illustrate this view of classification, by

taking the case of languages. If we possessed a perfect pedigree of

mankind, a genealogical arrangement of the races of man would afford the

best classification of the various languages now spoken throughout the

world; and if all extinct languages, and all intermediate and slowly

changing dialects, had to be included, such an arrangement would, I

think, be the only possible one. Yet it might be that some very ancient

language had altered little, and had given rise to few new languages,

whilst others (owing to the spreading and subsequent isolation and

states of civilisation of the several races, descended from a common

race) had altered much, and had given rise to many new languages and

dialects. The various degrees of difference in the languages from the

same stock, would have to be expressed by groups subordinate to

groups; but the proper or even only possible arrangement would still be

genealogical; and this would be strictly natural, as it would connect

together all languages, extinct and modern, by the closest affinities,

and would give the filiation and origin of each tongue.

In confirmation of this view, let us glance at the classification

of varieties, which are believed or known to have descended from one

species. These are grouped under species, with sub-varieties under

varieties; and with our domestic productions, several other grades of

difference are requisite, as we have seen with pigeons. The origin

of the existence of groups subordinate to groups, is the same with

varieties as with species, namely, closeness of descent with various

degrees of modification. Nearly the same rules are followed in

classifying varieties, as with species. Authors have insisted on the

necessity of classing varieties on a natural instead of an artificial

system; we are cautioned, for instance, not to class two varieties of

the pine-apple together, merely because their fruit, though the most

important part, happens to be nearly identical; no one puts the swedish

and common turnips together, though the esculent and thickened stems

are so similar. Whatever part is found to be most constant, is used in

classing varieties: thus the great agriculturist Marshall says the horns

are very useful for this purpose with cattle, because they are less

variable than the shape or colour of the body, etc.; whereas with sheep

the horns are much less serviceable, because less constant. In classing

varieties, I apprehend if we had a real pedigree, a genealogical

classification would be universally preferred; and it has been attempted

by some authors. For we might feel sure, whether there had been more

or less modification, the principle of inheritance would keep the forms

together which were allied in the greatest number of points. In tumbler

pigeons, though some sub-varieties differ from the others in the

important character of having a longer beak, yet all are kept together

from having the common habit of tumbling; but the short-faced breed has

nearly or quite lost this habit; nevertheless, without any reasoning

or thinking on the subject, these tumblers are kept in the same group,

because allied in blood and alike in some other respects. If it could be

proved that the Hottentot had descended from the Negro, I think he would

be classed under the Negro group, however much he might differ in colour

and other important characters from negroes.

With species in a state of nature, every naturalist has in fact brought

descent into his classification; for he includes in his lowest grade,

or that of a species, the two sexes; and how enormously these sometimes

differ in the most important characters, is known to every naturalist:

scarcely a single fact can be predicated in common of the males and

hermaphrodites of certain cirripedes, when adult, and yet no one dreams

of separating them. The naturalist includes as one species the several

larval stages of the same individual, however much they may differ from

each other and from the adult; as he likewise includes the so-called

alternate generations of Steenstrup, which can only in a technical sense

be considered as the same individual. He includes monsters; he includes

varieties, not solely because they closely resemble the parent-form, but

because they are descended from it. He who believes that the cowslip

is descended from the primrose, or conversely, ranks them together as

a single species, and gives a single definition. As soon as three

Orchidean forms (Monochanthus, Myanthus, and Catasetum), which had

previously been ranked as three distinct genera, were known to be

sometimes produced on the same spike, they were immediately included as

a single species. But it may be asked, what ought we to do, if it could

be proved that one species of kangaroo had been produced, by a long

course of modification, from a bear? Ought we to rank this one

species with bears, and what should we do with the other species?

The supposition is of course preposterous; and I might answer by the

argumentum ad hominem, and ask what should be done if a perfect kangaroo

were seen to come out of the womb of a bear? According to all analogy,

it would be ranked with bears; but then assuredly all the other species

of the kangaroo family would have to be classed under the bear genus.

The whole case is preposterous; for where there has been close descent

in common, there will certainly be close resemblance or affinity.

As descent has universally been used in classing together the

individuals of the same species, though the males and females and larvae

are sometimes extremely different; and as it has been used in classing

varieties which have undergone a certain, and sometimes a considerable

amount of modification, may not this same element of descent have been

unconsciously used in grouping species under genera, and genera under

higher groups, though in these cases the modification has been greater

in degree, and has taken a longer time to complete? I believe it has

thus been unconsciously used; and only thus can I understand the several

rules and guides which have been followed by our best systematists. We

have no written pedigrees; we have to make out community of descent by

resemblances of any kind. Therefore we choose those characters which,

as far as we can judge, are the least likely to have been modified

in relation to the conditions of life to which each species has been

recently exposed. Rudimentary structures on this view are as good as, or

even sometimes better than, other parts of the organisation. We care not

how trifling a character may be--let it be the mere inflection of

the angle of the jaw, the manner in which an insect's wing is folded,

whether the skin be covered by hair or feathers--if it prevail

throughout many and different species, especially those having very

different habits of life, it assumes high value; for we can account for

its presence in so many forms with such different habits, only by its

inheritance from a common parent. We may err in this respect in regard

to single points of structure, but when several characters, let them

be ever so trifling, occur together throughout a large group of beings

having different habits, we may feel almost sure, on the theory of

descent, that these characters have been inherited from a common

ancestor. And we know that such correlated or aggregated characters have

especial value in classification.

We can understand why a species or a group of species may depart, in

several of its most important characteristics, from its allies, and yet

be safely classed with them. This may be safely done, and is often

done, as long as a sufficient number of characters, let them be ever so

unimportant, betrays the hidden bond of community of descent. Let two

forms have not a single character in common, yet if these extreme forms

are connected together by a chain of intermediate groups, we may at

once infer their community of descent, and we put them all into the same

class. As we find organs of high physiological importance--those

which serve to preserve life under the most diverse conditions of

existence--are generally the most constant, we attach especial value to

them; but if these same organs, in another group or section of a

group, are found to differ much, we at once value them less in

our classification. We shall hereafter, I think, clearly see why

embryological characters are of such high classificatory importance.

Geographical distribution may sometimes be brought usefully into play in

classing large and widely-distributed genera, because all the species of

the same genus, inhabiting any distinct and isolated region, have in all

probability descended from the same parents.

We can understand, on these views, the very important distinction

between real affinities and analogical or adaptive resemblances.

Lamarck first called attention to this distinction, and he has been ably

followed by Macleay and others. The resemblance, in the shape of the

body and in the fin-like anterior limbs, between the dugong, which is a

pachydermatous animal, and the whale, and between both these mammals and

fishes, is analogical. Amongst insects there are innumerable instances:

thus Linnaeus, misled by external appearances, actually classed an

homopterous insect as a moth. We see something of the same kind even

in our domestic varieties, as in the thickened stems of the common and

swedish turnip. The resemblance of the greyhound and racehorse is hardly

more fanciful than the analogies which have been drawn by some authors

between very distinct animals. On my view of characters being of real

importance for classification, only in so far as they reveal descent, we

can clearly understand why analogical or adaptive character, although of

the utmost importance to the welfare of the being, are almost valueless

to the systematist. For animals, belonging to two most distinct lines

of descent, may readily become adapted to similar conditions, and thus

assume a close external resemblance; but such resemblances will not

reveal--will rather tend to conceal their blood-relationship to their

proper lines of descent. We can also understand the apparent paradox,

that the very same characters are analogical when one class or order is

compared with another, but give true affinities when the members of the

same class or order are compared one with another: thus the shape of

the body and fin-like limbs are only analogical when whales are compared

with fishes, being adaptations in both classes for swimming through the

water; but the shape of the body and fin-like limbs serve as characters

exhibiting true affinity between the several members of the whale

family; for these cetaceans agree in so many characters, great and

small, that we cannot doubt that they have inherited their general shape

of body and structure of limbs from a common ancestor. So it is with

fishes.

As members of distinct classes have often been adapted by successive

slight modifications to live under nearly similar circumstances,--to

inhabit for instance the three elements of land, air, and water,--we can

perhaps understand how it is that a numerical parallelism has sometimes

been observed between the sub-groups in distinct classes. A naturalist,

struck by a parallelism of this nature in any one class, by arbitrarily

raising or sinking the value of the groups in other classes (and all our

experience shows that this valuation has hitherto been arbitrary), could

easily extend the parallelism over a wide range; and thus the septenary,

quinary, quaternary, and ternary classifications have probably arisen.

As the modified descendants of dominant species, belonging to the larger

genera, tend to inherit the advantages, which made the groups to which

they belong large and their parents dominant, they are almost sure to

spread widely, and to seize on more and more places in the economy

of nature. The larger and more dominant groups thus tend to go on

increasing in size; and they consequently supplant many smaller and

feebler groups. Thus we can account for the fact that all organisms,

recent and extinct, are included under a few great orders, under still

fewer classes, and all in one great natural system. As showing how

few the higher groups are in number, and how widely spread they are

throughout the world, the fact is striking, that the discovery of

Australia has not added a single insect belonging to a new order; and

that in the vegetable kingdom, as I learn from Dr. Hooker, it has added

only two or three orders of small size.

In the chapter on geological succession I attempted to show, on the

principle of each group having generally diverged much in character

during the long-continued process of modification, how it is that the

more ancient forms of life often present characters in some slight

degree intermediate between existing groups. A few old and intermediate

parent-forms having occasionally transmitted to the present day

descendants but little modified, will give to us our so-called osculant

or aberrant groups. The more aberrant any form is, the greater must be

the number of connecting forms which on my theory have been exterminated

and utterly lost. And we have some evidence of aberrant forms having

suffered severely from extinction, for they are generally represented by

extremely few species; and such species as do occur are generally very

distinct from each other, which again implies extinction. The genera

Ornithorhynchus and Lepidosiren, for example, would not have been less

aberrant had each been represented by a dozen species instead of by

a single one; but such richness in species, as I find after some

investigation, does not commonly fall to the lot of aberrant genera. We

can, I think, account for this fact only by looking at aberrant forms

as failing groups conquered by more successful competitors, with a

few members preserved by some unusual coincidence of favourable

circumstances.

Mr. Waterhouse has remarked that, when a member belonging to one group

of animals exhibits an affinity to a quite distinct group, this affinity

in most cases is general and not special: thus, according to Mr.

Waterhouse, of all Rodents, the bizcacha is most nearly related to

Marsupials; but in the points in which it approaches this order, its

relations are general, and not to any one marsupial species more than

to another. As the points of affinity of the bizcacha to Marsupials are

believed to be real and not merely adaptive, they are due on my theory

to inheritance in common. Therefore we must suppose either that all

Rodents, including the bizcacha, branched off from some very ancient

Marsupial, which will have had a character in some degree intermediate

with respect to all existing Marsupials; or that both Rodents and

Marsupials branched off from a common progenitor, and that both groups

have since undergone much modification in divergent directions.

On either view we may suppose that the bizcacha has retained, by

inheritance, more of the character of its ancient progenitor than have

other Rodents; and therefore it will not be specially related to any one

existing Marsupial, but indirectly to all or nearly all Marsupials, from

having partially retained the character of their common progenitor, or

of an early member of the group. On the other hand, of all Marsupials,

as Mr. Waterhouse has remarked, the phascolomys resembles most nearly,

not any one species, but the general order of Rodents. In this case,

however, it may be strongly suspected that the resemblance is only

analogical, owing to the phascolomys having become adapted to habits

like those of a Rodent. The elder De Candolle has made nearly similar

observations on the general nature of the affinities of distinct orders

of plants.

On the principle of the multiplication and gradual divergence in

character of the species descended from a common parent, together with

their retention by inheritance of some characters in common, we can

understand the excessively complex and radiating affinities by which all

the members of the same family or higher group are connected together.

For the common parent of a whole family of species, now broken up by

extinction into distinct groups and sub-groups, will have transmitted

some of its characters, modified in various ways and degrees, to all;

and the several species will consequently be related to each other by

circuitous lines of affinity of various lengths (as may be seen in the

diagram so often referred to), mounting up through many predecessors.

As it is difficult to show the blood-relationship between the

numerous kindred of any ancient and noble family, even by the aid of a

genealogical tree, and almost impossible to do this without this aid,

we can understand the extraordinary difficulty which naturalists have

experienced in describing, without the aid of a diagram, the various

affinities which they perceive between the many living and extinct

members of the same great natural class.

Extinction, as we have seen in the fourth chapter, has played an

important part in defining and widening the intervals between the

several groups in each class. We may thus account even for the

distinctness of whole classes from each other--for instance, of birds

from all other vertebrate animals--by the belief that many ancient forms

of life have been utterly lost, through which the early progenitors of

birds were formerly connected with the early progenitors of the other

vertebrate classes. There has been less entire extinction of the forms

of life which once connected fishes with batrachians. There has been

still less in some other classes, as in that of the Crustacea, for here

the most wonderfully diverse forms are still tied together by a long,

but broken, chain of affinities. Extinction has only separated groups:

it has by no means made them; for if every form which has ever lived

on this earth were suddenly to reappear, though it would be

quite impossible to give definitions by which each group could be

distinguished from other groups, as all would blend together by steps

as fine as those between the finest existing varieties, nevertheless

a natural classification, or at least a natural arrangement, would be

possible. We shall see this by turning to the diagram: the letters, A

to L, may represent eleven Silurian genera, some of which have produced

large groups of modified descendants. Every intermediate link between

these eleven genera and their primordial parent, and every intermediate

link in each branch and sub-branch of their descendants, may be supposed

to be still alive; and the links to be as fine as those between the

finest varieties. In this case it would be quite impossible to give any

definition by which the several members of the several groups could be

distinguished from their more immediate parents; or these parents from

their ancient and unknown progenitor. Yet the natural arrangement in the

diagram would still hold good; and, on the principle of inheritance, all

the forms descended from A, or from I, would have something in common.

In a tree we can specify this or that branch, though at the actual fork

the two unite and blend together. We could not, as I have said, define

the several groups; but we could pick out types, or forms, representing

most of the characters of each group, whether large or small, and thus

give a general idea of the value of the differences between them. This

is what we should be driven to, if we were ever to succeed in collecting

all the forms in any class which have lived throughout all time

and space. We shall certainly never succeed in making so perfect a

collection: nevertheless, in certain classes, we are tending in this

direction; and Milne Edwards has lately insisted, in an able paper, on

the high importance of looking to types, whether or not we can separate

and define the groups to which such types belong.

Finally, we have seen that natural selection, which results from the

struggle for existence, and which almost inevitably induces extinction

and divergence of character in the many descendants from one dominant

parent-species, explains that great and universal feature in the

affinities of all organic beings, namely, their subordination in group

under group. We use the element of descent in classing the individuals

of both sexes and of all ages, although having few characters in common,

under one species; we use descent in classing acknowledged varieties,

however different they may be from their parent; and I believe this

element of descent is the hidden bond of connexion which naturalists

have sought under the term of the Natural System. On this idea of the

natural system being, in so far as it has been perfected, genealogical

in its arrangement, with the grades of difference between the

descendants from a common parent, expressed by the terms genera,

families, orders, etc., we can understand the rules which we are

compelled to follow in our classification. We can understand why we

value certain resemblances far more than others; why we are permitted to

use rudimentary and useless organs, or others of trifling physiological

importance; why, in comparing one group with a distinct group, we

summarily reject analogical or adaptive characters, and yet use these

same characters within the limits of the same group. We can clearly see

how it is that all living and extinct forms can be grouped together

in one great system; and how the several members of each class

are connected together by the most complex and radiating lines of

affinities. We shall never, probably, disentangle the inextricable web

of affinities between the members of any one class; but when we have

a distinct object in view, and do not look to some unknown plan of

creation, we may hope to make sure but slow progress.

MORPHOLOGY.

We have seen that the members of the same class, independently of

their habits of life, resemble each other in the general plan of their

organisation. This resemblance is often expressed by the term "unity of

type;" or by saying that the several parts and organs in the different

species of the class are homologous. The whole subject is included under

the general name of Morphology. This is the most interesting department

of natural history, and may be said to be its very soul. What can be

more curious than that the hand of a man, formed for grasping, that of a

mole for digging, the leg of the horse, the paddle of the porpoise, and

the wing of the bat, should all be constructed on the same pattern, and

should include the same bones, in the same relative positions? Geoffroy

St. Hilaire has insisted strongly on the high importance of relative

connexion in homologous organs: the parts may change to almost any

extent in form and size, and yet they always remain connected together

in the same order. We never find, for instance, the bones of the arm and

forearm, or of the thigh and leg, transposed. Hence the same names can

be given to the homologous bones in widely different animals. We see the

same great law in the construction of the mouths of insects: what can

be more different than the immensely long spiral proboscis of a

sphinx-moth, the curious folded one of a bee or bug, and the great jaws

of a beetle?--yet all these organs, serving for such different purposes,

are formed by infinitely numerous modifications of an upper lip,

mandibles, and two pairs of maxillae. Analogous laws govern the

construction of the mouths and limbs of crustaceans. So it is with the

flowers of plants.

Nothing can be more hopeless than to attempt to explain this similarity

of pattern in members of the same class, by utility or by the doctrine

of final causes. The hopelessness of the attempt has been expressly

admitted by Owen in his most interesting work on the 'Nature of Limbs.'

On the ordinary view of the independent creation of each being, we can

only say that so it is;--that it has so pleased the Creator to construct

each animal and plant.

The explanation is manifest on the theory of the natural selection of

successive slight modifications,--each modification being profitable

in some way to the modified form, but often affecting by correlation of

growth other parts of the organisation. In changes of this nature, there

will be little or no tendency to modify the original pattern, or to

transpose parts. The bones of a limb might be shortened and widened to

any extent, and become gradually enveloped in thick membrane, so as to

serve as a fin; or a webbed foot might have all its bones, or certain

bones, lengthened to any extent, and the membrane connecting them

increased to any extent, so as to serve as a wing: yet in all this great

amount of modification there will be no tendency to alter the framework

of bones or the relative connexion of the several parts. If we suppose

that the ancient progenitor, the archetype as it may be called, of all

mammals, had its limbs constructed on the existing general pattern,

for whatever purpose they served, we can at once perceive the plain

signification of the homologous construction of the limbs throughout the

whole class. So with the mouths of insects, we have only to suppose that

their common progenitor had an upper lip, mandibles, and two pair

of maxillae, these parts being perhaps very simple in form; and then

natural selection will account for the infinite diversity in structure

and function of the mouths of insects. Nevertheless, it is conceivable

that the general pattern of an organ might become so much obscured as to

be finally lost, by the atrophy and ultimately by the complete abortion

of certain parts, by the soldering together of other parts, and by the

doubling or multiplication of others,--variations which we know to be

within the limits of possibility. In the paddles of the extinct gigantic

sea-lizards, and in the mouths of certain suctorial crustaceans, the

general pattern seems to have been thus to a certain extent obscured.

There is another and equally curious branch of the present subject;

namely, the comparison not of the same part in different members of a

class, but of the different parts or organs in the same individual.

Most physiologists believe that the bones of the skull are homologous

with--that is correspond in number and in relative connexion with--the

elemental parts of a certain number of vertebrae. The anterior and

posterior limbs in each member of the vertebrate and articulate classes

are plainly homologous. We see the same law in comparing the wonderfully

complex jaws and legs in crustaceans. It is familiar to almost every

one, that in a flower the relative position of the sepals, petals,

stamens, and pistils, as well as their intimate structure, are

intelligible on the view that they consist of metamorphosed leaves,

arranged in a spire. In monstrous plants, we often get direct evidence

of the possibility of one organ being transformed into another; and we

can actually see in embryonic crustaceans and in many other animals, and

in flowers, that organs, which when mature become extremely different,

are at an early stage of growth exactly alike.

How inexplicable are these facts on the ordinary view of creation! Why

should the brain be enclosed in a box composed of such numerous and such

extraordinarily shaped pieces of bone? As Owen has remarked, the

benefit derived from the yielding of the separate pieces in the act of

parturition of mammals, will by no means explain the same construction

in the skulls of birds. Why should similar bones have been created in

the formation of the wing and leg of a bat, used as they are for such

totally different purposes? Why should one crustacean, which has an

extremely complex mouth formed of many parts, consequently always have

fewer legs; or conversely, those with many legs have simpler mouths?

Why should the sepals, petals, stamens, and pistils in any individual

flower, though fitted for such widely different purposes, be all

constructed on the same pattern?

On the theory of natural selection, we can satisfactorily answer these

questions. In the vertebrata, we see a series of internal vertebrae

bearing certain processes and appendages; in the articulata, we see the

body divided into a series of segments, bearing external appendages;

and in flowering plants, we see a series of successive spiral whorls of

leaves. An indefinite repetition of the same part or organ is the common

characteristic (as Owen has observed) of all low or little-modified

forms; therefore we may readily believe that the unknown progenitor of

the vertebrata possessed many vertebrae; the unknown progenitor of

the articulata, many segments; and the unknown progenitor of flowering

plants, many spiral whorls of leaves. We have formerly seen that

parts many times repeated are eminently liable to vary in number and

structure; consequently it is quite probable that natural selection,

during a long-continued course of modification, should have seized on

a certain number of the primordially similar elements, many times

repeated, and have adapted them to the most diverse purposes. And as

the whole amount of modification will have been effected by slight

successive steps, we need not wonder at discovering in such parts or

organs, a certain degree of fundamental resemblance, retained by the

strong principle of inheritance.

In the great class of molluscs, though we can homologise the parts of

one species with those of another and distinct species, we can indicate

but few serial homologies; that is, we are seldom enabled to say that

one part or organ is homologous with another in the same individual. And

we can understand this fact; for in molluscs, even in the lowest members

of the class, we do not find nearly so much indefinite repetition of

any one part, as we find in the other great classes of the animal and

vegetable kingdoms.

Naturalists frequently speak of the skull as formed of metamorphosed

vertebrae: the jaws of crabs as metamorphosed legs; the stamens and

pistils of flowers as metamorphosed leaves; but it would in these cases

probably be more correct, as Professor Huxley has remarked, to speak

of both skull and vertebrae, both jaws and legs, etc.,--as having been

metamorphosed, not one from the other, but from some common element.

Naturalists, however, use such language only in a metaphorical sense:

they are far from meaning that during a long course of descent,

primordial organs of any kind--vertebrae in the one case and legs in the

other--have actually been modified into skulls or jaws. Yet so strong

is the appearance of a modification of this nature having occurred,

that naturalists can hardly avoid employing language having this plain

signification. On my view these terms may be used literally; and the

wonderful fact of the jaws, for instance, of a crab retaining numerous

characters, which they would probably have retained through inheritance,

if they had really been metamorphosed during a long course of descent

from true legs, or from some simple appendage, is explained.

EMBRYOLOGY.

It has already been casually remarked that certain organs in the

individual, which when mature become widely different and serve for

different purposes, are in the embryo exactly alike. The embryos, also,

of distinct animals within the same class are often strikingly similar:

a better proof of this cannot be given, than a circumstance mentioned

by Agassiz, namely, that having forgotten to ticket the embryo of some

vertebrate animal, he cannot now tell whether it be that of a mammal,

bird, or reptile. The vermiform larvae of moths, flies, beetles, etc.,

resemble each other much more closely than do the mature insects; but

in the case of larvae, the embryos are active, and have been adapted

for special lines of life. A trace of the law of embryonic resemblance,

sometimes lasts till a rather late age: thus birds of the same genus,

and of closely allied genera, often resemble each other in their first

and second plumage; as we see in the spotted feathers in the thrush

group. In the cat tribe, most of the species are striped or spotted

in lines; and stripes can be plainly distinguished in the whelp of

the lion. We occasionally though rarely see something of this kind in

plants: thus the embryonic leaves of the ulex or furze, and the first

leaves of the phyllodineous acaceas, are pinnate or divided like the

ordinary leaves of the leguminosae.

The points of structure, in which the embryos of widely different

animals of the same class resemble each other, often have no direct

relation to their conditions of existence. We cannot, for instance,

suppose that in the embryos of the vertebrata the peculiar loop-like

course of the arteries near the branchial slits are related to similar

conditions,--in the young mammal which is nourished in the womb of its

mother, in the egg of the bird which is hatched in a nest, and in the

spawn of a frog under water. We have no more reason to believe in such

a relation, than we have to believe that the same bones in the hand of

a man, wing of a bat, and fin of a porpoise, are related to similar

conditions of life. No one will suppose that the stripes on the whelp

of a lion, or the spots on the young blackbird, are of any use to these

animals, or are related to the conditions to which they are exposed.

The case, however, is different when an animal during any part of its

embryonic career is active, and has to provide for itself. The period of

activity may come on earlier or later in life; but whenever it comes on,

the adaptation of the larva to its conditions of life is just as perfect

and as beautiful as in the adult animal. From such special adaptations,

the similarity of the larvae or active embryos of allied animals is

sometimes much obscured; and cases could be given of the larvae of two

species, or of two groups of species, differing quite as much, or

even more, from each other than do their adult parents. In most cases,

however, the larvae, though active, still obey more or less closely the

law of common embryonic resemblance. Cirripedes afford a good instance

of this: even the illustrious Cuvier did not perceive that a barnacle

was, as it certainly is, a crustacean; but a glance at the larva shows

this to be the case in an unmistakeable manner. So again the two main

divisions of cirripedes, the pedunculated and sessile, which differ

widely in external appearance, have larvae in all their several stages

barely distinguishable.

The embryo in the course of development generally rises in organisation:

I use this expression, though I am aware that it is hardly possible to

define clearly what is meant by the organisation being higher or lower.

But no one probably will dispute that the butterfly is higher than the

caterpillar. In some cases, however, the mature animal is generally

considered as lower in the scale than the larva, as with certain

parasitic crustaceans. To refer once again to cirripedes: the larvae in

the first stage have three pairs of legs, a very simple single eye, and

a probosciformed mouth, with which they feed largely, for they increase

much in size. In the second stage, answering to the chrysalis stage of

butterflies, they have six pairs of beautifully constructed natatory

legs, a pair of magnificent compound eyes, and extremely complex

antennae; but they have a closed and imperfect mouth, and cannot feed:

their function at this stage is, to search by their well-developed

organs of sense, and to reach by their active powers of swimming, a

proper place on which to become attached and to undergo their final

metamorphosis. When this is completed they are fixed for life: their

legs are now converted into prehensile organs; they again obtain a

well-constructed mouth; but they have no antennae, and their two eyes

are now reconverted into a minute, single, and very simple eye-spot.

In this last and complete state, cirripedes may be considered as

either more highly or more lowly organised than they were in the larval

condition. But in some genera the larvae become developed either into

hermaphrodites having the ordinary structure, or into what I have called

complemental males: and in the latter, the development has assuredly

been retrograde; for the male is a mere sack, which lives for a short

time, and is destitute of mouth, stomach, or other organ of importance,

excepting for reproduction.

We are so much accustomed to see differences in structure between the

embryo and the adult, and likewise a close similarity in the embryos of

widely different animals within the same class, that we might be led to

look at these facts as necessarily contingent in some manner on growth.

But there is no obvious reason why, for instance, the wing of a bat, or

the fin of a porpoise, should not have been sketched out with all the

parts in proper proportion, as soon as any structure became visible in

the embryo. And in some whole groups of animals and in certain members

of other groups, the embryo does not at any period differ widely from

the adult: thus Owen has remarked in regard to cuttle-fish, "there is no

metamorphosis; the cephalopodic character is manifested long before

the parts of the embryo are completed;" and again in spiders, "there

is nothing worthy to be called a metamorphosis." The larvae of insects,

whether adapted to the most diverse and active habits, or quite

inactive, being fed by their parents or placed in the midst of proper

nutriment, yet nearly all pass through a similar worm-like stage of

development; but in some few cases, as in that of Aphis, if we look to

the admirable drawings by Professor Huxley of the development of this

insect, we see no trace of the vermiform stage.

How, then, can we explain these several facts in embryology,--namely

the very general, but not universal difference in structure between the

embryo and the adult;--of parts in the same individual embryo, which

ultimately become very unlike and serve for diverse purposes, being

at this early period of growth alike;--of embryos of different species

within the same class, generally, but not universally, resembling each

other;--of the structure of the embryo not being closely related to its

conditions of existence, except when the embryo becomes at any period

of life active and has to provide for itself;--of the embryo apparently

having sometimes a higher organisation than the mature animal, into

which it is developed. I believe that all these facts can be explained,

as follows, on the view of descent with modification.

It is commonly assumed, perhaps from monstrosities often affecting the

embryo at a very early period, that slight variations necessarily

appear at an equally early period. But we have little evidence on

this head--indeed the evidence rather points the other way; for it is

notorious that breeders of cattle, horses, and various fancy animals,

cannot positively tell, until some time after the animal has been born,

what its merits or form will ultimately turn out. We see this plainly in

our own children; we cannot always tell whether the child will be tall

or short, or what its precise features will be. The question is not, at

what period of life any variation has been caused, but at what period

it is fully displayed. The cause may have acted, and I believe generally

has acted, even before the embryo is formed; and the variation may be

due to the male and female sexual elements having been affected by

the conditions to which either parent, or their ancestors, have been

exposed. Nevertheless an effect thus caused at a very early period, even

before the formation of the embryo, may appear late in life; as when

an hereditary disease, which appears in old age alone, has been

communicated to the offspring from the reproductive element of one

parent. Or again, as when the horns of cross-bred cattle have been

affected by the shape of the horns of either parent. For the welfare of

a very young animal, as long as it remains in its mother's womb, or in

the egg, or as long as it is nourished and protected by its parent,

it must be quite unimportant whether most of its characters are fully

acquired a little earlier or later in life. It would not signify, for

instance, to a bird which obtained its food best by having a long beak,

whether or not it assumed a beak of this particular length, as long as

it was fed by its parents. Hence, I conclude, that it is quite possible,

that each of the many successive modifications, by which each species

has acquired its present structure, may have supervened at a not very

early period of life; and some direct evidence from our domestic animals

supports this view. But in other cases it is quite possible that each

successive modification, or most of them, may have appeared at an

extremely early period.

I have stated in the first chapter, that there is some evidence to

render it probable, that at whatever age any variation first appears

in the parent, it tends to reappear at a corresponding age in the

offspring. Certain variations can only appear at corresponding ages, for

instance, peculiarities in the caterpillar, cocoon, or imago states of

the silk-moth; or, again, in the horns of almost full-grown cattle. But

further than this, variations which, for all that we can see, might have

appeared earlier or later in life, tend to appear at a corresponding

age in the offspring and parent. I am far from meaning that this is

invariably the case; and I could give a good many cases of variations

(taking the word in the largest sense) which have supervened at an

earlier age in the child than in the parent.

These two principles, if their truth be admitted, will, I believe,

explain all the above specified leading facts in embryology. But first

let us look at a few analogous cases in domestic varieties. Some authors

who have written on Dogs, maintain that the greyhound and bulldog,

though appearing so different, are really varieties most closely allied,

and have probably descended from the same wild stock; hence I was

curious to see how far their puppies differed from each other: I was

told by breeders that they differed just as much as their parents, and

this, judging by the eye, seemed almost to be the case; but on actually

measuring the old dogs and their six-days old puppies, I found that

the puppies had not nearly acquired their full amount of proportional

difference. So, again, I was told that the foals of cart and race-horses

differed as much as the full-grown animals; and this surprised me

greatly, as I think it probable that the difference between these two

breeds has been wholly caused by selection under domestication; but

having had careful measurements made of the dam and of a three-days old

colt of a race and heavy cart-horse, I find that the colts have by no

means acquired their full amount of proportional difference.

As the evidence appears to me conclusive, that the several domestic

breeds of Pigeon have descended from one wild species, I compared young

pigeons of various breeds, within twelve hours after being hatched; I

carefully measured the proportions (but will not here give details) of

the beak, width of mouth, length of nostril and of eyelid, size of

feet and length of leg, in the wild stock, in pouters, fantails, runts,

barbs, dragons, carriers, and tumblers. Now some of these birds, when

mature, differ so extraordinarily in length and form of beak, that

they would, I cannot doubt, be ranked in distinct genera, had they been

natural productions. But when the nestling birds of these several breeds

were placed in a row, though most of them could be distinguished from

each other, yet their proportional differences in the above specified

several points were incomparably less than in the full-grown birds. Some

characteristic points of difference--for instance, that of the width

of mouth--could hardly be detected in the young. But there was one

remarkable exception to this rule, for the young of the short-faced

tumbler differed from the young of the wild rock-pigeon and of the other

breeds, in all its proportions, almost exactly as much as in the adult

state.

The two principles above given seem to me to explain these facts in

regard to the later embryonic stages of our domestic varieties. Fanciers

select their horses, dogs, and pigeons, for breeding, when they are

nearly grown up: they are indifferent whether the desired qualities

and structures have been acquired earlier or later in life, if the

full-grown animal possesses them. And the cases just given, more

especially that of pigeons, seem to show that the characteristic

differences which give value to each breed, and which have been

accumulated by man's selection, have not generally first appeared at

an early period of life, and have been inherited by the offspring at a

corresponding not early period. But the case of the short-faced tumbler,

which when twelve hours old had acquired its proper proportions,

proves that this is not the universal rule; for here the characteristic

differences must either have appeared at an earlier period than usual,

or, if not so, the differences must have been inherited, not at the

corresponding, but at an earlier age.

Now let us apply these facts and the above two principles--which latter,

though not proved true, can be shown to be in some degree probable--to

species in a state of nature. Let us take a genus of birds, descended

on my theory from some one parent-species, and of which the several new

species have become modified through natural selection in accordance

with their diverse habits. Then, from the many slight successive steps

of variation having supervened at a rather late age, and having been

inherited at a corresponding age, the young of the new species of our

supposed genus will manifestly tend to resemble each other much more

closely than do the adults, just as we have seen in the case of

pigeons. We may extend this view to whole families or even classes. The

fore-limbs, for instance, which served as legs in the parent-species,

may become, by a long course of modification, adapted in one descendant

to act as hands, in another as paddles, in another as wings; and on the

above two principles--namely of each successive modification supervening

at a rather late age, and being inherited at a corresponding late

age--the fore-limbs in the embryos of the several descendants of the

parent-species will still resemble each other closely, for they will not

have been modified. But in each individual new species, the embryonic

fore-limbs will differ greatly from the fore-limbs in the mature animal;

the limbs in the latter having undergone much modification at a rather

late period of life, and having thus been converted into hands, or

paddles, or wings. Whatever influence long-continued exercise or use on

the one hand, and disuse on the other, may have in modifying an organ,

such influence will mainly affect the mature animal, which has come

to its full powers of activity and has to gain its own living; and the

effects thus produced will be inherited at a corresponding mature age.

Whereas the young will remain unmodified, or be modified in a lesser

degree, by the effects of use and disuse.

In certain cases the successive steps of variation might supervene, from

causes of which we are wholly ignorant, at a very early period of life,

or each step might be inherited at an earlier period than that at which

it first appeared. In either case (as with the short-faced tumbler) the

young or embryo would closely resemble the mature parent-form. We have

seen that this is the rule of development in certain whole groups of

animals, as with cuttle-fish and spiders, and with a few members of the

great class of insects, as with Aphis. With respect to the final cause

of the young in these cases not undergoing any metamorphosis, or closely

resembling their parents from their earliest age, we can see that this

would result from the two following contingencies; firstly, from the

young, during a course of modification carried on for many generations,

having to provide for their own wants at a very early stage of

development, and secondly, from their following exactly the same habits

of life with their parents; for in this case, it would be indispensable

for the existence of the species, that the child should be modified at

a very early age in the same manner with its parents, in accordance with

their similar habits. Some further explanation, however, of the embryo

not undergoing any metamorphosis is perhaps requisite. If, on the other

hand, it profited the young to follow habits of life in any degree

different from those of their parent, and consequently to be constructed

in a slightly different manner, then, on the principle of inheritance at

corresponding ages, the active young or larvae might easily be rendered

by natural selection different to any conceivable extent from their

parents. Such differences might, also, become correlated with successive

stages of development; so that the larvae, in the first stage, might

differ greatly from the larvae in the second stage, as we have seen to

be the case with cirripedes. The adult might become fitted for sites or

habits, in which organs of locomotion or of the senses, etc., would be

useless; and in this case the final metamorphosis would be said to be

retrograde.

As all the organic beings, extinct and recent, which have ever lived on

this earth have to be classed together, and as all have been connected

by the finest gradations, the best, or indeed, if our collections were

nearly perfect, the only possible arrangement, would be genealogical.

Descent being on my view the hidden bond of connexion which naturalists

have been seeking under the term of the natural system. On this view

we can understand how it is that, in the eyes of most naturalists, the

structure of the embryo is even more important for classification than

that of the adult. For the embryo is the animal in its less modified

state; and in so far it reveals the structure of its progenitor. In

two groups of animal, however much they may at present differ from each

other in structure and habits, if they pass through the same or similar

embryonic stages, we may feel assured that they have both descended from

the same or nearly similar parents, and are therefore in that degree

closely related. Thus, community in embryonic structure reveals

community of descent. It will reveal this community of descent, however

much the structure of the adult may have been modified and obscured; we

have seen, for instance, that cirripedes can at once be recognised by

their larvae as belonging to the great class of crustaceans. As the

embryonic state of each species and group of species partially shows us

the structure of their less modified ancient progenitors, we can clearly

see why ancient and extinct forms of life should resemble the embryos of

their descendants,--our existing species. Agassiz believes this to be a

law of nature; but I am bound to confess that I only hope to see the

law hereafter proved true. It can be proved true in those cases alone in

which the ancient state, now supposed to be represented in many embryos,

has not been obliterated, either by the successive variations in a long

course of modification having supervened at a very early age, or by the

variations having been inherited at an earlier period than that at which

they first appeared. It should also be borne in mind, that the supposed

law of resemblance of ancient forms of life to the embryonic stages of

recent forms, may be true, but yet, owing to the geological record not

extending far enough back in time, may remain for a long period, or for

ever, incapable of demonstration.

Thus, as it seems to me, the leading facts in embryology, which are

second in importance to none in natural history, are explained on the

principle of slight modifications not appearing, in the many descendants

from some one ancient progenitor, at a very early period in the life of

each, though perhaps caused at the earliest, and being inherited at a

corresponding not early period. Embryology rises greatly in interest,

when we thus look at the embryo as a picture, more or less obscured, of

the common parent-form of each great class of animals.

RUDIMENTARY, ATROPHIED, OR ABORTED ORGANS.

Organs or parts in this strange condition, bearing the stamp of

inutility, are extremely common throughout nature. For instance,

rudimentary mammae are very general in the males of mammals: I presume

that the "bastard-wing" in birds may be safely considered as a digit

in a rudimentary state: in very many snakes one lobe of the lungs is

rudimentary; in other snakes there are rudiments of the pelvis and hind

limbs. Some of the cases of rudimentary organs are extremely curious;

for instance, the presence of teeth in foetal whales, which when grown

up have not a tooth in their heads; and the presence of teeth, which

never cut through the gums, in the upper jaws of our unborn calves. It

has even been stated on good authority that rudiments of teeth can be

detected in the beaks of certain embryonic birds. Nothing can be plainer

than that wings are formed for flight, yet in how many insects do we see

wings so reduced in size as to be utterly incapable of flight, and not

rarely lying under wing-cases, firmly soldered together!

The meaning of rudimentary organs is often quite unmistakeable: for

instance there are beetles of the same genus (and even of the same

species) resembling each other most closely in all respects, one

of which will have full-sized wings, and another mere rudiments of

membrane; and here it is impossible to doubt, that the rudiments

represent wings. Rudimentary organs sometimes retain their potentiality,

and are merely not developed: this seems to be the case with the mammae

of male mammals, for many instances are on record of these organs having

become well developed in full-grown males, and having secreted milk. So

again there are normally four developed and two rudimentary teats in

the udders of the genus Bos, but in our domestic cows the two sometimes

become developed and give milk. In individual plants of the same

species the petals sometimes occur as mere rudiments, and sometimes in

a well-developed state. In plants with separated sexes, the male flowers

often have a rudiment of a pistil; and Kolreuter found that by crossing

such male plants with an hermaphrodite species, the rudiment of the

pistil in the hybrid offspring was much increased in size; and this

shows that the rudiment and the perfect pistil are essentially alike in

nature.

An organ serving for two purposes, may become rudimentary or utterly

aborted for one, even the more important purpose; and remain perfectly

efficient for the other. Thus in plants, the office of the pistil is to

allow the pollen-tubes to reach the ovules protected in the ovarium at

its base. The pistil consists of a stigma supported on the style; but in

some Compositae, the male florets, which of course cannot be fecundated,

have a pistil, which is in a rudimentary state, for it is not crowned

with a stigma; but the style remains well developed, and is clothed with

hairs as in other compositae, for the purpose of brushing the pollen out

of the surrounding anthers. Again, an organ may become rudimentary for

its proper purpose, and be used for a distinct object: in certain fish

the swim-bladder seems to be rudimentary for its proper function of

giving buoyancy, but has become converted into a nascent breathing organ

or lung. Other similar instances could be given.

Rudimentary organs in the individuals of the same species are very

liable to vary in degree of development and in other respects. Moreover,

in closely allied species, the degree to which the same organ has been

rendered rudimentary occasionally differs much. This latter fact is well

exemplified in the state of the wings of the female moths in certain

groups. Rudimentary organs may be utterly aborted; and this implies,

that we find in an animal or plant no trace of an organ, which analogy

would lead us to expect to find, and which is occasionally found

in monstrous individuals of the species. Thus in the snapdragon

(antirrhinum) we generally do not find a rudiment of a fifth stamen; but

this may sometimes be seen. In tracing the homologies of the same

part in different members of a class, nothing is more common, or more

necessary, than the use and discovery of rudiments. This is well shown

in the drawings given by Owen of the bones of the leg of the horse, ox,

and rhinoceros.

It is an important fact that rudimentary organs, such as teeth in the

upper jaws of whales and ruminants, can often be detected in the embryo,

but afterwards wholly disappear. It is also, I believe, a universal

rule, that a rudimentary part or organ is of greater size relatively to

the adjoining parts in the embryo, than in the adult; so that the organ

at this early age is less rudimentary, or even cannot be said to be in

any degree rudimentary. Hence, also, a rudimentary organ in the adult,

is often said to have retained its embryonic condition.

I have now given the leading facts with respect to rudimentary organs.

In reflecting on them, every one must be struck with astonishment: for

the same reasoning power which tells us plainly that most parts and

organs are exquisitely adapted for certain purposes, tells us with equal

plainness that these rudimentary or atrophied organs, are imperfect and

useless. In works on natural history rudimentary organs are generally

said to have been created "for the sake of symmetry," or in order "to

complete the scheme of nature;" but this seems to me no explanation,

merely a restatement of the fact. Would it be thought sufficient to

say that because planets revolve in elliptic courses round the sun,

satellites follow the same course round the planets, for the sake of

symmetry, and to complete the scheme of nature? An eminent physiologist

accounts for the presence of rudimentary organs, by supposing that they

serve to excrete matter in excess, or injurious to the system; but can

we suppose that the minute papilla, which often represents the pistil

in male flowers, and which is formed merely of cellular tissue, can thus

act? Can we suppose that the formation of rudimentary teeth which are

subsequently absorbed, can be of any service to the rapidly growing

embryonic calf by the excretion of precious phosphate of lime? When a

man's fingers have been amputated, imperfect nails sometimes appear on

the stumps: I could as soon believe that these vestiges of nails have

appeared, not from unknown laws of growth, but in order to excrete horny

matter, as that the rudimentary nails on the fin of the manatee were

formed for this purpose.

On my view of descent with modification, the origin of rudimentary

organs is simple. We have plenty of cases of rudimentary organs in our

domestic productions,--as the stump of a tail in tailless breeds,--the

vestige of an ear in earless breeds,--the reappearance of minute

dangling horns in hornless breeds of cattle, more especially, according

to Youatt, in young animals,--and the state of the whole flower in the

cauliflower. We often see rudiments of various parts in monsters. But

I doubt whether any of these cases throw light on the origin of

rudimentary organs in a state of nature, further than by showing that

rudiments can be produced; for I doubt whether species under nature ever

undergo abrupt changes. I believe that disuse has been the main agency;

that it has led in successive generations to the gradual reduction of

various organs, until they have become rudimentary,--as in the case of

the eyes of animals inhabiting dark caverns, and of the wings of birds

inhabiting oceanic islands, which have seldom been forced to take

flight, and have ultimately lost the power of flying. Again, an organ

useful under certain conditions, might become injurious under others,

as with the wings of beetles living on small and exposed islands; and in

this case natural selection would continue slowly to reduce the organ,

until it was rendered harmless and rudimentary.

Any change in function, which can be effected by insensibly small steps,

is within the power of natural selection; so that an organ rendered,

during changed habits of life, useless or injurious for one purpose,

might easily be modified and used for another purpose. Or an organ

might be retained for one alone of its former functions. An organ, when

rendered useless, may well be variable, for its variations cannot be

checked by natural selection. At whatever period of life disuse or

selection reduces an organ, and this will generally be when the being

has come to maturity and to its full powers of action, the principle

of inheritance at corresponding ages will reproduce the organ in its

reduced state at the same age, and consequently will seldom affect or

reduce it in the embryo. Thus we can understand the greater relative

size of rudimentary organs in the embryo, and their lesser relative size

in the adult. But if each step of the process of reduction were to

be inherited, not at the corresponding age, but at an extremely early

period of life (as we have good reason to believe to be possible) the

rudimentary part would tend to be wholly lost, and we should have a case

of complete abortion. The principle, also, of economy, explained in a

former chapter, by which the materials forming any part or structure, if

not useful to the possessor, will be saved as far as is possible, will

probably often come into play; and this will tend to cause the entire

obliteration of a rudimentary organ.

As the presence of rudimentary organs is thus due to the tendency

in every part of the organisation, which has long existed, to

be inherited--we can understand, on the genealogical view of

classification, how it is that systematists have found rudimentary

parts as useful as, or even sometimes more useful than, parts of high

physiological importance. Rudimentary organs may be compared with the

letters in a word, still retained in the spelling, but become useless

in the pronunciation, but which serve as a clue in seeking for its

derivation. On the view of descent with modification, we may conclude

that the existence of organs in a rudimentary, imperfect, and useless

condition, or quite aborted, far from presenting a strange difficulty,

as they assuredly do on the ordinary doctrine of creation, might

even have been anticipated, and can be accounted for by the laws of

inheritance.

SUMMARY.

In this chapter I have attempted to show, that the subordination of

group to group in all organisms throughout all time; that the nature of

the relationship, by which all living and extinct beings are united by

complex, radiating, and circuitous lines of affinities into one

grand system; the rules followed and the difficulties encountered by

naturalists in their classifications; the value set upon characters, if

constant and prevalent, whether of high vital importance, or of the most

trifling importance, or, as in rudimentary organs, of no importance; the

wide opposition in value between analogical or adaptive characters, and

characters of true affinity; and other such rules;--all naturally follow

on the view of the common parentage of those forms which are considered

by naturalists as allied, together with their modification through

natural selection, with its contingencies of extinction and divergence

of character. In considering this view of classification, it should be

borne in mind that the element of descent has been universally used in

ranking together the sexes, ages, and acknowledged varieties of the same

species, however different they may be in structure. If we extend the

use of this element of descent,--the only certainly known cause of

similarity in organic beings,--we shall understand what is meant by the

natural system: it is genealogical in its attempted arrangement,

with the grades of acquired difference marked by the terms varieties,

species, genera, families, orders, and classes.

On this same view of descent with modification, all the great facts in

Morphology become intelligible,--whether we look to the same pattern

displayed in the homologous organs, to whatever purpose applied, of the

different species of a class; or to the homologous parts constructed on

the same pattern in each individual animal and plant.

On the principle of successive slight variations, not necessarily

or generally supervening at a very early period of life, and being

inherited at a corresponding period, we can understand the great leading

facts in Embryology; namely, the resemblance in an individual embryo of

the homologous parts, which when matured will become widely different

from each other in structure and function; and the resemblance in

different species of a class of the homologous parts or organs, though

fitted in the adult members for purposes as different as possible.

Larvae are active embryos, which have become specially modified in

relation to their habits of life, through the principle of modifications

being inherited at corresponding ages. On this same principle--and

bearing in mind, that when organs are reduced in size, either from

disuse or selection, it will generally be at that period of life when

the being has to provide for its own wants, and bearing in mind how

strong is the principle of inheritance--the occurrence of rudimentary

organs and their final abortion, present to us no inexplicable

difficulties; on the contrary, their presence might have been even

anticipated. The importance of embryological characters and of

rudimentary organs in classification is intelligible, on the view that

an arrangement is only so far natural as it is genealogical.

Finally, the several classes of facts which have been considered in

this chapter, seem to me to proclaim so plainly, that the innumerable

species, genera, and families of organic beings, with which this world

is peopled, have all descended, each within its own class or group, from

common parents, and have all been modified in the course of descent,

that I should without hesitation adopt this view, even if it were

unsupported by other facts or arguments.

14. RECAPITULATION AND CONCLUSION.

Recapitulation of the difficulties on the theory of Natural Selection.

Recapitulation of the general and special circumstances in its favour.

Causes of the general belief in the immutability of species. How far the

theory of natural selection may be extended. Effects of its adoption on

the study of Natural history. Concluding remarks.

As this whole volume is one long argument, it may be convenient to the

reader to have the leading facts and inferences briefly recapitulated.

That many and grave objections may be advanced against the theory of

descent with modification through natural selection, I do not deny. I

have endeavoured to give to them their full force. Nothing at first can

appear more difficult to believe than that the more complex organs and

instincts should have been perfected, not by means superior to, though

analogous with, human reason, but by the accumulation of innumerable

slight variations, each good for the individual possessor. Nevertheless,

this difficulty, though appearing to our imagination insuperably great,

cannot be considered real if we admit the following propositions,

namely,--that gradations in the perfection of any organ or instinct,

which we may consider, either do now exist or could have existed, each

good of its kind,--that all organs and instincts are, in ever so slight

a degree, variable,--and, lastly, that there is a struggle for existence

leading to the preservation of each profitable deviation of structure or

instinct. The truth of these propositions cannot, I think, be disputed.

It is, no doubt, extremely difficult even to conjecture by what

gradations many structures have been perfected, more especially amongst

broken and failing groups of organic beings; but we see so many strange

gradations in nature, as is proclaimed by the canon, "Natura non facit

saltum," that we ought to be extremely cautious in saying that any organ

or instinct, or any whole being, could not have arrived at its present

state by many graduated steps. There are, it must be admitted, cases of

special difficulty on the theory of natural selection; and one of the

most curious of these is the existence of two or three defined castes

of workers or sterile females in the same community of ants; but I have

attempted to show how this difficulty can be mastered.

With respect to the almost universal sterility of species when first

crossed, which forms so remarkable a contrast with the almost universal

fertility of varieties when crossed, I must refer the reader to the

recapitulation of the facts given at the end of the eighth chapter,

which seem to me conclusively to show that this sterility is no more

a special endowment than is the incapacity of two trees to be grafted

together, but that it is incidental on constitutional differences in the

reproductive systems of the intercrossed species. We see the truth of

this conclusion in the vast difference in the result, when the same two

species are crossed reciprocally; that is, when one species is first

used as the father and then as the mother.

The fertility of varieties when intercrossed and of their mongrel

offspring cannot be considered as universal; nor is their very general

fertility surprising when we remember that it is not likely that either

their constitutions or their reproductive systems should have been

profoundly modified. Moreover, most of the varieties which have been

experimentised on have been produced under domestication; and as

domestication apparently tends to eliminate sterility, we ought not to

expect it also to produce sterility.

The sterility of hybrids is a very different case from that of first

crosses, for their reproductive organs are more or less functionally

impotent; whereas in first crosses the organs on both sides are in a

perfect condition. As we continually see that organisms of all kinds

are rendered in some degree sterile from their constitutions having been

disturbed by slightly different and new conditions of life, we need

not feel surprise at hybrids being in some degree sterile, for their

constitutions can hardly fail to have been disturbed from being

compounded of two distinct organisations. This parallelism is supported

by another parallel, but directly opposite, class of facts; namely, that

the vigour and fertility of all organic beings are increased by slight

changes in their conditions of life, and that the offspring of slightly

modified forms or varieties acquire from being crossed increased vigour

and fertility. So that, on the one hand, considerable changes in the

conditions of life and crosses between greatly modified forms, lessen

fertility; and on the other hand, lesser changes in the conditions of

life and crosses between less modified forms, increase fertility.

Turning to geographical distribution, the difficulties encountered

on the theory of descent with modification are grave enough. All the

individuals of the same species, and all the species of the same genus,

or even higher group, must have descended from common parents; and

therefore, in however distant and isolated parts of the world they are

now found, they must in the course of successive generations have passed

from some one part to the others. We are often wholly unable even to

conjecture how this could have been effected. Yet, as we have reason to

believe that some species have retained the same specific form for very

long periods, enormously long as measured by years, too much stress

ought not to be laid on the occasional wide diffusion of the same

species; for during very long periods of time there will always be a

good chance for wide migration by many means. A broken or interrupted

range may often be accounted for by the extinction of the species in

the intermediate regions. It cannot be denied that we are as yet very

ignorant of the full extent of the various climatal and geographical

changes which have affected the earth during modern periods; and

such changes will obviously have greatly facilitated migration. As an

example, I have attempted to show how potent has been the influence

of the Glacial period on the distribution both of the same and of

representative species throughout the world. We are as yet profoundly

ignorant of the many occasional means of transport. With respect to

distinct species of the same genus inhabiting very distant and isolated

regions, as the process of modification has necessarily been slow,

all the means of migration will have been possible during a very long

period; and consequently the difficulty of the wide diffusion of species

of the same genus is in some degree lessened.

As on the theory of natural selection an interminable number of

intermediate forms must have existed, linking together all the species

in each group by gradations as fine as our present varieties, it may be

asked, Why do we not see these linking forms all around us? Why are

not all organic beings blended together in an inextricable chaos? With

respect to existing forms, we should remember that we have no right to

expect (excepting in rare cases) to discover DIRECTLY connecting links

between them, but only between each and some extinct and supplanted

form. Even on a wide area, which has during a long period remained

continuous, and of which the climate and other conditions of life change

insensibly in going from a district occupied by one species into another

district occupied by a closely allied species, we have no just right to

expect often to find intermediate varieties in the intermediate zone.

For we have reason to believe that only a few species are undergoing

change at any one period; and all changes are slowly effected. I have

also shown that the intermediate varieties which will at first probably

exist in the intermediate zones, will be liable to be supplanted by the

allied forms on either hand; and the latter, from existing in greater

numbers, will generally be modified and improved at a quicker rate than

the intermediate varieties, which exist in lesser numbers; so that

the intermediate varieties will, in the long run, be supplanted and

exterminated.

On this doctrine of the extermination of an infinitude of connecting

links, between the living and extinct inhabitants of the world, and at

each successive period between the extinct and still older species, why

is not every geological formation charged with such links? Why does

not every collection of fossil remains afford plain evidence of the

gradation and mutation of the forms of life? We meet with no such

evidence, and this is the most obvious and forcible of the many

objections which may be urged against my theory. Why, again, do whole

groups of allied species appear, though certainly they often falsely

appear, to have come in suddenly on the several geological stages? Why

do we not find great piles of strata beneath the Silurian system, stored

with the remains of the progenitors of the Silurian groups of fossils?

For certainly on my theory such strata must somewhere have been

deposited at these ancient and utterly unknown epochs in the world's

history.

I can answer these questions and grave objections only on the

supposition that the geological record is far more imperfect than most

geologists believe. It cannot be objected that there has not been time

sufficient for any amount of organic change; for the lapse of time has

been so great as to be utterly inappreciable by the human intellect. The

number of specimens in all our museums is absolutely as nothing compared

with the countless generations of countless species which certainly have

existed. We should not be able to recognise a species as the parent

of any one or more species if we were to examine them ever so closely,

unless we likewise possessed many of the intermediate links between

their past or parent and present states; and these many links we

could hardly ever expect to discover, owing to the imperfection of the

geological record. Numerous existing doubtful forms could be named which

are probably varieties; but who will pretend that in future ages so

many fossil links will be discovered, that naturalists will be able

to decide, on the common view, whether or not these doubtful forms are

varieties? As long as most of the links between any two species are

unknown, if any one link or intermediate variety be discovered, it will

simply be classed as another and distinct species. Only a small portion

of the world has been geologically explored. Only organic beings of

certain classes can be preserved in a fossil condition, at least in any

great number. Widely ranging species vary most, and varieties are often

at first local,--both causes rendering the discovery of intermediate

links less likely. Local varieties will not spread into other and

distant regions until they are considerably modified and improved; and

when they do spread, if discovered in a geological formation, they will

appear as if suddenly created there, and will be simply classed as new

species. Most formations have been intermittent in their accumulation;

and their duration, I am inclined to believe, has been shorter than the

average duration of specific forms. Successive formations are separated

from each other by enormous blank intervals of time; for fossiliferous

formations, thick enough to resist future degradation, can be

accumulated only where much sediment is deposited on the subsiding bed

of the sea. During the alternate periods of elevation and of stationary

level the record will be blank. During these latter periods there will

probably be more variability in the forms of life; during periods of

subsidence, more extinction.

With respect to the absence of fossiliferous formations beneath the

lowest Silurian strata, I can only recur to the hypothesis given in the

ninth chapter. That the geological record is imperfect all will admit;

but that it is imperfect to the degree which I require, few will be

inclined to admit. If we look to long enough intervals of time, geology

plainly declares that all species have changed; and they have changed in

the manner which my theory requires, for they have changed slowly and

in a graduated manner. We clearly see this in the fossil remains from

consecutive formations invariably being much more closely related to

each other, than are the fossils from formations distant from each other

in time.

Such is the sum of the several chief objections and difficulties

which may justly be urged against my theory; and I have now briefly

recapitulated the answers and explanations which can be given to them. I

have felt these difficulties far too heavily during many years to doubt

their weight. But it deserves especial notice that the more important

objections relate to questions on which we are confessedly ignorant;

nor do we know how ignorant we are. We do not know all the possible

transitional gradations between the simplest and the most perfect

organs; it cannot be pretended that we know all the varied means

of Distribution during the long lapse of years, or that we know how

imperfect the Geological Record is. Grave as these several difficulties

are, in my judgment they do not overthrow the theory of descent with

modification.

Now let us turn to the other side of the argument. Under domestication

we see much variability. This seems to be mainly due to the reproductive

system being eminently susceptible to changes in the conditions of life;

so that this system, when not rendered impotent, fails to reproduce

offspring exactly like the parent-form. Variability is governed by many

complex laws,--by correlation of growth, by use and disuse, and by

the direct action of the physical conditions of life. There is

much difficulty in ascertaining how much modification our domestic

productions have undergone; but we may safely infer that the amount has

been large, and that modifications can be inherited for long periods.

As long as the conditions of life remain the same, we have reason to

believe that a modification, which has already been inherited for many

generations, may continue to be inherited for an almost infinite number

of generations. On the other hand we have evidence that variability,

when it has once come into play, does not wholly cease; for new

varieties are still occasionally produced by our most anciently

domesticated productions.

Man does not actually produce variability; he only unintentionally

exposes organic beings to new conditions of life, and then nature acts

on the organisation, and causes variability. But man can and does select

the variations given to him by nature, and thus accumulate them in any

desired manner. He thus adapts animals and plants for his own benefit or

pleasure. He may do this methodically, or he may do it unconsciously by

preserving the individuals most useful to him at the time, without

any thought of altering the breed. It is certain that he can largely

influence the character of a breed by selecting, in each successive

generation, individual differences so slight as to be quite

inappreciable by an uneducated eye. This process of selection has been

the great agency in the production of the most distinct and useful

domestic breeds. That many of the breeds produced by man have to a large

extent the character of natural species, is shown by the inextricable

doubts whether very many of them are varieties or aboriginal species.

There is no obvious reason why the principles which have acted so

efficiently under domestication should not have acted under nature.

In the preservation of favoured individuals and races, during the

constantly-recurrent Struggle for Existence, we see the most powerful

and ever-acting means of selection. The struggle for existence

inevitably follows from the high geometrical ratio of increase which is

common to all organic beings. This high rate of increase is proved by

calculation, by the effects of a succession of peculiar seasons, and by

the results of naturalisation, as explained in the third chapter. More

individuals are born than can possibly survive. A grain in the balance

will determine which individual shall live and which shall die,--which

variety or species shall increase in number, and which shall decrease,

or finally become extinct. As the individuals of the same species

come in all respects into the closest competition with each other, the

struggle will generally be most severe between them; it will be almost

equally severe between the varieties of the same species, and next in

severity between the species of the same genus. But the struggle will

often be very severe between beings most remote in the scale of nature.

The slightest advantage in one being, at any age or during any season,

over those with which it comes into competition, or better adaptation

in however slight a degree to the surrounding physical conditions, will

turn the balance.

With animals having separated sexes there will in most cases be a

struggle between the males for possession of the females. The most

vigorous individuals, or those which have most successfully struggled

with their conditions of life, will generally leave most progeny. But

success will often depend on having special weapons or means of defence,

or on the charms of the males; and the slightest advantage will lead to

victory.

As geology plainly proclaims that each land has undergone great physical

changes, we might have expected that organic beings would have varied

under nature, in the same way as they generally have varied under the

changed conditions of domestication. And if there be any variability

under nature, it would be an unaccountable fact if natural selection

had not come into play. It has often been asserted, but the assertion is

quite incapable of proof, that the amount of variation under nature is

a strictly limited quantity. Man, though acting on external characters

alone and often capriciously, can produce within a short period a

great result by adding up mere individual differences in his domestic

productions; and every one admits that there are at least individual

differences in species under nature. But, besides such differences, all

naturalists have admitted the existence of varieties, which they think

sufficiently distinct to be worthy of record in systematic works. No one

can draw any clear distinction between individual differences and slight

varieties; or between more plainly marked varieties and sub-species,

and species. Let it be observed how naturalists differ in the rank

which they assign to the many representative forms in Europe and North

America.

If then we have under nature variability and a powerful agent always

ready to act and select, why should we doubt that variations in any way

useful to beings, under their excessively complex relations of life,

would be preserved, accumulated, and inherited? Why, if man can by

patience select variations most useful to himself, should nature fail in

selecting variations useful, under changing conditions of life, to her

living products? What limit can be put to this power, acting during long

ages and rigidly scrutinising the whole constitution, structure, and

habits of each creature,--favouring the good and rejecting the bad? I

can see no limit to this power, in slowly and beautifully adapting

each form to the most complex relations of life. The theory of natural

selection, even if we looked no further than this, seems to me to be in

itself probable. I have already recapitulated, as fairly as I could,

the opposed difficulties and objections: now let us turn to the special

facts and arguments in favour of the theory.

On the view that species are only strongly marked and permanent

varieties, and that each species first existed as a variety, we can

see why it is that no line of demarcation can be drawn between species,

commonly supposed to have been produced by special acts of creation,

and varieties which are acknowledged to have been produced by secondary

laws. On this same view we can understand how it is that in each region

where many species of a genus have been produced, and where they now

flourish, these same species should present many varieties; for where

the manufactory of species has been active, we might expect, as a

general rule, to find it still in action; and this is the case if

varieties be incipient species. Moreover, the species of the larger

genera, which afford the greater number of varieties or incipient

species, retain to a certain degree the character of varieties; for

they differ from each other by a less amount of difference than do the

species of smaller genera. The closely allied species also of the larger

genera apparently have restricted ranges, and they are clustered in

little groups round other species--in which respects they resemble

varieties. These are strange relations on the view of each species

having been independently created, but are intelligible if all species

first existed as varieties.

As each species tends by its geometrical ratio of reproduction to

increase inordinately in number; and as the modified descendants of each

species will be enabled to increase by so much the more as they become

more diversified in habits and structure, so as to be enabled to seize

on many and widely different places in the economy of nature, there

will be a constant tendency in natural selection to preserve the most

divergent offspring of any one species. Hence during a long-continued

course of modification, the slight differences, characteristic of

varieties of the same species, tend to be augmented into the greater

differences characteristic of species of the same genus. New and

improved varieties will inevitably supplant and exterminate the older,

less improved and intermediate varieties; and thus species are rendered

to a large extent defined and distinct objects. Dominant species

belonging to the larger groups tend to give birth to new and dominant

forms; so that each large group tends to become still larger, and at

the same time more divergent in character. But as all groups cannot thus

succeed in increasing in size, for the world would not hold them, the

more dominant groups beat the less dominant. This tendency in the large

groups to go on increasing in size and diverging in character, together

with the almost inevitable contingency of much extinction, explains the

arrangement of all the forms of life, in groups subordinate to groups,

all within a few great classes, which we now see everywhere around us,

and which has prevailed throughout all time. This grand fact of the

grouping of all organic beings seems to me utterly inexplicable on the

theory of creation.

As natural selection acts solely by accumulating slight, successive,

favourable variations, it can produce no great or sudden modification;

it can act only by very short and slow steps. Hence the canon of "Natura

non facit saltum," which every fresh addition to our knowledge tends to

make more strictly correct, is on this theory simply intelligible. We

can plainly see why nature is prodigal in variety, though niggard in

innovation. But why this should be a law of nature if each species has

been independently created, no man can explain.

Many other facts are, as it seems to me, explicable on this theory. How

strange it is that a bird, under the form of woodpecker, should have

been created to prey on insects on the ground; that upland geese, which

never or rarely swim, should have been created with webbed feet; that a

thrush should have been created to dive and feed on sub-aquatic insects;

and that a petrel should have been created with habits and structure

fitting it for the life of an auk or grebe! and so on in endless other

cases. But on the view of each species constantly trying to increase in

number, with natural selection always ready to adapt the slowly varying

descendants of each to any unoccupied or ill-occupied place in nature,

these facts cease to be strange, or perhaps might even have been

anticipated.

As natural selection acts by competition, it adapts the inhabitants

of each country only in relation to the degree of perfection of their

associates; so that we need feel no surprise at the inhabitants of

any one country, although on the ordinary view supposed to have been

specially created and adapted for that country, being beaten and

supplanted by the naturalised productions from another land. Nor ought

we to marvel if all the contrivances in nature be not, as far as we can

judge, absolutely perfect; and if some of them be abhorrent to our ideas

of fitness. We need not marvel at the sting of the bee causing the bee's

own death; at drones being produced in such vast numbers for one

single act, and being then slaughtered by their sterile sisters; at the

astonishing waste of pollen by our fir-trees; at the instinctive hatred

of the queen bee for her own fertile daughters; at ichneumonidae feeding

within the live bodies of caterpillars; and at other such cases. The

wonder indeed is, on the theory of natural selection, that more cases of

the want of absolute perfection have not been observed.

The complex and little known laws governing variation are the same, as

far as we can see, with the laws which have governed the production of

so-called specific forms. In both cases physical conditions seem to have

produced but little direct effect; yet when varieties enter any zone,

they occasionally assume some of the characters of the species proper

to that zone. In both varieties and species, use and disuse seem to have

produced some effect; for it is difficult to resist this conclusion

when we look, for instance, at the logger-headed duck, which has wings

incapable of flight, in nearly the same condition as in the domestic

duck; or when we look at the burrowing tucutucu, which is occasionally

blind, and then at certain moles, which are habitually blind and have

their eyes covered with skin; or when we look at the blind animals

inhabiting the dark caves of America and Europe. In both varieties and

species correlation of growth seems to have played a most important

part, so that when one part has been modified other parts are

necessarily modified. In both varieties and species reversions to

long-lost characters occur. How inexplicable on the theory of creation

is the occasional appearance of stripes on the shoulder and legs of the

several species of the horse-genus and in their hybrids! How simply is

this fact explained if we believe that these species have descended from

a striped progenitor, in the same manner as the several domestic breeds

of pigeon have descended from the blue and barred rock-pigeon!

On the ordinary view of each species having been independently created,

why should the specific characters, or those by which the species of

the same genus differ from each other, be more variable than the generic

characters in which they all agree? Why, for instance, should the colour

of a flower be more likely to vary in any one species of a genus, if

the other species, supposed to have been created independently, have

differently coloured flowers, than if all the species of the genus have

the same coloured flowers? If species are only well-marked varieties,

of which the characters have become in a high degree permanent, we can

understand this fact; for they have already varied since they branched

off from a common progenitor in certain characters, by which they have

come to be specifically distinct from each other; and therefore these

same characters would be more likely still to be variable than the

generic characters which have been inherited without change for an

enormous period. It is inexplicable on the theory of creation why a part

developed in a very unusual manner in any one species of a genus,

and therefore, as we may naturally infer, of great importance to the

species, should be eminently liable to variation; but, on my view, this

part has undergone, since the several species branched off from a common

progenitor, an unusual amount of variability and modification, and

therefore we might expect this part generally to be still variable. But

a part may be developed in the most unusual manner, like the wing of a

bat, and yet not be more variable than any other structure, if the part

be common to many subordinate forms, that is, if it has been inherited

for a very long period; for in this case it will have been rendered

constant by long-continued natural selection.

Glancing at instincts, marvellous as some are, they offer no greater

difficulty than does corporeal structure on the theory of the natural

selection of successive, slight, but profitable modifications. We

can thus understand why nature moves by graduated steps in endowing

different animals of the same class with their several instincts. I have

attempted to show how much light the principle of gradation throws

on the admirable architectural powers of the hive-bee. Habit no doubt

sometimes comes into play in modifying instincts; but it certainly is

not indispensable, as we see, in the case of neuter insects, which leave

no progeny to inherit the effects of long-continued habit. On the view

of all the species of the same genus having descended from a common

parent, and having inherited much in common, we can understand how it is

that allied species, when placed under considerably different conditions

of life, yet should follow nearly the same instincts; why the thrush of

South America, for instance, lines her nest with mud like our British

species. On the view of instincts having been slowly acquired through

natural selection we need not marvel at some instincts being apparently

not perfect and liable to mistakes, and at many instincts causing other

animals to suffer.

If species be only well-marked and permanent varieties, we can at once

see why their crossed offspring should follow the same complex laws

in their degrees and kinds of resemblance to their parents,--in being

absorbed into each other by successive crosses, and in other such

points,--as do the crossed offspring of acknowledged varieties. On

the other hand, these would be strange facts if species have been

independently created, and varieties have been produced by secondary

laws.

If we admit that the geological record is imperfect in an extreme

degree, then such facts as the record gives, support the theory of

descent with modification. New species have come on the stage slowly and

at successive intervals; and the amount of change, after equal intervals

of time, is widely different in different groups. The extinction of

species and of whole groups of species, which has played so conspicuous

a part in the history of the organic world, almost inevitably follows on

the principle of natural selection; for old forms will be supplanted

by new and improved forms. Neither single species nor groups of species

reappear when the chain of ordinary generation has once been broken. The

gradual diffusion of dominant forms, with the slow modification of their

descendants, causes the forms of life, after long intervals of time, to

appear as if they had changed simultaneously throughout the world.

The fact of the fossil remains of each formation being in some degree

intermediate in character between the fossils in the formations above

and below, is simply explained by their intermediate position in the

chain of descent. The grand fact that all extinct organic beings belong

to the same system with recent beings, falling either into the same or

into intermediate groups, follows from the living and the extinct being

the offspring of common parents. As the groups which have descended

from an ancient progenitor have generally diverged in character, the

progenitor with its early descendants will often be intermediate in

character in comparison with its later descendants; and thus we can see

why the more ancient a fossil is, the oftener it stands in some degree

intermediate between existing and allied groups. Recent forms are

generally looked at as being, in some vague sense, higher than ancient

and extinct forms; and they are in so far higher as the later and more

improved forms have conquered the older and less improved organic beings

in the struggle for life. Lastly, the law of the long endurance of

allied forms on the same continent,--of marsupials in Australia, of

edentata in America, and other such cases,--is intelligible, for within

a confined country, the recent and the extinct will naturally be allied

by descent.

Looking to geographical distribution, if we admit that there has been

during the long course of ages much migration from one part of the world

to another, owing to former climatal and geographical changes and to the

many occasional and unknown means of dispersal, then we can understand,

on the theory of descent with modification, most of the great leading

facts in Distribution. We can see why there should be so striking a

parallelism in the distribution of organic beings throughout space, and

in their geological succession throughout time; for in both cases the

beings have been connected by the bond of ordinary generation, and the

means of modification have been the same. We see the full meaning of the

wonderful fact, which must have struck every traveller, namely, that on

the same continent, under the most diverse conditions, under heat and

cold, on mountain and lowland, on deserts and marshes, most of the

inhabitants within each great class are plainly related; for they will

generally be descendants of the same progenitors and early colonists.

On this same principle of former migration, combined in most cases with

modification, we can understand, by the aid of the Glacial period, the

identity of some few plants, and the close alliance of many others,

on the most distant mountains, under the most different climates; and

likewise the close alliance of some of the inhabitants of the sea in

the northern and southern temperate zones, though separated by the whole

intertropical ocean. Although two areas may present the same physical

conditions of life, we need feel no surprise at their inhabitants

being widely different, if they have been for a long period completely

separated from each other; for as the relation of organism to organism

is the most important of all relations, and as the two areas will have

received colonists from some third source or from each other, at various

periods and in different proportions, the course of modification in the

two areas will inevitably be different.

On this view of migration, with subsequent modification, we can see why

oceanic islands should be inhabited by few species, but of these, that

many should be peculiar. We can clearly see why those animals which

cannot cross wide spaces of ocean, as frogs and terrestrial mammals,

should not inhabit oceanic islands; and why, on the other hand, new and

peculiar species of bats, which can traverse the ocean, should so often

be found on islands far distant from any continent. Such facts as the

presence of peculiar species of bats, and the absence of all other

mammals, on oceanic islands, are utterly inexplicable on the theory of

independent acts of creation.

The existence of closely allied or representative species in any two

areas, implies, on the theory of descent with modification, that the

same parents formerly inhabited both areas; and we almost invariably

find that wherever many closely allied species inhabit two areas, some

identical species common to both still exist. Wherever many closely

allied yet distinct species occur, many doubtful forms and varieties of

the same species likewise occur. It is a rule of high generality that

the inhabitants of each area are related to the inhabitants of the

nearest source whence immigrants might have been derived. We see this in

nearly all the plants and animals of the Galapagos archipelago, of Juan

Fernandez, and of the other American islands being related in the most

striking manner to the plants and animals of the neighbouring American

mainland; and those of the Cape de Verde archipelago and other African

islands to the African mainland. It must be admitted that these facts

receive no explanation on the theory of creation.

The fact, as we have seen, that all past and present organic beings

constitute one grand natural system, with group subordinate to group,

and with extinct groups often falling in between recent groups, is

intelligible on the theory of natural selection with its contingencies

of extinction and divergence of character. On these same principles

we see how it is, that the mutual affinities of the species and genera

within each class are so complex and circuitous. We see why certain

characters are far more serviceable than others for classification;--why

adaptive characters, though of paramount importance to the being, are

of hardly any importance in classification; why characters derived from

rudimentary parts, though of no service to the being, are often of high

classificatory value; and why embryological characters are the most

valuable of all. The real affinities of all organic beings are due

to inheritance or community of descent. The natural system is a

genealogical arrangement, in which we have to discover the lines of

descent by the most permanent characters, however slight their vital

importance may be.

The framework of bones being the same in the hand of a man, wing of

a bat, fin of the porpoise, and leg of the horse,--the same number of

vertebrae forming the neck of the giraffe and of the elephant,--and

innumerable other such facts, at once explain themselves on the theory

of descent with slow and slight successive modifications. The similarity

of pattern in the wing and leg of a bat, though used for such different

purpose,--in the jaws and legs of a crab,--in the petals, stamens, and

pistils of a flower, is likewise intelligible on the view of the

gradual modification of parts or organs, which were alike in the early

progenitor of each class. On the principle of successive variations

not always supervening at an early age, and being inherited at a

corresponding not early period of life, we can clearly see why the

embryos of mammals, birds, reptiles, and fishes should be so closely

alike, and should be so unlike the adult forms. We may cease marvelling

at the embryo of an air-breathing mammal or bird having branchial slits

and arteries running in loops, like those in a fish which has to breathe

the air dissolved in water, by the aid of well-developed branchiae.

Disuse, aided sometimes by natural selection, will often tend to reduce

an organ, when it has become useless by changed habits or under changed

conditions of life; and we can clearly understand on this view the

meaning of rudimentary organs. But disuse and selection will generally

act on each creature, when it has come to maturity and has to play its

full part in the struggle for existence, and will thus have little power

of acting on an organ during early life; hence the organ will not be

much reduced or rendered rudimentary at this early age. The calf, for

instance, has inherited teeth, which never cut through the gums of the

upper jaw, from an early progenitor having well-developed teeth; and we

may believe, that the teeth in the mature animal were reduced, during

successive generations, by disuse or by the tongue and palate having

been fitted by natural selection to browse without their aid; whereas in

the calf, the teeth have been left untouched by selection or disuse,

and on the principle of inheritance at corresponding ages have been

inherited from a remote period to the present day. On the view of each

organic being and each separate organ having been specially created, how

utterly inexplicable it is that parts, like the teeth in the embryonic

calf or like the shrivelled wings under the soldered wing-covers of some

beetles, should thus so frequently bear the plain stamp of inutility!

Nature may be said to have taken pains to reveal, by rudimentary organs

and by homologous structures, her scheme of modification, which it seems

that we wilfully will not understand.

I have now recapitulated the chief facts and considerations which have

thoroughly convinced me that species have changed, and are still slowly

changing by the preservation and accumulation of successive slight

favourable variations. Why, it may be asked, have all the most eminent

living naturalists and geologists rejected this view of the mutability

of species? It cannot be asserted that organic beings in a state of

nature are subject to no variation; it cannot be proved that the amount

of variation in the course of long ages is a limited quantity; no clear

distinction has been, or can be, drawn between species and well-marked

varieties. It cannot be maintained that species when intercrossed are

invariably sterile, and varieties invariably fertile; or that sterility

is a special endowment and sign of creation. The belief that species

were immutable productions was almost unavoidable as long as the history

of the world was thought to be of short duration; and now that we have

acquired some idea of the lapse of time, we are too apt to assume,

without proof, that the geological record is so perfect that it would

have afforded us plain evidence of the mutation of species, if they had

undergone mutation.

But the chief cause of our natural unwillingness to admit that one

species has given birth to other and distinct species, is that we are

always slow in admitting any great change of which we do not see the

intermediate steps. The difficulty is the same as that felt by so many

geologists, when Lyell first insisted that long lines of inland cliffs

had been formed, and great valleys excavated, by the slow action of the

coast-waves. The mind cannot possibly grasp the full meaning of the

term of a hundred million years; it cannot add up and perceive the full

effects of many slight variations, accumulated during an almost infinite

number of generations.

Although I am fully convinced of the truth of the views given in this

volume under the form of an abstract, I by no means expect to convince

experienced naturalists whose minds are stocked with a multitude of

facts all viewed, during a long course of years, from a point of view

directly opposite to mine. It is so easy to hide our ignorance under

such expressions as the "plan of creation," "unity of design," etc., and

to think that we give an explanation when we only restate a fact. Any

one whose disposition leads him to attach more weight to unexplained

difficulties than to the explanation of a certain number of facts

will certainly reject my theory. A few naturalists, endowed with

much flexibility of mind, and who have already begun to doubt on the

immutability of species, may be influenced by this volume; but I look

with confidence to the future, to young and rising naturalists, who will

be able to view both sides of the question with impartiality. Whoever

is led to believe that species are mutable will do good service by

conscientiously expressing his conviction; for only thus can the load of

prejudice by which this subject is overwhelmed be removed.

Several eminent naturalists have of late published their belief that

a multitude of reputed species in each genus are not real species; but

that other species are real, that is, have been independently created.

This seems to me a strange conclusion to arrive at. They admit that

a multitude of forms, which till lately they themselves thought were

special creations, and which are still thus looked at by the majority of

naturalists, and which consequently have every external characteristic

feature of true species,--they admit that these have been produced by

variation, but they refuse to extend the same view to other and very

slightly different forms. Nevertheless they do not pretend that they

can define, or even conjecture, which are the created forms of life, and

which are those produced by secondary laws. They admit variation as a

vera causa in one case, they arbitrarily reject it in another, without

assigning any distinction in the two cases. The day will come when this

will be given as a curious illustration of the blindness of preconceived

opinion. These authors seem no more startled at a miraculous act of

creation than at an ordinary birth. But do they really believe that at

innumerable periods in the earth's history certain elemental atoms have

been commanded suddenly to flash into living tissues? Do they believe

that at each supposed act of creation one individual or many were

produced? Were all the infinitely numerous kinds of animals and plants

created as eggs or seed, or as full grown? and in the case of mammals,

were they created bearing the false marks of nourishment from the

mother's womb? Although naturalists very properly demand a full

explanation of every difficulty from those who believe in the mutability

of species, on their own side they ignore the whole subject of the first

appearance of species in what they consider reverent silence.

It may be asked how far I extend the doctrine of the modification of

species. The question is difficult to answer, because the more distinct

the forms are which we may consider, by so much the arguments fall away

in force. But some arguments of the greatest weight extend very far.

All the members of whole classes can be connected together by chains of

affinities, and all can be classified on the same principle, in groups

subordinate to groups. Fossil remains sometimes tend to fill up

very wide intervals between existing orders. Organs in a rudimentary

condition plainly show that an early progenitor had the organ in a

fully developed state; and this in some instances necessarily implies

an enormous amount of modification in the descendants. Throughout whole

classes various structures are formed on the same pattern, and at an

embryonic age the species closely resemble each other. Therefore I

cannot doubt that the theory of descent with modification embraces all

the members of the same class. I believe that animals have descended

from at most only four or five progenitors, and plants from an equal or

lesser number.

Analogy would lead me one step further, namely, to the belief that all

animals and plants have descended from some one prototype. But analogy

may be a deceitful guide. Nevertheless all living things have much in

common, in their chemical composition, their germinal vesicles, their

cellular structure, and their laws of growth and reproduction. We see

this even in so trifling a circumstance as that the same poison often

similarly affects plants and animals; or that the poison secreted by

the gall-fly produces monstrous growths on the wild rose or oak-tree.

Therefore I should infer from analogy that probably all the organic

beings which have ever lived on this earth have descended from some

one primordial form, into which life was first breathed. When the views

entertained in this volume on the origin of species, or when analogous

views are generally admitted, we can dimly foresee that there will be a

considerable revolution in natural history. Systematists will be able

to pursue their labours as at present; but they will not be incessantly

haunted by the shadowy doubt whether this or that form be in essence

a species. This I feel sure, and I speak after experience, will be no

slight relief. The endless disputes whether or not some fifty species

of British brambles are true species will cease. Systematists will

have only to decide (not that this will be easy) whether any form be

sufficiently constant and distinct from other forms, to be capable of

definition; and if definable, whether the differences be sufficiently

important to deserve a specific name. This latter point will become a

far more essential consideration than it is at present; for differences,

however slight, between any two forms, if not blended by intermediate

gradations, are looked at by most naturalists as sufficient to raise

both forms to the rank of species. Hereafter we shall be compelled to

acknowledge that the only distinction between species and well-marked

varieties is, that the latter are known, or believed, to be connected

at the present day by intermediate gradations, whereas species

were formerly thus connected. Hence, without quite rejecting the

consideration of the present existence of intermediate gradations

between any two forms, we shall be led to weigh more carefully and to

value higher the actual amount of difference between them. It is quite

possible that forms now generally acknowledged to be merely varieties

may hereafter be thought worthy of specific names, as with the primrose

and cowslip; and in this case scientific and common language will come

into accordance. In short, we shall have to treat species in the same

manner as those naturalists treat genera, who admit that genera are

merely artificial combinations made for convenience. This may not be a

cheering prospect; but we shall at least be freed from the vain search

for the undiscovered and undiscoverable essence of the term species.

The other and more general departments of natural history will rise

greatly in interest. The terms used by naturalists of affinity,

relationship, community of type, paternity, morphology, adaptive

characters, rudimentary and aborted organs, etc., will cease to be

metaphorical, and will have a plain signification. When we no longer

look at an organic being as a savage looks at a ship, as at something

wholly beyond his comprehension; when we regard every production of

nature as one which has had a history; when we contemplate every complex

structure and instinct as the summing up of many contrivances, each

useful to the possessor, nearly in the same way as when we look at

any great mechanical invention as the summing up of the labour, the

experience, the reason, and even the blunders of numerous workmen; when

we thus view each organic being, how far more interesting, I speak from

experience, will the study of natural history become!

A grand and almost untrodden field of inquiry will be opened, on the

causes and laws of variation, on correlation of growth, on the effects

of use and disuse, on the direct action of external conditions, and so

forth. The study of domestic productions will rise immensely in value.

A new variety raised by man will be a far more important and interesting

subject for study than one more species added to the infinitude of

already recorded species. Our classifications will come to be, as far as

they can be so made, genealogies; and will then truly give what may be

called the plan of creation. The rules for classifying will no doubt

become simpler when we have a definite object in view. We possess no

pedigrees or armorial bearings; and we have to discover and trace

the many diverging lines of descent in our natural genealogies, by

characters of any kind which have long been inherited. Rudimentary

organs will speak infallibly with respect to the nature of long-lost

structures. Species and groups of species, which are called aberrant,

and which may fancifully be called living fossils, will aid us in

forming a picture of the ancient forms of life. Embryology will reveal

to us the structure, in some degree obscured, of the prototypes of each

great class.

When we can feel assured that all the individuals of the same species,

and all the closely allied species of most genera, have within a not

very remote period descended from one parent, and have migrated

from some one birthplace; and when we better know the many means

of migration, then, by the light which geology now throws, and will

continue to throw, on former changes of climate and of the level of the

land, we shall surely be enabled to trace in an admirable manner

the former migrations of the inhabitants of the whole world. Even at

present, by comparing the differences of the inhabitants of the sea

on the opposite sides of a continent, and the nature of the various

inhabitants of that continent in relation to their apparent means of

immigration, some light can be thrown on ancient geography.

The noble science of Geology loses glory from the extreme imperfection

of the record. The crust of the earth with its embedded remains must not

be looked at as a well-filled museum, but as a poor collection made

at hazard and at rare intervals. The accumulation of each great

fossiliferous formation will be recognised as having depended on an

unusual concurrence of circumstances, and the blank intervals between

the successive stages as having been of vast duration. But we shall be

able to gauge with some security the duration of these intervals by a

comparison of the preceding and succeeding organic forms. We must be

cautious in attempting to correlate as strictly contemporaneous

two formations, which include few identical species, by the general

succession of their forms of life. As species are produced and

exterminated by slowly acting and still existing causes, and not

by miraculous acts of creation and by catastrophes; and as the most

important of all causes of organic change is one which is almost

independent of altered and perhaps suddenly altered physical conditions,

namely, the mutual relation of organism to organism,--the improvement of

one being entailing the improvement or the extermination of others; it

follows, that the amount of organic change in the fossils of consecutive

formations probably serves as a fair measure of the lapse of actual

time. A number of species, however, keeping in a body might remain for a

long period unchanged, whilst within this same period, several of these

species, by migrating into new countries and coming into competition

with foreign associates, might become modified; so that we must not

overrate the accuracy of organic change as a measure of time. During

early periods of the earth's history, when the forms of life were

probably fewer and simpler, the rate of change was probably slower; and

at the first dawn of life, when very few forms of the simplest structure

existed, the rate of change may have been slow in an extreme degree. The

whole history of the world, as at present known, although of a length

quite incomprehensible by us, will hereafter be recognised as a mere

fragment of time, compared with the ages which have elapsed since

the first creature, the progenitor of innumerable extinct and living

descendants, was created.

In the distant future I see open fields for far more important

researches. Psychology will be based on a new foundation, that of the

necessary acquirement of each mental power and capacity by gradation.

Light will be thrown on the origin of man and his history.

Authors of the highest eminence seem to be fully satisfied with the view

that each species has been independently created. To my mind it accords

better with what we know of the laws impressed on matter by the Creator,

that the production and extinction of the past and present inhabitants

of the world should have been due to secondary causes, like those

determining the birth and death of the individual. When I view all

beings not as special creations, but as the lineal descendants of some

few beings which lived long before the first bed of the Silurian system

was deposited, they seem to me to become ennobled. Judging from the

past, we may safely infer that not one living species will transmit its

unaltered likeness to a distant futurity. And of the species now living

very few will transmit progeny of any kind to a far distant futurity;

for the manner in which all organic beings are grouped, shows that the

greater number of species of each genus, and all the species of many

genera, have left no descendants, but have become utterly extinct. We

can so far take a prophetic glance into futurity as to foretel that it

will be the common and widely-spread species, belonging to the larger

and dominant groups, which will ultimately prevail and procreate new

and dominant species. As all the living forms of life are the lineal

descendants of those which lived long before the Silurian epoch, we may

feel certain that the ordinary succession by generation has never once

been broken, and that no cataclysm has desolated the whole world.

Hence we may look with some confidence to a secure future of equally

inappreciable length. And as natural selection works solely by and for

the good of each being, all corporeal and mental endowments will tend to

progress towards perfection.

It is interesting to contemplate an entangled bank, clothed with many

plants of many kinds, with birds singing on the bushes, with various

insects flitting about, and with worms crawling through the damp earth,

and to reflect that these elaborately constructed forms, so different

from each other, and dependent on each other in so complex a manner,

have all been produced by laws acting around us. These laws, taken in

the largest sense, being Growth with Reproduction; Inheritance which is

almost implied by reproduction; Variability from the indirect and direct

action of the external conditions of life, and from use and disuse; a

Ratio of Increase so high as to lead to a Struggle for Life, and as a

consequence to Natural Selection, entailing Divergence of Character and

the Extinction of less-improved forms. Thus, from the war of nature,

from famine and death, the most exalted object which we are capable

of conceiving, namely, the production of the higher animals, directly

follows. There is 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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