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On the Origin of Species
by Means of Natural Selection
or the
Preservation of Favoured Races
in the Struggle for Life

“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*.*

“The only distinct meaning of the word ‘natural’ is stated, fixed or settled, since what is natural as much requires and presupposes an intelligent agent to render it so – i.e. to effect it continually or at stated times, as what is supernatural or miraculous does to effect it for once.”

– Butler, *Analogy of Revealed Religion*.*

“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*.*

Introduction

When on board HMS *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 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 judgement.

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 co-adaptation 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 mistletoe, 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 mistletoe, 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 co-adaptations 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 co-adaptation. 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 the high geometrical ratio of their 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 leads to 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

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judgement 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.

Chapter I

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 – METHODOICAL 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 subvariety of our older cultivated plants and animals, one of the first points which strikes us is that they generally differ 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 great 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 organization 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 Saint-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 function of the reproductive system – this system appearing to be far more susceptible than any other part of the organization 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 discovered 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.

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 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 Müller 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, 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 are 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 or 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 Saint-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 worthwhile 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 subvarieties differ slightly from each other. The whole organization 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 any deviation of structure often appears, and we see it in the father and child, we cannot tell whether it may not be due to the same cause having acted 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. Everyone 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 a peculiarity in different individuals of the same species, or 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 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 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 naturalizing, 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 the same conditions, and whilst kept in a considerable body, so that free intercrossing might check, by blending together, any slight deviations in their 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 racehorses, 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 closely allied species, 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 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 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 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 bulldog, 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 natural species – for instance, of the many foxes – inhabiting different quarters of the world. I do not believe, as we shall presently see, that the whole amount of difference between the several breeds of the dog has been produced under domestication; I believe that some small part of the difference is due to their being descended from distinct species. In the case of some other domesticated species, there is presumptive, or even strong evidence that all the breeds have descended from a single wild stock.

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 reindeer, 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 wild 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. Knowing, as we do, that savages are very fond of taming animals, it seems to me unlikely, in the case of the dog genus, which is distributed in a wild state throughout the world, that since man first appeared one single species alone should have been domesticated. 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 anyone, 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 have all 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 bulldog 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 only get 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, bulldog, 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 experimented for