# LAMARCK AND MODERN GENETICS

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*by*

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F.R.S.

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*To the memory of*
*my distinguished predecessor*
SYDNEY JOHN HICKSON
*on the occasion of the centenary of his birth*

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# PREFACE

I HAVE written this book for two reasons, one pleasant and the other merely irritating. My pleasant task is to write on this 150th anniversary of the publication of Lamarck's *Philosophie Zoologique* and tell the true story of this greatest of all French biologists, the man who has with all justification been named the Founder of Evolution.

My other task arises out of the fact that a couple of years ago I wrote a book,[1] *The Evolution of Living Things*, in which I gave, as far as I was then able, the real significance of Lamarck's work and at the same time I attacked the modern Neo-Mendelian theory of genetics *as applied to the theory of evolution* and pointed out some of its more obvious inadequacies. Now during the last year there were great celebrations in connection with the centenary of the famous Darwin-Wallace meeting of the Linnean Society as a result of which the whole scientific world became evolution conscious. In addition there was the meeting of the International Congress of Zoology at which the opening address was given in honour of Darwin. At these meetings, with their adjuncts of broadcasts and publications, all my criticisms were completely ignored. But what was much worse, the name of Lamarck was traduced in a manner that it is scarcely possible to believe would occur in an enlightened scientific world. Despite the fact that some years ago I published a paper[2] 'What Lamarck really said', in which I pointed out some

of the grosser ways in which the views of Lamarck had been misrepresented in the middle of last century and the beginning of this, and despite the appearance of my book early last year, the pundits who pulled the strings behind the celebrations persisted in repeating in a most aggressive manner the calumnies that had been perpetrated against Lamarck.

Now things cannot go on like this. The orthodox geneticists are certainly very well entrenched. Their researches, admittedly brilliant, 'displayed in impressive settings of mathematical learning'[3] have impressed their physical colleagues and let us not forget that it is the physicists who rule the scientific world. But they are bound ultimately to face up to the mounting barrage of opposition which is being published against them. They cannot for ever protect themselves in their trenches of cabalistic formulae. To the average non-biologist it is of course heresy, or merely being silly, to question any of the profundities of the Neo-Mendelian. And it goes without saying that the vast number of biologists who make no pretence at being geneticists, accept without question that the Mendelian mechanism which, there is no doubt, controls the production of our domestic animals of revolting obesity and our cabbages of monstrous superfluity, must be the same as that which underlies the evolution of all the superb adaptations of wild types. But now listen to what Goldschmidt said as early as 1940. Nobody can deny his authority. In the early twenties when I had the privilege of meeting him at Bateson's house, he was one of the leading figures in the Mendelian world. After

all it is perhaps not too much to say that his work on inter-sexes paved the way for the Neo-Mendelian outburst. But now, after a long period of Mendelian research, this is what he said.[4] 'The neo-Darwinian theory of the geneticists' being 'no longer tenable' must be abolished. It is incompatible with the picture of evolution, and it accordingly 'blocks progress in evolutionary thought'. So you see I am not alone, mine is not a voice crying in the wilderness. There are other criticisms that come from most distinguished sources (see p. 113) and they cannot be ignored.

The reception of my small book has been very interesting. There have not been many reviews. The two most distinguished journals, to my surprise, published excellent reports. In the few cases where out and out geneticists took the trouble to notice and condemn the book, it is illuminating that never once did they attempt to answer any of my criticisms. They merely indicated that I was an ignorant fellow and really ought to learn something about genetics. Very well then, the present book can be looked on as a sequel to the first and it is in the form of a challenge.

In the opening chapters I have given an account from the original sources of what it was that Lamarck said just one hundred and fifty years ago and have shown what a travesty indeed is being disseminated even at the present time at all educational levels under the name of Lamarckism. Then I have considered Darwinism and shown that the only part of his hypothesis which has survived is the belief in the survival of the fittest and that, of course, is

no more Darwin's than it is anybody else's, for it is simply a truism which cannot be denied. In chapter six I have criticized the orthodox mutation-selection hypothesis of the Neo-Mendelians whose attitude nowadays appears to be that the one great point about Darwinism is that it shows that they are right in their own fantastic hypothesis! I have put my criticism in the form of certain questions which I maintain must be answered since they deal with the fundamental basis on which all the Neo-Mendelian superstructure is founded. Therein lies the challenge. Let my genetical colleagues attempt to answer my queries. If they can do so, and can show that I am completely on the wrong track, at least, we shall have progressed. Finally I have shown that whatever the answers may be to my questions, if only the leaders of the modern orthodox school of genetics would take the trouble to understand the real central theme of Lamarck's views they would then realize that the whole edifice of Neo-Mendelism is riddled through and through by true Lamarckism—a truly Gilbertian situation.

I have to thank my colleague Professor Eugène Vinaver for his authoritative advice over translating certain critical passages from the French, and Mr. T. L. Jones, Secretary to the Press, for correcting some of my passages in English. He has also given other valuable advice in the preparation of the manuscript. Once again I have to thank Gordon Blower for his persistent but stimulating criticism.

H. GRAHAM CANNON

*Chapel-en-le-Frith,*
*Jan. 1959.*

CHAPTER I

# THE MAN

How many people know that, if it had not been for a piece of tomfoolery, of sheer horse-play, we would never have heard of Lamarck the scientist. We might have heard of Général de Lamarck, a famous military commander, but not of the biologist.

Jean Baptiste Pierre Antoine de Monet, Chevalier de Lamarck,* was the eleventh and last child of his parents. He was born on August 1, 1744, in the squire's house in the little village of Bazentin-le-Petit, a district in Picardy, in the very centre of some of the grimmest episodes of trench warfare of the First World War. It lies about four miles east of Albert on the road to Bapaume and so on to Cambrai. The family of Monet was very martial-minded, and one of them, 'Etienne de Monet, esquire, second of the name', married in 1612 Jeanne de Lamarque and so the seigneury of Lamarque came into the family of the Monets. How our Lamarck came to acquire the title of Chevalier is something of a mystery. It can only be assumed that all his elder brothers died before him and somehow, by a family arrangement maybe, he was allowed to take over the family honours. However his parents being relatively poor and he being very much a

* For more detailed information the reader is referred to *Lamarck, The Founder of Evolution*, by A. S. Packard, 1901 (Longmans, London).

younger son he was entered for the Church. There seems some doubt as to whether he entered the College of the Jesuits at Amiens or whether it was the ancient college of Rue Poste de Paris. Be that as it may, in 1760 his father died and he, being now sixteen years of age, could not be persuaded to continue his clerical studies. So he chucked the Church and returned home and bought a horse, a sorry nag if all accounts are true. And he went forth, like a knight of old accompanied by a lad from the village as his esquire, to satisfy his martial desires. The French army seems to have been doing the same thing at the same time and they met at the little village of Fissingshausen between Lippstadt and Hamm, a district incidentally of some of the grimmest bombing exploits of the Second World War. Apparently through complete lack of co-ordination between the two French commanders, who were both, strangely enough, trying independently to fight the same battle, the battle was lost. However our day-old recruit, Lamarck, finding himself in a tight corner with all his officers killed, calmly took charge and ordered the troops to stand firm until orders came for them to evacuate their position. Orders did come through eventually after some very tricky scouting work and young Lamarck and his companions retired to safety. But because of the initiative that he had shown in the field he was promoted there and then to a commission.

The peace came and the regiment to which Lamarck was attached was ordered on garrison duty first to Toulon and then to Monaco. It was in the latter place that one day in a rough and tumble among the officers some lunatic

lifted up Lamarck by his head! How he did not receive more serious injury is a miracle but it is said that as the result of this fooling the glands in his neck were disturbed and became inflamed. Whether this is the true explanation or not the glands were affected and they were operated upon and Lamarck ever afterwards carried the scars of that operation. But they did not get well and Lamarck was forced to resign from the army on medical grounds and on a microscopic pension.

While he was recuperating from the effects of his operation he decided to study medicine in Paris. To enable him to do so he had to do something to eke out his meagre pension and so he took a job as a bank clerk. Now it was at this period of his life that there occurred a chance meeting that maybe changed the whole of Lamarck's life. In his spare time from his work and his studies he used to go walking round Paris and it was on these rambles that he met the great French philosopher, Jean Jacques Rousseau. Now Rousseau was a keen botanizer and he infected Lamarck. He drew him away from medicine and got him interested in science generally, but particularly in botany.

It is interesting to compare this history of events with Darwin's early life. Both Darwin and Lamarck studied medicine and for the Church, but in reverse order. Lamarck chose his own studies whereas Darwin's father chose the studies for his son. Both met distinguished scholars in their spare time rambling. Darwin met Henslow, who was to have such an impact on his mind, and Lamarck met Rousseau.

Now it is certain that Lamarck must have learned a lot more from Rousseau than mere hedgerow botany. Rousseau was interested in things in general and more particularly in the conditions of life of mankind. See what he wrote in his *Discourse on the Origin and Foundations of Inequality among Men* which he published ten years after Lamarck was born. He is discussing the conditions of life in Sparta and he says, 'Accustomed from infancy to the severity of the weather and the rigours of the seasons, trained to undergo fatigue, and obliged to defend naked and without arms their life and their prey against ferocious beasts, or to escape them by flight, the men acquired an almost invariably robust temperament; the infants *bringing into the world the strong constitution of their fathers*, and *strengthening themselves by the same kind of exercise as produced it*, have thus acquired all the vigour of which the human species is capable. Nature used them precisely as did the law of Sparta the children of her citizens. She rendered strong and robust those with a good constitution and *destroyed all the others*.' In the three passages I have italicized, the first and the second represent the ideas which Lamarck incorporated later (in 1815) into his fourth and third law of evolution. But speaking more generally, the first and third passages probably represent the first published mention of the inheritance of acquired characters and of the survival of the fittest.

Whether Rousseau was responsible or not the attraction of botany became too great and Lamarck finally abandoned medicine which was to have been his profession, the bank which at least kept the wolf from the

door, and music which was his great delight, and concentrated all his energies on the study of the French Flora. For nine or ten years he worked on this and finally produced his Flora of France in three octavo volumes. To say of him as Cuvier did that his Flora appeared after 'six months of unremitting labour' is a complete distortion—it may have taken him a whole six months merely to write by hand the final draft—but this is not the only denigration of Lamarck's work that Cuvier made, as we shall see later.

The book brought him immediate fame, and election to the French Academy came almost at once. Then the wealthy Buffon, who had already helped Lamarck by getting his Flora published at the expense of the Government and now wanted a companion for his son to escort him around foreign lands, chose Lamarck for this purpose, but not wanting him to appear as a mere tutor obtained for him a commission as Royal Botanist. This was in 1781 when Lamarck was thirty-seven years old. How he managed to live on his small *pension alimentaire* it is impossible to say but the new position at first was certainly honorary. He was a married man with several children and so must have found it excessively hard to make both ends meet.

The details of the following ten years are not of much significance. He continued his botanical work largely on the plants that he had collected on his travels and those which were continually arriving at the Royal Botanical Garden as a result of contacts he had made with botanists in other parts of Europe.

It was in 1793, the year of the 'Terror', that the Royal Garden was suppressed as being an annexe of the King's Palace. In its place there appeared the Jardin des Plantes. This was merely a change of name. At the same time it was reorganized on the basis of a memorandum which Lamarck, as a result of most unsatisfactory conditions in the establishment, had drawn up in 1790. The Museum of Natural History was established and along with it, not the six professors to teach the sciences as Lamarck had asked for, but twelve! And strange as it may seem, when these had all been handed out to the more favoured ones, there was only one left over for Lamarck and that was the Professorship in charge of Invertebrates. So that, after what in those days was an average lifetime spent on botany, he became a zoologist at the age of forty-nine!

After this, little more need be said. For the first seven years of his Professorship he published nothing zoological. It was not until the opening of the new century, as I shall explain later on, that in his course of lectures to his students he first broached the idea of evolution. This work appeared in 1801. It was not until 1809, however, that is just one hundred and fifty years ago, and fifty years before Darwin published the *Origin*, that he put before the public a logical account of the evolution of living things. But whichever publication is taken there is no doubt, as Haekel said[5] some seventy years later, 'To him will always belong the immortal glory of having for the first time worked out the Theory of Descent, as an independent scientific theory of the first order, and as the philosophical foundation for the whole science of Biology.'

Lamarck lived to a great age. His life was one of long unending poverty. No patrimony apparently came with the family title. Yet he was married four times and outlived all his wives. He had seven children, of whom three died before him. And then at an advanced age his sight failed, and for the last ten years of his life he was completely blind. We do not know the names of any of his wives or any of his children except one, Cornelie, and she looked after him in his blindness. One of the few kind things that Cuvier said was that at her first walk out into fresh air after her father's death the change was too much from the circumstances to which she had become accustomed and she was nearly overcome. Lamarck died on 18 December 1829, at the age of eighty-five years.

He was buried in a pauper's grave.

CHAPTER II

# THE EULOGY

In the French Academy, as in our own Royal and other learned societies, it is customary on the death of a Fellow to publish an account of his life and work. This has happened in some cases for centuries. But there is a subtle difference. We call our publications obituary notices and the aim of the author is always, as it should be with all obituaries, to give as objective an account of the deceased member's researches and position in the learned world as possible. Praise is given where it is merited, but if adverse criticism is called for, it is not, or should not be, omitted. In the French Academy, however, such publications are referred to as eulogies—this is something quite different. From the derivation of the word a eulogy means simply a recital in praise of the subject and nothing else.

Now the man called upon to write the eulogy on Lamarck was M. le Baron Cuvier, and a more unfortunate choice it seems difficult to imagine. Cuvier had been a close associate of Lamarck's at the Paris museum. It is even said that he owed his position in Paris partly to the support given to him by Lamarck. They must have worked together so there is no doubt that he was fully acquainted with Lamarck's views. Moreover, he was an experienced writer of obituaries. His collected eulogies ran to three volumes! Whereas generally he is fair and

kindly in his writing, in his eulogy of Lamarck this is far from being the case. He ridicules indiscriminately Lamarck's views on all the multifarious subjects which he dealt with during his long life. Certainly Lamarck's published opinions on physical and chemical subjects were even then open to adverse criticism, but it is doubtful if, even so, Cuvier was really in a position to speak on them with authority. But his particular views on evolution, these must have been well known and understood by Cuvier, and it remains inexplicable why he made such dogmatic and, as I shall show, inaccurate attacks on them. Of course he disagreed with them, for he was one of the most catastrophic of catastrophists. His views were in great favour, especially in this country, because they were supposed to give a scientific flavour to the story of Noah's flood.[6] Maybe it was because of this support that he gave to the teachings of the Church that his reputation was immense, but surely, this fact in itself should have made him very careful in attacking someone not so well known as himself with whom he happened to disagree.

We speak of Cuvier's eulogy of Lamarck; in fact, Cuvier did not read it. He died in 1830, the year after Lamarck, so that this obituary must have been one of his last publications. It was read by M. le Baron Silvestre on November 26, 1832.[7] Even then we do not know exactly what it was that Cuvier wrote and presumably Silvestre read, for the Academy 'did not allow it to be printed in the form it was pronounced'.[8] According to de Blainville, who was one of the immediate successors of Lamarck and who was without doubt present at the meeting, only

an expurgated edition was published to the world. The original must have been bad indeed. Packard, in his usual kindly manner, says[9] that it forms the only stain on Cuvier's life, and that it was unworthy of the great man. Maybe so, but it was a very large stain.

However, the eulogy as we know it, and a translation of which I shall describe shortly, caused an amount of harm to Lamarck's reputation that is hard to estimate, and which persists to the present day. The whole cause of the trouble was a misuse of words, which it is difficult to imagine as anything other than deliberate, followed by a twisting of phrases which produced a mere travesty of Lamarck's real views.

The key to the whole unfortunate affair occurs in a critical passage (see Appendix, p. 134) where Cuvier purports to be describing how in Lamarck's opinion new structures and new habits occurred in the course of time —an aspect, of course, that is of fundamental significance in the evaluation of Lamarck's evolutionary views—and this is how he puts it. 'Other needs, other desires, produced by circumstances, will lead to other efforts, which will produce other organs.'[10] Not other needs *and* other desires but just as I have stated it. Now is it possible that Cuvier did not understand that 'needs' and 'desires' are not synonymous? I am afraid not. However, the seed of ridicule having been sown, Cuvier continues by giving examples of what he pretends are Lamarck's views. Aquatic birds, he said, acquired their webbed feet by dint of wishing to swim. from continually wishing to fly, the bird developed its wings and feathers: from con-

tinually going to the water's edge but wishing to avoid wetting the body, the long-legged wading bird appeared. The whole passage is sheer distortion, but what makes matters so much worse is that Cuvier states positively, without any shadow of doubt, that in order that no one will suspect him of either adding to Lamarck's views or of suppressing anything, he has used his actual words! This, to put it mildly, is simply an inaccurate statement.

The passage he is 'paraphrasing' is perfectly clear. It occurs in Chapter VII in the first volume of Lamarck's *Philosophie Zoologique*, which is headed 'On the influence of circumstances on the actions of animals . . as causes which modify their organization . . .' To begin with, the case of the evolution of wings and feathers in birds is sheer imagination. It does not occur here, and I have been unable to find this reference elsewhere. No doubt Lamarck would have partly agreed with it. Undoubtedly he believed that the wings and feathers and other adaptations did arise through the efforts of the nascent birds to fly, but not through *wishing* to fly. Now remember Cuvier said that he was using Lamarck's actual words! However, the case of the evolution of webbed feet in some water birds, and the origin of long-legged wading birds—about these there is no doubt. Lamarck states:[11] 'The bird which necessity drives to the water to find there the prey on which it lives, spreads out the toes of its feet when it wishes to strike the water to propel itself on its surface.' And then: ' . . . the shore bird, which is not inclined to swim, and which however has the need of approaching the edge of the water there to find its prey,

is in continual danger of sinking in the mud. Now this bird, wishing to act so that its body shall not plunge into the water, makes every effort to extend and elongate its legs.' Now these are the two quotations on which all the criticism of Lamarck originally rested. Admittedly they both contain the verb 'to wish', but how else should Lamarck have expressed his ideas. In English as in French we might say—if a fish wishes to swim it wags its tail, if a bird wishes to fly it flaps its wings, and there would be no ambiguity about what was meant. We should not mean that the fish or the bird deliberately thought about the matter and acted accordingly. All the evidence suggests that a fish cannot think, and so if we were to attempt to explain the matter in strict scientific terms this would lead to such involved phrases that no one would see the wood for the trees. We should have to say something like this: 'If the conditions in the environment of the fish change in such a way that, for its own sake, it had better move off, then (always assuming that the fish does the right thing on every occasion) it wags its tail!' The whole thing becomes as absurd as unnecessary

What Cuvier did was to emphasize the verb 'to wish', which is the same as 'to desire', in all the statements he misquotes from Lamarck. In his opening remarks, he indicated quite erroneously, as I have pointed out, that desires, or wishes, were synonymous with needs. Then why did he not use the verb 'to need' instead of 'to wish'? If he had done so there would have been no cause for ridiculing Lamarck's views. The *need* for swimming might reasonably lead to the evolution of a tail, but the

wish to swim—wishful thinking—as a cause of the appearance of the tail is clearly absurd. But that is how Cuvier traduced Lamarck.

Even if we are charitable and ascribe the distortion to a genuine mistake there is no real excuse for a man in Cuvier's position. He *ought* to have known what was the real meaning of the word, for Lamarck always makes it abundantly clear what he means by '*les besoins*'—the needs —of an organism, as compared with what he meant by the 'will'. In fact he devotes a whole chapter to considering the will in relation to conscious thinking and subconscious activity, and in this chapter, as I have recently pointed out,[12] he showed an appreciation of animal behaviour far in advance of his time. *Les besoins*, he says,[13] means the necessities that are essential to the animal to allow it to compete successfully with its fellow creatures, and under no circumstances whatever does it mean the actual desires of the organism. He states, without any reservations, that those animals which have not the organ necessary for intelligence cannot have an idea, cannot execute a judgment. He is here distinguishing between man and what he calls the higher vertebrates, where there is evidence of a certain degree of choice of action. But among the vast majority of animal forms there is not even this choice. He continues: 'It follows then that these animals before acting, do not consider the matter, neither do they make any judgments before responding. The unconscious responsive system (sentiment intérieur) directly stimulated by the need, and then directed . . . by the nature of this need, puts into action the appropriate parts

of the body. *Thus the actions which result from this stimulus are not preceeded in any way by actual desire.*' Can anything be plainer than this?

Cuvier's mistranslation on this point has recently been described[14] as 'one of the scandals of science', and, especially in view of its consequences, so it is. It seems so trivial, and yet it ruined Lamarck's reputation in his own country. This however cannot be the whole story. Lamarck first put forward his views in the very opening years of the century. It was not until thirty years later that Cuvier published his damaging obituary. What had happenened in the meantime? Lamarck had been completely overlooked. As Haeckel said many years later[15]—Lamarck's views were not understood by most men, 'and for fifty years were not spoken of at all'. It must have been Cuvier's immense reputation that impressed all the rising young biologists to such an extent that Lamarck was treated almost as a joke. This is not exaggerating, for years later, at the end of the century, Lacaze-Duthiers, himself a veteran Professor at the Jardin des Plantes, wrote to Packard in America and told him that in his early days in Paris in discussing Lamarck with 'very great learned men' who had known Lamarck personally (but whose names he did not wish to write), they simply treated him as mad![16]

Amazing as it may seem to us nowadays, one of the main points held against him was that, among his multifarious interests he practised weather forecasting! He rented a top-floor flat in Paris, and from the conditions of the clouds issued predictions as to what the weather

would be. Is this not unjust? Why—every peasant in those days regarded himself as a weather prophet. Is it much different even nowadays despite our wireless propaganda? If peasants were allowed to act in this way, why should not a scientist? Lamarck was probably correct in his forecasts more often than not.

No, there is no doubt that Lamarck did not get a square deal in France, but in this country things simply went from bad to worse. In 1836 someone saw fit to publish a translation of Lamarck's eulogy, or at least what purported to be a translation. I say someone because we do not know with complete certainty who was the author —or culprit. The article is not signed. It was published in the *Edinburgh New Philosophical Journal*.[17]* This was edited by Robert Jameson, who was Regius Professor of Natural History and Keeper of the Museum in the University of Edinburgh, and it looks suspiciously as though he was responsible for the publication. All the evidence seems to point this way. Jameson, who was primarily a mineralogist, went as a young man to Freiburg to study under Werner, and Werner was one of the most confirmed catastrophists of his time. Jameson was so impressed that later, in 1808, when he was only thirty-four he founded in Edinburgh the Wernerian Natural History Society. Now one of the functions of this society was to keep the British scientific world in touch with scientific developments abroad. It is not surprising therefore that we find that between 1817 and 1827 Jameson

* I have to thank Canon C. Raven, D.Sc., for finding this reference. In Packard's book the reference is incorrectly quoted.

produced four editions of Cuvier's *Essay on the Theory of the Earth*. In this work Cuvier 'threw the whole weight of his influence into the scale of Catastrophism'.[18] It looks from this that it would have been difficult to choose a more unsuitable translator of Lamarck's eulogy than Jameson. He must have been so steeped in the ideas of the Catastrophists—and remember, at that time they were considered the orthodox scientists—that it scarcely seems possible that he could have given to the scientific world an unbiased translation of a eulogy of a man who appeared to be a complete heretic. Be that as it may, at least we do know for a certainty that Jameson *edited* the translation, and this would be a disgrace to a schoolboy. The critical phrase of Cuvier that caused all the trouble—other needs, other desires—is translated as 'other wants *and* desires' (my italics) (see Appendix, p. 134). Later on the words which correctly should be translated 'It is by dint of wanting to swim' have been rendered 'It is the desire and the attempt to swim . . .' In all that follows only the 'desire' is mentioned. The idea of needs or requirements or wants is simply not there. The mistranslation is so puerile that it seems unlikely that it was due to inexperience—especially if it was Jameson himself who was responsible for it. It looks much more probable that whoever did the translation had a vicarious knowledge of Lamarck's views on evolution, and already had preconceived ideas of what he expected to find in the eulogy. The critical passage ends with the words 'A system established on such foundations may amuse the imagination of a poet; a metaphysician may derive from it an entirely

new series of systems; but it cannot for a moment bear the examination of anyone who has dissected a hand, an entrail or even a feather.' What sarcasm, what a travesty of judgment, but also what ignorance!

It has been said recently[19] that it would be strange if generations of biologists had all mistranslated the simple word '*besoin*', but in view of what I have said I do not think this is anything to be wondered at. This translation of the eulogy, emanating as it did from such a centre of learning as Edinbugh, was probably read by the great majority of scientists. The journal in which it appeared contained articles on all sorts of topics, historical, engineering, archaeological, geological and so on. The sub-title states that 'it exhibits a view of the progressive discoveries and improvements in the sciences and the arts', and it was issued quarterly so that, without doubt, it was very popular among the intelligentsia. How many people actually read the translated eulogy it is, of course, impossible to say, but at the time of its publication it does not appear to have called forth any comment. Its real effect came later in the century, after 1859, when everybody had become evolution-conscious through the publication in that year of the *Origin of Species*, and when a dispassionate comparison between Darwin's views and those of scientists who preceded him was so important. Then it looks suspiciously as though the general biologist —and even those in high places—instead of going to the original source, obtained their information of Lamarck's views at third hand from the translated travesty, and as a result they built up a story so fantastic that, as Packard

pointed out[20] more than fifty years ago, it led in many cases to the wholesale rejection of all Lamarck's views. Perhaps the most erroneous, and most damaging story was that Lamarck believed that while plants were evolved by the direct action of the physical environment animals evolved indirectly—they only got what they wished for![21] Thus T H. Huxley in a chapter that he wrote in the *Life and Letters of Charles Darwin*[22] states that Lamarck's 'one [*sic*] suggestion as to the gradual modification of species —effort excited by change of conditions—was, on the face of it, inapplicable to the whole vegetable world.' And in a footnote to this page, presumably written by the editor, Darwin's son Francis, it is stated that really Darwin's grandfather, Erasmus was the one who first thought out all Lamarck's 'fundamental conceptions' but that he was more logical, for he applied them to plants as well. Lamarck therefore by inference did not! This absurdity we all thought nowadays was 'in the deep bosom of the ocean buried', but oh no! It has recently been revived[23] by one of the High Priests of Mendelian orthodoxy—who ought to have known better. After making the gratuitous statement that Lamarck had brought the subject of the causes of evolution into disrepute 'by their fanciful nature' he remarks that Lamarck's hypothesis is totally inapplicable to plants—presumably because they cannot think and therefore have no desires! Of course, Lamarck said nothing of the sort. To him plants just as well as animals had their wants, their requirements—call them what you will as long as you do not call them 'desires', and evolved in the same way. We should not

forget that Lamarck was an established botanist thirty years and more before he became a zoologist, and so undoubtedly based his evolutionary ideas just as much on plants as on animals.

I have now described what must have been the course of things that lead to the misunderstanding of Lamarck's views about the middle of last century when the evolutionary kettle was boiling so fast. But this cannot be the whole story because in the early thirties, in fact, shortly before Cuvier's eulogy was publicly read, there appeared an account of Lamarck's evolutionary views, which ought to have been read and understood by the scientific public. This occurred in the second volume of Lyell's *Principles of Geology*. It is very readable, and, of course, the book was an immense success. The account has been described[24] as 'the best, most catholic, and just exposition of Lamarck's views . . . and . . . still worth reading', but unfortunately it also contains an unaccountable misrepresentation. In the middle of an admirable and concise account Lyell breaks off[25] to make most stringent criticisms accusing Lamarck of 'disregard to the strict rules of induction . . and . . . resorts to fiction, as ideal as the "plastic virtue" and other phantoms of the geologists of the middle ages'! His criticism is this—according to him Lamarck said that 'organs no longer in use are impoverished and diminished in size . . . are sometimes entirely annihilated, while in their place new parts are insensibly produced for the discharge of new functions'. He complains that 'no positive fact is cited to exemplify the substitution of some *entirely new* (the italics are his!) sense, faculty or organ in

the room of some other suppressed as useless'. But Lamarck did not say this. On the page quoted by Lyell[26] is the statement 'that every new need necessitating new action to satisfy it, demands of the animal that experiences it, either (*soit*) the more frequent use of such parts as had been less used before, which develops and considerably enlarges them, or (*soit*) the use of new parts which the needs have caused insensibly to arise in it . . . .' (the animal concerned). Nowhere is there any suggestion that 'entirely new' organs arise *to take the place of organs* vestigial to extinction. How it came about that Lyell was unable to translate this simple and very clear statement is a mystery * However he only makes this one criticism and the remainder of the passage is a straightforward account of Lamarck's views, containing here and there a comment but no other criticism. Elsewhere throughout the book there are three or four passages where Lyell begs to differ from Lamarck on more or less minor points—but there is no prolonged attack on his views generally.

Lyell's real attitude to Lamarck is to be found among his letters. He was of course, to start with, a lawyer and it was in 1827 when he was returning to London from the Oxford circuit that he amused himself by 'devouring Lamarck *en voyage*' to use his own phrase. Which bit of Lamarck he does not say, but presumably it was his *Philosophie Zoologique* as his textbook on invertebrates which is the book where the fullest expressions of his

* Strange to say even Packard[27] does not render the passage correctly for he translates '*soit* *soit*' as 'both . . and'.

views is given, ran to seven volumes, and he would hardly cart round seven octavo volumes in his legal peregrinations! He says[28] he was delighted with the book more than with any novel he had read. His real trouble came when he saw that, if Lamarck's views were fully accepted, it would mean that man evolved from the anthropoid apes, or as he puts it, 'men may have come from the Ourang-Outang'. He was however so impressed that it looks as though he became an evolutionist there and then. Lamarck in his geological efforts was an uniformitarian; that is, he believed that all the physical characteristics of the earth's surface came about by continuous actions of the elements rather than by cataclysmic changes. And this, of course, was Lyell's main thesis in his famous book, so that it is not surprising that he received Lamarck's views with such enthusiastic sympathy But he did not consider it expedient to admit publicly his belief in evolution. This he felt would have done harm to the reception of his views. In this he was acting as Darwin acted later in the publication of the *Origin* where he deliberately omitted any special reference to the origin of man. What Lyell did was to write his book in such a way as to leave it to be inferred that he accepted the idea of evolution. To say, as has been said recently[29] in connection with the Darwin-Wallace Centenary, that in Lyell's book the possibility of evolution was firmly rejected is simply erroneous. In a letter in reply to one from Adam Sedgwick,[30] the elder, asking him whether he really believed in the creation of new species at the present day, he said that he did, and said that he *wished it to be inferred*

from his book 'that the substitution of new species fill the vacancies'—caused by the extinction of forms—'which must always be occurring, has also been going on'. And then he adds 'though *how* is a point we are as ignorant of as the manner of God's creating the first man'. Now all this is important historically because it is certain that Darwin owed more to this book of Lyell than to any other in laying the foundations of his evolutionary ideas. It looks as though Lyell held out that hidden bait and Darwin fell for it.

The whole story makes it all the more inexplicable why Lyell attacked Lamarck in such a violent manner on such a trifling—and misunderstood—point. It merely provided ammunition for those who wished to decry Lamarck which was seized upon with ill-concealed relish. Later, some thirty years after he had published his book, and after he had become a convert to the idea of Natural Selection, Lyell repented. Writing to Darwin[31] he said 'when I came to the conclusion that after all Lamarck was going to be shown to be right, and that we must "go the whole orang" I re-read his book, and remembering when it was written, I felt I had done him injustice'. And then even more forcibly, in another letter[32] to Darwin, he states that while he cannot go as far as Huxley in swallowing all his (Darwin's) views, hook, line and sinker, he feels how satisfactory it would be to accept Lamarck's views 'improved by yours'.

Now when one remembers the close corporation of the evolutionists of the early sixties—Lyell, Hooker, Huxley and Darwin—how they met together or corresponded

with each other daily, how they must have been so well acquainted with each other's views, one would have thought Lyell's misguided criticism might soon have been forgotten. But this was not to be—for that scientific politician, T H. Huxley, revived it in an equally misguided manner in an article[33] he wrote in 1887 'On the Reception of the *Origin of Species*'. Here, after making most disparaging remarks about what he maintains Lamarck had said in his *Philosophie Zoologique*, he states that at the time of writing half of what Lamarck had said 'was obsolete and the other half erroneous or defective . . . ' and then goes on to refer to 'Lyell's trenchant and effectual criticism'! Now, in fact, it is doubtful whether Huxley actually read Lyell's criticism. He simply *must* have studied Lyell's Principles, for it was one of the most famous scientific books of the day, but it looks suspiciously as though he skipped the passage dealing with Lamarck's views. I say this with a fair degree of certainty because the criticism he brings against Lamarck was *not* the one Lyell raised—it was the old 'wishful thinking' mistake that Cuvier and his translator made in the eulogy! At the time of writing his article Huxley states that some thirty five years previously he had 'studied Lamarck attentively'. But had he? Did he go to the original or did he merely accept the translation of the eulogy as an accurate statement of Lamarck's views? There is every reason to believe that, if he did refer to any source at all, the latter is most probably the case.

It was in 1863, that is four years after the first appearance of the *Origin*, when interest in the subject of

evolution was rising to its maximum, that Huxley gave a course of 'Six Lectures to Working Men'.[34] I suppose these must have been some of the earliest forerunners of our Workers Educational Association lectures. It is in the last of these six that he attacks Lamarckism, and from the criticism that he made there is no doubt that he must have got his ideas direct from the eulogy Thus he starts by accusing Lamarck of having said that 'an animal may be modified . . . . in consequence of its *desires* and consequent actions'. But then he continues, 'Lamarck . . . said, for example, that short-legged birds which live on fish, had been converted into long-legged waders *by desiring to get the fish without wetting their feet*, and so stretching their legs more and more through successive generations.' A more ridiculous statement it is difficult to imagine because, of course, however long or short the birds' legs might be, if they had to go to the water to catch their fish, they would get their feet wet! What Lamarck really said I have already quoted. It was that these shore-feeding birds would put all their efforts into extending their legs, in order to prevent the *body* from plunging into the water —not their feet! What Lamarck said, whether one accepts his views or not, was perfectly rational. One has only to think of such a bird as a heron standing motionless with its long legs in the water at the edge of a loch to understand Lamarck's point. Obviously, if such a bird waded in so that its body 'plunged into the water', all the fish nearby would be scared away. Huxley's mis-statement of Lamarck's views is simply a disgrace. He may, of course, have got his information from some other ignor-

ant person. In fact, it is difficult to believe that he could have referred either to the original eulogy or its translation. But, in any case, to ascribe to another scientist whose opinions are under discussion a statement which is patently absurd is inexcusable.

His mistake was soon pointed out to him, but by this time the harm was done. And that, after all, was all that apparently mattered to Huxley. He was out to damn Lamarck. In his own words[35] he said it is 'Mr. Darwin's hypothesis or nothing'! He was not going to tolerate any opposition. It was Darwin's daughter, Henrietta who, apparently some time in 1863, sent a cryptic little note to Huxley in one of the letters that her father was writing to him. In it she simply asks 'wetting *feet* (her italics) or bodies?'[36]

There is no doubt, I am afraid, that Huxley's propaganda did untold damage to Lamarck's views. But what was the attitude of the master himself, Charles Darwin? After all Huxley was only the disciple. What did Darwin think about the matter? That is a complete puzzle. In his published works whenever he does refer to Lamarck, and that is extremely rarely, he speaks of him in high terms. Thus in the historical sketch which acts as the preface to the third edition of the *Origin** he places Lamarck the first among the thirty four authors who had anticipated himself in putting forward evolutionary views,

* In fact, the 'Historical Sketch of recent progress of opinion on the *Origin of Species*' appeared first of all in the German translation of the *Origin* by H. G. Bronn and is dated as a manuscript from Darwin's house February 1860, that is only three months after the appearance in this country of the first edition.

and he refers to him as 'this justly celebrated naturalist'. He gives a short but good account of Lamarck's views and does *not* follow Cuvier or Huxley and repeat the grotesque mis-statement about wishful thinking. Moreover as was pointed out years ago, whenever his own views fell to the ground, he simply took those of Lamarck—incidentally always without acknowledgement. This is especially so, of course, in his adoption of the idea of the inheritance of acquired characters. But, despite all this, in his correspondence he is most outspoken and irritable, confining himself to generalization but condemning Lamarck outright. His criticisms I have dealt with recently, and there is no need to repeat them in detail, but the point I want to make here is that his criticisms all appear to be based on the eulogy. Now is it possible that he never actually referred to the original source of Lamarck's views? It hardly seems so. He was after all a very great invertebrate anatomist before he became the greatest evolutionist of his day. He was awarded the highest honour which the Royal Society can bestow—the Copley Medal—in 1862, that is when the *Origin* had been out for three years, but in the presentation he was admonished that this award was for his anatomical work and was *not* for his evolutionary views! His reputation was based largely on a long monograph on barnacles that he wrote, and it is indeed a magnificent work. It is in many respects still a standard work on this group of animals. Now he must have referred during the course of this work to the greatest contemporary textbook on the invertebrates which, of course, was written by Lamarck. It is here, as I shall explain later,

that Lamarck gives the fullest expression, although not the best, of his views. It was published originally in seven volumes, and later in 1835–45 in eleven volumes, just when Darwin was in the throes of his barnacle work. It is, of course, possible that Darwin, who hated big books, did not refer to the actual volume in which the laws of evolution are stated. In one of his letters to Lyell he refers to reading Lamarck's book, but does not say which book. Presumably it was his *Philosophie Zoologique*, but he says that he got 'not a fact or idea' from 'the extremely poor' work.[37]

Now as I have said all his direct criticisms of Lamarck, that is, those cases where he raises specific points, deal with the absurd wishful thinking idea. He asks, for example,[38] how can a man of Lamarck's status suggest 'that insects which never see their eggs, should *will* . to be of particular forms' and so on. Incidentally why insects never see their eggs, or why such a fact if true would be of any significance in this connection, is one of those little profundities that we find scattered through Darwin's legacy to us. However all this comes from the misrepresentations in the eulogy. But now, just to complicate matters, in a letter to Huxley, Darwin[39] accuses Lamarck of getting all his ideas from his, Darwin's own grandfather, Erasmus. He points out in a most unpleasant manner that Lamarck first published his ideas in 1809,* while Erasmus Darwin's *Zoonomia* appeared in 1794, and further that *Zoonomia* was translated into many languages

* He could not even get the title of the book right for he refers to the *Hist. Zoolog.*—there is no such book!

and then leaves one to draw one's own conclusion! Mine is that Lamarck most probably never heard of Erasmus Darwin. After all *Zoonomia* was an enormous medical treatise. However, these opinions which Lamarck is supposed to have stolen all deal with the inheritance of acquired characters; that is what nowadays is almost universally taken to represent the whole of Lamarckism. Now this aspect *is not mentioned in the eulogy*, a fact which I feel is appreciated by very few poeple today. It does however follow from this that Darwin must have got at least some of his ideas of what Lamarck said from sources other than the eulogy

Of course he may have relied simply on hearsay But there is one thing we know for certain, and that is that on his voyage round the world he received the second volume of Lyell's *Principles* which contains the summary of Lamarck's views on evolution. Darwin was given a copy of the first volume by his mentor, Henslow, at Cambridge just before he left England, but he was warned to pay little attention to it. In fact, if it had not been for this book, it is possible that Darwin would only be remembered as an authority on barnacles! Now we know that by the time Darwin reached S. America in the *Beagle* he had become an ardent geologist—and this after giving up the subject as hopeless after his experience at both Cambridge and Edinburgh. It is only natural therefore that, on arriving at Monte Video in 1832,[40] he received with great enthusiasm the second volume of Lyell which was awaiting him there. Now it is here that Lamarck's views are put forward in such an admirable

and concise style. Is it possible that Darwin omitted to read this part of the book? At first sight one would say—no—but in the autobiography recently published by Darwin's granddaughter we are given for the first time a statement in Darwin's own words which throws extraordinary light on his attitude of mind.

It is in connection with the unfortunate quarrel between Darwin and Samuel Butler over the publication of a translation of a life of Erasmus Darwin. In this article there appeared a foreword by Charles himself which suggests, although it does not say so positively, that in the translation nothing has been added to or subtracted from the original—rather, in fact, like Cuvier's statement about using Lamarck's actual words! It turned out that in the course of translation the original biography had been edited, and added to considerably and, what made matters worse, adversely to Butler. This naturally annoyed Butler, and he wrote to Darwin about it. Darwin was equally annoyed that his good faith should be called in doubt, but whatever were the merits or demerits of the case it led to some illuminating correspondence—or rather to some letters which the Darwin family conclave suppressed. It was the first of these letters which Darwin drafted to the Editor of the *Athenaeum*, to which I refer.[41] This was vetoed by the family 'for reasons that turned mainly on saving' . . . Darwin's . . . dignity'.[42] More particularly one paragraph was objected to. This refers to Samuel Butler's book *Evolution Old and New* in which, of course, there is a review of the evolutionary opinions of Buffon, Erasmus Darwin, Lamarck and Charles Darwin himself, that is

certainly not in favour of the latter. Darwin says in his letter, 'I did not even look at the part about evolution.'* This is bad enough for the whole book is about evolution, but worse follows. He continues that he did not read this '*for I had found in his former work that I could not make his views harmonize with what I knew*'. No wonder the family objected to it! Here in Darwin's own words is an unsolicited admission that he did not read things with which he knew that he would disagree. Such a confession cannot be and should not be overlooked, and, if he could do this about Samuel Butler, is this not also the obvious explanation of his reactions to Lamarck. He simply did not read him because he did not want to. After all, Darwin was in a very sheltered position. He was wealthy, he held no official appointment, he did not have to worry about promotion, so why should he worry? Also, it should be remembered he was human, although in this centenary year, from the writings of some of the hagiographers, one would hardly think so. Fortunately his granddaughter takes a realistic view of his character. She says that he 'craved for approbation,'[44] and there is no doubt that his family encouraged him in this little weakness, and so naturally he did not like criticism, and did not like being anticipated in his views. There seems no other explanation of his utterly illogical attitude to Lamarck. He praises him publicly, and damns him privately. He

* This however did not prevent him sending a copy of the book to Dr. Krause who was the nigger in the woodpile in the Darwin-Butler quarrel, and telling him 'not to expend much powder and shot on Mr. Butler, for he really is not worth it. His work is merely ephemeral.'[43]

scorns his supposed 'wishful thinking' ideas, and yet his real views on the inheritance of acquired characters he takes to himself without acknowledgment. Clearly he never studied Lamarck seriously, and must have relied simply on misguided gossip. Whatever may be the real truth, it is very galling to remember that it was Charles Darwin himself who said[45] 'Great is the power of steady misrepresentation' and then added 'but the history of science shows, how fortunately, this power does not long endure'!

CHAPTER III

# THE INHERITANCE OF ACQUIRED CHARACTERS

On July 1, 1858, at a meeting of the Linnean Society of London, Alfred Russell Wallace communicated a paper[46] 'On the Tendency of Varieties to depart indefinitely from the Original Types'. At the same meeting, Darwin submitted his paper[47] 'On the Variation of Organic Beings in a State of Nature', and this was the beginning of Evolution for Everyman. Darwin's paper, as might be expected, is difficult to follow, but then his style could never be described as sparkling. Wallace's essays stands out for its absolute clarity. There is a minimum use of words, and what words he does use are clear and to the point. He goes on all right until near the end of the paper when he talks about Lamarck; here he repeats the Cuvierian mistake. He says,[48] 'Neither did the giraffe acquire its long neck by desiring to reach the foliage of the more lofty shrubs.'*

In 1912 Judd, writing on the history of *The Coming of Evolution*,[49] and Doncaster, that most meticulous of workers, writing a general book on *Heredity*,[50] both deal with Lamarck's views by emphasizing that the real

* Maybe Wallace knew his bible better than his Lamarck and remembered Matthew VI. 27, 'Which of you by taking thought can add one cubit unto his stature?'

essence of his hypothesis is a firm belief in the inheritance of acquired characters. Then they both take as an example the neck of a giraffe. Lamarck, they said, believed that the giraffe elongated its neck by stretching it to reach the higher branches of the trees and this elongation was inherited and so increased in successive generations.

All three, Wallace, Judd and Doncaster, were wrong in referring to the *neck* of the giraffe. As usual Lamarck was being misquoted, and this could easily have been seen if any of them had read Lamarck in the original text. As I have explained at length in *The Evolution of Living Things*, it was the front legs which Lamarck referred to primarily, not the neck; besides which, nobody can stretch his neck! I shall have occasion to refer to this again in more detail. What I want to emphasize here is that these two misstatements, Wallace's on the one hand and Judd's and Doncaster's on the other, represent the two aspects of Lamarckian criticism, ancient and modern. The ancient 'wishful thinking' travesty that Wallace used, the origin of which I have described in the last chapter, died a natural death towards the end of the century, except for the revival that I have mentioned (see Chapter II, p. 18), when it was resurrected in this centenary year as a little partisan propaganda for Darwin. However, that slip does not seem to have created much of a disturbance, and will no doubt soon be forgotten. The modern travesty is now, and has been ever since the beginning of the century, the almost universal belief that the *only thing* that Lamarck believed in was the inherited effects of use and disuse. That was the view which I was taught as a student over forty

years ago, and I can say from experience that that was the only aspect of Lamarckism which was considered in the early twenties when Bateson was still alive and we had those splendid discussions on the subject—especially good if they were enlivened by the presence of MacBride in one of his more explosive moods. Later on I shall show how this confining of Lamarckism to just one view that he expressed did as much harm to his reputation as the absurd mistranslations which led to the wishful thinking idea. But the extraordinary thing was that while there *were* full accounts of the whole of Lamarck's views, nobody referred to them. There was a complete life of Lamarck and a study of his views written by Packard and published in this country and America in 1901. This book now is unfortunately very difficult to come by. But Dendy who was Professor of Zoology at King's College, London, brought out a second edition of his *Evolutionary Biology* in 1923, that is just at the time when we were all in the thick of the 'Lamarckian' discussion, and this contains a complete and concise statement of all of Lamarck's views. And yet nobody seemed to refer to them.

This misconception, this belittling of Lamarck's work, is nowhere more evident than in a verbatim edition of the first edition of the *Origin of Species*, which was brought out a few years ago, and which contains an introduction that an Oxford Professor saw fit to write. After referring[51] to 'the discarded and disreputable theory of Lamarck' he continues by referring to it[52] as that 'ancient superstition of the inheritance of acquired characters'! He maintains that Darwin at first did not believe in this stupid nonsense,

and only later did he succumb to 'Lamarckian' heresy. And then he even blames him for having the courage to change his views! Now in fact it looks suspiciously as though the Professor had not only not read his Lamarck but it is doubtful whether he had read his Darwin! For in the *first* edition of the *Origin*—that is the one that was being reproduced verbatim—there occurs on page nine[53] the passage where Darwin points out that the leg bones of the domestic duck are proportionately more massive and the wing bones less so than in the wild form, and says that this 'may be safely attributed to the domestic duck flying much less, and walking much more, than its wild parent'. There can be no case here of Darwin's idea of Nature picking out chance variations because in the barnyard Natural Selection is not at work. It is human selection, and surely domestic ducks are not selected by their breeder on account of the size of their legs.

No, without doubt Darwin was a believer in the inheritance of acquired characters, that is, according to our contemporary idea, a 'Lamarckian' at the time of the first publication of the *Origin*. But there is evidence that he was thinking in this direction even earlier. In a short abstract of his theory that he wrote in 1842—an essay of thirty-five pages—he points out in the first paragraph[54] that an organism placed in a new environment sometimes varies in a variety of ways, among others, in its habits. 'Also habits of life develop certain parts. Disuse atrophies.' This is the old idea of the blacksmith's arm that has never been challenged—the more it is used the bigger and stronger it becomes. In the original manuscript he wrote

after the words just quoted, 'Most of these slight variations tend to become hereditary', and then crossed this out. However a dozen or so pages on[55] he wrote, 'It must I think be admitted that habits, whether congenital or acquired . .', then he wrote 'sometimes' and crossed this out and ended up with 'often', become inherited'. From this it is safe to assume that even in these early days he had strong 'Lamarckian' tendencies.

But (as so often happens when we try to find out what was really at the back of Darwin's mind) there is a reference[56] in between the two passages just quoted that only seems to confuse the issue. In a paragraph dealing with selection he ends up, without any apparent reference to what he had just written, with the sentence, 'Introduce here contrast with Lamarck—absurdity of habit or chance or external conditions, making a woodpecker adapted to a tree.' He seems to be condemning not only Lamarck's views but his own reliance on fortuitous selection of variations which looms so large in his hypothesis. As Professor Thompson said recently,[57] and so truly, Darwin was not interested 'in the exact definition of the terms he used'.

Whatever views Darwin held before 1859 there is no doubt that as edition after edition of the *Origin* appeared he became a firm believer in what is generally accepted as Lamarckism. For in the sixth edition there appears a new chapter inserted between the original chapters six and seven, and in this—referring to the evolution of the giraffe—he explains[58] how according to his own theory it could have been brought about, and then adds the words

'combined no doubt in a most important manner with the inherited effects of the increased use of parts'. He thus not only came to believe in what is nowadays considered to represent Lamarckism, but attributed to it an important part in the process of evolution.

There is no doubt, despite all the obscurities of Darwin's writings, that his views *did* alter considerably as regards the inheritance of acquired characters. And why not? Darwin had the courage to change his views which, after all, is surely the sign of an intelligent man. But Darlington,[59] apparently for this reason dubs him 'not by any means what is now called an intellectual. He did not often have bright ideas.' Maybe not, but when he did they shook the world.

It was this change in outlook that in 1868 caused Darwin to put forward his theory of Pangenesis. He felt that he had to produce some concrete mechanism to explain how acquired characters, that is effects produced during the lifetime of an organism by its own activities, could possibly affect the germ cells so that those characters would appear in the next generation. And so he conceived pangenes. These were purely hypothetical particles that were given off by every organ in the blood-stream. Then if an organ altered by use *or* disuse, its pangenes would alter accordingly, and so when these, in the course of their cirulation around the body, reached the gonads they would register a change in the germ cells. Lamarck has recently been accused[60] of bringing the subject of evolution into disrepute by the fanciful nature of his views. But could anything be more fanciful than this idea of

Pangenesis? The whole conception was soon abandoned, and is now only of historical interest.

However, this idea of the pangenes was a definite *ad hoc* hypothesis to support the belief in the inherited effects of use and disuse and the essence of the scheme was a continuity between all parts of the body and the germ cells, so that changes in distant parts would be recorded on the germ cells via the blood-stream. Now in 1885, for exactly the opposite reason, that is to show the impossibility of the inheritance of acquired characters, Weismann put forward his Continuity of the Germplasm and the essence of this scheme was that there is complete *discontinuity* between the germ cells and all the rest of the body! It was an absurd situation, and it is difficult to see how any serious-minded person would come to believe in Weismann's fantasies, but believe they did!

Weismann stressed the idea that the germ cells were isolated from the rest of the body, and this conception gained support from the embryological studies that were becoming so fashionable just about that time. It is a fact that in many embryos the germ cells, that is the primordial germ cells which later on in life will form the adult gonads, are separated from the rest of the body at a very early stage. They appear as a small group of a few cells which remains intact and separate from all the rest of the body while this is going through its development to lead to all the specializations necessary to carry the animal up to sexual maturity. It is only when this stage is reached that the small group of primordial cells becomes active again and starts to multiply. It does look as though the

germ cells are sometimes in a certain degree autonomous, and it was probably this that gave rise to the extraordinary idea of the isolation of the germ cells.*

However the idea has its devotees even today. Only last year one of our senior zoologists,[62] describing Weismann's ideas, said, 'the gonad, though resident in the body and dependent on it for food and maintenance, is otherwise independent; it may be regarded as a parasite on the soma. This relationship of soma and gonad, *undeniable when once pointed out* (my italics), made the possibility of any form of Lamarckian inheritance very difficult to credit.' But is it undeniable? Replace the word gonad by 'spleen' or 'kidney' and see how absurdly it reads. Of course there is no isolation. A gonad is no more isolated than any other separate organ. As Martin[63] states: 'The Weismannian absolute gulf between somatoplasm and germplasm is certainly an intellectual artifact corresponding to nothing that exists in nature'. Clearly every organ in the body *must* have its own blood supply, so that every organ is continuous with every other part of the body. How the idea ever caught on is a complete mystery, but catch on it did, and from that time 'the almost universal disbelief among biologists in Lamarckian inheritance dates'. Apart from its impact on Lamarckian principles, how anyone nowadays with our wide knowledge

* When asked by Professor E. B. Poulton what it was that first put the idea of the continuity of the germplasm in his mind 'He replied that when he was working upon the germ cells of Hydrozoa he came to realize that he was dealing with material which—early and late in the history of the individual—was most carefully preserved, as though it were of the most essential importance for the species.'[61]

of internal secretion can solemnly suggest that gonads are in any way isolated is something more than a mystery!

However, Weismann's ideas produced a profound effect. More particularly they lead to an absurd spate of experimentation—experiments to prove (or more often disprove) the possibility of the inheritance of acquired characters. They were commenced by Weismann himself, who cut the tails off mice as soon as they were born for generation after generation and showed that the experiments did not produce a Manx mouse. Of all the idiotic experiments—as Giard, a staunch supporter of Lamarckian tradition, is reputed to have said—one might just as well study the inheritance of a wooden leg! As Martin said[64] with such pungency—such experiments 'are puerile in conception'. Let us leave it at that.

Fortunately few workers were found to follow the absurd example set by Weismann, but nevertheless experiments *were* carried out and they were all of the same type. An animal was placed from its normal surroundings, which we may call environment *A*, into a new environment *B* and kept there. If now it changed, either itself or in the course of successive generations, and that change was something that could be measured and hence recorded, then the experiment could go on. After establishing the new character—the 'acquired character'—the animal was put back into its normal environment *A* to see if the new induced character would persist. If now it did persist Lamarckism was said to be proved, but if on the other hand it disappeared, then Lamarckism was shown to be without foundation. This was the logic of the ex-

periments, but of course it was all false. MacBride who, after all, was one of the greatest culprits in his partisanship of 'Lamarckism', was the one who first pointed out the utter absurdity of this experimentation. At a meeting of the British Association in 1916 at Newcastle-upon-Tyne, when he was President of the Zoological section, that is some years *before* he was bitten by the 'Lamarckian' bug, listen to what he said:[65] 'If a change from one environment to another causes a change in one generation, a change in the opposite direction should be sufficient to reverse it in an equal amount of time.' Is not this obvious? It stands to reason that the character would disappear when the animal was returned to its normal environment, that is, if the environment had any control whatever over the characters! Of course, if the character produced happened to be an induced Mendelian mutation, that is something which we nowadays know is so easy to produce, then the results would be different. The character would persist and be inherited according to the simple Mendelian Law.

Now this is just what happened. In some experiments[66] that were being carried out on the inheritance of a black variety of a certain moth it was found that by feeding the larvae of the moth on their normal food which had been sprayed with a solution containing manganese, incidentally a sort of contamination that one might expect in any industrial smoke, the resulting moths which turned up were the black varieties. This was an induced variation, a mutation, call it what you will, but it was inherited! Well do I remember the excitement of MacBride—I was

his assistant at the time—when he heard of this work. Here was the experimental proof that he had been looking for. But well do I remember also when he heard later that this new character behaved in a Mendelian fashion. To think that he had given his blessing to something that was merely Mendelian was indeed a sore disappointment to that great man.

Now the so-called Lamarckian experiments were of two kinds. The first were those experiments of the type I have just described in which the disturbing condition, the factor which caused the appearance of the so-called 'acquired character', bore no relation to the *normal* life of the experimental animals. All sorts of intricate experiments have been carried out along these lines.* Thus animals have been subjected to injection of substances of excruciating complexity which bear no relation whatever to the metabolism of the unfortunate subject and if, as a result, a new character turns up then the animals were used for further experiments. But there is no need to go further than the experiments I have already mentioned—the melanin induced through the action of certain chemicals. I mention this case especially because of the song that is being made about it by the Neo-Mendelians. Here we have a case of an abnormal agent, a manganese salt, merely causing the animal to mutate and produce a black variety. It is abnormal in the sense that it is one of those products that has been produced by man in his industrial activities. And this of course is abnormal to a wild animal.

* For a further account of this type of experiment, see Medawar, P. B. (1957), *The Uniqueness of the Individual* (London, Methuen).

If man had never become civilized and developed his factories, there would never have been a deposit of manganese on the leaves of the trees in the industrial areas, and hence there would not have been melanic varieties in those localities (see also later, p. 100).

No—this type of experiment is nowadays well understood and has no Lamarckian significance. Certainly the modern Neo-Mendelians who claim this experiment as one of the greatest pillars of support for their own theory would not deny this. It is the other type of experiment that is most significant, the experiment in which the disturbing condition is one which the animal might expect to meet, or at least would not be surprised to meet, in its normal activities. Thus an animal might be caused to carry out a lot more muscular activity than normal to see whether its particular muscles were increased in size. If so then the inheritance of this might be studied. A more successful type of experiment was to change the food of the experimental animal. This is obviously a normal change. Thus the gooseberry saw fly larva normally only feeds on gooseberry but, as some of us know to our cost, when it has eaten all the gooseberries it will start on the currants. Here is the type of experiment that MacBride used when he experimentally caused the stick insect to feed on ivy rather than on its normal food, privet.

Now strange to say it was this second more reasonable type of experiment that led to the ultimate disaster—to the state of affairs that enabled Darlington, quite illogically, to refer to 'Lamarckism' as disreputable. The culprit was of course, the unfortunate Kammerer. He certainly

was guilty of carrying out some Weismannian mutilation experiments. He cut off the siphons of sea squirts to see what inherited effect this would produce in subsequent generations. He maintained that the regenerated siphons became larger than normal, and that this increased length was inherited. This claim, like most of his work, was subsequently shown by Munro Fox in a very accurate series of experiments to be without foundation. However, one of his more reasonable experiments was to keep the yellow and black salamander on different coloured backgrounds, particularly yellow and black, to see if he could change the colour of the animal. He mantained that the yellow colour did increase the yellowness, and that this was inherited, and similarly with a black background. But of course when his animals were subsequently transferred to normal conditions the experimental change gradually disappeared. It seems now at this length of time difficult to understand how any zoologist of the time took the trouble to contradict Kammerer, but they most certainly did. It was however another experiment, much more logically reasonable, that lead to his final defeat. He worked on the mid-wife toad *Alytes obstetricans*. This toad unlike our normal toad, breeds on land rather than in the water. Now in our toad as in our frog the male has a black horny pad on the thumb and the wrist that functions in holding on to the female in the nuptial embrace. In the wet conditions of breeding under water the skin would appear to be very slippery and the horny pad serves to provide friction, so that the forearms of the male clasped round the female shall not slip. In the case of *Alytes*, how-

ever, this is unnecessary as the skin is merely moist and not slippery. Hence *Alytes* do not exhibit the nuptial callosity. Now Kammerer maintained that by forcing *Alytes* to breed under water it developed the horny pad and that this was inherited in subsequent generations.

The true story of Kammerer's experiments is so little known and what has been published is so partisan that I I feel that it is only right that I should record what I know of the trouble. Kammerer's *Alytes* work was first published in a short paper just before the First World War. Now what is not well known, and what was certainly overlooked by most people after that war, was that Kammerer maintained that the new acquired character, the horny pad, which he had induced, was inherited in a Mendelian fashion, but what is much more significant, in what was then a unique variation of the ordinary Mendelian principles. This naturally interested Bateson, who was then the leading geneticist in the country, if not in the world. So he decided after some unsatisfactory correspondence to go to Vienna to see the experimental animals for himself. He went there, but was unable to make contact with Kammerer. Always there were excuses. Then the war came, and Bateson was unable to pursue the matter further. Immediately peace came again Bateson took up the matter where he left off, but always with the same result. Kammerer eluded him. So he wrote an account[67] of what happened in *Nature* and washed his hands of the matter. In the meantime MacBride had become the ardent partisan for what he, like other people, thought represented Lamarckism,

namely the inheritance of acquired characters, and either at his instigation or at least with his support, Kammerer was invited to this country to lecture on his work. It was just about this time that the Austrian currency collapsed, and so naturally it was an invitation that Kammerer could scarcely refuse. He accepted and said he would bring his specimen of *Alytes* with him. This he did, and I was present in MacBride's room at the old Royal College of Science—T. H. Huxley's old department—when he arrived. The only other person present was Bateson. Kammerer produced the specimen in a glass tube, and MacBride glanced at it, literally for about a couple of seconds, and then handed it to me. I took it across to Bateson, who inspected it with a hand lens and then said under his breath to me, 'It looks to me like a spot of black ink'! He was, of course, referring to the supposed black horny pad which was said to have been produced expermentally. And how right he was only came out some months later when it was shown in America that the specimen was in fact a forgery. The nuptial callosity *was* indian ink.

Looking back on the whole affair it seems so sordid. And so it was, but the remarkable fact was that no one seemed to notice, or if they did they said nothing about it, that the obvious fault of the whole story was, not so much the forgery, as that Kammerer said openly that he had brought his specimen with him—his single specimen! Where were all the others? If he had in fact carried out an experiment which lead to Mendelian results, did not this imply that there must have been a number of treated

animals, a number large enough to yield significant results. It was quite wrong of MacBride to publish to the world[68] that Kammerer visited England, 'bringing his critical specimens with him'. There were not specimens, there was one single specimen. There was no excuse. All the animals which were said to have inherited the experimentally produced acquired character could have been preserved immediately after having been used for breeding. If this had been pointed out at the time surely Kammerer would have been bound to have refused to visit us. It seems impossible that MacBride did not see this point at the time, but he was so partisan that he made excuses for anything that supported his fanatical belief in 'Lamarckism', even after the final tragedy of Kammerer's death.

In a recent review[69] of the situation Martin, the distinguished anatomist from McGill, has pointed out that among modern Neo-Mendelians it is firmly believed that it has been demonstrated experimentally beyond all possibility of doubt that an acquired character . . . cannot become hereditary And then he points out that, when one looks into the matter, such evidence is not to be found. If what I have explained is accepted, then this is not surprising. All experiments devised to establish the point one way or another are doomed to failure *ab initio*, and 'are just so much waste of time'.[70] Martin however does give an excellent account of a variety of experiments, which, by playing on the normal environment of the animal, produced modifications—acquired characters—which then linger on for a varying amount of time but in ever diminishing degree when the animals are returned

to their normal environment. He calls these lingering modifications, a translation of the term first put forward by Jollos in 1921 in his experimental work on *Paramecium*. But these results are of no interest to the out and out 'Lamarckian'. He is not interested in ephemeral characters. What MacBride was after was to induce a new character in an organism, a new adaptive character, which would be permanent under any conditions. Frankly that is how he considered evolution to have taken place. If he had succeeded in his experiments then he would have considered that Lamarck had been vindicated. But that only showed his ignorance of Lamarck's views. So let us see what it was that Lamarck really said.

CHAPTER IV

# WHAT LAMARCK REALLY SAID

IT is a fact which cannot be denied that Lamarck was himself partly to blame for the misunderstanding of his views that arose and still persists. For the way in which he presented his final views on evolution in 1815 was just asking for trouble. It must be remembered of course that he had by then passed the normal expectation of life of three score years and ten. He wrote remarkably clearly for his age, but he did not see that, in enumerating his laws of evolution in a totally different scheme from that which he had adopted in his *Philosophie Zoologique* six years previously, he was very much confusing matters.

It was in 1809 in his *Philosophie Zoologique* that he summarized his evolutionary views as two laws. The first of these[71] may be translated as 'In every animal that is not senile, the more frequent and sustained use of any organ gradually strengthens this organ, develops it and makes it larger and gives it a strength proportional to the length of such use; while the constant lack of use of such an organ imperceptibly weakens it, deteriorates it, progressively diminishes its faculties and finishes by causing it to disappear.'

The opening sentence of this law is a good example of the sometimes irritating but always meticulous style of Lamarck's writing. It is quite unnecessary to confuse the

issue by starting off with this. All sorts of other provisos might have been added. He might have started by saying, 'in every animal in a normal state of health' and so on. However, the rest of the law is easy to follow There is no doubt what he is driving at. It is the simple case of the blacksmith's arm. He saw, what is commonplace, that a blacksmith by using his arms continually from day to day develops very large arm muscles. But then he generalized on this and said that all organs if used much enlarge and, although this is the one part of Lamarck's hypothesis which is never denied, it is strictly incorrect. Many, in fact most, organs do not enlarge however frequently they are used. People with a very high rate of heart-beat do not necessarily have enlarged hearts or wider blood vessels. People who chew gum do not have enlarged salivary glands. Organs will certainly strengthen after continual use, if by strengthen is meant become more efficient. Thus the passage of a nerve impulse along a nerve makes that nerve more efficient from the point of view of sending subsequent messages down it. But there is no increase in size. An interesting example is the case of the sensitivity of the finger-tips of compositors of the old days when type was set up by hand. They developed an extraordinary power of being able to identify a minute isolated piece of type just by touching its type face. But their skins did not become thicker. They remained thin and pliable. This inaccurate generalization may seem a trivial point, but it becomes important when considering Lamarck's final four laws which he published later.

He follows this law by the more famous, or ought I to

say notorious, law of the inheritance of acquired characters. 'Everything that nature has caused individuals to acquire or lose by the influence of the circumstances to which their race finds itself exposed for a long time, and consequently, by the predominant use of any organ or by that of the continued lack of use of any such part; it preserves by heredity in those forms which arise from such individuals. . .' It is ponderously put, but there is no doubt about its implication. With its frillings removed it is simply a confession of faith in the inherited effects of use and disuse.

Lamarck was sixty-five when he published these laws. Six years later he again published his views in the introduction to the first volume of his *Invertebrate Zoology*, but this time as four laws.[72] Again his extreme caution in enunciating these laws did not add to their clarity, but they are better than his first attempt and they can be briefly stated as follows:

*First Law*

Life, by its own force, tends continually to increase the volume of every living body and to extend the dimensions of its parts, up to a limit which it imposes.

*Second Law*

The production of a new organ in an animal body results from a new need (*besoin*) which continues to make itself felt, and from a new movement that this need brings about and maintains.

*Third Law*

The development and effectiveness of organs are proportional to the use of those organs.

*Fourth Law*

Everything acquired or changed during an individual's lifetime is preserved by heredity and transmitted to that individual's progeny.

From this it is obvious that his first and second laws of 1809 become his third and fourth laws respectively of 1815. No wonder there was a muddle!

The 1815 first law can be dismissed briefly. Lamarck believed[73] that growth in an animal was due to the movements of its body fluids. In minute animalcules the 'movements of the body fluids are slow and sluggish', and so these never attain a large size. But 'according as the fluids of this little body are accelerated, so the force of life will grow in proportion', and a larger body becomes possible. There will thus be a progressive increase in size as more and more complex forms arise. The law in fact follows from his third law. If activity leads to increase in size then increased activity will lead *a fortiori* to increased size. It is a necessary corollary of the third law, and hence as a separate law redundant. While Lamarck's reasoning may well be wrong, his conclusion is without doubt correct. There has been on the whole a progressive increase in the size of organisms as one progresses up the branches of the evolutionary tree. Within any one group the ancestral types are nearly always the smallest. This is, in

fact, a very useful generalization, but Lamarck himself, after enunciating this law, leaves it severely alone and we can do the same.

It is the second law[74] which forms the great addition to his theory—the law which says that if the relation between the organism and environment demands the appearance of a new organ then the organ will appear. *This is the essence of real Lamarckism.* However, Lamarck states[75] that 'The foundation of this law derives its proof from the third', which he says cannot be disputed. But this is not so. The third law states that organs increase in size and effectiveness with use, and so, says Lamarck, one can be sure that the same forces which control this increase in size[76] 'would necessarily give birth to the organ adapted to satisfy' the new need. But can one be sure of this? Surely this is the stumbling-block for most people, at least those who take Lamarckism seriously. Everybody appears to accept this third law because it is easy to envisage how an acting organ can increase itself. The works are there, and so by use they can become more powerful and efficient. But when the works are not there it is harder to picture how new organs can originate, and to many of our present-day geneticists the whole idea smacks of mysticism or at least vitalism. But let us remember that such a process does not involve any greater stretch of the imagination than is required to accept the modern orthodox theory which leaves everything to chance!

Now if Lamarck had said that the third law depended on the second he would have been nearer the truth. Surely if a new want leads to the appearance of a new

structure, that structure will appear in definite relation to the magnitude of that want. Further, the use of that new organ will be in the same relationship. The organism will not use the organ more than the need requires. Hence the power behind the second law, whatever it may be, will cause the appearance of the new organ *to just the right degree*, and so the third law becomes really dependent on the second law and not the reverse as Lamarck suggests.

But what about the fourth law, the cause of all the trouble? This states that whatever appears during the lifetime of an individual will be passed on to the next generation. Now this acquired character has been produced through the action of the second law, or if you like, the combined second and third laws. Whatever the environment demands, or rather, whatever the interaction of organism with environment demands, will appear. But, when that organism reproduces, the environment for all intents and purposes will be the same from one generation to the next. Hence the demands of the environment on the progeny will be just the same as on the adult. Now let us remember that those demands, the positive effects of the environment on the organism, will commence to make themselves felt *immediately the offspring develops an individuality*. Thus consider first of all the simple case of an asexually reproducing organism such as Hydra. Directly it begins to push out its body wall to form a bud the environment will act on this bud in just the same way as on the parent. Now clearly there is no essential difference between this case and that of sexually produced forms: when their gametes are set free they

will emerge into the same environment as the parents which produce them and so come under the same influence. Hence when spermatozoon meets ovum and a zygote, a new individual, is formed, this offspring will be affected *immediately*, from its very inception, in exactly the same manner as its parents. Does it not follow therefore inexorably that whatever the parents acquired through the influence of the environment *must*, for the same reasoning, appear in the offspring. *Whether the new character is considered to have been inherited or not is completely beside the point.* In other words the notorious fourth law is redundant. It is completely unnecessary, as any effects that it might produce, were it true, would be produced already by the powers behind the second law

I find that there is remarkable reluctance among biologists, especially anatomists, to accept this argument. While some are prepared to agree with Lamarck's second law, or at least they see what he is driving at, they don't like to see the inheritance of acquired characters thrown overboard. They base their protest largely on the well-known phenomenon of the precocious appearance of certain types of what are taken to be acquired characters. Thus the thickening of the soles of the feet appears in the human embryo *in utero*. The callosities that occur on the sternum and on the pubis of the ostrich appear in the late embryos, that is, before hatching. Obviously these epidermal thickenings are correlated with the rubbing of the skin on the hard ground, and there is plenty of experimental evidence for this, so they may be considered as acquired characters. But it has been suggested that the fact

that they occur precociously is positive proof of their inheritance. But is it? If a character is acquired at a particular time in adult life, if the only force acting on the organism was that behind Lamarck's fourth law, then that character would appear in the offspring *at that same particular time.* But it does not—it appears earlier. What is it that makes it appear earlier? Surely it must be some other force altogether. Once this is admitted, clearly there is no need to call in the hypothetical force behind the inheritance of acquired characters. The callosities are required in adult life. Therefore they appear. The fact that they appear before they are necessary is really beside the point, for it is reasonable to deduce that the precocious appearance is an advantage and even a necessity, in which case, it will be Lamarck's second law which controls the phenomenon and not the fourth.*

Now all this is Lamarck's real tragedy He did not see that by enunciating his belief in the inherited effects of use and disuse as a separate law he drew attention away from the central theme. This led to a belittling of his ideas, to a false conception of the magnitude of his hypothesis.

* In a book published in 1957 Medawar[77] has ignored all recent criticism and has restated the usual mistake that defines Lamarckism as being a belief in the inheritance of acquired characters. But he has gone further, he has added that acquired characters appear precociously. This is entirely without authority. As far as I can see Lamarck never mentioned the point. Certainly it does not occur in the enunciation of his four laws of 1815. Unfortunately Medawar publishes these two mis-statements as two separate inserts in small print as though they were quotations and, in the absence of any reference, the reader is left to deduce that they are from Lamarck. This is not so, they are entirely Medawar's irresponsibility.

It was not so to begin with. Originally, as I have explained, criticism of Lamarck was a result of faulty reading. But nowadays, and since the opening of the century, it is the result of the absence of reading. Despite the fact that there are plenty of easily accessible accounts of the complete hypothesis of Lamarck, modern writers persist in confining Lamarckism to just that part of his views which I have persistently shown to be redundant. And so it is not surprising that at all educational levels it has become almost an article of faith to treat the whole matter as something rather ridiculous.

I have given an account of how Lamarck published his ideas to the world in his two greatest books of 1809 and 1815. But it was, in fact, earlier than all this, at the very opening of the century, in the introductory lecture of his course on the Invertebrates which he delivered in the Museum of Natural History, that he presented to the world the idea of the evolution of living things. The lecture was given in late April or in the first part of May in 1800, and it forms the preface to his *Système des Animaux sans Vertèbres* which appeared in 1801.[78] Here for the first time in the history of biology, as Packard so rightly remarks,[79] we have the essential principles of evolution put forward in a broad scientific manner. More than this, there is nothing missing in this first account, The essentials of his hypothesis are all there. Having read this there is scarcely need to go further. In fact it is even possible that Cuvier never took the trouble to read anything of Lamarck's except this first treatise—that must have so shocked his 'Catastrophic conscience'—for the

passage that he so scandalously mistranslated in the eulogy occurs here in all essentials.

In 1802 Lamarck once again published an introductory lecture to his course on the Invertebrates in a book[80] *Recherches sur l'organisation des Corps vivants* . . . and here he repeats all that he had published the year before. He emphasizes his ideas, but as far as I can see adds nothing of significance. These two books should be combined together because in them he puts forward for the first time two generalizations far in advance of his time.

But to return to the major thesis of Lamarck, his second law, this is unequivocally stated in these two earlier writings without any law being laid down. Thus in 1802 he states:[81] 'It is not the organs . of an animal which give rise to its habits and its particular abilities, but it is, on the contrary, its habits, its manner of life . . . which have, with time, brought about the form of the body, the number and condition of its organs and finally, the abilities that it possesses.' This inclusion of the words 'the number . of its organs' clearly shows that, at the earliest time that Lamarck put forward his evolutionary views he believed in the origination and not merely the development and extension of new organs in relation to new requirements. He makes his views absolutely clear in 1809 where he states that when an animal is urged to any particular action, ' . . . the organs which could carry out this are immediately stimulated. The result is that the repetition of these acts strengthen, extend, develop *and even create the organs that are necessary.*'

And so we may sum up what it was that Lamarck really

said. His first law is simply a small part of his third law unnecessarily split off. But his third law itself depends on his second while his fourth law is meaningless in the face of this same second law. Thus the one law—the expressions of belief in the inheritance of acquired characters—the main basis of the contumely to which Lamarck is subjected by the vast majority of modern biologists, this law, if only the decriers would take the trouble to read what else it was that Lamarck wrote, and believed in, would be found to be redundant. What an absurd situation!

Now having got to this point it is possible to consider further the calumny made by Darwin in which he insinuated that really Lamarck got all his ideas from his—Charles Darwin's—grandfather, Erasmus. Incidentally this false charge is made much more positively in *Darwin's Life and Letters* which was edited by his son Francis. It occurs also in a chapter written by T. H. Huxley in a footnote[82] which baldly states: 'Erasmus Darwin first promulgated Lamarck's fundamental conceptions. . . .' Whether Francis Darwin or Huxley was responsible for this utterly misleading insertion it is now, of course, impossible to say. But the fact is that, while Erasmus Darwin and Lamarck both believed in the inheritance of acquired characters, on the really essential part of Lamarckism, his second law, they are diametrically opposed.

I have already referred to the letter in which Charles Darwin suggested that Lamarck was guilty of plagiarism. In addition to this, however, he made the veiled suggestion publicly in the Historical Sketch that he wrote to the sixth edition of the *Origin*. There,[83] in a footnote, he

states: 'It is curious how largely my grandfather, Dr. Erasmus Darwin, anticipated the views and erroneous grounds of opinion of Lamarck in his *Zoonomia* . . . published in 1794.' Notice that he says 'the views . . ' and not some or one of the views, but by implication *all* the views. Fortunately he gives the page reference. He refers to 'vol. I, pp. 500–10'.[84]

Now this occurs in the middle of a large chapter on 'Generation' Here Erasmus describes what he considers to be the course of sexual reproduction. Briefly put, he considers[85] that the female produces 'a certain quantity of nutritive particles' among which is inserted a 'living filament' from the male, and then he goes on to describe how he considers this composite mass develops into the foetus. This leads him on to his idea of evolution, and the passage in which he enunciates this is too good to miss. He says,[86] 'From thus meditating on the great similarity of the structures of the warm-blooded animals, and at the same time of the great change they undergo both before and after their nativity; and by considering in how minute a portion of time many of the changes of animals . . . have been produced; would it be too bold to imagine, that in the great length of time, since the earth began to exist, perhaps millions of ages before the commencement of the history of mankind, would it be too bold to imagine, that all warm-blooded animals have arisen from one living filament which THE GREAT FIRST CAUSE endowed with animality, with the power of acquiring new parts, attended with new propensities, directed by irritations, sensations, volitions and associations; and thus possessing

the faculty of continuing to improve by its own inherent activity, and of delivering down those improvements by generation to its posterity, world without end!' This is rhetoric indeed; yet, if the complete chapter is read, it is obvious that Erasmus treated the whole matter as something incidental to the main theme, something almost trivial. He was much more concerned with the actual process of embryology. In the synopsis at the beginning of the chapter[87] he includes the whole vast idea in the phrase, 'All warm-blooded animals derived from one living filament', and that is all, and this alongside such headings as 'Eggs with double yolks', 'Male pigeon gives milk' and 'Snails and worms are hermaphrodite, yet cannot impregnate themselves'! Little did he realize how much controversy would be caused by his innocent essay Even in the summary[88] at the end of the chapter he seems to have been more concerned with the fact that the idea of 'the gradual generation of all things seems to have been as familiar to ancient philosophers as to the modern ones'.

However, in this general statement of the idea of evolution there is little, as far as I can see, with which Charles Darwin would disagree, and let us not forget that it was the *erroneous* views of Lamarck that he suggested derived from his grandfather, so that presumably he considered Erasmus just as bad. The popular idea would be that he would disagree with the last sentence, '. . . and of delivering down these improvements by generation to its posterity . . .', because this involved a simple belief in the inheritance of acquired characters, but this, as I have shown, held out no terrors to Charles. He accepted the

idea and continually relied on it. It is another passage, in the pages he quoted, that probably influenced his attitude to Lamarck. It runs as follows:[89] '. . . from their first rudiment . to the termination of their lives, all animals undergo perpetual transformations; which are in part produced by their own exertions in consequence of their desires and aversions, of their pleasures and their pains, or of irritations, or of associations; and many of these acquired forms or propensities are transmitted to their posterity ' Here is Lamarck's fourth law true enough, but completely transmogrified by the 'pleasures' and 'pains', the 'desires' and 'aversions', into the wishful thinking caricature about which I have spoken at such great length. It seems to me that this paragraph was probably the cause of all the trouble. Darwin having read what his grandfather wrote, but never having read Lamarck seriously and only knowing from hearsay that he also believed in the inheritance of acquired characters, quite erroneously assumed that Lamarck accepted the contemporary but fanciful idea of the acquired characters being acquired through 'desires and aversions'.

In fact, if Charles Darwin had had any sound idea of Lamarck's views he would have seen that his grandfather, in the chapter from which he quotes, but three pages before those he enumerates, made a statement which, far from anticipating Lamarck's views is diametrically opposed to them. Here it is stated[90] that an organ must have *preceded* the power to use it. Thus 'the lungs must be previously formed before their exertion to obtain fresh air can exist' Again a little further on 'with every new

change, therefore, of organic form, or addition of organic parts, I suppose a new kind of irritability or of sensibility to be produced'. That is, the organ comes first, the irritability follows. Now compare this with Lamarck's positive affirmation which I have already quoted more fully 'it is not the organs . . of an animal which have given rise to its habits . . . but . . on the contrary its habits, its manner of life . . . which have brought about . . . the number and condition of its organs' The two statements are poles apart.

Now surely it must be presumed that Charles read the whole chapter from which he quotes, and not merely the ten pages which he enumerates, and so he must have seen this passage. There is, as far as I can see, only one explanation as to why it meant nothing to him, it was because he only had the vaguest idea of what Lamarck really said, and even that from hearsay, not from the original source; and so we may leave an unsavoury subject.

I said a few pages back that in Lamarck's earlier writings he made two fundamental generalizations. He stresses first that the study of both animal and plant life shows that there is a gradation from the structurally simplest up to the most complex. But then he adds emphatically that this does not mean that there is a linear series, one existing form being ancestral to the next higher up in the chain. There is no such thing as a series of 'missing links'. What a pity it is that our Victorian ancestors did not know of this critical passage! It is the *constitution* of the groups which form the levels in the evolutionary series. At each level in relation to different

habits and habitats there will be, to use his own words, 'many lateral ramifications, whose extremities offer truly isolated points.'[91] In other words he put forward in all clarity the modern conception of the evolutionary tree, the ultimate twigs representing the existing forms and all the rest just bygone ancestral types.

Secondly, in a remarkable passage he shows, although he does not mention any definite principle, that he had a very clear idea of Natural Selection. He points out[92] that owing to the great rate of reproduction and multiplication of the invertebrates, if it were not for the fact that these lower forms on the whole constitute the food of 'the first classes and first orders' or the animal kingdom, the latter would simply be wiped out. But, he says, Nature has anticipated the dangerous effects of this 'vast power . She has prevented it on the one hand by limiting considerably the duration of life of the simpler forms of life, and on the other by making these animals the prey of each other. Further, he remarks that each type of animal will have its own particular environment, so that if it fails to reach this, it will succumb. 'By means of these wise precautions of nature everything is well balanced and in order.'[93]

Later he amplifies this statement and points out that in the competition among living forms it is 'the strongest and best armed' that come out top. But the most convincing evidence that he clearly saw the action of Natural Selection and the survival of the fittest is that he continues 'only man . . . seems capable of multiplying indefinitely, because his intelligence and his resources secure

him from seeing his increase arrested by the voracity of any animal'. It has been complained that Lamarck, having so clearly and precisely anticipated Darwin and all the others, spoils the effect by not pursuing the matter further. But one has only to read the whole of these passages to realize that he saw no reason for further elaborating them. They seemed to him so obvious. He merely accepted them as simple truisms for the further elaboration of his views.

He looked upon Nature as one dynamic system that was controlled by this interaction of animals and plants, of large animals and small animals, of predators and prey—by what we would nowadays call Natural Selection. He said in 1801[94] 'By means of these wise precautions of nature everything is well balanced and in order.' To him the organism was one part of an equilibrium. It was one side of a balance, and was meaningless unless the counterpoise could be defined. And if that counterpoise changed then the organism adjusted itself accordingly and brought itself back again into balance. But Lamarck saw further that these changes to which the organism responded could not have been violent. He had no time for catastrophes. He summarized his views in a chapter 'On fossils' which he included at the end of his *System of Invertebrates* of 1801.[95] Here he maintains that throughout the great length of geological time the physical environment remained practically the same in general, but that there were continual gradual changes which produced corresponding gradual changes in the habitat, and it was these that upset the balance and caused the organisms to modify themselves

accordingly. But, especially from his later writings, it is clear that he did not consider the animal balanced simply against its external environment, its habitat. Its own structure and habits came into the picture. If one part of the animal varied then this was to be considered as a disturbance of the balance in respect of the rest of the body, and as a result, the rest of the body varied accordingly. It was simply the whole organism that varied in the appropriate manner. There is really no question as to whether the habits came first or the organs, and this is another example of how Lamarck so confused matters by talking at length on things which were really beside the point. As Samuel Butler put it so neatly[96], they must be 'considered as Siamese twins begotten together, conceived together, born together and inseparable always from each other'. But to appreciate this fully in the light of our modern knowledge we must leave it and return to it again after we have discussed its relation to the modern orthodox theory of genetics.

CHAPTER V

# DARWINISM AND THE ORTHODOX THEORY

THE trouble is to find the orthodox view We may take Julian Huxley's *Modern Synthesis*[97] and we come across the statement '. . . it may be legitimately argued that the majority of all inherited characters must rest on a Mendelian basis'. Unfortunately he does not tell us how the minority of characters are inherited, neither does he give us any indication as to what type of character is included in this minority On the other hand Ford[98] only last year published the unequivocal statement that 'the characters studied by geneticists do in fact differ fundamentally from those which appear to be responsible for evolutionary progress in nature'. This may account for the attitude of Waddington who in a broadcast,[99] after asserting positively that the characters, that is all the characters or an organism, are due to the genes that they carry, has to call in to his assistance a mysterious power, 'an harmonious control', which sees that the course of evolution does not run off the rails. But then to confuse matters, Darlington,[100] in a review of three books by authors who had had the temerity to write on evolution, states with somewhat sarcastic pungency that of course inheritance is not entirely the responsibility of the nucleus!

During this last year, as I have already stated, we have

been celebrating the centenary of the meeting of the Linnean Society at which the joint communication of Darwin and Wallace was read, which had the effect of bringing to the notice of the scientific world the idea of evolution which had been enunciated fifty years previously by Lamarck. In connection with these celebrations, Sir Gavin de Beer has written and broadcast several articles; surely we are justified in taking these to represent the latest orthodox view. de Beer does not mince matters. He states[101] pontifically that modern genetical research has 'established *as firmly as Newton's laws of motion* (my italics) that hereditary resemblances are determined by discrete particles, the genes, situated in the chromosomes of the cells. . . .' Then with amazing ignorance of what Lamarck really said, he condemns him outright while he lauds Darwin to the skies. His laudation however consists almost entirely of pointing out that the real merit of Darwinism is that it establishes the fact that the modern orthodox theory of the gene complex is correct. Or perhaps more accurately it is a case of the spirits of Darwinism and Neo-Mendelism reciprocally patting each other on the back. The present chapter consists essentially of a consideration of the justness of de Beer's claims.

It is clear throughout his articles that de Beer does not yet realize the omnipotence of the Neo-Mendelian fabric. It allows for all eventualities and this idea is nothing new. In 1940 that great negro biologist E. E. Just said,[102] 'The gene theory . . . gives us a set of demons, the genes, capable of extricating the theorist from any difficult situa-

tion. . .' de Beer in fact, makes this point very clear when, after reiterating that modern genetical work does nothing but confirm Darwin's ideas, he adds,[103] 'even if new formulations are needed as knowledge increases'! There in that last sentence you have the modern method. If something cannot be explained at once, then simply make a few assumptions, and *from the nature of the original Mendelian hypothesis* it is always possible to make them fit the occasion.

His main argument is that the modern orthodox theory of Neo-Mendelism proves that Darwin was right when he postulated the existence of Natural Selection as an essential factor in the evolutionary process. But, of course, the action of Natural Selection is completely independent of the way in which the varieties arise on which Nature makes its selections. Animals may vary according to the Neo-Mendelian idea, but also they may vary in the true Lamarckian sense. But even if they vary through an act of special creation there will always be some organisms which will be more fit and some less fit than their fellows. And it is on these differences that Natural Selection will act. And then, as long as there *is* competition between the organisms, there will be the survival of the fittest. That is what Darwin and Wallace, and many others, pointed out There is nothing complicated about it, because it is a simple truism, and surely a truism needs no proof. But even if proof were considered necessary, how the elaboration of any particular mode of inheritance, be it Neo-Mendelian or any other, can possibly provide that proof it is difficult to say *Even if there were no heredity*, and like

did *not* beget like, there would still be Natural Selection and the survival of the fittest! If a pig gave birth to a litter of porcupines there would still be competition between the baby porcupines and other piglets to see which was fittest to survive.

One of the strangest aspects of contemporary evolutionary work is the immense amount of thought and argument and experiment to prove the existence of Natural Selection. The amount of excruciating mathematics that has been published on the problem is as amazing as it is unnecessary. The survival of the fittest in the struggle for existence is something which cannot be denied,* not of course for the individual organism, but, as long as it is large enough, for the species or group being considered. The critical point is that there must be the struggle for existence, there must be competition. Now if all the experimental work had been carried out to prove positively that there *is* competition, the matter would be more understandable. But no, the existence of the competition is taken for granted and denied by no one.

This attitude is nowhere more marked than in connection with the problem of the spread of industrial melanism which I have already mentioned (see p. 41). This has been described[104] as 'the most striking evolutionary phenomenon ever actually witnessed'. And yet what is all the

* According to ancient Japanese mythology, the Karashishi, the Korean Lion or Dog-Fo was said to cast its offspring over the edge of a precipice. Those which survived this ordeal were considered fit to live! So that to the Japanese at least the idea of the survival of the fittest was commonplace.

fuss about? It is as I have stated simply that man through his industrial activities has produced a lot of smoke that settles on the neighbouring trees and local moths feed on those trees. Now this smoke contains two things, metallic salts and soot. The former, as was shown experimentally many years ago, may be capable of inducing a sudden mutation producing blackness in moths which may imbibe them with their food. The local moths cannot help doing this in their normal feeding, and so numbers of them go black. But now the other constituent of the smoke, the soot, makes the trees much blacker and hence the darker the moth the more it is hidden against its normal background and hence the more it is protected from its enemies. Is it surprising therefore that Nature selects the dark varieties for survival? It would be surprising if it were otherwise (see however later, p. 100).

But the modern geneticist gives a tremendous importance to this very simple phenomenon. There was a time, says de Beer,[105] when the expression 'survival of the fittest' was considered to be tautological because 'the fittest' could only be determined as those which survived. But he continues, this objection 'melts like snow before the objective and positive proof' provided by these experiments. For, he says, they show on the contrary that it is on the average not the fittest which survive but it is the unfittest which die! What the argument is behind this extraordinary statement it is difficult to imagine. But no amount of argument will alter the fact that the survival of the fittest by means of natural selection *is* a truism. The expression 'survival of the fittest' *is* tautological, and why

not? If it is useful to us in our consideration of evolution then let us use it. And moreover the whole idea is not the property of Darwin nor of the Neo-Mendelians.* Neither of them has any more right to claim the idea of the survival of the fittest as their own than any particular mathematician can lay claim to the elementary problems of Euclid—or to the idea that the shortest distance between two points is a straight line! And so let us leave Natural Selection and see what else of Darwin's hypothesis has survived to the present day.

According to de Beer[108] the only part of Darwin's hypothesis that has to be abandoned, in view of the illuminating effect of the modern orthodox view of genetics, is his reliance on the 'effects of use and disuse as a source of variation'. In fact, as I shall show later, it is probably the only ingredient of Darwinism that has survived in the modern Neo-Mendelian cauldron.

The first major point in Darwin's hypothesis, and one incidentally that is rarely mentioned, is that Darwin

* Professor Osborn in *From the Greeks to Darwin* (1894) refers to the obvious appreciation of the power of Natural Selection which Tennyson shows in his *In Memoriam*, the dedication of which *precedes the 'Origin' by ten years.*

> Nature red in tooth and claw
> With .....

and

> .... finding that of fifty seeds
> She often brings but one to bear.

When taxed about the matter the aged Tennyson replied, 'The fact is that long before Darwin's work appeared these ideas were known and talked about.'[106] Incidentally it was Wood-Jones who said, 'I still cling to the belief that whoever wins to a great scientific truth will find a poet before him in the quest.'[107]

believed in blending inheritance.* He believed of course that if two different varieties are crossed together the offspring are always intermediate between the two parents. And it must be remembered that this is fundamental to his ideas. de Beer suggests[109] that Darwin was not in a position to know better. Mendelism was only rediscovered eighteen years after Darwin had died. And yet is it not surprising that, after all the experimenting that Darwin did about which we hear such a lot, he did not stumble on the idea of dominant and recessive characters? After all, Mendel did. What is much more significant is that Darwin did have before him definite evidence of Mendelian segregation, but did not see its significance. In his book[110] on animals and plants under domestication, he refers in four places to the results that Andrew Knight obtained in various crossing experiments. These were published in the Philosophical Transactions of the Royal Society of London in 1799. Now Darwin mentions that Knight crossed two sorts of field pea, one large one small, reciprocally, and found that the resultant hybrids were all tall. He does not mention that Knight further pointed out that the hybrids resembled each other in the smallest detail and also that often in pea hybrids two sorts of seeds corresponding to the two parent forms occur side by side in the same shell, *but no intermediate forms* occur. Perhaps he never read this part of the paper. He does however mention that Knight crossed a peach with an almond and obtained fruit which was typically peach. Surely with all this

* Why de Beer does not include this as one of the things to be abandoned in Darwin's theory remains a puzzle.

evidence in front of him, it is amazing that he apparently never attempted to repeat these experiments, but more significantly, never saw the fallacy of blending inheritance, at least as regards the inheritance of pronounced mutations. He described the results as an example of the 'stimulative effects of crossing breeds'—an entertaining prophecy of 'hybrid vigour'—but he apparently did not see how this phenomenon could affect his hypothesis.

Now as regards the inheritance of small continuous variations, this is of course absolutely essential to Darwin's hypothesis. Darwin considered that these small fluctuations *were* inherited but, what is just as important, that the offspring showed the same range of variation as the parents. Thus suppose that two individuals, which were a small but definite amount taller than the average, mated and produced offspring, Darwin maintained that the chances were even that they would also produce offspring which were that same definite amount taller than themselves and so on through succeeding generations, thereby leading to increased stature with, of course, Natural Selection acting as the controlling influence and ultimately the brake. Galton however maintained that he could show experimentally that this was not so,[111] and it was this mathematical attack on Darwinism that was used so effectively to deride Darwin in the early days of Mendelism. The slogan in those days was—what an experiment shows to be wrong must be wrong. Today the Neo-Mendelians think differently. It is convenient to overlook the work of the earlier biometricians, whether true or not, and assume that these small variations which

were so significant to Darwin's ideas *must* be inherited after all. Then all that is necessary is to devise a Neo-Mendelian 'explanation' and the matter is settled. This attitude was broadcast to the nation without any ambiguity only a few years ago by one of our leading geneticists.[112]

As regards the nature of these small continuous variations, it is commonly stated that Darwin took the view that they were completely haphazard. They occurred without rhyme or reason in all directions and it was this reliance on chance rather than destiny that brought such coals of fire on Darwin's head. However this belief has been taken up by the Neo-Mendelian in a completely uncompromising manner. Thus Waddington states definitely that 'new hereditary changes occur spontaneously and at random'. de Beer[113] says 'mutation and variation are random and undirected'. But now it requires a very large stretch of the imagination to reconcile this proposition with what we know of the amazing complexity of adaptation of both animals and plants. de Beer[114] however finds no difficulty He apparently bases his argument, incredible as it may seem, on an estimate made by Muller of the number of mutations required to transform an amoeba into a horse! He points out that this is an amazingly large number, which is hardly surprising. However he says there is no need to worry, for the most improbable things come to pass. Thus it is very improbable that any man alive today will have a son, who will have a son, who will have a son and so on for a hundred generations. And yet every man alive today had a father,

who had a father, who had a father and so on for a hundred generations. Why de Beer stops at one hundred I do not know, because surely this argument goes back to Adam and beyond! In other words because everybody has had a continuous series of paternal ancestors stretching back to the mists of antiquity, we are asked to believe that this is evidence that the evolution of the most complex adaptation can take place by chance variation. If ever there was a scientific red herring, it is this!

However, de Beer goes further.[115] He states 'that the operation of natural selection cannot be regarded as the results of chance; on the contrary, it is vigorously determined by multiple conditions which, although more complex than the laws which govern the movement of the heavenly bodies, are no less exact'. Here he is merely repeating with similar extravagant hyperbole the utterly misleading argument put forward by T. H. Huxley[116] to counter the criticisms of the mid-Victorians who protested against the impropriety of Darwin's hypothesis, or as he put it, the singular and perhaps immortal fallacy 'which charges Mr. Darwin with having attempted to reinstate the old pagan goddess, Chance'. This occurs in the article that he wrote 'on the reception of the *Origin of Species*.' His argument is to ask the critics what they mean by chance. 'Do they believe that anything in this universe happens without reason or without cause?' Naturally the answer is 'no' and so presumably there is no such thing as chance! According to Huxley's argument gambling by means of spinning a coin would not be a game of chance

because the movement of a rotating coin through the air is controlled by clearly defined physical forces! I am emphasizing this passage because it again illustrates the extent to which T. H. Huxley was preparing to go in his partisan propaganda for Darwin.

The whole argument is of course beside the point. What both Huxley and de Beer say *is* theoretically correct. But natural selection, especially as Darwin envisaged it, was far from theoretical—it was a very practical matter. Now no one will deny that when a spermatozoon meets an egg, it moves towards that egg and finally unites with it, always under the influence of definite physico-chemical forces. But we all know what we mean when we say that it is pure chance which spermatozoon fertilizes any particular egg. And nobody, certainly not the Neo-Mendelians, will deny that the characters that appear in the offspring are a result of that chance meeting. And natural selection will act on those characters. To say that this is not a matter of chance is simply playing with words.

But now, after all this, did Darwin really believe that evolution took place solely through chance variation as do the Neo-Mendelians? Maybe he did originally but when he came to publish the sixth edition of the *Origin* he certainly did not. This was in 1872, that is thirteen years after the first appearance of the book, so that it is reasonable to deduce that any alterations represent Darwin's mature and well considered opinions. He introduced a new Chapter VII and this contains the famous passage[117] where he discusses the evolution of the giraffe, the passage

incidentally in which he does not mince matters about calling in the aid of the inheritance of acquired characters. In this he refers to the reproduction of offspring inheriting the same bodily peculiarities as their parents or '*with a tendency to vary again in the same manner*' (my italics). There is no ambiguity about this. Animals, according to Darwin, did not *only* vary 'spontaneously and at random' as our Neo-Mendelians would have us believe, but might have tendencies, that is something inherent coming from within the organism, to vary continuously along definite functional lines such as, for example, might lead to the progressive elongation of the fore limbs of a giraffe. But now listen to de Beer,[118] '— . . . all theories . . . which seek to explain evolution as a result of control of the direction in which mutation occurs, by the supposed effects of . . hypothetical "inner urges", are negatived by mathematical demonstration confirmed by experimental evidence' So you see that here is another point of major significance where, far from supporting each other, Darwinism and the modern orthodox theory are poles apart.*

But now to return to this major problem of explaining the evolution of complex adaptations. This has, at long last, come to tax the ingenuity of the Neo-Mendelians. It also gave Darwin furiously to think. It is of course central to the whole idea of evolution and it is amazing

* Of course, one has only to postulate the existence of a gene (turning up at random, needless to say) that directs future mutation in a particular direction (for genes are omnipotent, after all) and the whole of my argument falls to the ground!

that Darwin treated it so lightly. The difficulty is, in the face of these random variations, to explain how the amazing correlations between the different organs of a body, or the different parts of a mechanism, can come about. Darwin at times seems to consider correlations almost frivolously as what we would nowadays describe as examples of linkage of non-functional characters, or of pleiotropisms, and nothing else. But in other places he treats the evolution of functioning mechanisms in a manner which is completely illogical. It cuts right across his own hypothesis and is completely opposed to the modern Neo-Mendelian view.

To attempt to understand Darwin's argument fully it must be remembered that he considered Natural Selection as being of such power that it acted on extremely minute variations. 'Insensibly fine gradations' were the words[119] he used in the first edition of the *Origin*. In later editions he significantly omitted the word 'insensibly', but even so there is no doubt what great power he attributed to selection and in this he is in full accord with the modern orthodox view.

There is no question about this, but when it comes to Darwin's attitude as to whether or not all characters are to be considered as functional, then we are at a complete loss to know what it was that he really thought. Thus he stated categorically in the first edition of the *Origin*[120] that 'every detail of structure in every living creature must be of use to the organism or must have been so to one of its ancestors'. But then in the sixth edition he contradicts this. He says,[121] 'I fully admit that many structures are

now of no use to their possessors, *and may never have been of any use to their progenitors.*'*

Now all this is of critical importance if we are to pass a fair judgment on the value of Darwin's evolutionary ideas, for there is no doubt that on this point he made a fundamental mistake. He did not distinguish between the correlated characters that *must* occur in all adaptations and the correlated characters that *may* occur without the slightest functional significance to the adaptation with which they are associated. Thus he considers[122] 'quite whimsical' the fact that white cats with blue eyes may be deaf, but from his argument it appears certain that he did not realize for example the complete correlation that *must* occur between the front and hind limbs of those same cats.

He does not appear to have had any sound idea about the functional relationship that occurs between the various constituents of any adaptation. Thus a gland will have its duct of a particular size, it will have a blood supply, it will have a certain proportion of glandular secreting cells. Or take a muscle, it will always have its antagonistic fellow and also like the gland will require a particular blood supply, and so on. Now all these constituents will *ex hypothesi* (*his* hypothesis) vary and so through the sorting

* 26 lines later he appears to contradict himself again for he says 'but with these important exceptions, we may conclude that the structure of every living creature either now is, or was formerly, of some direct or indirect use to its possessor' Unfortunately he does not make clear what are the 'important exceptions' The only cases he deals with in the 26 lines are structures that *have* been of functional importance to ancestral types!

and sifting of Natural Selection each will be present in its particular optimum strength or size in relation to all the other constituents, so that collectively, they will produce the adaptation. Clearly then the variation of any one constituent, without the corresponding variation of the remainder of the complex, must lead to a decrease in efficiency and, of course, this will be pounced upon by natural selection for extinction.

There is no question about the inadequacy of Darwin's views on this point. Thus in 1875 he says,[123] 'We can seldom say which of two correlated parts first varies' Now this is not objectionable if it refers only to non-functional characters. But then just previously in the same book he states:[124] 'We do not know that the initial . . variations of the several parts were in any way related: slight modifications or individual differences may have been preserved, first in one and then in another part, until the final and perfectly co-adapted structure was acquired'. Here there is no doubt that he is considering a correlated functional system since he refers to co-adaptation. But three years earlier in the sixth edition he drops an even bigger brick. He states[125] 'Although every part of the body varies slightly, it does not follow that the necessary parts always vary in the right direction and to the right degree'—but it *does* follow—according to his hypothesis!

This weakness is nowhere more evident than in the famous passage where he is describing the evolution of the giraffe through the agency of natural selection.* Thus

* Of course as Wood-Jones points out[126] the average female giraffe is *two feet* (not a mere inch or so) shorter than the male, and so, were

he states[127] that among the nascent giraffes 'those individuals which had some one part or several parts of their bodies rather more elongated than usual would generally have survived'. That is where he makes the mistake. Then he continues, 'These will have intercrossed and left offspring . . . whilst the individual, less favoured in the same respects, will have been most liable to perish.' Again the reverse of what he said is, *according to his own hypothesis*, the truth. Surely if the nascent giraffe has had all the proportions of its body sifted and settled by natural selection then any deviation from this optimum condition of any one of them must lead to a less efficient form. Thus the ancestral type that turned up by chance with longer legs, but not a corresponding longer neck, would be at a disadvantage when it came to drinking water off the ground. Similarly an animal with a longer neck would be hampered because of the mere extra weight of the neck if the front limbs were not proportionately enlarged to support the bigger neck at the same time. Hence it will be just these hypothetical animals which vary in one direction and not in another correlated direction that will be inefficient, and so will be exterminated and leave no offspring!

And now finally, as regards the question of Darwin's belief in the inheritance of acquired characters: this, says de Beer,[128] is the only part of his hypothesis that has had to be abandoned. But has it? Martin[129] in his admirable criticism of the modern genetical concept says that there

there even a grain of truth in Charles Darwin's thesis, all the females would die off in hard times and leave only the old males to survive and carry on the race!

is, in the modern biological creed, 'an article which is especially favourable to the mutation-selection theory (Neo-Mendelism). This is that a long series of carefully contrived experiments has demonstrated beyond all possibility of doubt that an acquired character or modification cannot become hereditary ' But does such a series of experiments exist. 'Astonishing to relate', he said, 'it is not to be found.' Of course not. Nobody has proved experimentally that acquired characters can *not* be inherited any more than the reverse is the case. The idea has been abandoned by the vast majority of geneticists simply because it does not fit in with the abacus of Neo-Mendelian philosophy. This only shows what I have already emphasized, that they do not realize the omnipotence of their hypothesis. It is quite easy to postulate a Mendelian mechanism for the inheritance of acquired characters. In fact Waddington, who after all is one of our leading Neo-Mendelians, has done so.[130]

And now to sum up this fantastic claim that the Darwinian and Neo-Mendelian hypotheses reciprocally support each other. Darwin believed in blending inheritance, the modern orthodox view relies entirely on Mendelian inheritance. Darwin considered that offspring show the same range of variation about their characteristics as their parents did about theirs. The Mendelian knows this to be wrong. Darwin believed that in any type of organism there may be an inherited tendency to vary continuously in one particular direction. The Mendelians emphatically deny this. To them all variation is strictly at random. Darwin was completely ignorant of

the necessity of functional correlation between the evolving parts of any adaptation, and so based his hypothesis on a faulty premise. The Neo-Mendelians, as I shall show in the next chapter, have seen this fault in Darwin's ideas. And then finally we have Darwin's belief in the inheritance of minute variations and in the inheritance of acquired characters. The former of these *has* been analysed experimentally and the results indicate that there is no such inheritance. The Neo-Mendelian says therefore that the experiments must be wrong! The latter has *not* been analysed experimentally with any success either one way or the other—simply because the problem does not lend itself to such treatment. Nevertheless the Neo-Mendelians adopt most emphatically the positive view that acquired characters are *not* inherited. It all makes sorry reading!

The only real point of agreement between the two sides is that they both agree in the efficacy of Natural Selection as an evolutionary force. And this, as I have tried to show, nobody can contradict because it is a truism. But for the modern Neo-Mendelian to claim, as they appear to do, that their researches establish the reality of Natural Selection is as fantastic as it is illogical.

CHAPTER VI

# THE ORTHODOX THEORY AND LAMARCKISM

BEFORE considering the relationship of true Lamarckism to the orthodox mutation-selection hypothesis, I am putting forward certain critical points which appear to me to hit at the very foundations of the modern view These are partly in addition to and partly amplify those stumbling blocks that I have already set out in *The Evolution of Living Things* and in the original papers which preceded it. Unfortunately the upholders of the modern view appear to be adopting the principle, especially during these centenary years, of completely ignoring any such criticism. But they go further. de Beer even suggests[131] that 'to seek to impugn Darwin's conclusions'—and hence those of the Neo-Mendelian—is simply to make oneself ridiculous. Nothing, he says, 'but ignorance or effrontery could occasion such exhibitions in the present state of our knowledge'. Now these are strong words and presumably were published advisedly Are we then to understand that Darwin and de Beer are above criticism? Surely, if only in the interests of science, it would be better if de Beer, rather than pontifying in this manner, were to attempt to answer some of the outstanding criticisms that have so recently been levelled against him. Assuredly the longer

the modern geneticists refuse to reply, the weaker will their case become.

In order that there shall not be any misunderstanding about what I consider the weak points in the modern orthodox view, I am putting my queries under separate headings. And to each one of these headings I feel that every biologist has the right to expect an answer. Each question I consider affects the fundamental basis on which the Neo-Mendelian explanation of the mechanism of evolution is constructed. If my criticism *cannot* be shown conclusively to be without foundation, then why should biologists generally be expected to take notice of the fantastic superstructure that the Neo-Mendelians have built on such shifting sands?

*Is inter-specific sterility a Mendelian phenomenon?*

Let us start with something that we can say we know for a certainty We know for certain that Mendel's Law is the law which governs the inheritance of the differences that occur between domestic varieties. Nearly three generations of research has established this fact beyond doubt. If you care to use the hyperbole of de Beer you can say that this is established with the certainty with which we accept Newton's Laws of Motion. But we do not know with the same degree of assurance that this same law governs the inheritance of differences between species for the simple reason that species, in the vast majority of cases, will not interbreed! And hence we do not know for certain that the real significant difference between two species has anything to do with Mendelian inheritance. We may study

the inheritance of colour patterns of the common English frog, *Rana temporaria*, and discover its Mendelian mechanism. We may do the same with the French frog, *Rana esculenta*. But this does not really throw any light on the characters that ultimately distinguish *Rana temporaria* from *Rana esculenta*. All we know is that varieties of one will all breed together and the same applies to the other. But no variety of one will breed with a variety of the other. And we do not know—with Newtonian certainty—that this fact has any connection at all with Mendelian inheritance. This has merely been assumed by the Neo-Mendelians. To some of us this development of inter-specific sterility may be something quite outside the range of Mendelian control. And yet it is of fundamental importance to the evolution of different types. For surely when sexual reproduction first evolved, and it was only at this stage that inter-specific sterility became essential, if an antipathy had *not* become established between gametes of different types, how could those types have become canalized into separate species. There is no reason why this should not be a genic effect—in fact—the evolution of genes may have been primarily for this purpose and all their other effects purely secondary and of little consequence. We know, of course, many cases, especially among plants, where sterility between what it is probably correct to call species *is* due to a chromosome mechanism. But we have no right to assume that inter-specific sterility has been shown positively to be in all cases a Mendelian effect. It is reasonable to argue that it may be simply due to a property of specific protoplasm—that one gamete will

not unite with a strange gamete simply because its protoplasm is in some way repelled by any other protoplasm than its own. Is this any more difficult to envisage than a genic explanation?

After all, as Just says,[132] all the true species 'now in the world can doubtless be distinguished from each other on the basis of the chemical constitution of their proteins'. He points out moreover that these proteins are only one constituent of the mixture that we know as protoplasm. It is not surprising therefore that there *is* an antipathy between gametes of different species and that only when the cortical integrity of the egg has been injured in some way, which will naturally result in a change in its chemical constitution, is cross fertilization by a foreign sperm possible.[133]

*Is there not a basic continuous variation in all individuals quite apart from any which may result from polygenes?*

Now secondly, the modern orthodox view is to ascribe evolutionary steps to the action of natural selection on very small continuous variations. The old mutation theory of de Vries based on large discontinuous changes has been abandoned except for certain peculiar types.* In fact, the orthodox geneticist cannot really escape this conclusion, relying as he does to such an extent on natural selection and random variation. For if all characters are varying continuously in *all* directions it follows that practically all

* The fact that 'the most striking evolutionary phenomenon ever actually witnessed'—industrial melanism—is a case of large mutation is conveniently overlooked (see later, p. 100).

forms must be perfectly adapted. Only in those excessively rare cases where by pure chance the optimum variation has not turned up would it be possible to find a badly adapted form, and this would only be slightly out of adjustment. Clearly then, this being so, any large deviation *must* lead to inefficiency, and so to extinction.

Having adopted this view it is now assumed—and let us remember that it is only an hypothesis—that these small varying characters are each supposed[134] to be 'controlled not by one or a few genes but certainly by a large number, and perhaps by all the genes, which interact with each other to give the character. . . .' Further the organism is looked upon as the end product of a sequence of chemical reactions and mechanical developments all of which are completely conditioned by what has gone before, and according to the orthodox geneticist, each one of these processes is supposed to have its genic control. Thus an organism may develop to a certain length before a particular set of genes comes into action and initiates some particular reaction. After this stage has been reached it may continue to develop to a particular weight before another set of genes directs its development along another route. It will then proceed along these lines until more genes come into action and, let us say, produce a deposit of pigment in the embryo, and so on. Thus as each set of genes comes into action it finds the embryo developed up to a certain point. But will that point be the same for each individual even if they have the same genetic constitution? Naturally not. There must be differences if only for the reason that embryos must occupy different points in space.

Thus even if it were assumed that all the eggs laid by a frog were identical to start with, which is of course absurd, the position of an egg in the egg mass would affect that egg slightly in such a thing as obtaining oxygen or getting rid of waste products. Now when one realizes that it is not one single series of reactions that constitutes an embryology but an ever increasing spread of reactions, one reaction conditioning the future appearance of several and those several leading to a much greater number and so on, does it not follow that the end result of such embryologies must vary among themselves merely because of these vicissitudes?

The point I am trying to emphasize is that because of the minute differences which we know occur between egg cells, and because of the differences that must occur in each process in any chain of chemical reactions that collectively form the embryology, the resultant embryos hatching *must* vary among themselves. And this variation will be independent of whether the individual chemical reactions are controlled by genes or whether it is the organism as a whole that controls these processes.

Let us take a concrete example. Suppose you started with a certain quantity of two organic chemicals A and B, and as a result of their interactions, with all the extreme accuracy that is nowadays possible in a chemical laboratory, you synthesized a certain amount of C. Then with this interacting with a certain amount of another chemical D you synthesized a chemical E and so on through the whole alphabet. You would end up with a product Z which would be present in a definite quantity. Now sup-

pose you repeated the experiment. Would you expect to obtain the same quantity of Z? Of course not. In fact you might not have any Z at all, for, because of the variation in all the preceding syntheses the series might have come obligatorily to an end at, say, S. Now this alphabet of syntheses can logically be compared with the sequence of events in any embryology, and the same arguments will apply. In fact the minute continuous variation on which Darwin relied so much must be a *sine qua non* of any development.

Now the orthodox geneticist believes that this continuous variation is due to the cumulative effects of a lot of polygenes.[135] But how can this cumulative genic effect be distinguished from the normal chemical variation that I have just described. It cannot be analysed by experiment because as Waddington points out[136] these purely hypothetical genes have too small an effect to allow of their identification individually If one could measure the degree of variance in the two cases and so compare them, then there might be some significance in all the soul-destroying mathematics that have been expended in analysing the power of Natural Selection. But until this can be done, and frankly from the nature of things it is doubtful whether it ever can be done, I maintain that all mathematical analyses of the effect of natural selection acting on small continuous variations (not large discontinuous ones) may provide entertainment for the armchair mathematician but they have little, if any, other use.

The difficulty, as I see it, is that it is assumed that

individuals with the same gene complex, apart from obvious environmental effects, will appear the same and exhibit the same degree of efficiency relative to natural selection. But will they? I maintain that they will no more be alike than a precision instrument made in a factory will be identical with another. The minute variations that must occur in all the processes, however carefully controlled, that lead to the manufacture of a high grade instrument, always leads to *slight* differences between the end products. And these very small differences which will satisfy the degree of tolerance permitted in the manufacture may make all the difference to the ultimate performance of the finished article. Thus to take a well known example, cylinders of motor-cars and their related pistons are precision made. It takes an excessively fine instrument to measure the differences between the actual dimensions of pistons and cylinders in separate cars. And yet we all know that if the piston is on the small side to a minute extent and the cylinder is similarly on the large side, then that car will not be nearly as efficient as one that varied in the opposite direction. Now these inequalities in efficiency are inherent in the method of manufacture. Similarly in living organisms there is the variance that is inherent in the fact that all development is a sequence of chemical reactions and at the same time there is the hypothetical variance due to the action of polygenes. Neither can be measured. They will be superimposed on each other without rhyme or reason, and so natural selection will select for survival a bad complex just as often as a good.

*Can the Neo-Mendelian give any sound explanation of the regression of structures which have ceased to be used?*

Now, thirdly, according to Darwin, natural selection acted only on 'profitable variations.' 'Unless profitable variations do occur, natural selection can do nothing'.[137] He did not worry himself unduly over the obvious fact that there must be any number of characters in an organism which have no function apart, of course, from forming a constituent of the complex that we refer to as the organism. After all, if you build a motor-car it must be of some particular colour—if you wear a hat it must have some distinctive shape. There have been biologists who take the view that it is only through the feebleness of our knowledge that we take this view. If we only knew more we should find that everything has a purpose. The type of character I refer to is too well known to discuss at length. I have recently given[138] some illuminating cases but one that always intrigues me is the case of the neck shield of the extinct giant *Triceratops*. This great sheet of armour projecting back over the vital neck region has been given a variety of functions, but why is its edge always beautifully scalloped? Whatever function the neck shield had this could hardly be effected by the frills along its margin.

The modern geneticist, and I feel biologists generally, all accept the idea of non-functional characters. The problem is how they can evolve. To the modern Neo-Mendelian, in fact ever since the early days of Mendelism, there has been no difficulty. It is simply assumed that

every non-functional character is linked *somehow* to a functional character so that natural selection, in always producing an organism perfectly adapted to its environment, at the same time endows it with a lot of useless characters—our finger-prints, the pattern of our hair, the coiling of our gut and so on. Now Martin,[139] a distinguished anatomist at MacGill, has recently attacked this idea in a very forceful chapter on the regression of organs, or the inherited effects of disuse.

He first of all takes the well known case of the femur of the whale. In the case he quotes he says that the bone of this enormous animal, which as far as we can see is functionless, only weighs two ounces. There must however have been a time when it weighed four ounces. Now it would be absurd to consider that this weight difference which, relative to the total weight of the animal, is minute, could have any survival value. How therefore has it evolved according to the modern mutation-selection hypothesis? Clearly there is no difficulty if one assumes that there is a gene which leads to progressive diminution which is linked with some part of the gene complex controlling some functional thing such as, for example, the size of the tail flukes. Why should there not be?—to the Neo-Mendelian anything is possible.

But then Martin takes the numerous cases of flightless birds. He wisely omits consideration of the ostrich and its allies and the penguin, as these, for all we know, may be cases of primary flightlessness. He points out that flightless birds 'are mostly located on, and are characteristic of, . . . the small oceanic islands', which 'afford safety with-

out any stimulus to fly'. Now the Neo-Mendelians point out that in these circumstances a capacity to fly may have become positively dangerous. As long as the bird remains on the ground where aparently there are few predators it is safe. But if it attempts to fly then in the presence of a gale it might be blown out to sea and perish. Hence a gene complex which leads to the inability to fly could be selected for survival. But then the trouble arises when it becomes necessary to explain the fact that the wings of the birds must have continued to regress long after they became incapable of flight. And this has occurred in a considerable number of different types, as widely different as a rail, a cormorant and a grebe. Now how is this to be explained on the modern mutation-selection basis? Any one case may be explained by assuming the existence of a gene similar to that conjured up to explain the diminishing femur of the whale. But to suggest that this gene has turned up (and remember mutations are supposed to occur purely at random and without any necessary relation to the needs of the organism) time and time again in very distantly related birds, this, as Martin remarks, 'would carry us beyond the confines of science'

The extremely interesting point that Martin brings out is that all the birds which have become flightless belong to those groups of birds which may be termed reluctant fliers. On the other hand the birds associated with these flightless forms which themselves have not become flightless belong to groups 'like the plovers and sandpipers which are eager fliers and would continue to fly in any environment'. Now here is an observation which would

have warmed the heart of Lamarck. But can the Neo-Mendelians account for it? I suppose the answer would be that a gene leading to the progressive disappearance of the wings is located on the same chromosome or has, as its pleiotropic effect, that which leads to reluctant flying!

*What is the Neo-Mendelian explanation of abnormal regeneration?*

Another case which stretches the imagination almost to the limit is that of the regeneration of the tail of a lizard. It is well known that if a lizard is chased by an enemy which grabs its tail, it has the power of throwing it off, so that the enemy only gets the tail to eat and the lizard lives to grow another one. This is said to be an advantage and so it can be supposed that a gene appears that controls this autotomy and this will be selected for survival. Of course this argument suggests that the tail is therefore of no real purpose, as otherwise tailless lizards would themselves be wiped out by natural selection in competition with those that had not been unfortunate enough to lose their tails. However, let us pass that by; the really interesting thing about the process is that while the regenerated tail appears to be a very good copy of the original one, the skeleton inside is quite different from the normal candal vertebrae that run down its length. Now this must involve a new set of genes to control this new process. How were they selected for incorporation in the gene complex? Is this another case of linkage or pleiotropism or must we postulate that there is some especial advantage in forming the skeleton in the re-

generated tail in a different way? However, be this as it may, I am still awaiting an explanation of how it is that the newt has developed an entirely new way of re-forming the lens of its eye when this is removed.[140] This appears to be characteristic of newts; the ability does not occur in frogs, and therefore it must be represented according to the Neo-Mendelians in the gene complex of the newt *and established there* by a process of Natural Selection. I ask once again, 'How often does a newt have the lens of its eye accidentally extracted so that Natural Selection can pick out for survival the fortunate newt that is able to regenerate its lens by an amazingly lucky chance of having developed a specially new process just for the occasion?'

To my mind, to explain these cases on the selection-mutation hypothesis takes one merely into the realms of fantasy Is it not more probable that their explanation is to be found in the wonderful researches of Spemann rather than Mendel? Spemann in his work on amphibian embryos demonstrated conclusively, at least so I maintain, the existence of an organismal control.

Compare the two arguments. The Mendelians by crossing a variety with the normal type demonstrate the existence of genes. Therefore they deduce that there must have been genes in the normal type before the variety turned up, and few people will disagree with them, but remember, it is only a deduction. In Spemann's work if a patch of cells in some part of the body of an early embryo of a newt is removed and replaced by a strange patch of cells, the embryo as a whole controls that patch and

forces it to develop appropriately. Therefore there must be an organismal control which brings this about. Hence by an argument parallel to that of the Mendelians, it can be deduced that the normal patch of cells in the ordinary course of events is also controlled by the organism and made to develop properly. Here is the proof that would even satisfy de Beer's Newtonian test of exactitude that there *is* an organismal control. But this control looks after the functional aspect of development. It makes the group of embryonic cells develop into organs appropriate to the position of those groups in the body, but it does not worry itself about the pattern of the first organs. This as I have suggested it leaves to the Mendelian mechanism. I quoted[141] the case where a newt larva can force a patch of strange cells from another species to develop into gills but cannot control the pattern of those gills. If now the Neo-Mendelians admit the existence of this organismal control it is up to them to suggest a way in which the appropriate gene complex for such a control could have evolved. If they admit that it exists but is *not* under the control of the gene complex—a strange attitude to take, but one apparently adopted by Waddington—then they must consider what effect this admission would have on the significance of all those statistical profundities that emerge when mathematicians deal with the problem of evolution. For after all, unless you can evaluate this personal control of the organism in comparison with random variation it is difficult to see how evolution can be ascribed to the power of one or the other.

*Genes can alter existing things but can they bring about the appearance of new functional structures, and what is more important, of correlated systems of new structures?*

Now no one, or at least very few, will deny that the Mendelians years ago published the objective existence of genes which by their influence bring about *alterations* in existing structures of characters generally. And the problem of how this is brought about, let us admit at once, is one of which only the slight fringes have been tackled. de Beer's positive statement[142] that genic action is mostly a matter of 'competition between enzymes, and ecological efficiency, at the level of ions and molecules', is mere verbiage. After all would not this non-committal statement apply to any physiological process? However the orthodox Neo-Mendelian goes further and deduces that all the characters of an organism are under the influence of genes and therefore evolved under the same influence. Now is this not illogical? We know that blood is red, that bones are hard and that protoplasm is aqueous. By analogy with results that have been obtained in genetical research we also know that genes might turn up which would diminish the haemoglobin content of the blood or might soften bones or might produce a dropsical protoplasm. They might all be lethal but that is beside the point. What I want to emphasize is that their production would not prove that a gene was necessary for blood to be red, for bones to be rigid or for protoplasm to be a watery mixture. And yet these are three vital characters of the vertebrate body. Genes can *alter* things but they cannot

produce new things. At least that is my contention. That is what I have published at various times during the last three or four years, but the Neo-Mendelians will not reply Let us have from them a case of a new functioning organ arising definitely by a gene mutation. If they cannot produce this evidence then, I maintain, their hypothesis falls to the ground when it comes to explaining all the major steps in the evolutionary tree.

Of course, I suppose de Beer, who has lost his heart to industrial melanism, could say that *there* was a case of a gene mutation producing a new character—blackness—of vital importance to the organism. But this melanic variety has *not* been produced by Natural Selection. It has been produced by Human Selection! Human beings have carried out a genetical experiment on a gigantic scale without knowing it. By their own activities they have inadvertently sprayed their trees with a particular chemical that induces the gene mutation. But then at the same time they have blackened the trees with soot. Hence the melanic forms (see p. 42). I am sure Darwin would have understood this case. He would have put it in chapter I of the *Origin*, 'Variation under Domestication' and not in chapter II, 'Variation under Nature'.

I have argued this point elsewhere in a number of publications.[143–5] I will only repeat here one case—the transition of vertebrates to a complete terrestrial existence —the emancipation from water which was brought about when the amphibia evolved into reptiles. At this stage a complete new method of mating had perforce to be evolved since gametes cannot lead an aerial existence.

Hence some method of copulation became essential. Then the larval life was given up and hence a heavily yolked egg appeared. This had to be provided with a supply of water for the developing embryo and hence white of egg first appeared. But then waste products could not diffuse away as in the aquatic amphibian egg and so the cloacal bladder developed precociously as the allantois. This then took on a secondary function of a respiratory organ in the later embryos, corresponding to the amphibian larvae, to take the place of their gills. Then all this elaborate egg had to be protected and hence there appeared the egg shell. But now how was the baby reptile to escape out of the egg? The answer is that it developed a special *ad hoc* structure, a built-in tin-opener, with which to cut its way out of the egg. Now here are a lot of new structures. To be perfectly fair the large amount of yolk and the allantois may be considered as modifications of pre-existing structures, and perhaps also the method of copulation, and so no one can deny that they *might* have evolved under the action of genes. But the white of egg, the shell and the egg-opener, these are entirely new. They were wanted and therefore they appeared. To claim that there is any evidence that they can have evolved by natural selection acting on the products of gene complexes only shows, if I may be allowed to use de Beer's words, 'ignorance'—but I refrain from adding 'effrontery'!

Not only were they wanted, but they were wanted all at the same time. So that even if it were allowed that they were under the influence of genes we have to postulate that, *by pure chance*, all these seven major changes occurred

at just the appropriate time. And Lamarck is laughed at for the fanciful nature of his views! Surely his explanation is more acceptable than this fantastic hypothesis or the Neo-Mendelian.

*We can define genes—can we define the characters they control? This leads to the question—what is the unit of genic influence?*

The next query that I want to put concerns the relation between the genes and the 'characters'. The genes we know are particulate. We know their position in the nucleus. It might even be said that we are beginning to study their molecular anatomy. In any case they are clearly definable. But are the characters whose evolution they are supposed to direct equally definable? We know of course that the original idea of one gene for one structure has been abandoned and now it is thought that a multiplicity of genes all cumulatively affect some particular part. But that does not answer my point; for however many polygenes are envisaged, the statement that they control a character is of little value unless that character can be clearly defined.

Thus I have argued[146] that it is impossible to define the limits of the human eye as it leads to the inclusion of parts of the brain in the definition. And if you try to define a muscle—a living one—you end up by including the whole muscular system of the body. Take the case of the heart. Where does it end? Does it include any part of the main vessels leaving and entering it and if so how much? Who can say where the mouth—the buccal cavity

—begins and ends? No, it is perfectly obvious that individual organs cannot be defined. In every case, to be strictly logical, the definition leads ultimately to the whole organism. On the other hand alteration to organs *can* be defined. We can define accurately the change in the colour of an eye, an alteration in the place of attachment of a muscle, an increase in the size of a mouth or a change of one chamber of the heart relative to the other. And for these *changes* it is easy to conceive of a genic mechanism. We simply state that the presence of a gene or a particular chromosome is correlated with the occurrence of some disturbance in the normal development of a structure which thus results in that structure turning up different from the normal. But can we say any more than this?

It will probably be argued that the fallacy lies in talking of the heart as a whole or the eyes as a whole. Maybe this is correct, but one can hardly overlook the vast number of references in Mendelian literature to genes causing the presence or absence of an eye! However let us forget that. The correct attitude is perhaps to divide the organ up into smaller constituents. Let us accept this idea. Then where do we stop? Is there one gene for an arm, or for a muscle of that arm, or for the muscular tissue in that muscle, or for each individual muscle cell? Where does it all end? After all it is the orthodox geneticists who stick to their genes. Then let them say what is the unit of genic influence. There must be some limit because Huxley[147] and de Beer[148] and many others believe in a *definite number* of mutations being necessary for some particular evolutionary transition. Is this limit the cell? After all,

Goldschmidt in some of his earlier work enumerated and mapped out the cells of nematodes and showed that there was remarkable constancy. But it would be perfectly illogical to stop even at the cell. The contents of the cell, their constitution and their arrangement are absolutely essential to the functioning of the cell. In fact, as far as it has been possible to analyse structure by means of the electron microscope, there are always to be found structures of amazing complexity but constancy of pattern or even structures of definite functional significance. Thus Parke's work[149] on the anatomy of certain minute dinoflagellates has shown at the magnification of $\times 30{,}000$ that there are present skeletal structures with a characteristic pattern as complicated as the type of sculpture one sees so easily in the skeleton of a radiolarian under the low power of the microscope. Are there genes for the separate characteristics of these patterns? As far as I can see the Neo-Mendelians cannot deny this.

Another even more interesting example is to be found in the structure of the cuticle of the earthworm as revealed by the electron microscope. Thus it has been shown that the thin sheet of material has a warp and a weft like any woven fabric. The difference is that the separate fibres of warp and weft do not interlace. They lie at right angles across each other and appear to be stuck together where they touch by some particular substance. Now the earthworm can elongate its body when it becomes narrower and *vice versa*, but always the cuticle adheres close against the outer body wall. This is made possible by the fibres of the cuticle lying diagonally to the long axis of the animal.

As everyone knows, if it is desired that a garment should cling to the figure the material is cut on the skew. Now remember in this beautiful work it is hardly an exaggeration to say that we are seeing molecular structure. The individual 'fibres' would appear to be ropes of long chain molecules. Incidentally they even show a twist as if they had been spun. Now all this structure, which I am going to have the temerity to refer to as molecular, is of functional importance. Therefore surely the orthodox view must insist on it having a genic representation so that natural selection can act on it and establish it.

As I see the problem, the othodox geneticist is committed by the extension of the original Mendelian Law of Heredity which he has developed to provide a genic representation for every detail of structure, whether functional or mere pattern, that we may discover, however minutely we may carry our anatomical investigation. If he stops short at any level then *he must show cause why he does so.*

But now, see the absurdity to which this leads. The eye is an organ of very precise structure. It must be represented by a complex of genes. Surely no orthodox geneticist will deny this. But also its parts must have their own genes to ensure that each part puts in its appearance in proper, spatial relationship to the rest of the eye. Again no Neo-Mendelian can deny this. Very well then—similarly the chromosome is an anatomical feature whose details of structure are just as rigidly fixed as are those of the eye. Hence it must also be represented by its appropriate genes to ensure that it occurs in the right place in each cell of

the organism. But now just as the eye has its parts so also has the chromosome—the genes. Moreover, each of these occupies its own particular place on the chromosome. Clearly then there must be a set of genes to ensure this obligatory spatial relationship, just as much as there must be a gene complex to ensure that, let us say, the lens of the eye occurs in the right place, relative to the rest of the eye. In other words, for any particular gene to occur on any chromosome it follows that there must be other genes to ensure its appearance at its appropriate place. And so for every gene there must occur another gene or genes and where does this lead except to a vicious circle, a *reductio ad absurdum*?

*By what right do the Neo-Mendelians carry the Mendelian mechanism of heredity back to the levels before cells, nuclei, mitosis and sexual reproduction were established?*

Now lastly, before considering the relation of the modern theory to true Lamarckism, there is one more query I must raise again. I have pointed out in several publications that Mendelian inheritance can go back as far as the first cellular organism because it is at this level that true chromosomes first appear. But it can go no further. Chromosomes are necessary for the existence of genes and Mendelian inheritance cannot get on without them. I said therefore that there can be no Mendelian genic inheritance in bacteria because at this level there are no nuclei and hence no chromosomes and hence no genes. This has indeed brought coals of fire on my head, but I am unrepentant. My opponents point out (but not in

print) that papers have been published recently which show definite chromosomes in bacteria. I therefore referred to the report of the sixth syposium of the Society for General Micro-Biology held in 1956. There I found the paper by Delamater on 'Bacterial Chromosomes and their Mechanism of Division'. The whole paper appears a typical example of *a posteriori* reasoning. It argues that since there is genetic inheritance there must be chromosomes. But, as a matter of fact, the two plates illustrating the paper could be taken as proof positive that there are *no* chromosomes! There may be however division centres. These would appear to be present in some preparations, but that is all one can say The chromosomes are nothing more than lumps of matter with an affinity for stains—chromatin masses if you will. When these lumps appear in line they are termed chromosomes!

Now what is one to do in a case like this? I am afraid that the answer is that if you *want* to find Mendelian genes in bacteria you accept this paper, and pay no attention at all to the paper which precedes it in the same collection *and flatly contradicts it!*

Really the bacteriologists should make up their mind before contradicting me. What I said was that I did not deny that *simulacra of chromosomes* could appear at levels below the cellular forms but these were simulacra, that is bodies which appear to be similar in some ways, and probably *are* similar in some ways, but taken by and large are really quite different bodies. Now a chromosome cannot play about with fate like this. If it is to be a structural body incorporating in itself a string of beads—the genes

—all arranged in a particular order and endowed with an instinct to go through all the gymnastic contortions that we know are characteristic of the chromosomes of cellular animals and plants, and are essential for any Mendelian inheritance, then it must *be* this and not merely an elongated mass of substance that has a particular affinity for biological stains! Every geneticist knows that the slightest displacement of a gene upsets its effect. Each gene *must* occupy the same position relative to all the other genes in the thread that we call the chromosome. By what right then do those unorthodox bacteriologists claim that the scaffolding necessary for the unfolding of Mendelian inheritance is present in bacteria? Lumps of staining matter—chromatin—yes, but not chromosomes.

I am not denying, and have never denied, that the primordia out of which the Mendelian mechanism may have evolved is present at the bacterial level. To my mind it must be. In fact unless this is accepted it becomes necessary to envisage the first appearance of nuclei, diploid number of chromosomes, mitosis and, in fact, sexual reproduction, all occurring at some sudden step or mutation. What I have said[150] is that out of all the welter of conditions at the bacterial level—level, mark you, and not in any particular bacteria—conditions which included among them that strange process known as conjugation, there ultimately evolved the sexual process, and with it Mendelian inheritance.

Now just consider these chromatin masses which, there is no doubt, occur in some bacteria. Those who want to see Mendelism carried back to this level say that changes in

these lumps control and produce changes in the bacterium. But is not this wishful thinking? If these lumps do it on their own, this means that they have in themselves the power of changing the whole organism in which they occur in the right direction for its survival. Remember, Professor Hinshelwood in all his experiments shows that the bacteria are able to adjust themselves to changing conditions. In other words they do the right thing. If then you ascribe this power to the individual pieces of chromatin, is this not sheer mysticism? Is it not far more probable that the external environment acting on the organism *as a whole*, that is producing its effect through the surface of the organism, produces a reaction in the organism as a whole that fits it to the changed conditions? And then that this change registers itself on the chromatin masses. In other words it is the change in the organism that affects the chromatin masses and not the reverse.

I cannot overemphasize this point. If you imagine that these dispersed masses of staining material that occur in the bacteria are capable of spontaneously changing themselves then you are endowing them with mystical attributes far exceeding the entelechy of Driesch. Is it not clear that these bodies can only be affected through changes that occur around them? Now this will be in the general cytoplasm of the body. Or, as Just puts it more precisely[151] 'The ectoplasm, standing between the protoplasmic system's inner substance and the outside world, reacts first to environmental stimuli and thus conditions the responses of the whole system.' *It* will be in contact with the outside world and so will react directly to any change in

that environment. Now imagine any critical alteration in the conditions of the immediate surroundings to take place. This will have a definite reaction on the bacteria. But on which part? Obviously it will act on the general cytoplasm of the body; and this will then react in such a way as to protect itself and preserve itself and its offspring. Clearly if it did not do so it would be annihilated by this simple change. It is reasonable then to deduce that this general and superficial protoplasm, or cytoplasm if you will, will change in an adaptive manner. But now this change will upset the equilibrium that must occur between the cytoplasm of the bacterial body and the chromatin masses that it envelops. The latter naturally alter accordingly to maintain the equilibrium. Is it not therefore far more probable that these hypothetical changes in the chromatin bodies, that have been called chromosomes, are *propter hoc* rather than *ad hoc*? Is it not more likely that the changes in the body as a whole are catalogued in the chromatin bodies, that later may evolve into the chromosomes of cellular forms, than that *they*, the lumps of chromatin, are the sources of power that produce the adaptive responses of the whole organism?

The idea which has encumbered the development of cytology for at least forty years is this belief that any minute constituent of a living cell can do a thing by itself. At one time it was the Golgi bodies that gave rise to fats and at another that the mitochondria transformed themselves into a particular secretion. But of course the true state of affairs was that Golgi bodies inter-acting with their surrounding cytoplasm may have produced fat.

Mitochondria with the help of their neighbouring protoplasm may have produced the secretion. Now this is a different thing altogether, for clearly the cytoplasm is just as important in the reaction as the other constituent. Surely this same argument must apply to the adaptive responses of those bacteria which contain chromatin masses.

Is it not better to regard bacteria as little envelopes containing a heterogeneous living mass and that it is this chemical mixture which reacts *as a whole* with the surrounding medium through the external envelope which, incidentally, Stacey has shown[152] to be itself extremely complex? Is there any more reason for ascribing changes in the organism to the lumps of matter, which, by chance have an affinity for stains and so can easily be seen, than to some critical component of the external envelope? Why choose one rather than the other?

Here then are some of the criticisms that I raise against the modern orthodox view of genetics. They are not the vapourings of a tyro. I have taught the foundations of genetics for nigh on forty years. I have seen the science of genetics change from the days of Bateson and Morgan when it was reasonable and understandable, to the phantasmagoria of contemporary genetical statistics. And is not this the real essence of the rot? The science of genetics has been allowed to drift into the control of the mathematicians who, however commendable may be their efforts at understanding the problem of evolution, can hardly be expected to have a real insight into what is

meant by a living organism. And it is living organisms that have evolved. Remember, Bateson and Doncaster, Morgan and Punnett and all the earlier Mendelians, were established biologists long before they transferred their affections to genetics. They knew what a living organism meant, they knew what an embryology meant. They could envisage the implications of a living organism struggling for existence. They most certainly realized their limitations. But nowadays it seems to be accepted without protest that anybody with a penchant for mathematics can dabble in the problem of evolution and, whether or not there is any evidence that they have had some experience of the study of living organisms, their findings are accepted *ex cathedra*.* Is it surprising that an anonymous author recently, in a very distinguished journal, described[153] the modern orthodox Mutation-Selection hypothesis as consisting of 'assigning names to hypothetical lumps of stuff in order to confer magical virtues on them, so that a crowd of lumps can bring order out of chaos and unity out of multiplicity'?

I have already said that I consider every biologist is

* A striking example of this is to be found in a book, *Darwin's Century*, by Loren Eiseley, (Gollancz, London), which appeared in this country after the manuscript of the present book was submitted for publication. Here, after an admirable and most courageous account of the rise and fall of Darwinism, the author breaks off about 1900 and suggests that since then the mathematicians have adequately dealt with the matter and interpreted the whole of the evolutionary process in terms of Mendelism. Unfortunately the two mathematicians whom he quotes as being mainly responsible for this revolutionary advance appear to be at complete loggerheads! (See Sheppard, 1958, *Natural Selection and Heredity*, Hutchinson, London, p. 135.)

entitled to an answer to the queries I have just enumerated. At the same time I feel bound to emphasize that all these points have already been raised by me in reputable scientific journals, quite apart from a separate book, but they have been completely ignored by the High Priests of Mendelian Orthodoxy behind the scenes in this centenary year of celebration. Not one mention has been made of them either in the printed word or over the air. What is the explanation? Do they find themselves unable to answer? After all, in the scientific world there is no difficulty in answering criticism.

And let it not be thought that I am alone in kicking against the pricks. There are others among contemporary writers. There is Professor Thompson[154] who, in his introduction to the reprint of the sixth edition of the *Origin*, questions completely the efficacy of the power of Natural Selection on which the orthodox geneticist relies so strongly Then there is Professor Martin[155] who refuses to accept the attitude adopted in such a partisan manner by de Beer and his friends that it has been proved experimentally that acquired characters cannot be inherited. There is Professor Vandel[156] of Tropical France who has a conception of the living organism much more in line with Lamarck and, I may add, myself, and will have little to do with Neo-Mendelism.

Also among recent writers there is of course that great anatomist, Wood-Jones. In his last writings he put forward examples which would tax the ingenuity of the most abstruse Neo-Mendelian to explain away on a genic hypothesis. Unfortunately he spoiled his own case by

denying Mendelism as a whole. But then there is Caullery who, at the time of his death last year, was the doyen of zoologists in Lamarck's own country. He opposed Mendelism completely as a mechanism for explaining the evolution of parasitism, and when we realise, that, among animals at least, there are probably more parasitic forms than there are free living, his criticism, based as it was on a life time study of animal relationships, cannot be lightly ignored. And then, earlier but still within the Neo-Mendelian era, is that erudite zoologist, E. E. Just, who in his devastating manner said,[157] 'The gene-theory, however much it pretends to be mechanistic, is only crudely vitalistic. . . For one entelechy it sets up many'!

But perhaps most significant of all are the mature views of Johannsen, the man who was responsible for coining the word 'gene' He asks,[158] 'Is the whole of Mendelism perhaps nothing but an establishment of very many chromosomal irregularities, disturbances or diseases of enormously practical and theoretical importance but without deeper value for an understanding of the "normal" constitution of natural biotypes? The Problem of Species, Evolution, does not seem to be approached seriously through Mendelism nor through the related modern experiences in mutations.'

These are all criticisms by men of mature years and long experience, and I mention them specifically not so much in support of my own views, as that I feel that the ordinary scientific reader, and particularly the senior non-biological scientists, should be made to realize that there is a strong and, I feel, growing opposition to the monopolistic atti-

tude of the modern geneticists. It is useless for the latter to adopt their ostrich-like posture and ignore criticism. They must surely soon take their heads out of the sand. Then let them refute my criticisms and, if they can do so, then at least biological science will go forward.

And now what is the position of true Lamarckism in the orthodox theory? Let us assume for the sake of argument that the modern Neo-Mendelian conception is correct after all and my criticisms can be passed by There still remains the problem of how adaptations arise, for these are all coordinated systems. The orthodox mutation-selection hypothesis demands that the mutations are at random, and yet the various parts always remain in a co-ordinated balance during their evolution.

I am taking my account of the Neo-Mendelian explanation from Carter's *Hundred Years of Evolution*. I do this deliberately as it received the imprimatur of a senior geneticist.[159] Carter starts by saying[160] that it is the evolution of the small changes that lead up to the formation of species—often called micro-evolution—that forms the fundamental problem of evolution and very few people would deny that. He takes two cases. He says, suppose as a result of some gene mutation an organ varies slightly Then all the other organs associated with it will vary accordingly, that is adaptively But this will be phenotypically—it will not have anything to do with genes. It will thus result in the coordinated system changing as a whole through a gene mutation affecting one central part.

Now in his second case he says, suppose that this central part evolves further, by chance of course, along the same

lines and the changes required in the associated structures become larger than can be provided for by this phenotypic adaptation.* We are then asked to believe that the organism goes on reproducing with the central organ considerably modified while, by random 'recombination and mutation of the genes under selection', the necessary genic changes in the gene complex are brought about. But of course the mistake here is the same as that made by Darwin. An animal with one part of a coordinated system out of balance with the rest will obviously be at a disadvantage and so the animal will be wiped out by natural selection. The same mistake has been made by Sheppard[161] in another recent book that has met with approval. But Sheppard does not even see the need for preliminary phenotypic adjustment. He states categorically that the required changes in the associated structures 'necessitate the selection of a new balanced system of polygenes'. The example he takes is of an animal increasing its height. He says that this 'may have to be followed by an increase in the strength of the bone of the leg'. There is no 'may have to' about it. If it does not do so clearly it will be weak about the legs and so natural selection which he says a few lines before, 'is often intense', will wipe it out of

* Of course the assumption that there *is* a limit to the extent of phenotypic adjustment is entirely gratuitous. The fact that animals and plants can adapt themselves only to a certain limited extent to the crude changes of environment that we bring about in our short lives by domestication, or deliberately instil experimentally, does not mean that organisms are so limited in their adaptive reponses to the vastly smaller changes occurring over vastly longer periods that must have occurred in the aeons of evolutionary time.

existence. This whole idea of one part of a functioning system varying without the other is absurd. Supposing for instance a femur *did* increase in length by any means, genes or otherwise, does it not follow that the muscles *must* increase accordingly and the blood vessels and the nerves? If the muscles did not do so they would be permanently on the stretch! If the blood vessels did not do so some part of the leg would go short of blood! The whole idea is fantastic and it is amazing that Darwin never saw the fallacy in it.* Of course he thought in terms of whole limbs, or 'parts of the body' to use his own words, and this may be the explanation. But the Neo-Mendelian who postulates genes for each part of every organ—or if he does not do so, it is up to him to show the reason why —for him there is no excuse. So the second fantasy we may ignore.

Now to come back to Carter's first example. Let us take again a concrete example, one that I have discussed before, the case of the biceps muscle of the upper arm. Suppose this muscle by a gene mutation increases in strength then clearly certain resultant changes in the muscular system *must* occur at the same time, and these will occur, according to the Neo-Mendelian, phenotypically. Thus all the muscles holding the shoulder steady must increase in strength because while the biceps pulls the forearm up it tends to pull the shoulder down. It *must*

* It is perhaps even more amazing that those people who resisted Darwinism even up to the advent of Mendelism and more recently appear to have overlooked this fundamental flaw in Darwin's hypothesis, or, if they see it, they keep quiet about it.

do so—there is no escaping this conclusion as a muscle pulls equally on both its attachments. In other words the development of a stronger biceps creates a need—the need of stronger shoulder muscles. Hence they appear phenotypically! But is this not sheer Lamarckism? Of course it is. *The whole of the Neo-Mendelian fabric is saturated with true Lamarckism!*

But, says the orthodox geneticist, these secondary changes are only phenotypic temporarily Very soon through the mystical action of the polygenes they will all be gathered in to the genic fold and become genotypic. And how this is supposed to come about is surely the central problem of the whole of the orthodox Neo-Mendelian hypothesis. But accounts differ—reliance is usually placed on those blessed words 'recombination' and 'reassortment' which cover such a multitude of sins. Sheppard simply states[162] that for each of these Lamarckian responses—he calls them adjustments—there will be of necessity 'the selection of a new balanced system of polygenes' affecting those adjustments. But how Natural Selection can pick out any one collection of polygenes rather than another it is difficult to see, for all the forms will be *ex hypothesi* alike. In the absence of any phenotypic adaptive effect the argument might be sound, but the Neo-Mendelians have to admit the existence of this power of adjustment and in its presence the scheme is unworkable. Each organism will make all the necessary adjustments, just so much and no more. Hence why should Natural Selection pick on one rather than on the other?

But, however the orthodox geneticists explain this

critical point, will all the phenotypic changes be translated into the gene complex? For a mutation in the single biceps muscle there must be a score or two of the ancillary muscles* each of which will vary adaptively in spite of the genes and not by virtue of them. Is it not therefore safe to deduce that long before they have all been gathered in and have received the comfort of the gene complex to control their structure and function, one or other of them will themselves mutate? But this will at once cause another phenotypic reaction among all the others and so all the good work of providing each muscle with its own gene control will be undone and the game has to begin all over again. Does it not follow from all this that *on the orthodox view*, in any organism, for every Mendelian effect there are always scores of characters that are purely Lamarckian in origin? I have taken a case from the muscular system because it is simple and can be studied by anybody on themselves but any functioning system would have served my purpose. Thus if the heart, by this hypothetical gene mutation, became more powerful, is it not obvious that the whole of the blood system must adjust itself accordingly? The capillary control must alter

* This can be realized very clearly if one considers the action of combing the hair with the elbow lifted above the level of the neck. This appears at first sight to be simply the action of the biceps and triceps sweeping the forearm through the air. But, of course, the upper arm is being held in position all the time by a complicated system of muscles attached to the scapula and thence to the vertebrae, all of which are in a nice balance with the biceps and so any alteration of the power of that biceps must naturally affect all those associated muscles.

—the veins must enlarge to supply more blood to the more powerful pump and so on. Or take the kidney—suppose that it mutates in such a way that its capacity for extracting the waste products of metabolism alters, does it not follow that the means of getting rid of them out of the body must alter accordingly And then suppose the liver varied in such a way that the composition of the bile altered slightly. This slight alteration would affect the whole digestive system and if this did not adapt itself in the appropriate manner it would be the end of that organism.

In fact, it is surely obvious that no essential functioning character can possibly change without affecting a great many other organs or even the whole body This is why Waddington[163] in his dilemma had to call in to his help his harmonious control, and in doing so no geneticist ever made a wiser move. But is not this control simply Lamarck's second law in action? Here are Waddington's own words, and remember these were broadcast to the nation, and have never been contradicted by any of the other senior geneticists. He says 'There is no reason . . . why a chance mutation should not affect the whole organism in a harmonious way' . . 'A random change in a hereditary factor will . . . not usually result in an alteration in just one element of the adult animal; it . . . may . . . alter a complex organ as a whole', for, he adds, the different parts of organs influence one another. Yes, of course they do, but they influence each other *in the right direction*. That is the crux of the whole matter and it is that point that the geneticists pretend not to notice. They are

so intoxicated with the idea that hereditary changes 'are not guided so as to fit into the environment'—Waddington again—that they do not see that for every trifling little hypothetical Mendelian change the whole organism reacts in a true Lamarckian manner.

There you have the irony of the present situation. The powers that be behind the modern Neo-Mendelian ramp took pains at their recent centenary celebrations to advertize their scorn of Lamarck but, in their ignorance of what he said—or is it pretended ignorance—the whole of their fabric is based on the essence of Lamarckism. They are merely following in Darwin's footsteps. As we have seen, he took to himself Lamarck's ideas, whether right or wrong, whenever it suited him and made no acknowledgement. de Beer and his friends go one better. They build their fantastic hypothesis around Lamarck's great inspiration and then go out of their way to abuse him. Darwin at least only did this in private!

Now all this argument was based on the hypothesis that the original cause of the disturbance was a change brought about by a small gene mutation, that is, it originated inside the organism. But suppose that the change comes from the environment. Again let us take an actual example. Let us consider an aquatic form. Suppose that the oxygen content of the surrounding water diminished slightly. This will at once affect the gills. Now why should not these gills respond at once and increase their surface so as to bring about equilibrium again?

There are two answers to this question. First, the hard-boiled Neo-Mendelian will say that this is heresy. The

characters of an organism are controlled by its genes and these mutate at random without any relation to its needs. Hence the aquatic organism cannot adapt itself to changes in its environment except through the many-millions-to-one chance of the right gene turning up at the right time. Now see where this argument leads. Suppose that the heart of this aquatic organism alters by a gene mutation. Suppose that it becomes stronger, then the gills, *according to the orthodox view*, will modify themselves accordingly *at once*, and that will be phenotypically. On the other hand, if the change comes from outside, if the oxygen content around the gills alters, then the gills are unable to do anything about it—they have to wait for Providence to act through the gene complex! The whole argument is a gallimaufry of ideas!

But the more cautious Neo-Mendelian will say that there is no cause for alarm. He would argue that if the oxygen content varied around the gills these would adapt themselves phenotypically and then sit like patience on a monument, waiting for all the necessary genes to turn up. But I have already pointed out where this leads; long before this upset is settled there will be—there must be, according to Neo-Mendelian ideas—other mutations among some of the affected parts which will again upset the apple-cart. So that once again there will appear a host of Lamarckian effects.

Now if this second answer is really the modern Neo-Mendelian point of view, then their reiterated statement that all organisms vary at random and definitely *not* in relation to the need of the organism becomes meaningless.

If the various parts of the body can adapt themselves to changes phenotypically then why worry any more? Why not accept Lamarckism straight away for that is all that he said? He was not concerned with genes and after all, genes in relation to functional matters are only figments of the imagination!

There surely lies the fundamental mistake of the Neo-Mendelians. They assume that all characters are of one type. They do not distinguish between the unimportant trivialities on the inheritance of which their own brilliant work is based and the essential functioning characters of the organism which do not lend themselves to Mendelian experiment. And these functioning characters are all adaptations and adaptation is the essence of evolution. So each man to his last. Let the Neo-Mendelians continue the good work of producing more and more monstrous forms which man keeps alive to satisfy his aesthetic desires or his scientific curiosity—but let them leave the problem of evolution to those who really understand what was the real fundamental basis of Lamarck's views.

CHAPTER VII

# CONCLUSION

In the earlier chapters of this book I showed how an infamous distortion of the published works of Lamarck lead to a wholesale ridicule of all his views. I went on to explain how this perverted story was exploited when it became necessary to evangelize Darwinism in the latter half of last century Then with the rediscovery of Mendelism, and an apparent mechanism which would explain all hereditary processes, the line of attack on Lamarck changed. In fact, very few people nowadays know of the original criticism. Today Lamarck is attacked solely on the grounds that he believed in the Inheritance of Acquired Characters.

I then continued by pointing out that this represents only one, the fourth, of his four laws of evolution and that, in fact, this law was redundant in the presence of the second law. It is this latter law which represents true Lamarckism. And yet, despite the fact that all the four laws have been published on several occasions within this century, and despite my published contention that only the second law matters, the modern upholders of the Neo-Mendelian Mutation-Selection Hypothesis insist on shutting their eyes to these facts.

Then taking Lamarckism in its true sense I argued that, on the one hand it showed the utter inadequacy of

Darwin's hypothesis and yet, on the other was unwittingly being used by the Neo-Mendelians in their misguided efforts at translating what they consider to be Darwin's views into the terms of the orthodox mutation-selection hypothesis.

What is the explanation of this apparent paradox? I think it lies in the fact that in evolution there are three distinct processes at work. There is first, the continuous change of the functional system in close correlation with the changing environment which nobody can deny *must* come under the control of natural selection. Secondly there is the changing of pattern and colour and shape and such-like things which may be of no functional importance to the organism and so have no direct relationship to natural selection. And lastly there is the origin of interspecific sterility.

The second process there seems no reason to doubt is somehow related to the existence, or at least to the potentiality of there being, a gene complex, as I pointed out in the last chapter. I say potentially because after all, it must be remembered that, in the vast majority of organisms, no gene complex has been proved to exist. Unless a variety turns up that can be bred with the normal form the existence of a gene complex can only be inferred. But from the universal occurrence of chromosomes in cellular organisms we can safely say that at least there is provision made for the occurrence of genes in such types. And we know from breeding experiments of varieties that genes do control such things as colour and pattern when they bear no functional significance. So that for

example we can accept a gene complex mechanism for the colouration of the vast majority of birds. More particularly the shape and colour of flowers and leaves in many respects we can safely hand over to the Mendelians.

The third process, the development of inter-specific sterility I have suggested elsewhere may be the fundamental process of which the gene complex is merely a secondary result. Varieties are universally taken to represent incipient species, and species are only established when sterility is finally developed between the variety and the normal type. But the varieties differ from the species in a manner which is controlled by the gene mechanism. It follows that the varietal difference *may* be merely an indication of an evolving sterility.

But now, the first process, the continual evolution of adaptedness, this has never been shown to bear any relation to Mendelian inheritance. All that the Mendelians can do is to produce specimens distorted from the perfect adaptation which they know, by their reliance on the intensity of Natural Selection, must occur in all wild types. They have never yet been able to produce by Mendelian mutation a new functioning organ or an adaptation to natural conditions, that is, conditions not interfered with by the artificiality of man.

The mistake made by modern geneticists occurred, as I see it, at the opening years of the century when Mendelism suddenly came to the fore. The geneticists, disgruntled as they were with the obvious inadequacy of Darwinism, jumped at this heaven-sent gift of a method of testing the inheritance of characters experimentally.

And it all fitted in so well with the mutations that de Vries had discovered a dozen or so years previously in his evening primrose.* They did not notice that all the characters that distinguished these sports were such as the shape of the leaf or the fruit, the colour of the flower, and that the same thing applied to the characters on which Mendel worked. It does not appear to have struck any of them that all of these characters are of such a nature that there is no obvious reason for supposing that they have any particular functional significance. But of course the realization of this came ultimately and Neo-Mendelism was born, a cult whose devotees feel free to discard the results of experiment at will and who rely in the final analysis completely on hypothesis. In the last chapter I have shown where this hypothesis leads, to the absolute reliance for the majority of their evolutionary changes on the action of true Lamarckism. Thus I maintain, whichever view you adopt, whether or not you accept the gene theory which as Just[164] described 'gives us a set of demons, the genes, capable of extricating the theorist from any difficult situation . . ', you are finally canalized—if I may use a Neo-Mendelian term—into the true Lamarckian stream.

In the last chapter[165] of my book *The Evolution of Living Things*, I have tried to demonstrate how the great law of Lamarck *must* result ultimately from the very nature of living substance. And the first living substance, the first protoplasm must have appeared long before cells

* It is ironical that de Vries owed his reputation largely to work on a plant named after Lamarck—*Oenothera Lamarckiana*!

and all their complicated appurtenances. That is why the discussion as to whether or not bacteria have nuclei is really only of trivial interest, because surely no one—or at least no one who knows anything about the complexity of the nucleus and the division centre and mitosis—can really believe that there was no pre-nuclear stage of life. It was at this pre-nuclear level—call it virus level if you like—that the only thing which governed the organism was its simple relation with its physicochemical environment. And this *demanded* that, if the protoplasm was to retain its identity, it had to obey the laws of equilibrium of all chemical mixtures, for it was simply a mixture of particular types of chemicals which collectively had certain properties which we know to be characteristic of living forms. And the one inexorable law which controlled this process was simply that if a change takes place affecting the mixture, that reaction will take place which tends to bring the system back into equilibrium. But quite apart from the physicochemical laws, is not all this obvious *and obligatory*, for clearly if such a reaction did *not* take place the mixture would cease to exist and continued existence in a particular form at a particular time is surely essential to evolution.

Now we may imagine the appearance of the first protoplasm not necessarily at only one place on one occasion. In fact such a thing seems highly improbable. It is surely more realistic to take the view that when conditions on the earth became possible for living matter, then wherever the necessary chemicals were present, they would unite together and form themselves protoplasm.

To begin with, each protoplasm as it added to itself and spread—it must have done this as this is one of the characteristics of living matter—would react and change with changing physicochemical environments, and with each change characteristic types of protoplasm would appear, each one perfectly adapted to its surroundings. 'Organic invention', as Vandel said,[166] is a property of protoplasm. There would be a variety of forms in a multitude of places and this would go on until type met type and the environment of each ceased to be simply physicochemical and became biotic. Life would thus have become a variety of types all existing in various physicochemical environments and all being in equilibrium *and having the power to remain in equilibrium* with each other and with the surroundings. But now is not this process simply Lamarck's second law stated in general terms? This ability, this property of the primordial protoplasms, to act in such a way as to preserve themselves—is this not merely an expression of Lamarckism? What is needed will appear. But how will a need arise? It will occur through a change in one or other of the constituents of the biotic environment. This will upset the equilibrium and so the form under consideration will do what is necessary to preserve itself. But now, that is not only the essence of Lamarckism, it is the essence of Evolution. No wonder Packard included as a sub-title in his biography of Lamarck 'the Founder of Evolution'.

Now I believe that this balanced population of varied types of protoplasm, changed with changing conditions, in other words, evolved, each type becoming more and

more complex until ultimately forms with particular shape and size appeared. Of course each shape and size would be controlled by the overriding necessity of remaining in equilibrium in the biotic environment. Ultimately forms with cellular organization appeared, and from then on the *course* of evolution almost ceases to be a matter of speculation. How this change from an amorphous condition to a cellular organization with particulate form came about, we cannot say I am merely underlining the fundamental principle that *must* have governed the transition. I emphasize this because so often I meet the criticism that I cannot demonstrate either experimentally or theoretically how the course of evolution, as I suggest it, can have taken place. No, maybe I cannot, but neither can the Neo-Mendelians explain how *they* think different types have evolved. Do the modern orthodox geneticists believe that they have any real insight as to how their genes produce the eye in the tail feather of a peacock? To talk of it in the terms of de Beer that I have already quoted is simply bathos. I do at least base my argument on a fundamental physicochemical law—le Chatelier's Principle—and, after all, can a biologist go further than interpret his views on a mathematical basis?

Now my views on evolution, as I have put them forward, start from the simplest and work up to contemporary conditions. But that is not the way in which they originated. The thing which impressed me and set me off on my particular line of thought was that wonderful conception[167] of my namesake, W. B. Cannon, of homeostasis—of the power of the organism, if one part of its

body changed, then the rest adjusted themselves accordingly Is not this obvious? To any student of physiology is it not clear as crystal—that any change in the body *must* be followed by adjustments in all other parts of the body How else in view of the omnipotence of natural selection —how else would the body continue to exist. If the heart works faster, must not the capillaries act accordingly? If a motor nerve is continually used, does not the muscle respond by becoming more facile? I saw all this, but then I saw that surely, if every part of a body reacted in such a way as to preserve that body, in any changing environment that body *must* act in a similar way, in a way to preserve itself. If *parts* of the body in relation to other parts can behave in such a way as to preserve their equilibrium within the body, surely the body as a whole, in equilibrium with its surroundings, *must* behave in a similar way, to preserve its own existence. Again as Vandel says[168] 'the genius of Lamarck is shown in having demonstrated that adaptation is an active reaction of the organism *vis-à-vis* the external environment'.

And that is my conception. A living body is not something isolated, neither is it a collection of parts as Darwin envisaged it, like, as I have said before,[169] so many marbles in a box. That is the tragedy of modern genetics. The devotees of the Neo-Mendelian hypothesis regard the organism as so many characters controlled by so many genes. Say what they like about polygenes—that is the essence of their fantastic hypothesis. They do not appear to realize that, apart from all the characters that may or may not be represented in their gene complexes, there is

that phenotypic adaptive power of all organisms which they have been forced to call in to their aid, which *vis-à-vis* Natural Selection renders meaningless all those abstruse analyses of the gymnastics of their gene complexes which are displayed in such 'impressive settings of mathematical learning'. And I think I see how this state of affairs has come about. When Mendelism was rediscovered we were nearing the end of that spate of morphological work which had resulted from the publication of the *Origin*. Biologists all over the world were studying the anatomy of forms, largely under the influence of the museums, with an object in view—to establish the branches and twigs of the evolutionary tree by which modern forms evolved. Dead animals and embryos were being dissected *ad nauseam*, living things were passed by The days of experimental biology had not yet occurred. These only started after the First World War, but by that time the particulate theory of heredity by its brilliant successes in breeding animals and plants under domestication had become firmly established. It became an article of faith of all students of biology to scorn Darwin, to ignore Lamarck, and to accept as the be-all and the end-all of evolution the gospel according to Mendel.

But a living organism cannot be considered as something on its own like a corpse, something isolated, to chop and change independently of what goes on around it. 'It can never be divorced from its milieu.' As Lindsay said,[170] 'In one sense, life is always and everywhere, in all its forms, characterized by . oscillatory changes, . . . tensions of stability and instability by which an organism

preserves both its continuous structural existence and its polarity with its environment.' Is not this conception more satisfying than that unnatural fantasy of sudden and repeated changes occurring without rhyme or reason and without direction? 'The best system of philosophy, then, is that which recognizes living things and outside world as one interdependent continuum' [171]

A living organism is no more an isolated thing than is the earth in its orbit. It is simply part of a superb equilibrium that we call the living world. It is the expression of a power that is beyond man's comprehension and maybe always will be. Can I do better than end in a way in which a very great anatomist ended a lecture. I refer to my friend Wood-Jones. These are the lines of Francis Thompson with which he finished and I can find no better·

> All things by immortal power,
> Near and far,
> Hiddenly
> To each other linked are,
> That thou canst not stir a flower
> Without troubling of a star.

# APPENDIX

Passage from Cuvier's *Eulogy* (1835), p. xix.

D'autres besoins, d'autres désirs, produits par les circonstances, amèneront d'autres efforts, qui feront naître d'autres organes; car, par une hypothèse de plus ajoutée à toutes les autres, ce ne sont pas les organes, c'est-à-dire la nature et la forme des parties, qui donnent lieu aux habitudes et aux facultés; ce sont les habitudes, la manière de vivre, qui, avec le temps, font naître les organes: c'est à force de vouloir nager qu'il vient des membranes aux pieds des oiseaux d'eau; à force d'aller à l'eau, à force de ne vouloir pas se mouiller, que les jambes s'allongent à ceux de rivage; à force de vouloir voler, que les bras de tous se produisent en ailes, et que les poils et les écailles s'y développent en plumes: et que l'on ne croie pas que nous ajoutions ni retranchions rien, nous employons les propres termes de l'auteur.

Translation of same (Anon., 1836) p. 14.

Other wants and desires, produced by circumstances, will lead to other efforts, which will produce other organs: for, according to a hypothesis inseparable from the rest, it is not the organs, that is to say, the nature and the form of the parts, which give rise to habits and faculties; but it is the latter which in process of time give birth to the organs. It is the desire and the attempt to swim that produces membranes in the feet of aquatic birds; wading in the water, and at the same time the desire to avoid wet, has lengthened the legs of such as frequent the sides of rivers; and it is the desire of flying that has con-

verted the arms of all birds into wings, and their hairs and scales into feathers. In advancing these illustrations, we have used the words of the author, that we may not be suspected either of adding to his sentiments or detracting anything from them.

Passage from *Philosophie Zoologique* from which the passage adopted by Cuvier as representing Lamarck's views has been taken (1873 edition), vol. I, p. 248.

L'oiseau, que le besoin attire sur l'eau pour y trouver la proie qui le fait vivre, écarte les doigts de ses pieds lorsqu'il veut frapper l'eau et se mouvoir à sa surface. La peau, qui unit ces doigts à leur base, contracte, par ces écartements des doigts sans cesse répétés, l'habitude de s'étendre; ainsi avec le temps, les larges membranes qui unissent les doigts des canards, des oies, etc., se sont formées telles que nous les voyons. Les mêmes efforts faits pour nager, c'est-à-dire pour pousser l'eau afin d'avancer et de se mouvoir dans ce liquide, ont étendu de même les membranes qui sont entre les doigts des grenouilles, des tortues de mer, de la loutre, du castor, etc.

Au contraire, l'oiseau, que sa manière de vivre habitue à se poser sur les arbres et qui provient d'individus qui avaient tous contracté cette habitude, a nécessairement les doigts des pieds plus allongés et conformés d'une autre manière que ceux des animaux aquatiques que je viens de citer. Ses ongles, avec le temps, se sont allongés, aiguisés et courbés en crochet, pour embrasser les rameaux sur lesquels l'animal se repose si souvent.

De même l'on sent que l'oiseau de rivage, qui ne se plaît point à nager et qui cependant a besoin de s'approcher des bords de l'eau pour y trouver sa proie, est continuellement exposé a s'enfoncer dans la vase. Or, cet oiseau, voulant faire

en sorte que son corps ne plonge pas dans le liquide, fait tous ses efforts pour étendre et allonger ses pieds. Il en résulte que la longue habitude que cet oiseau et tous ceux de sa race contractent d'étendre et d'allonger continuellement leurs pieds, fait que les individus de cette race se trouvent élevés comme sur des échasses, ayant obtenu peu à peu de longues pattes nues, c'est-à-dire dénuées de plumes jusqu'aux cuisses et souvent au delà.

*Note:* A closely similar passage occurs even earlier in the account of the introduction to a course of lectures given by Lamarck in the very opening months of the nineteenth century. This appears as the preface to the *Système des Animaux sans vertèbres* published in 1802.

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