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IS EVOLUTION PROVED?

H. S. SHELTON *has also written :*

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MORE DIFFICULTIES OF THE EVOLUTION THEORY
MAN : A SPECIAL CREATION
A CHALLENGE TO EVOLUTIONISTS
GAME BIRDS
BIRDS AT THE NEST
INDIAN BIRD LIFE
ETC., ETC.

IS EVOLUTION PROVED?

A DEBATE BETWEEN

DOUGLAS DEWAR

AND

H. S. SHELTON

WITH AN INTRODUCTION BY THE EDITOR

ARNOLD LUNN

LONDON
HOLLIS AND CARTER
1947

THIS BOOK IS PRODUCED IN COMPLETE CONFORMITY WITH THE AUTHORIZED ECONOMY STANDARDS
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INTRODUCTION

(1)

In the autumn of 1944 I learned from H. S. Shelton that he had just completed a controversial book in which he had collaborated with Douglas Dewar. I asked if I might see the manuscript, read it with immense interest, and recommended it enthusiastically to Messrs. Hollis & Carter who accepted it for publication.

Many years ago Shelton sent me a paper which he had contributed to *Mind*, which might without disrespect be described as the trade journal of professional philosophers. Shelton's paper struck me as the best essay on that particular subject that I had ever seen. The combination of philosopher and scientist is not common, with the result that most champions of evolution are very weak in logic. Sir Arthur Keith, for instance, in his book on Darwinism points out that there has been, in the course of ages, evolution in everything, in watches, for instance. But the question at issue is not whether men are different from fishes, or whether men appeared on the surface of the planet subsequent to fishes, but whether men are *descended* from fishes and unless Sir Arthur Keith could prove that my wrist watch is a blood relation of a sixteenth-century watch, his analogy has not the least relevance to the issue which is in dispute.

Shelton, who is a Bachelor of Science of London University, has published a large number of papers in *Mind*, *Journal of Philosophy*, *The International Journal of Ethics*, the *Proceedings of the Aristotelian Society* and *Science Progress*. He has written with great lucidity on the methods of reasoning employed in science, a good preparation for this book, since it is not as a rule the facts which are in doubt in this controversy but the inferences to be drawn from them. He has written two books on education, and one of them, *The Theory and Practice of General Science*, is the only book on the theory of general science as yet published in this country. He has also written *The Credibility of the Christian Faith*. He will attempt to show in this correspondence that the truth of organic evolution is so well established as to justify the teaching of it as a fact in an educational course.

Douglas Dewar took a degree in natural science in 1895, and then entered the Indian Civil Service. In India he took up ornithology as a hobby. His observations on birds led him to reject

successively Darwinism, the Darwinian theory of sexual selection and finally the whole concept of organic evolution. The results of his investigations are set forth in *Indian Bird Life* (1921). In 1931 he published his *Difficulties of the Evolution Theory* which was, as I happen to know, widely read among orthodox evolutionists, but in the main ignored in scientific journals. When Dewar, whose competence as a zoologist is admitted, submitted the statistics which he and Levett-Yeats had compiled on the all-important question of the fossil record (see chapter 2), these were not accepted by the Zoological Society of London on the ground that 'this kind of evidence led to no valuable conclusion,' a 'valuable' conclusion being a conclusion in accord with evolutionary orthodoxy. My own interest in evolution began at Oxford where I read Samuel Butler's attacks on Darwinism. I have been reading evolutionary literature, on and off, for the last thirty years, without coming to any very definite conclusions. I reject the theory of mechanistic evolution but I have an open mind on the question as to whether evolution has occurred. I am one of an increasing number of people who acquiesce in the fact that evolutionists are in a great majority, but who are not prepared to accept a dictatorship of evolutionists. We do not want a debatable doctrine rammed down our throats as if it had been proved beyond all possible doubt. We suspect that if the case for the opposition were as weak as it is represented to be, it would be less difficult than it is to get that case ventilated in scientific papers and in B.B.C. broadcasts.

Dewar's admirable persistence has, to some extent, broken down this conspiracy of silence. The evolutionists have been unable to leave his books unanswered, for in 1937 Dr. Morley Davies published his *Evolution and Its Modern Critics*, which was primarily designed as a refutation of Dewar's book.

There is, as I know from my correspondence, an increasing demand for a comprehensive statement of the pros and cons of evolution, a demand which this book should satisfy, for both Shelton and Dewar are masters of their subject. My role as Editor has been largely confined to the watching brief which I hold for the general reader who has no technical knowledge of this controversy, and both disputants have complied with suggestions for clarifying the more difficult points. The order in which the points are discussed is designed to familiarise the general reader with the subject before reaching the more technical chapters. Shelton would have preferred to lead off with the discussion on morphology which

he thinks to be the logical order. He will, however, have an opportunity in the last chapter of stating his case in his own way.

The general reader should not only consult the chart at the end of the book, but study it with some care so as to start the book with a general conception of the geological periods and the order in which the principal types appeared on the surface of the globe. He will find it useful to *memorise* the paragraph which follows. *Organisms are divided into two kingdoms, the animal and the vegetable kingdom. The animal kingdom is divided into phyla. Each phylum is divided into classes, each class into orders, each order into families, each family into genera, each genus into species.*

Thus a fox terrier and a spaniel are different *varieties* of the same *species*. The dog, wolf and jackal are different *species* of the same genus. The wolf and the fox are of different genera of the Canidae or dog *family*. This is one of the families of the *order* Carnivora. The Carnivora belong to the *class* Mammalia (animals that suckle their young), and the mammals belong to the *phylum* Vertebrata (backboned animals). Every animal belongs to a species, genus, family, order, class and phylum. Biologists give every species a double name, denoting its genus and species, thus the domestic dog is CANIS FAMILIARIS—the species FAMILIARIS of the genus CANIS.

The species is the smallest of the divisions which any special creationist would regard as the 'unit of creation.' Species are subdivided into varieties or breeds. Thus the pekinese and the St. Bernard are different breeds or varieties of the dog species.

The word OCtoPus may help the general reader to remember the relative order of the bigger divisions, for O.C.P. are the initial letters of Order, Class, Phylum.

In the original correspondence, Shelton adopted my use of the expressions *major* and *minor* evolution, instead of the more technical terms—*macro-evolution* and *micro-evolution*. By *major evolution* I mean evolution which transcends the limits of the *family*, by *minor evolution*, evolution within the limits of the family.

(2)

My task in this introduction is partly to define the issue, and to make it clear that there are other possibilities than mechanistic evolution and special creation, and partly to explain why a book, such as this, is particularly timely. It is not my task either to

defend or to attack evolution, but rather to expose the foolish conspiracy which represents evolution as a demonstrated fact which only cranks, fundamentalists or eccentrics, comparable to flat-earthers, could possibly deny. If this were true no publishing house of repute would publish this book, and no monthly of the high standing of the *Nineteenth Century and After* would publish three articles within eighteen months attacking evolution.

The man in the street still believes that Darwinism ranks with Copernican astronomy as a scientific certitude which fought its way to recognition in spite of ecclesiastical opposition, but it is at least arguable that Darwinism was accepted, in spite of the weakness of the scientific evidence, for theological or rather for theophobic reasons. Darwin has been anticipated by Buffon, Lamarck and Erasmus Darwin in the promulgation of evolution, and by Patrick Matthew in his advocacy of Natural Selection as the principal agent in the evolutionary process, and his immense success was partly due to the fact that he happened to restate these theories at the precise moment when theophobia (the fear or dislike of God) was on the increase, and when a majority of scientists were looking for some alternative to what Huxley calls the 'untenable theory of special creation.'

Darwin himself was a confused theist, but Darwinism was eagerly exploited by men who were searching for an answer to that great argument from design which was developed by St. Thomas Aquinas and popularised by Paley. Paley compared the intricate structure of the eye, with its numerous delicate adjustments, to the telescope and argued that it would be no easier to believe that the eye could be produced by chance than a telescope. We infer the designer from the design of the telescope, and the creator from the design of the eye.

Darwinism was supposed to provide an alternative explanation for the existence of design in nature, an explanation which rendered unnecessary the hypothesis of a Creator. 'If you can realise,' writes Mr. Bernard Shaw, 'how insufferably the world was oppressed by the notion that everything that happened was an arbitrary personal act of an arbitrary personal God of dangerous, jealous and cruel personal character, you will understand how the world jumped at Darwin.'

Fortunately for the world there are other alternatives than atheism and Calvinistic theism, but given Shaw's premise, the jump to the Darwinism conclusion was not unreasonable.

The argument of Darwinism may be briefly summarised as follows :

Individual members of a species vary, and these variations, however slight, affect the individual's chances of survival. Some individuals, for instance, will be fleeter than others and therefore better able to escape their enemies. A larger proportion of such individuals will survive long enough to reproduce themselves. Favourable variations are perpetuated, unfavourable variations tend to disappear. The gradual and progressive accumulation of small variations, under the influence of natural selection, will produce first a distinct variety, then a distinct species, and then transform one type of animal or plant into another type. Pure chance replaces the Creator. Favourable variations are blindly selected by a mindless environment.

Thus Darwinism appears to render superfluous the hypothesis of a Creator, which explains its attraction not only for atheists but also for all those scientists who would agree with P. Broca: 'Creation c'est le miracle en permanence, c'est la nature asujetté à une volunté et non à des lois—et alors s'il n'y a plus de lois, il n'y a plus de science.' Or as Sabatier puts it: 'God who is the final reason of everything is the scientific explanation of nothing.'*

Darwinism was accepted in spite of the fact that the geological record is eloquent in its witness to the suddenness with which new types appear and to the impossibility of tracing one family into another family by a time lineage series of fossils.

'In answer to the question,' wrote Huxley, 'what does an impartial survey of the positively ascertained truths of palæontology† testify in relation to the common doctrines of progressive modification? I reply: It negatives these doctrines, for it either shows us no evidence of such modification, or demonstrates such modification as has occurred to have been very slight.'

Huxley met Darwin some years before *The Origin of Species* was published, and expressed his 'belief in the sharpness of the lines of demarcation between natural groups and in the absence of transitional forms.' But by 1857 he was 'feeling that some working hypothesis must be found respecting the origin of known organic

* 'Creation implies the permanence of miracle, that is, nature subjected to a Will and not to laws. And if there be no more laws, there is no more science.' Quoted by Ronald Campbell Macfie in his excellent book *The Theology of Evolution* (Unicorn Press), p. 103.

† *Palaeontology*. This word which will recur again and again in this book means the study of *extinct* species whose characteristics can be inferred from their fossil remains.

forms *to replace the untenable separate creation theory*.' Why 'untenable'? Uncongenial no doubt. The ancestry of the horse helped Huxley to escape from the unwelcome hypothesis of a creating Deity.

'We must assume,' wrote Weismann,* 'Natural Selection to be the principle of the explanation of the metamorphoses because all other apparent principles of explanation fail us, and it is inconceivable that there should be another capable of explaining the adaptation of organisms *without assuming the help of a principle of design*.'[1]

'I am thoroughly persuaded,' wrote the great biologist Yves Delage in 1903, 'that one is or is not a transformist *not so much for motives deduced from natural history, as for motives based on personal philosophic opinions*. If there existed some other scientific hypothesis besides that of descent to explain the origin of species many transformists would abandon their present opinion as not being sufficiently demonstrated . . . If one takes his stand upon *the exclusive ground of facts* it must be acknowledged that the *formation of one species from another species has not been demonstrated at all*.'[2]

This same Delage, after pointing out with infinite regret the weak points in Darwinism, added: 'Whatever may befall this theory in the future, Darwin's everlasting title to glory will be that he explained the seemingly marvellous adaptation of living things by the mere action of natural factors without looking to a divine intervention, without resorting to any finalist or metaphysical hypothesis.'

In other words, Darwin's everlasting title to fame is that he provided the atheist with a plausible if untenable answer to Paley's argument from design.

Darwinism, as the great scientist *von Uexhull* said, '*is more a religion than a science*. Its logical consistency leaves as much to be desired as the accuracy of the facts on which it is based. That is why all arguments against it remain ineffective. It is nothing but the embodiment of the determination to rid nature at any cost of the principle of order (Planmässigkeit). In this way the idea of evolution has become the sacred conviction of thousands, *a conviction that has no longer anything to do with unbiased scientific research*.'

Theophobia as the motive for the acceptance of Darwinism

* Italics always mine unless the contrary is stated. The italics, which are Weismann's, emphasise the horror with which he rejected the alternative of divine creation, but there is nothing scientific about the confession 'We must *assume*' to be true the only plausible alternative to the hypothesis of a creating God.

emerges very clearly in the following quotation from *du Bois-Reymond* : 'Whoever does not place all activity wholesale under the sway of Epicurean chance, whoever gives only his little finger to teleology will inevitably arrive at Paley's discarded *Natural Theology*, and so much the more necessarily the more clearly he thinks and the more independent his judgment . . . The possibility, ever so distant, of banishing from nature its seeming purpose, and putting a blind necessity everywhere in the place of final causes, appears, therefore, as one of the greatest advances of the world of thought, from which a new era will be dated in the treatment of these problems. To have somewhat eased the torture of the intellect which ponders over the world-problem will, as long as philosophical naturalists exist, be Charles Darwin's greatest title to glory.'

Professor D. M. S. Watson who was the principal speaker in a series of broadcasts (October 2nd to December 18th, 1942) informed a body of scientists at Cape Town that '*Evolution itself is accepted by zoologists not because it has been observed to occur or can be proved by logically coherent evidence to be true*, but because the only alternative, special creation, is clearly incredible.'[3]

Biologists have, of course, been always free to criticise particular theories of evolution, such as Darwinism, but the truth of organic evolution has been accepted as a quasi-religious dogma which it would be blasphemous to criticise. Whereas a Christian can be a Darwinist, an atheist cannot reject evolution. Mechanistic evolution is *de fide* for the atheist, and consequently evolutionary controversy is embittered by the odium anti-theologicum. Evolution, as Sir Arthur Keith remarks (*Darwinism and Its Critics*), is a basic dogma of rationalism.

There are welcome signs that the religiosity which has so far protected the dogma of evolution from scientific scrutiny is breaking down. Dr. W. R. Thompson, after remarking that 'the concept of organic evolution' was 'an object of genuinely religious devotion,' adds : 'this probably is the reason why severe methodological criticism employed in other departments of biology has not yet been brought to bear against evolutionary speculation. *There are, however, indications that this criticism will not long be delayed.*'[4]

The great majority of biologists still accept organic evolution, and the appeal to authority would therefore tell heavily in Shelton's favour, but the quotations which follow establish the one fact which I am concerned in this introduction to establish, the existence of an important minority of distinguished scientists who either

reject evolution or who realise that evolution still lacks adequate proof. I shall only quote a small selection of representative men. It is perhaps hardly necessary to point out that the letters F.R.S. (Fellow of the Royal Society) designate a member of the scientific *élite* of Great Britain.

The minority of those who have the courage to challenge the dogma of evolution is, of course, far smaller than the number of those who dare not express their doubts. It is, as a Fellow of the Royal Society once remarked to me, professional suicide for a biologist to attack organic evolution as such. 'The tyranny of the *Zeitgeist* in the matter of evolution,' wrote Dwight, Parkman Professor of Anatomy at Harvard, 'is overwhelming to a degree of which outsiders have no idea. Not only does it influence (as I admit it does in my own case) our manner of thinking, but there is oppression as in the days of the Terror. How very few of the leaders of science dare to tell the truth concerning their own state of mind.'[8]

And now for a few quotations from the sceptics who *have* the courage to proclaim their scepticism. *Sir Ambrose Fleming, F.R.S.*, was a convinced creationist. He rejected evolution. *Sir J. William Dawson, F.R.S.* (obit. 1899), Professor of Geology and Principal of McGill University, writes: 'the evolutionist doctrine is itself one of the strangest phenomena of humanity, but that in our day a system destitute of any shadow of proof, and supported merely by vague analogies and figures of speech, and by the arbitrary and artificial coherence of its own parts, should be accepted as a philosophy, and should enable adherents to string upon its thread of hypotheses our vast and weighty stores of knowledge is surpassing strange.'[9]

Dr. Fairfield Osborn, former Curator of the American Museum of Natural History, an evolutionist troubled by doubts, writes: 'Between the appearance of the *Origin of Species* in 1859 and the present time there have been great waves of faith in one explanation and in another; each of these waves of confidence has ended in disappointment, until finally we have reached a stage of very general scepticism.'[7]

Dr. Austin H. Clark, of the United States National Museum, Washington, writes: 'Thus so far as concerns the major groups of animals, the creationists seem to have the better of the argument. There is not the slightest evidence that any of the major groups arose from any other.'[8]

Professor D. H. Scott, F.R.S.: 'For the moment at all events, the Darwinian period is past. We can no longer enjoy the com-

fortable assurance which once satisfied so many of us, that the main problem has been solved, all is again in the melting pot.'

Professor T. H. Morgan, Nobel prize winner in 1933 : 'Within the period of human history we do not know of a single instance of the transformation of one species into another, if we apply the most rigid and extreme tests used to distinguish wild species from each other. It may therefore be claimed that the theory of descent is lacking in the most essential feature that it needs to place the theory on a scientific basis.'[10]

European scientists have, on the whole, been less uncritical in their acceptance of evolution than British scientists. *Oswald Spengler* was not a scientist, but he was well informed about scientific opinion, and the following passage from his famous book, *The Decline of the West*, is evidence as to growing scepticism about evolution. Spengler's religious views are difficult to discover, but his theology seems to be pantheistic. In the following passage he uses 'Darwinism' as a popular equivalent for 'evolution.'

'Palaeontology,' he writes, 'furnishes the most conclusive refutation of Darwinism. According to the laws of probability, fossil deposits are only test samples. Each sample should therefore represent a different phase of evolution, and in this case there would be no transitional forms, no boundaries, and also no species. Instead of this we find completely stable and unchanging forms persisting through long ages, forms which have not evolved in accordance with the principle of adaptation, but appear suddenly and at once in their final form.' And elsewhere Spengler remarks that 'the Materialism and the Monism and the Darwinism, which stirred the best minds of the nineteenth century to such passion, have become the world-view proper to country cousins.'[11]

The most remarkable evidence of increasing scepticism about evolution is contained in the volume which lies before me as I write, the fifth volume of the *Encyclopédie Française*, which is devoted to *Les Êtres Vivants. Plantes et Animaux.* To this volume, published like its predecessors by the famous house of *Larousse*, eminent French scientists contribute. The editor, Paul Lemoine, was a former Director of the National Museum of Natural History at Paris. The concluding essay in this volume is by Lemoine and is entitled 'Que Valent Les Theories de l'évolution ?' 'What are the theories of evolution worth ?' Lemoine answers, in effect, that they are worth nothing. 'The theories of evolution,' he writes, 'in which our student youth was cradled, constitute a dogma which all

the world continues to teach : but each in his speciality, zoologist or botanist, comes to the conclusion that none of the available explanations are adequate.' Lemoine then analyses in detail the difficulty of reconciling evolution with palaeontology and biogeography. And he concludes his chapter with the following words : 'Il resulte de cet exposé que la theorie de l'évolution est impossible. . . . Cela il faut avoir le courage de le dire, pour que les hommes de la génération future orientent leurs recherches d'une autre façon.' (It results from this summary that the theory of evolution is *impossible*. . . . It is necessary to say this in order that future generations may orientate their researches in another fashion.)

(3)

The evolutionist who is forced to concede the strength of the case against evolution usually asserts that 'evolution is the only *scientific* hypothesis for explaining the origin of species.' I disagree. 'All the new types appeared suddenly on the surface of the globe' has as much right to be considered a scientific hypothesis as the statement : 'All new types have evolved *gradually* from existing types.' These rival hypotheses must be compared with reference to the palaeontological, morphological and embryological evidence. If the evidence suggests that new types appeared suddenly, we can still defend the hypothesis that they were spontaneously generated, for it is only the defect of imagination which blinds us to the complexity of the first living organism and which makes it easier for us to accept the spontaneous generation of a one-celled organism than of a bird. Admittedly if we could establish the fact that new types had appeared suddenly, most people would accept special creation, but why this conclusion, *if in accord with the facts*, should be less 'scientific' than evolution I cannot see. If, on the other hand, we mean by 'evolution' the theory that species have evolved by a purely natural process, and if by 'scientific hypothesis' we mean a hypothesis which excludes supernatural agencies, the statement : 'Evolution is the only scientific hypothesis' is a glimpse of the obvious.

Consider the two following hypotheses. *First, the hypothesis of mechanistic evolution.* Life appeared on the surface of the cooling planet by an action of spontaneous generation. From the mud, rocks, mists and sea of the primeval planet all living things evolved by a purely natural process. *Second*, the hypothesis that you can't extract plus from minus, or qualities in an effect which were not

present in the cause. No purely natural agency would therefore have sufficed to transform the mud, rocks, mists and seas of the primeval planet into the minds which enriched the world with the sonnets of Shakespeare or with Beethoven's *Ninth Sonata*.

'Science' means knowledge, and a good scientific hypothesis is a hypothesis which takes into account all available and relevant knowledge. If supernatural agencies have played a part in the origin and development of species, a valid scientific hypothesis will allow for such agencies. Nothing could be less scientific than to rule out in advance any particular class of agencies, natural or supernatural, as unworthy of investigation.

It is, of course, the duty of the scientist to make every possible attempt to explain phenomena in terms of natural agencies, but it would be unscientific, as Shelton would agree, to cling tenaciously to an unsound hypothesis merely because its rejection would involve the admission that supernatural agencies were partially responsible for the phenomena under investigation.

I have sometimes wondered whether the irrational and fanatic resistance to the possibility of miracles was wholly unconnected with the mental climate of the England in which Deism was popularised. Deism, the theory that God created the world, and then left it severely alone, became fashionable in England at the time of the 'Glorious Revolution' of 1688, the revolution which substituted a limited for an absolute monarch. The god of Deism is a constitutional monarch. The King of England has a theoretic right to veto laws approved by Parliament, and the God of Deism has a theoretic right to veto the laws of nature, but neither the King of England nor the King of Kings would venture to exercise these theoretic rights. Miracles are as objectionable to the Deist as the arbitrary acts of an absolute monarch to the historian of the Whig tradition. Deism, in fact, might be defined as constitutional Theism.

Before attempting to summarise the attempts to explain the origin of species, it will be as well to determine in advance our standards of proof. The statement that the depth of the sea is greater than five feet admits of *coercive* proof. The sceptic would be coerced into this belief by throwing himself off the deck of a cross-channel steamer. Neither Shelton nor Dewar expect to *coerce* the unbiased reader into acceptance of their respective positions. All that they claim to offer is *persuasive* proof, proof which should persuade an intelligent and reasonable reader.

Nobody has ever witnessed an act of special creation, and

nobody has witnessed the transformation of species, so far, at least, as the animal kingdom is concerned. Both the evolutionists and the special creationists rely on inferences from the geological record and from the structure of living things in the world to-day. Evolutionists sometimes remark with airy denigration that 'special creation' is one of those theories that one can neither prove nor disprove. Much the same might be said of evolution. Neither the evolutionist nor the special creationist claim that they can provide coercive proof. Both claim to provide proof which is believed to be 'persuasive.' The burden of proof, both in the case of evolution and in the case of special creation, lies on him who asserts. If, for instance, you were to assure me that the dog who has just come into the room in which we are sitting was specially created in the course of to-day I should reply by quoting Bishop Butler's remark: 'Probabilities are the guide to life.' It might be difficult to disprove your assertion, but having acted as midwife to a spaniel and seen dogs born and met dogs at every stage of existence from new-born puppies to senile and decrepit veterans, I shall continue to believe that the dog who has just entered the room was born in the normal way unless you produce coercive proof to the contrary. I have never seen a dog specially created and I have never seen one type of animal evolve into another type, and I shall therefore continue to record a verdict of 'not proven' until either the special creationist or the evolutionist produce persuasive proof in favour of their views.

We may reject a hypothesis which cannot be coercively refuted, if it is unsupported by adequate evidence or if it conflicts with conclusions based on other premises. Thus Mr. Philip Gosse's theory, described below, that the world was created in six working days of twenty-four hours each, complete with fossils to suggest that the planet had been in existence for millions of years, conflicts with the conclusions of rational theology.

Mr. Gosse, who did not believe in evolution, was embarrassed by the fossils in the geological record, and one might therefore reasonably expect that the evolutionist would not only welcome the appeal to the geological record, but also make every effort to prove that this record is reliable, but in the original form of the correspondence between Dewar and Shelton, it was Shelton who was urging with eloquence that the geological record is imperfect and fragmentary, and Dewar who was maintaining that it is, on the contrary, representative and sufficiently complete to serve as the basis for definite conclusions.

The truth is that the geological record, though it negatives the crude fundamentalism which asserts that the world was created in six working days of twenty-four hours each, is, perhaps, easier to reconcile with special creationism than with evolution, for all the new types appear suddenly in that record. It is impossible to bridge the gap between any of the families in nature by means of a *true lineage series of fossils*, by which I mean a gently graded series of fossils linking an ancestor A, with a descendant B, each member of the series being slightly less like A and slightly more like B than its immediate predecessor in the series. Not only is it impossible to produce a true lineage series between families, and *a fortiori* between classes, orders, or phyla, but as Vialleton, for fifty years professor of comparative anatomy at the University of Montpellier, says: 'It has only rarely been found possible to trace a genus, step by step, and without artifice into an earlier genus, moreover, when this can be done, it is never a case of two creatures essentially different in their organisation but of neighbouring forms of which the organisation continues in the same line.' No lineage series of fossils connecting families, much less different classes, orders, phyla have been found.

Shelton is not only a scientist but a metaphysician. I therefore hope he will solve what always seems to be a logical dilemma in the evolutionary case. Shelton would agree that the fossil of a horse in the earliest Cambrian strata or of a human skull in the secondary strata would knock the bottom out of the evolutionary theory. By what right does the evolutionist assert that the missing volumes in the geological record contain *no* evidence difficult to reconcile with evolution (e.g. *equus* fossils in the Cambrian) and *all* the evidence he requires to prove evolution, i.e., the millions and millions of missing links?

Why should the evolutionists be entitled and the anti-evolutionist not entitled to draw definite conclusions from the *absence* of fossils from any particular strata? Surely the geological record is either sufficiently complete to draw definite conclusions, in which case we can assert with confidence not only that fishes appeared before men, but also that the absence of missing *chains* (for it is the chains which are missing, the links which are present) is fatal to the theory of slow gradual evolution, or alternatively that the record is so fragmentary that no conclusions of any value, favourable to or hostile to evolution, can be drawn from it.

(4)

All theories which profess to explain the origin of species may be divided into four main groups.

1. *Atheistic evolution.* There is no God and therefore no supernatural creative activity. The origin of life and the origin of species are explicable as the result of natural agencies and natural law.

2. *Deistic evolution.* God exists and created the world, but does not interfere in the process of creation. There is nothing miraculous in the origin of life or in the origin of species. Both can be explained as the result of natural agencies without invoking supernatural intervention. For controversial purposes, atheistic and Deistic evolution are indistinguishable, but a man need not be a Deist in theology to be a deistic evolutionist. Many Catholic biologists are Deistic evolutionists, so far at least as the evolution of man's *body* is concerned.

3. *Special creation.* There is nothing in this theory repugnant, as Philip Gosse's theory is repugnant, to an exalted conception of God. Neither the philosopher nor the scientist can adduce a single valid reason against the *possibility* of special creation. The principal obstacle to the acceptance of special creation is neither science nor philosophy, but fashion. The mental climate of the day renders it difficult for us to accept special creation. Phrases such as 'fundamentalism,' 'the Bible belt,' etc., handicap the special creationist by importing emotional prejudices into what should be a purely scientific discussion, but our attitude to this hypothesis should be determined by the evidence. If, for instance, the evolutionist could produce true lineage series of fossils linking family with family, and class with class, and order with order, and phylum with phylum, I for one would have no hesitation in rejecting special creation. If, on the other hand, the special creationist had a completely satisfactory answer to the horse series, to vestigial remains, and to the embryological evidence, I should regard the suddenness with which new types appear as conclusive in favour of special creation.

4. *Theistic evolution,* differs from *special creation* in that it postulates the evolution of man's body from that of the simplest forms of life, and differs from *Deistic evolution* in that it invokes supernatural activity to bring about the more radical changes in the human pedigree. Natural agencies, according to this view, are

adequate for minor evolution but require to be supplemented by supernatural agencies to provoke major evolution.

Wallace who shares with Darwin the credit for promulgating the theory of Natural Selection did not believe that natural forces alone could account for the ascent of man. He suggested that there were 'three stages in the development of the organic world when some new cause or power must necessarily have come into action, the first when the first living cell was created, the second when the animal kingdom separated from the vegetable kingdom, and the third at the creation of man.'

We can either opt for one of these four theories, or can return a verdict of 'not proven' against any one or all of these theories. Many readers of this book will begin with strong prejudices for or against evolution, and such readers (and reviewers) will pounce with delight on anything which tells in favour of their views, and will ignore what tells against them, but there will be others who after weighing the difficulties of evolution against the difficulties of special creation will refuse to commit themselves either to special creation or to any form of evolution, atheistic, deistic or theistic.

For many years I studied snow and avalanches, and my theories have stood the test of searching examination by scientific investigators in different countries because I never felt under the least obligation to pretend that problems which were unsolved had, in fact, been solved. I refused to choose between rival theories if both theories seemed to me unsupported by evidence. Any form of research which affects men's lives or pockets discourages irresponsibility. A rash diagnosis of a snowslope, or even a faulty theory about snow, expressed in print may result in a fatal avalanche accident, but nobody is a penny the worse if an evolutionary pedigree proves to be false, all of which encourages a certain light-hearted irresponsibility, unknown in other branches of scientific research.

I am inclined to suspect that the agnosticism which returns a verdict of 'not proven' on the evolutionary issue is more common in scientific circles than it was. 'The only statement,' wrote the great biologist Reinke, 'consistent with her dignity that science can make is to say that she knows nothing about the origin of man.'[12] 'We have reached,' says Dr. Fairfield Osborn, 'a stage of very general scepticism.'[13] 'A student,' writes Sir Albert Seward, F.R.S., 'who takes an impartial retrospect soon discovers that the fossil record raises more problems than it solves . . . we want to know where we are; faith, as Dr. Bateson says, has given place to

agnosticism ; it is necessary to start afresh, to see things as they are, and not as we think they should be.'[14]

The transformation of agnosticism into faith will not be achieved by a process of piling up new evidence, so long as the unresolved difficulties remain unexplained.

(5)

St. Thomas Aquinas said that no rational argument was possible until a common premise had been discovered, and for this reason he began his famous five proofs of the existence of God with a premise common both to the theist and to the atheist, the fact that 'some things are in motion.'

It will clarify the discussion if we begin by deciding what premises are and what are not common to Shelton and to Dewar. Here then are my suggestions for premises which I believe that they will both accept, though Shelton may perhaps cavil at one or more of them.

1. *A hypothesis is not necessarily plausible because it cannot be refuted.*

You cannot refute the man who believes that he alone exists and that the world process exists only in his imagination.

2. *A hypothesis deserves consideration* if

(a) it is not inherently absurd, and *if*
(b) it is supported by plausible arguments and *if*
(c) a plausible reply can be made to the arguments against it.

3. *The hypothesis of a Creator is not inherently absurd.*

It would be an understatement to describe this as an understatement.

4. *We must not accept a miraculous explanation of any phenomenon before we have examined all possible explanations in terms of natural causes.*

Dewar and I began as evolutionists. Dewar is a special creationist and I am, so far as evolution is concerned, an agnostic, because all attempts to explain the evolutionary process solely in terms of natural law seem to be completely unconvincing.

5. *The hypothesis that the different species*, or *the different genera*, or *the different families came into existence by an act of special creation is not inherently absurd.*

Huxley admitted that 'the philosophical difficulties of theism are neither greater nor less than they have ever been since theism was invented.' He also admitted that ' "creation" in the ordinary sense of the term is perfectly conceivable' and that 'The *a priori* arguments

against theism, and given a deity, against the possibility of creative acts, appear to me devoid of reasonable foundation.'

In other words that there is nothing inherently improbable either in the existence of a Creator or in creative acts.

Special creation qualifies under 2(a), 2(b) and 2(c) as a hypothesis which deserves consideration because it is not inherently improbable, because it is supported by plausible arguments, and because a plausible reply can be made to the arguments against it. Special creation, the traditional belief of Christendom, is consistent with the noblest conception of God. It does not offend, as Philip Gosse's theory offends, our sense of ethical probabilities.

It is, of course, easy to understand that those whose mental outlook is entirely coloured by the prevailing fashions in thought often refer to special creationism as if it ranked with flat-earthism, forgetting that whereas no scientist of standing believes that the earth is flat, many distinguished scientists believe in special creation.

I can understand this attitude because to some extent I am affected by it. I find it difficult to believe in the creation of, say, a bird *ex nihilo*, not because there is *a priori* reason why the Creator should not create a bird *ex nihilo*, but because I am influenced by the mental climate of the age in which we live. And for this reason I can appreciate the attraction of Deistic creation, for this hypothesis relegates the act of creation to the remote past before the earth came into existence, and absolves us from the tiresome necessity to allow for creative acts in the evolution of species.

There is, of course, no *a priori* reason why creative acts should have come to a sudden end before the first living cell appeared on the surface of the planet.

6. The Ambi-Neuter Postulate.

Facts which are equally consistent with BOTH of two rival theories can be cited in support of NEITHER of these theories.

Ambi-neuter is a clumsy word, but until Shelton can think of a better, I propose to use it, for a clumsy word is better than none in so far as it saves space in future references to a postulate of immense importance in this controversy.

If special creationism and evolution are hypotheses, both of which deserve consideration, we must clearly not cite facts which are equally easy to reconcile with both hypotheses as if they told in favour of either of them.

Thus the fact that fishes appear on the surface of the planet before men is equally consistent with evolution and with the first

chapter of Genesis. Again there are many facts of geographical distribution which are as easily reconciled with the theory that evolution began on the mainland as with the theory that species which had been specially created on the mainland subsequently migrated across land bridges which have now disappeared, to the various islands in which they have been found.

Again an intermediate type wholly distinct from the two types between which it is alleged to be intermediate yet containing characteristics of each type is as easy to reconcile with the creationist as with the evolutionist hypothesis. There is no *a priori* reason why the Creator should begin *de novo* with each type, or why he should not introduce into a new type some features which have occurred in earlier types. The human creator does not begin *de novo* every time he tries to design a new car or a new plane. The Fighter Bomber is an intermediate between the Fighter and the Bomber, but the Fighter Bomber is not a blood relation of the Fighter.

Shelton may be tempted to retort that the *ambi-neuter* postulate might be invoked to justify absurd theories such as for instance the theory put forward in *Omphalos*. The author of *Omphalos* was Philip Gosse, a great naturalist and a Plymouth brother, and the thesis of Omphalos is that God created the world, just as it is, fossils and all complete. The world was created in six days, of twenty-four hours, somewhere about 4000 B.C. As the world and the fossils were created together, the world instantly presented the structural appearance of a planet on which life had existed for millions of years.

No coercive disproof of this hypothesis is possible, but Gosse's theory of creationism does not qualify as a hypothesis which needs to be taken seriously under 2(b) because it is not supported by plausible arguments.

Moreover we can reject it under 2(a), for it conflicts with our sense of ethical probabilities. It is, as Charles Kingsley said, 'Unthinkable that God has written on the rocks one enormous and superfluous lie.' Admittedly there are *many* facts which are equally easy to reconcile with Gosse's theory and with Dewar's conception of creationism, and *those* facts cannot be cited as evidence against Gosse. We reject Gosse's theory not by citing facts equally consistent with his theory and ours, but by an appeal, in the first place to 2(b), since Gosse's theory is unsupported by any evidence, and in the second place because it is inconsistent with rational theology.

Similarly if Shelton could produce a true lineage series of fossils linking family to family, class to class, and order to order, and phylum to phylum we should accept this as decisive evidence for evolution in spite of the fact that it would still be possible to assert that every species, represented in the graded lineage series linking class with class, had been specially created.

We should reply that coercive evidence, such as this, for evolution could only be rejected by those who were prepared to believe that God had 'written on the rocks one enormous and superfluous lie.'

The *ambi-neuter* postulate is self-evident. If, for instance, *all* the facts which Dewar and Shelton could produce were equally easy to reconcile with evolution and creationism, agnosticism would be the only reasonable attitude, but this is not the case. There are facts, which are easier to reconcile with evolution than with creationism and there are other facts which are easier to reconcile with creationism than with evolution.

Both theories, special creationism and evolution qualify under 2(a), (b) and (c) as theories which deserve consideration, and neither Shelton nor Dewar will therefore attempt to *sneer* their opponent out of court.

(6)

The omission of all discussion of the *causes* of evolution was a serious defect in the original correspondence. Dewar, at least, can suggest a *vera causa* for living species. Clearly if one believes in God there is no *a priori* reason why the different species (or genera, or families) should not have come into existence as acts of special creation. The atheistic or Deistic evolutionist must attempt to produce an adequate substitute for the creative power of God. He must suggest a *vera causa* capable of evolving from the mud, sand, mists and sea of the primeval planet the living things which we see around us, and also many other things which do not exist in inorganic matter, as for instance our reaction to beauty.

May I therefore invite Shelton to give his idea of the *vera causa* of evolution and then to comment on the following points and show how they can be explained in accordance with his theory.

1. Natural Selection is a negative process. It cannot select what is not there. The survival of the fittest, as somebody remarks, does not explain the arrival of the fittest. The presence of weeds in a garden is not explained by the fact that the gardener has not removed them. Natural Selection, Darwin argued, may be said

to create a new species out of fortuitous variations as truly as man may be said to create a new building out of the material provided by the stones of various shapes. He accepted variations as a fact, but made no attempt to explain the origin of these variations, out of which the negative action of Natural Selection was alleged to create new species. But unless we can explain the origin of variations, we cannot hope to explain the origin of species.

My first contention is therefore that purely natural forces are inadequate either to produce from the mud, seas, mist and rocks of the primeval planet the variations which are the raw material for Natural Selection, or to bridge the gulf between the different types.

2. Here is a passage which I quoted some years ago from *The Modern Adventure* by Mr. W. J. Blyton. Mr. Blyton gives no reference, but it would be an insult to Shelton to suggest that he would need the context to understand a passage which is characteristic of Whitehead's clear and logical reasoning. I draw Shelton's particular attention to the italicised passages in which Professor Whitehead suggests that Darwinism offers no explanation (a) why the process should ever have occurred (b) why the trend of evolution should be upward. What is Shelton's answer to (a) and (b)? Professor Whitehead points out that the doctrine of organic evolution does not in the least explain why the process should ever have occurred. '*The phrase "survival of the fittest" offers no explanation. For life itself has very little survival value compared with the inorganic matter from which it sprang.* A rock survives for hundreds of millions of years, whereas even a tree lasts only a thousand years. If survival was what Nature aimed at why should life appear at all? *Again why should the trend of evolution be upwards so that higher and higher types are evolved?* The doctrine of evolution does not explain this. *The upward trend cannot be due to the influence of environment, for the lower types are just as well adapted to their environment as are the higher types.*'

Professor A. N. Whitehead, F.R.S., is an eminent mathematician and philosopher. I should be grateful for Shelton's comments on the italicised passages.

3. It is difficult to understand how natural selection could in Darwin's words create new species out of fortuitous variations which in their stage are not advantageous. The feathers of the first bird are supposed to have evolved from the scales of a reptile. 'Fine feathers do not make a bird,' nor a few feathers a flyer. Feathers would have to be present not only in great quantity, but also in an advanced stage of development before the parent bird could

even glide downwards from a tree, let alone lift itself from the ground.

4. If evolution is a gradual process (and if a reptile is mutated into a bird overnight by a sudden *mutation*, you might as well postulate special creation) then there must have been a time in the past when the world was full of transitional types, reptiles with a few rudimentary feathers for instance. The evolutionist must therefore explain not only the apparent suddenness with which new types appear in the geological record, but also the sharpness of the lines which divide species in the world to-day. Darwin in the sixth chapter of his book tries to meet this difficulty. 'Why do we not see everywhere,' he writes, 'innumerable transitional forms? Why is not all Nature in confusion, instead of the species being as we see them, well defined?' Darwin's answer was that the evolution of species is determined among other things 'by the presence of other species, on which it lives, or by which it is destroyed, or with which it comes into competition, and as these species are already defined objects, not blending into each other by insensible graduations, the range of any one species, depending as it does on the range of others, will tend to be sharply defined.'

Now, in the first place, as Kellogg remarks, this is a *petitic principii:* 'The sharp definition of species, that we started out to account for, is explained by the sharp definition of other species.' And, in the second place, Darwin ignores the fact that there must have been a time in the past when all Nature was 'in confusion,' when the slow changes which he postulates were taking place. His explanation virtually assumes that evolution has ceased to operate.

5. It seems to me impossible to explain the evolution of very complex organs such as the eye, which consist of several parts, parts which cannot function satisfactorily unless they are accurately fitted together. 'One might possibly,' writes Wolff, 'imagine the adaptation between one muscle cell and one nerve end through selection among innumerable variations, but that such should take place in a thousand cases in one organism is inconceivable.'

As Berg, a Soviet scientist remarks, 'the probability that all useful variations will simultaneously occur is the probability of a miracle.'

If there be no directive and supernatural intelligence at work in the controlling and ordering of the development of life, we have to endow *pure chance* with miraculous power.

Is it conceivable that so complex an optical instrument

as the eye could be improved by *pure chance*? Mr. Noyes, in his book *The Unknown God*, quotes a Savilian professor of astronomy on this subject. 'Suppose, for instance, one of the surfaces of the crystalline lens of the eye to be accidentally altered, then I say that unless the form of the other surface is *simultaneously* altered in one only way *out of millions of possible ways*, the eye would not be optically improved. An alteration in the two surfaces of the crystalline lens, whether accidental or otherwise, would involve a definite alteration in the form of the cornea, or in the distance of its surface from the centre of the crystalline lens, in order that the eye may be optically better. All these alterations must be simultaneous and definite in amount, and these definite amounts must coexist in obedience to an extremely complicated law.

'To my apprehension then that so complicated an instrument as the eye should undergo a succession of millions of improvements by means of a succession of millions of accidental alterations is no less improbable than if all the letters in *The Origin of Species* were placed in a box and on being shaken and poured out millions and millions of times should at last come together in the order in which they occur in that fascinating and, in general, highly philosophic work.'

Again it is very difficult to explain by the unplanned selection of chance variations the existence in the same organism of identical structures. 'It cannot,' writes Wolff, 'be explained by selection how the carnivores, for example, can have developed through fortuitous yet always similar variations, two such structures agreeing in all details as back teeth, which have developed in course of time from small skin teeth. That a tooth can develop into such an admirable biting organ through chance variation can be explained by selection, but that the tooth standing next to it shall have varied always *in exactly the same way* so that the result of its development shall make it *identical with the other one*, is inexplicable by selection on a basis of fortuitous variation, but rather indicates that the change of form is ruled by law which we do not know.'

6. Finally how do you explain the sense of beauty? Was the emotion which the Wetterhorn, the *Sixth Symphony*, and Chartres evoke in me implicit in the mud, rocks, mists and sea of the primeval planet? Darwin's attempt to meet this difficulty was characteristically evasive. He assumed what it was his business to explain, the existence of a sense of beauty and then proceeded to discuss the influence of this sense on evolution. The female is attracted

by a beautiful mate, with the result that ugly varieties tend to end their days disconsolate bachelors, whereas beautiful varieties leave a numerous progeny. No doubt, but *why does the peahen think the peacock beautiful*? Is it conceivable that a blind and fortuitous process, undirected by intelligence, could have evolved from lifeless matter of the primeval planet the power to create and the power to appreciate beauty? The art of primitive man proves him to have possessed the power to create beauty, and those who accept Darwin's theory of sexual selection must attribute to the animal creation the power to appreciate beauty.

The points on which I invite you to comment raise no difficulty, either for the special creationist or for the theistic evolutionist. The latter believes that the body of man has evolved from the body of animals, but he affirms that this process has throughout been guided and directed and controlled not only by natural, but also by supernatural agencies. The theistic evolutionist does not believe that the blind and unplanned and fortuitous forces of nature are sufficient to evolve from the mud, sand, mists and seas of the primeval planet the brain that conceived and the minds that enjoy Beethoven's *Sixth Symphony*.

The Deistic evolutionist, on the other hand, affirms that it is unnecessary to postulate any supernatural forces or miraculous intrusions to account for the origin and evolution of species. And it seems to me, though I may be wrong, that the atheistic and Deistic evolutionists are necessarily forced to adopt some form of neo-Darwinism. If we reject a supernatural control of the evolutionary process, we must fall back on the blind and fortuitous processes, and thus inevitably rely more and more on natural selection as the key to the evolutionary process. For myself I agree with Dr. Inge that 'the notion that a species is evolved blindly and fortuitously could only be accepted when it was rammed down our throats.'

Let me end with a suggestion to the reader. If his mind is closed to evidence which conflicts with his beliefs he will, of course, 'floodlight' those parts of the book with which he is in agreement. If he is an evolutionist his pupils will dilate when he reads Shelton on the evolution of the horse and contract when he reads Dewar on the suddenness with which new types appear. Equally, if he is a special creationist his pupils will dilate when he reads Dewar on nascent organs and contract when he reads Shelton on vestigial remains.

If, on the other hand, he has an open mind on the whole question he will not adopt the floodlighting technique.

Whether a verdict arrived at by taking into consideration all the relevant facts would be in favour of or against evolution is for the reader to decide. I suggest that he should keep a careful round-by-round record of this controversy, crediting the two opponents with points as if he were the referee in a boxing match. Readers who adopt this method will differ as to who wins on points, but I am confident that they will be grateful to both controversialists for the skill with which they have stated the case for and against the hypothesis of evolution.

ARNOLD LUNN

The Athenaeum

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I

CAUSES OF EVOLUTION

DEAR DEWAR,

Well, as the saying is, here we are again. We are indebted to Lunn for finding us a publisher, in spite of war-time difficulties, and have agreed to revise our work so as to make it more easily understood by the plain blunt man. We are also indebted to Lunn for an introduction, the consideration of which will take the greater part of this letter.

This revision involves extra work on my part which I did not anticipate; still, on the whole, I think it worth while. I have considerable sympathy with minority views, as in many things I am in a minority myself. I believe in free discussion. If anyone has anything to say contrary to the general opinion, let him say it; let him publish it; discuss it fairly; accept it or reject it according to its value. If therefore anyone says there is an arguable case against the majority view, and can show *prima facie* evidence that there is more in it than old-fashioned prejudice, I am willing to discuss it. In the case of evolution there has always been a minority of some importance who are hostile or doubtful, and I think therefore that something is to be gained by discussing it once more.

At the same time it seems to me that there is too much of the air of grievance about Lunn's Introduction. I do not like this talk about 'foolish conspiracy' and 'conspiracy of silence.' The ordinary man of science, whose business it is to try to extend the bounds of human knowledge, cannot be expected to spend his time on what he regards as the controversies of the last century. Most of us, if we are so disposed, can formulate grievances; indeed I could make out a very good case myself. But generally speaking it does very little good to advertise grievances, real or imaginary, and, now the question has been raised, I feel it incumbent on me to say that, in my opinion, the grievances of the anti-evolutionists are mainly imaginary. In particular it does not seem to me fair or just to speak on the one hand of a conspiracy of silence, and on the other hand to speak of evolutionists 'being unable to leave his

book unanswered.' I know of no compelling reason why Dr. Morley Davies should write a reply except that he thought it a reasonable thing to do; nor is there any compelling reason why I should take part in this discussion except that I like to do so. I will put the whole matter in a nutshell by saying that, if there is a grievance, I am doing my best to remedy it, but must dissociate myself at the outset from any attacks on those who do not feel called upon to enter into controversy. I trust that I shall be allowed for the remainder of this book to discuss the question of evolution without anything further of this nature, which embarrasses me, as I hardly know whether to answer it or not.

All the same I have pleasure in accepting Lunn's invitation to continue and revise this discussion. You have difficulties about evolution. You have written two books to say so. I am aware of them in a general way, though you may spring a few new ones on me, and I do not agree with your conclusions. If you will state your difficulties clearly, each one in its appropriate chapter, and not too many of them, I will do my best to answer them, and if I cannot answer them I will say so. With the rest of the space at my disposal I will do my best to state the general case for evolution. I wish it to be understood, however, that the fact that I am undertaking this discussion does not imply the slightest doubt in my own mind that the case for evolution is sound. First of all I must devote some space to the questions raised by Lunn in his introduction.

Lunn's views are well known, and he has made no attempt to write an impartial introduction. I make no complaint, but, needless to say, I have to deal faithfully with what he says. As I have already said, I agree with him that there always has been a minority opposed to evolution, and there is still. It *is* a minority, and I do not wish to be drawn into any discussion about its size and importance; but will merely remark that the fact that I am taking part in this discussion indicates that I think it of sufficient importance to deserve serious notice. Apart from that I disagree with most of what he says and have now to say so. As he will not appear again in the discussion, you must consult with him and answer for him if necessary. He takes us into deep philosophical waters, and I agree that evolution is as much a philosophical question as a scientific one.

I will not trouble much with his history of the question. He underestimates the importance of Darwin. When an idea is in the air, it is often a matter of chance which exponent is most successful

in convincing the public. Darwin's success, in my opinion, was not due so much to his use of the concept natural selection, as to the enormous mass of material from all branches of biology, embryology and geology which he correlated, and showed how they all pointed to the same conclusion. Spencer, Huxley and Wallace all did their share, and perhaps in some ways were greater men, but Darwin actually succeeded more than the others, and it is merely foolish to try to deprecate him. But this is a side issue and does not matter greatly.

I will now try to clear up the tangle about evolution, creation and special creation. I think the majority of philosophers look back to creation in some form. At the present time this is true of the physicists. All physicists use the concept of entropy. Whenever they use it in any extensive sense they are postulating creation, for the concept of entropy definitely implies an *irreversible process*, and an irreversible process, however long it may take, looks back to a beginning and looks forward to an end. Personally I am not at all satisfied with the soundness of this concept, and have even succeeded in publishing an article against it,[1] but there it is, and it is a generally accepted idea. The physicist, however, who believes in entropy, will not expect a miracle to happen when he is working with a piece of copper, and find that it has assumed the properties of lead. Nor, when he finds a deposit of mineral formed (say) a thousand million years ago, will he admit the hypothesis that the mineral got there by a miraculous process. He looks for the natural causes, which, by the way, are exceedingly obscure. He accepts creation, but not *special creation*. Similarly the evolutionist may or may not believe in creation, but he certainly will not accept special creation.

That is why special creation is called unscientific. Lunn defines science as knowledge. That is the mediaeval use of the term. You will find it explained at length in Froude's famous essay on the relation between natural science and general science. Now, however, the meaning of the terms has changed, and it is no use squabbling about that; it is far better to use the current terminology. Science to-day means knowledge of the natural order—that is it corresponds to Froude's natural science. General science now means a certain method of approach in education. The science in Lunn's sense which is not included in natural science is now called metaphysics, or if you like the study of final causes. This does

[1] *Oxford and Cambridge Review*, January 1912.

not matter so far as you are concerned. I shall not shirk a metaphysical discussion. If you think you have evidence for special creation, by all means bring it forward. But what it amounts to, if it is brought forward, is the claim that something has occurred outside the scope of science which science cannot explain. If we are to make ourselves understood we really must use terms in the ordinary accepted sense, and to squabble about the meaning of terms gets us no further. Whether it is science or metaphysics does not matter. Whatever you have to say, I will do my best to answer it.

You, I understand, believe in special creation; at least you have written a book entitled *Man a Special Creation.* This I suppose implies that you regard man as *one* special creation. I must ask you to be quite explicit about this, as it will be relevant when we discuss the origin of man. What are the others? What was created and when and where? What sort of evidence (apart from geology) do you think will enable us to distinguish between evolution and special creation? Lunn, on the other hand, adopts what he calls the agnostic attitude, by which I think he means that he does not know whether living organisms were formed by evolution or by special creation. There is only one answer to this attitude and that is to keep on piling on the evidence. When the evidence becomes sufficient depends on the individual. There comes a time when this attitude becomes unreasonable. I might express a doubt whether the object causing the explosion which broke the windows of the room in which I am sitting was a bomb or a meteor. Both are possible. Either would account for the effect. But I doubt whether anyone would take the second suggestion seriously. One other question on this matter—are there any other possibilities? Is there any other theory which can be stated in plain English and which is worthy of consideration? Otherwise we are limited to the alternatives evolution or special creation, or must take refuge in the statement that the whole matter is an unfathomable mystery.

Lunn deals with this to some extent, and it is necessary for me to point out his inconsistency and confusion of thought. He has put forward a number of premises, most of which can be allowed to pass without comment. You can formulate your thoughts under these headings, or under other headings, and it does not matter much. But if you put forward inconsistent premises, the result is inextricable confusion. This is shown most clearly in the contradiction between his premise 4 and his premise 6. Premise 4 :—'We

must not accept a miraculous explanation of any phenomenon before we have examined all possible explanations in terms of natural causes'—I accept unreservedly. That way of putting it will suit me as well as any other, and I hope it will suit you too. I must add to it the obvious statement that special creation is 'a miraculous explanation,' or, to put it more correctly, the postulation of miracles. According to the premise I have accepted, therefore, all natural explanations, especially evolution, must be exhausted before we consider seriously anything of the kind. Very well; but in that case the so-called ambi-neuter postulate does not apply. 'Facts which are equally consistent with both of two rival theories can be cited in support of neither of those theories.' Yes, if the theories are both scientific, and both on the same level. But if one of them is a theory of miracles, *it does not apply at all.* Special creation, according to our agreed premise, or any disguised form of special creation, cannot be admitted as a rival theory. There is nothing 'ambi-neuter' about it. The only argument that can be admitted for special creation is that natural causes are unable to account for the facts. If you are going to assert your premise 4, then you must apply it, and you must not try to get out of it by saying 'evolution and special creation are hypotheses' and 'we must clearly not cite facts which are equally consistent with both.' On our agreed premise special creation is not admitted as an equal and rival hypothesis. If the evidence is explicable by either evolution or special creation, then it is evidence for evolution.

Lunn gives a good example of this principle in his answer to the suggestion that a stray dog has been specially created. He remarks 'probabilities are the guide of life.' Exactly, I agree; and I give the same answer to the same suggestion concerning the animal whose fossil bones are found in the Cretaceous. But all through this introduction we have the contrary assumption. Later on Lunn argues that the finding of an intermediate type is not valid evidence for evolution because it might have been specially created. It is, I suppose theoretically possible that the type might have been specially created, but what has become of our premise? The intermediate types *are* evidence for evolution. They give evidence that it is superfluous to invoke the miraculous. Otherwise it is necessary to admit that it is equally probable that the stray dog was specially created as that it was born in the ordinary natural way. I apply exactly the same standard to the past that I do to the present. If the evidence is strong enough, Lunn's assertion is

open to the objection he quotes from Kingsley about Gosse, that what is implied is that the Almighty has created a tremendous lie. These retorts, like curses, have a way of coming home to roost.

There is much in Lunn's Introduction which I cannot deal with at length. In particular I shall ignore his quotations. We have agreed that there is a sufficient minority opposing evolution to make this book desirable. I would remark, however, that quotations taken out of their context sometimes give a false impression ; but if any of those quoted are aggrieved they must see to the matter themselves. What is more important is that a number of statements of fact quoted by Lunn, especially those concerning the geologic record, though they might have been reasonable at the time they were written, could not have been made now by competent geologists. Some of the statements Lunn makes on his own are misleading. The reader should be left in no doubt that *new species have now been formed.* This has been done with some plants, and the new species are fertile among themselves but infertile with the parent species. This is known as polyploidy ; but you do not alter a fact by giving it a long name. And even with regard to the animal kingdom, which is all Lunn asserts, the statement is doubtful. We will discuss this in the appropriate chapter. Again, I never heard of anyone who postulated the spontaneous generation of anything that could rightfully be called an organism, a term which implies a highly evolved and specialised form of life. I shall say a word or two on this later.

With regard to atheistic evolution and deistic evolution[1] I have very little to say. The point I am concerned with is that *evolution* has occurred. Lydekker is as good an example as any of the deistic evolutionist. The final sentence in his book on the horse is as follows :

> 'That these marvellous changes and adaptations are not due to any mere "blind struggle for existence" or "survival of the fittest" but that they were directly designed and controlled by an Omniscient and Omnipotent Creator is the settled and final opinion of the author of this volume.'

This admirable sentence is an assertion of *religious* belief, and should be respected as such, but it is religious, not scientific, and I am sure Lydekker would have admitted that it does not arise

[1] So far as I understand Lunn's theistic evolution, it is a variable mixture of evolution and special creation, and so need not be considered separately.

directly from the facts. So far as this discussion is concerned I regard it as irrelevant. There is no doubt that organisms vary, and that, *on the whole*, the types best fitted to the environment survive. Personally I believe that the sort of variations which actually occur are sufficient to account for evolutionary change. If so you can explain them how you like. Nor do I intend to enter into any discussion of the physical causes of the changes. Personally I believe in a small factor of use inheritance, inappreciable in a single generation, but an important factor if extended over a long time. I say this so that you may know where I stand, but I do not intend to argue about it, or try to prove it. We will try to get this preliminary matter out of the way in this chapter, so as to get on to the real discussion. I should also like to say that I mean by chance variation, if I happen to use the term, variation or variations *of which the cause is unknown*. People have a way of giving a metaphysical meaning to chance which is quite illegitimate. I mean by a chance event, not one without a cause, but one of which I am unable to disentangle the causes. This I think is sufficient on the causes of evolution. The one thing I am stating somewhat positively is that the sort of variations that have been observed is all that is needed, and that it is quite superfluous to postulate any enormous variations, though considerable variations, greater than are usual, do occur from time to time.

Most of the special questions raised by Lunn, as for example 'true lineage of fossils' I shall defer to the appropriate chapters, but one or two I had better mention now. He suggests that I should welcome the appeal to the geologic record, instead of saying that it is fragmentary. I do both. The geologic record *does* give powerful evidence for evolution, and the record *is* fragmentary. Both are facts and easily provable. But obviously what you expect from the record depends on whether or no you think it reasonably complete. We will discuss this in the appropriate chapter.

Now I had better deal with Lunn's special difficulties, which he has numbered for my benefit.

He begins with a number of general remarks asking me to find a *vera causa* for the origin of life and for evolutionary change. I am dealing later with the origin of life; but it can be briefly summarised in the expression: I don't know. And the same applies to a considerable extent to the process of evolution. What I think is not clearly understood is that the term and idea of *vera causa* belongs to a metaphysical system which is not now usually accepted,

and is not a scientific question at all. Science is unable and *does not even attempt to give a vera causa of anything.* If a stone falls to the ground, we explain it (in a sense) by the law of gravitation. But this is not a *vera causa.* The law of gravitation is a short and convenient expression of the way matter behaves. It is a *description,* and not in the metaphysical sense an explanation. No one knows *why* one particle of matter attracts another. Nor is the recent 'explanation' in terms of relativity anything more than another form of description. With regard to evolution, I have stated that I think that the kind and size of the variations *that are actually observed to occur* are sufficient. The reason for these variations is obscure. This is not 'agnosticism,' it is merely a statement of the present condition of human knowledge. As science advances, we shall no doubt know more about the conditions under which these variations occur. In the scientific sense this amounts to discovering the causes ; *but not in the metaphysical sense.* I cannot imagine an extension of knowledge which will take us beyond the stage that has been reached in the case of gravitation, or, in other words, how we can possibly get down to a *vera causa* in Lunn's sense. In any case it is so far beyond the present state of knowledge that we need not trouble with it in this discussion.

Now we can deal with the particular questions :

1. The difficulty is not at all clear. I do not know what is meant by calling natural selection a negative process. How can a process be negative, or positive either for that matter? Lunn repeats his favourite phrase of 'mud, slime, mist, sands, sea etc.' I think life is generally supposed to have originated in the sea. If Lunn wishes to contend that natural forces are unable to account for the origin of life, that is a personal opinion to which he is quite welcome. Personally I do not know enough about 'natural forces' to assert even that. I think the other points have been dealt with already.

2. I must protest against the dragging in of the names of prominent philosophers and others without references. As Whitehead is mentioned, it is just as well to say that I have no reason to think he has any doubt about the occurrence of evolution. I would say also that Whitehead, even more than most writers, needs to be read in the context to find the purpose and meaning of what he is saying. In this case, as there is no reference, I cannot do that, and in any case I am discussing this matter with you and Lunn and not with Whitehead. Moreover, even if Lunn had read the

book (which I think he has not) I should not be at all confident that he had correctly understood the meaning. At any rate he has attributed to me the acceptance of a distinction between major and minor evolution (or macro-evolution and micro-evolution) which I repudiate as artificial and unnecessary. In the present context I cannot make much sense of this Whitehead-Blyton-Lunn difficulty.

'The phrase survival of the fittest offers no explanation' of what? If it means of the origin of life, of course it does not; no one thinks it does. It does not seem to me to make sense if we confuse such different concepts as the time of existence of a rock and the biological survival of an organism. As we deal with different forms and aspects of existing things we use different concepts to try to understand them, and no sense arises if we mix them. 'Why should the trend of evolution be upwards so that higher and higher types are evolved?' I am not prepared to say offhand whether the trend of evolution is upwards or not, and I should require upward and higher types to be defined. Because higher types are evolved it does not follow that the trend of evolution is towards those higher types. The term environment is meaningless unless we go into detail. There is no environment in general, though there are many environments in particular. We can only say that the lower types are adapted to certain environments and the higher types to others.

What all this amounts to is that I do not know what Whitehead is driving at, and could only find out by reading the passage in the context. Then I have no doubt that it would be interesting and intelligible. But it makes no sense that I can discover in this context.

3. This is a very old difficulty, which will probably recur in our discussion. The way it is put begs the question. Personally I do not think that any such changes have taken place. In other words I think the changes in their rudimentary stages *are* advantageous, though I cannot undertake to explain in every case exactly how and why. There is a possible exception if the change happens to be correlated with others which are advantageous, but I do not think this possibility probable enough to be important.

The one Lunn mentions about feathers arising from scales is not as difficult as some. I regard feathers not *in the first place* as an adaptation to flight—as the bats manage very well without them—but as an adaptation giving extra warmth. The prolongation

of scales and their splitting up into fibres obviously provides protection against cold even in rudimentary stages. Lunn will get the idea if he will examine the feathers on newly hatched birds, some of which, e.g., those of a pigeon, are very simple indeed.

4. One part of this difficulty deals with the apparent suddenness with which types appear in the geologic record. I note the use of the word apparent, and that is the subject of our next two chapters.

The rest of the question concerns a difficulty stated and answered by Darwin. I deprecate being asked to explain other peoples' expositions, even Darwin's. I will therefore only say that as Darwin takes half a chapter to deal with the difficulty, there is not much sense in taking out one passage and saying that it is the explanation.

If I may be allowed to leave Darwin out of it and say a word or two on my own, I would remark that we do as a matter of fact find many transitional types. When we find them we call them varieties, but varieties and species grade into each other in such a way as to make it often an academic question which is which.

5. In living organisms there are many grades between the primitive sensitiveness to light of (say) the anterior segment of an earthworm and the human eye, and in extinct animals probably very many more. Lunn will find some of them listed in the *Science of Life* (1938 popular edition pp. 1022 seq.). The eye did not come into existence suddenly or perfect. It has been evolving for hundreds of millions of years.

6. This last question is a good one. One difficulty is that it assumes the existence of consciousness. Here we are lost. We know nothing about the origin of consciousness. Most of the functions of organisms, including our own, are unconscious. And then somehow comes this consciousness, which is *sua generis*. I do not know, neither does anyone else, how to get consciousness out of anything else. You must understand, because we accept the plain evidence for evolution, it does not mean we can explain everything. Granted the consciousness, the sensations of pleasure and pain obviously have survival value. But I can't explain in every case why one thing gives pleasure and another pain. Usually it is obvious, but there are puzzles. This is one of them. The snow peaks as islands in a sea of cloud which I once saw was perhaps the most moving sight I ever remember. Why I have not the least idea. It is, however, a fact that this feeling, so far as we know, is a modern development. So far as I know it hardly occurs in ancient literature.

Indeed it seems to be eastern rather than western. Certainly it has developed exceedingly in the last few centuries, consequently, good as the example is, as we know it has developed recently, it has not much bearing on the problems of evolution.

I want to say a few words about questions in general, as I expect that later on you will ask me a number. I wish, if I can, to get to the root of this anti-evolutionary feeling. So far as putting questions out of order is concerned, Lunn has the excuse that he will now disappear from the discussion, and leave the anti-evolutionary case to you. Therefore he must say what he has to say at once. In your case I will ask that you state your difficulties under the agreed chapter headings, and in particular that in this chapter you will discuss only questions of a general nature. I also deprecate the mixing of difficulties with quotations from other people. If this is done the question arises whether the other person has been properly understood. Also do not ask leading questions, such as question 3. It does not help reasonable discussion to ask me to explain a thing which I think does not exist. Also I would ask you in every case to make it clear how the question or difficulty is relevant to the problem of evolution. I am making these remarks in view of the 'original correspondence.' It is no use, for example, asking me why the organs of a star fish are arranged radially. In the first place I don't know, and, what is more important, I do not see what it has to do with the matter. The statement that the forms of life have been formed by the process of evolution merely gives us one clue to the unravelling of the enormous complexity of living things. It does not mean that everything is clearly explained. To put it in mathematical terms, it is one equation where there are many unknowns.

All this gives me very little space to say anything about the case for evolution, and I defer detailed evidence to the appropriate chapters. One thing I think which needs to be clearly stated in the first place is the manner in which the idea of evolution naturally arises immediately we extend the scale of time. For many centuries the progress of human thought in these matters was inhibited because everyone took the first chapter of Genesis, if not absolutely literally, at least as absolutely true in the descriptive sense. A very large section thought that the world was created 6,000 years ago, or approximately so. If that were so, it was obvious that organisms had not had time to change much, and that the forms of life that were created were very like those we now know. Cats gave

birth to kittens, dogs to puppies, cows to calves. No special creation was required; everything was referred back to the original creation. Immediately you extend the scale of time, the matter is altered. That a succession of small variations could produce cats and tigers from the same ancestral form, or horses and asses, or indeed the whole animal and vegetable world, all depends on the necessary time being available. Even Darwin felt embarrassed by Lord Kelvin's very dogmatic and entirely erroneous assertion that the history of the Earth was limited to 100 millions of years or less.

This question of the scale of time is still important. It needs no elaboration that the 6,000 years which the early interpreters of Genesis assumed is now abandoned, and even 6 millions of years is absurdly small, but there may be some difference of opinion if we try to assess the time more exactly. The results generally accepted amongst geologists, and among those who have analysed radioactive minerals, for the time that has elapsed since the Lower Cambrian is of the order of 500 millions of years. This is a matter which I studied in detail some years ago,[1] and a number of my papers will be found in scientific journals. My own opinion is that this is an underestimate, and I will not now trouble to say by how much, but that it represents correctly the *order* of the time. For the purpose of this discussion only the order of the time is relevant. In any assertion I may make about geologic time I shall use Holmes' figures. I believe that you regard them as an over-estimate. That, within reasonable limits, does not matter either; indeed I do not think any argument I shall use in these letters will be appreciably affected if you reduce Holmes' figures by 50 per cent. Still, of course, in discussing evolutionary changes, the time element has some relevance. If, however, for the sake of argument, you are prepared to accept the current estimates as sound within the limits of 50 per cent, I need say very little more about it. Otherwise, of course, the question will need further elaboration, and I shall have to give it some attention in the next chapter.

Granted the scale of time, I will now put the principle of evolution in a form slightly different from that which is customary. You will remember that at the beginning of the last century it was commonly thought that maggots arose spontaneously in decaying meat; and still later that bacteria arose spontaneously in (say) an infusion of hay. Pasteur definitely settled this question. He proved that these

[1] See particularly *Journal of Geology*, Feb.–March 1910, and *Science Progress*, Oct. 1913 and July 1914. Holmes' latest figures will be found in his *Physical Geology*, p. 104.

forms of life all arose in a natural way from pre-existing forms. That is the principle of evolution. It extends Pasteur's principle from the present day to the very beginning. All through the ages life has been formed from pre-existing life, *and from no other source.* In the many millions of years there have been vast changes. Many forms of life have become extinct. Yet always some have remained and have given rise to others which have taken their place. The principle of evolution asserts that these new forms are descended directly by the ordinary processes of reproduction and change from some of the pre-existing forms.

I had better mention here a question which I am sure will arise, and which any anti-evolutionist is likely to use. The difficulty is that of credibility. There is such an enormous difference between the higher forms of life and the lower ones that it does seem at first sight incredible that the one should have arisen from the latter, or from anything like it, by the usual process of small variations. I grant that surface appearance of incredibility, but what I wish to point out is that this is a feature of all the greater conclusions of scientific research. I take a small telescope and look at that formless glow which is called the Great Nebula in Andromeda. By a process of reasoning which the plain blunt man can follow in some detail it is shown that it is so far off that the light takes about 800,000 years to reach us, and the light in question travels at the rate of 186,000 miles a second. How vastly incredible! Mr. Bernard Shaw in one of his characteristic semi-humorous passages described it as a stupendous lie. Yet we all accept it as true. Again, when we find that a formation of mountain limestone is a thousand feet thick, or often more, and we are told that it was all formed at the bottom of the sea by the slow accumulation of the skeletons of minute animalcules similar to those which to-day inhabit the ocean. How incredible! Yet we all accept it as true. Similarly that these stupendous changes in the forms of life have occurred by small variations is on the surface incredible. An anti-evolutionist may well say that they are impossible. Yet the majority of people competent in science accept it as true. That is the point we are discussing in this book. We have to decide on the balance of the evidence the details of which must be left to the succeeding chapters. I simply wish to point out that the apparent surface incredibility is not a valid argument.

But, you may ask, what about the origin of life? Life was not possible until the earth was sufficiently cooled down. Do you

not want a special creation here? My answer is that, if you wish to postulate this, there is not at present sufficient convincing evidence against it. I will not argue the matter at length, but personally I do not think the assumption necessary. Note my manner of approaching the question, for which the reasons have already been given. The natural formation of the beginnings of life is not so incredible now as it appeared 50 years ago. I will say in the first place that I do not believe in the spontaneous generation of anything so complicated as an amoeba, or even as a bacterium; nor do I know of anyone who does. But recently the photo-microscope using ultra-violet rays, and still more recently the electron microscope, which is still in its initial phases, have taken us much farther back. Similarly bio-chemistry has taken us farther forward. I fully expect that these two lines of research will eventually meet. As things are, we know that there are forms of life very much simpler than an amoeba or a bacterium. The bacteriophages are good examples. It is really now becoming an academic question whether certain forms are alive or not. One example is snake venom. It works as if it contained harmful bacteria, and can be countered in the same way by inoculating with the suitable anti-toxin. But no bacteria have been found, and I really do not know whether the active principle is alive or not. I think the general view is that it is not alive, but the properties of living and non-living do grade into each other in a surprising way. A gradual primitive evolution rising into rudimentary life is therefore not wholly impossible. I accept it as the most probable assumption, but shall not quarrel with you if in this place you care to make the other assumption, as I know as well as you do that my own assumption is a long way from definite proof.

This is hardly relevant to our discussion, which does not include the origin of life. But it does give rise to the relevant question whether the evolution of primitive life into a definite cell with a nucleus occurred once or more than once. Needless to say I cannot answer this, though I think the probability is once only. On the other hand the world is a big place, and the time is long, and it is just possible that it occurred more than once. If so we have to admit the possibility that all the phyla are not genetically related, except in so far as they all arise from primitive non-cellular life. I regard that as a remote speculation and improbable, indeed as a rule I should not regard the suggestion seriously. Yet it does envisage the possibility of polyphyletic evolution, in a sense. I wish you

therefore to notice that in admitting this possibility, I am doing so only in this sense and am not postulating any special creations. This will not arise often in the discussion, as very seldom shall I deal with the relations between the phyla. It will be quite sufficient if we can show the great probability (please note that philosophically all scientific conclusions of any complexity must be stated in these terms) of the common origin of all the diverse forms of each of the great phyla.

The next question that arises is what kind of proof can I offer of all this. Obviously it is what Lunn calls persuasive proof. I have no time machine, and cannot take you back to the old Triassic shore, nor can I go myself. I cannot bring you back again slowly through the ages, and show you that in all times the world moved much in the same way as it does to-day, and that the rabbits always came in the natural way out of the body of the mother, and never out of the hat. It is only thus that you could find coercive proof that your special creation is a delusion and that the world moved always as at present by natural laws.

In the place of a comparatively recent creation, we have a world existing through vast eras of time. Sea and land, tide and storm, day and night, go back to a vast antiquity. But though physical conditions have remained much the same, the forms of life have altered. If we go back only a small fraction of the time shown by the study of the rocks, most of the species we know to-day are no longer to be found. There were no men, no horses, no cattle, no lions—none of the higher animals now existing. The animal life is that of a very different world. But it is not entirely different. Though our present horses are not to be found, animals somewhat similar existed having three toes. Although there were neither tigers nor cattle similar to those now living, there were mammals adapted to vegetable feeding, and others which from their structure were clearly adapted to prey on their fellows. In some of the less highly developed forms of life : crustacea, foraminifera, and others, the similarity was still more striking. The old world was different, but there were great similarities. The life in two successive epochs is much more similar than that in epochs farther apart. Some forms of life flourish, others become extinct. But those that remain and those that become abundant always bear striking resemblances to some of those that existed previously.

Our hypothesis, therefore, is that of the continuity of life. The forms of life in any epoch are directly descended from *some* of the

forms in the previous one. The evidence must be left for the succeeding chapters. As I have already stated, I cannot offer you coercive proof, but merely persuasive proof. It is a large subject, and only those with a broad philosophic outlook can appreciate and co-ordinate the evidence from so many different lines of thought. It may be that some who accept evolution do so in a mechanical way, and have not fully grasped the intricacy of the subject. Lunn's quotation from Professor Watson, which I happen to have traced to its source, is a case in point. It certainly appears to me that the statement that evolution cannot 'be proved by logically coherent evidence to be true' shows a lack of appreciation of the methods of scientific reasoning. I am getting a little bored by that quotation, which I have heard much too often, but I think, in the context, where it is not properly explained, it does show the deficiency I have mentioned. As some of my special work has been the study of the type of reasoning used in scientific work, and especially in the larger questions such as these, I may perhaps be able now and again to show where you reason in the wrong way. The problem of the canons of scientific reasoning is equally interesting whether we are dealing with correct reasoning or incorrect. We shall not as a rule quarrel about the facts, and I have no doubt whatever about your competence in collecting these. Certainly you know more of the facts of natural history than I do. You have also been collecting facts and arguments on this matter for some years, which I have not. You have the advantage of an introduction[1] which is definitely on your side, and of Lunn's help throughout the discussion. I am willing that you should have these advantages, because I am convinced that your reasoning is fundamentally unsound.

One final word on the subject of authorities. Lunn has saved you the trouble of showing that there is a minority which agrees with you. We need not quarrel about the size and importance of that minority. If we are arguing from authority you have lost the case from the beginning. I shall refer to authorities only for facts. There we *must* rely on authority, because no one can personally be acquainted with all the facts concerned. But I do not propose to refer to authorities on opinions, or on methods of reasoning, and I hope you will follow the same rule. On this question I am discussing the question with you, and I do not wish to be obliged to make depreciatory remarks on any others who may happen to

[1] Since this was written, Lunn has informed me that on this account I am allowed a little extra space, which concession is thankfully accepted.

agree with you. That the great majority of scientific opinion is on my side is irrelevant, and I shall not mention it again—unless you force me to do so. Similarly it is quite unnecessary to add to Lunn's quotations. I am not sheltering myself behind Darwin, Spencer, Huxley, Wallace, and their successors too numerous to mention. I am expressing the evidence in my own way, and I hope you will do the same.

Yours sincerely,

H. S. SHELTON.

DEAR SHELTON,

In connection with your comments on Lunn's introduction I here quote a letter I have received from him :

'Shelton writes : "As Lunn will not appear again in this discussion you must consult with him and answer for him if necessary." I have just one or two points which you should put by way of clearing things up. I know that Shelton does not accept the distinction between major and minor evolution. All I intended to imply was that he had made use of my terminology to attack what he considers an unreal distinction.

'I note that Shelton regards the geological record as reliable when it tells in his favour and too fragmentary to be of any value when it tells in your favour.

'Creation, whenever it occurred, involves miracles and I wish Shelton would explain why it is more scientific to relegate this miracle to a period before the planet was created than to admit miracles into the evolutionary process. It seems to me that the atheistic evolutionist has the advantage in logic over the deistic evolutionist.

'As to theistic evolution, the deist differs from the theist in that the former rejects and the latter accepts miracles. Deistic evolution is a purely natural process. The theistic evolutionist accepts descent of man from lower species but does not believe that purely natural forces are sufficient to explain evolution. At various stages in the process mutations, inexplicable solely in terms of natural agencies, must be postulated. I am not myself a theistic evolutionist but an agnostic so far as all evolutionary theories are concerned.

'With regard to premise 4. I am, of course, ready to examine any theory of evolution and to accept it, if proved, and to give such theories priority over special creation, but I am not prepared to

admit the arrogant claim that "special creation cannot be admitted as a rival theory." It is, of course, a very respectable rival for reasons already set out in my introduction. And unless Shelton can produce some more convincing explanation of the causes and mechanism of evolution, the unbiased reader will have to choose between special creation, theistic creation or agnosticism.

'It argues a certain lack of confidence in his case that he has to begin by attempting to rig the laws of evidence in favour of evolution. Consider, for instance, his remark "if the evidence is explicable by either evolution or special creation then it is evidence for evolution." Dewar is the counsel for special creation. "Gentlemen of the jury," says Shelton, "there is a good deal of evidence in this case which is equally easy to reconcile with my case and with my learned friend's case. All such evidence must be treated as if it told only in favour of my case." I have read for and you have practised at the Bar, and we both know the kind of comments which a judge would pass on such a claim.

' "I may perhaps," Shelton writes, "be able to show you where you reason in the wrong way." Perhaps, but it is by no means excluded that you may be able to perform the same kind service for Shelton.

'Shelton's attempt to restrict the word "scientific" to explanations in terms of natural forces would not be accepted by such scientists as have investigated psychical phenomena. He often quotes from *The Science of Life*. He will, I think, find in that book a discussion of psychical phenomena which the authors admit to be worthy of investigation. The scientist begins, of course, by attempting to explain these phenomena in terms of natural forces, but he does not begin his investigation with a closed mind. He does not rule out in advance the possibility that these phenomena might be caused by supernatural (or praeternatural) agencies. Nothing could be less scientific than to exclude from the field of research any possible explanation of the phenomena under investigation. In the case of psychical phenomena, as in the case of evolution, we begin by trying to exhaust natural causes, but *priority of investigation does not imply that the ordinary rules of evidence can be violated in favour of the hypothesis with which we begin our investigation.* The fact that we must *begin* by examining hypothesis A before considering hypothesis B does not mean that A is more likely to be true than B. Le Verriere *began* by trying to explain the movements of planets on the hypothesis that there were no new planets to discover.

After proving that hypothesis A did not fit the facts he proceeded to hypothesis B and discovered the planet Uranus.

'I am beginning to think that my agnosticism about evolution only differs from Shelton's in degree. He does not explain (1) the origin of life, (2) the upward trend of evolution, (3) why the process should ever have started, (4) how the variations originated which are the raw material for evolution, (5) how natural selection which only selects what is there could possibly be creative, (6) how pure chance could produce so complex an organ as the eye, (7) how variations in their rudimentary form could be advantageous, (8) how the sense of beauty originated. Shelton picks out our feeling for mountains and asserts because "it has developed recently, it has not much bearing on evolution." Has the evolutionary process ceased? I wish you would ask him. Of course man's sense of beauty was present from the first, witness prehistoric paintings.

'One word more. The publishers commissioned this book on the distinct understanding that its thesis was "Is Evolution Proved?" Shelton must therefore prove his case, not disprove yours. He must not follow the bad precedent of Watson and others of assuming that depreciatory remarks about special creation are an adequate substitute for proof of evolution. You, for your part, are only concerned in this book to disprove evolution, and though the creationist hypothesis may be mentioned from time to time, it is no part of your case to prove special creation. Many but not all those readers whom Shelton fails to convince will be converted to special creation. Some will, perhaps, agree with me, that the evidence is insufficient to warrant us accepting either special creation or evolution.'

. . . . .

In the event of your making any comments on Lunn's letter that seem to require a reply, I will do my best to give the reply.

In connection with your answers to Lunn's questions in the introduction :

You have not attempted to tackle the problem that baffled Darwin (*Origin of Species* (1882 ed.), p. 133), viz., if evolution be true we should expect to see (a) innumerable transitional forms. (Note the word 'innumerable,' and that the transitional forms are between *species*), (b) 'all nature in confusion.' Instead of this (c) species, as we see them, are sharply defined. This you can easily see by looking at the plants growing on any piece of uncultivated ground. If all

nature were in confusion and species, genera, families, orders and classes were not sharply defined, but faded into one another, this could be adduced as evidence in favour of evolution. Of the above points you ignore (b) and (c), and try to avoid (a) by citing varieties within species as transitional forms, but these are as easy to reconcile with creationism as with evolution. Citing as evidence for evolution facts easily explicable on the theory of special creation will not get you far. In view of this I request you to comment on (a), (b) and (c) above.

In reply to Lunn's remarks about feathers you say you regard these as an adaptation to giving extra warmth, but you completely ignore Lunn's main difficulty, viz., how a bird became a flier. The immensely complicated structure of feathers (some of which are said to contain more than a million barbules) is not necessary if their function is only to assist in the retention of body heat. You make no attempt to explain how the fore-leg of a reptile became *gradually* converted into the wing of a bird—an implement that in a fraction of a second can be changed from a surface impervious to air into one through which air passes almost without resistance. This is but one of many gradual changes, or rather transformations, postulated by the evolution theory, which I believe to be impossible. One of the reasons why I reject the evolution theory is that it demands impossible changes. In my view the first flying bird was a special creation, i.e. it originated suddenly, *per saltum*, whether by the transformation of some other animal or otherwise is, I think, beyond the power of science to discover. Bear in mind that the change of an ambulatory or swimming leg into a bird's wing would, *inter alia*, necessitate the rearrangement of almost every bone and muscle in the body. It is up to you to explain how all these changes can have been effected by the accumulation of small variations. When we discuss the morphological difficulties of the evolution theory I shall ask you to say how you imagine this transformation took place.

As to Lunn's point No. 5 : How do you explain the evolution of very complex organs such as an eye? Your reply that there are many grades between the primitive sensitiveness to light of the anterior segment of an earthworm and the human eye is merely an evasion of the question. Do you realise that 'a primitive sensitiveness to light' involves not only a modification of a part of the integument, but a correlated modification of the nerve supplying it and the part of the brain or ganglion at which external impressions

are received and acted on? Even if this happened, of what use is this to the animal until this is provided with some mechanism for translating this stimulus into action? What advantage has a creature having what you call this sensitiveness to light in a certain part of its body over one lacking this sensitiveness? What do you imagine caused this supposed modification of skin, nerve and ganglion?

The eye is but one of many organs which I think cannot have evolved gradually. The existence of such structures is one of the greatest objections to the evolution theory. Lunn happens to have selected as an example the eye, an organ of which there are many kinds. But this is not the case for all such organs, and, as this objection is so important, I will in a later chapter deal with one of these, the web-spinning apparatus of spiders, which I propose to discuss in detail.

You write: 'A number of statements of fact quoted by Lunn, especially those concerning the geological record, though they might have been reasonable at the time they were made, could not have been made now by competent geologists.' I have not been able to find any such statements; will you please name them.

My observations on your letter are as follows: First, as Lunn has stated, you are the plaintiff and I am the defendant and our readers are the jury. You as plaintiff claim that evolution is true and is supported by cogent evidence which you are about to produce and by means of which you hope to induce the jury to find that you have established your case. But there are a number of theories of evolution, of which only one (if any) can be true. This being so it is incumbent on you as plaintiff to state in precise terms precisely the particular theory you seek to establish. Until you have done this, the jury cannot know what we are disputing about and I cannot know what I have to attack. This you have not done. At considerable pains I have read your letter more than once in the hope of discovering precisely what your case is. My impression is that the theory of evolution most commonly propounded is that millions of years ago inorganic matter, acted upon by natural forces, gave origin to a living organism which, owing to the action of the said forces, has evolved into all living and extinct animals and plants, including man.

So far as I can make out from your letter, you are not prepared to go so far as this. You say you do not think that life was specially created. You say that Pasteur showed that bacteria all arise in a

natural way from pre-existing bacteria. 'That,' you assert, 'is the principle of evolution.' In fact, however, the evolution theory asserts that on one occasion life arose from inorganic matter, i.e. just what Pasteur, according to you, disproved! Then you admit that you cannot say whether or not 'the evolution of primitive life into a cell with a definite nucleus occurred more than once.' All this seems to me very indefinite.

You seem to be equally indefinite regarding the origin of the great groups of animals known as phyla. As regards bodily structure every animal, living and extinct, is of one or other of a few types, each fundamentally different from all the others. The structure of back-boned animals is of one type, that of molluscs of another, that of worms of a third. (See the appendix to this letter.) *One of the most difficult problems the evolutionist has to solve is how all these phyla can have evolved out of a speck of protoplasm.*

The statements you have made in your letter seem to imply: (1) For all you know the phyla may not be genetically related, but you doubt this. (2) You are little interested in the relations between the phyla. (3) 'It will be quite sufficient if we can show the great probability . . . of the common origin of all the diverse forms of each of the great phyla.' I take all this to mean that you are completely in the dark (which is true of all evolutionists) as to how, when, and where the phyla originated. But this does not matter. All you need do is to show that it is extremely probable that all the members of each phylum are derived from a common ancestor, e.g. all vertebrates from a common vertebrate ancestor, all molluscs from a common molluscan ancestor, etc. If you can do this it automatically follows that all the phyla themselves are derived from a common ancestor. Thus, in your view, proof of the lesser is proof of the greater. This is indeed an ingenious way of proving evolution. But why take so much trouble? Why not be content with showing it to be extremely probable that all the sub-species of a species are derived from a common ancestor, and extend this to genera, families, etc.?

I assert that there is not an iota of evidence, either experimental, morphological, palaeontological, embryological, or of any other kind, that the phyla evolved from a simple nucleated cell, much less from 'primitive life' or from inorganic matter. Nor is there a shred of evidence that any phylum evolved from another phylum. The vertebrata form the highest phylum, but there is no evidence that this phylum is derived from any other. *Thus the notion that the phyla evolved from 'primitive life'*

has no scientific foundation. It is, if you like, a philosophy unsupported by any kind of evidence, a pure matter of faith.

If the phyla evolved from 'a simple nucleated cell' then it follows that more than five-sixths of the sum total of evolution that has taken place since life appeared on the earth was concerned with the evolution of the first members of each phylum. Of this huge slice of evolution there is no evidence. Thus the subsequent evolution or differentiation whereby the primitive members of each phylum gave rise to all the existing forms, constitutes less than one-sixth of the earlier evolution in the case of the vertebrates and much less in the case of the other phyla. I make this distinction because, according to the evolutionist, while the fishes that remained in the sea have undergone but little modification, one enterprising fish that took to life on land became an amphibian and from this reptiles, birds and mammals evolved. Nothing approaching this has happened in the case of any of the other phyla. The protozoa, echinoderms, etc., differ but little from their Cambrian ancestors. This apparent difference in respect of modification between the vertebrata and all the other phyla seems to me to be, for the evolutionist, a problem not easy to solve.

Will you please say whether or not I have interpreted correctly your statements to which I have referred, i.e. that all that you are attempting to prove is *evolution within the phylum.* If this be not so, will you kindly state precisely what you are trying to prove. Armed with this information I shall be in a better position than I now am to take up the other points you have raised. This I will do in my next letter.

Yours sincerely,

D. DEWAR.

APPENDIX

LIST OF THE PHYLA

ONE-CELLED ANIMALS

1. Protozoa.

MULTI-CELLED ANIMALS

2. Coelenterata (jelly-fishes, corals, etc.).
3. Porifera (sponges).
4. Platyhelminthes (flat-worms, tape-worms, etc.).
5. Nemertinea (marine worms).
6. Rotifera (wheel-animalcules).
7. Nematoda (thread-worms).
8. Annelida (round worms, such as the common garden worm).

9. Arthropoda (crabs, lobsters, insects, spiders, etc.).
10. Mollusca (cuttle-fishes, limpets, snails, mussels, etc.).
11. Echinodermata (star-fishes, sea-urchins, sea-cucumbers, etc.).
12. Brachiopoda (lamp-shells).
13. Polyzoa (a large group of microscopic animals that live in colonies).
14. Chaetopoda (arrow-worms).
15. Vertebrata (back-boned animals, fishes, amphibia, reptiles, birds, mammals, also amphioxus).

DEAR DEWAR,

I will try to be brief this time. We want to get these metaphysical discussions out of the way. I am not particularly interested in this argument about Deistic Evolution and Theistic Evolution. If anyone likes to say (as Wallace did) that the evolution of man from a lower animal implies a special exercise of Divine directive power, which appears to be what Lunn means by Theistic Evolution, I am not disposed to argue the matter. It is a religious attitude, and can neither be proved nor disproved. Neither can Lyddeker's Deistic Evolution. In this discussion I am merely trying to show that man *has* been evolved from other forms of life, and in general with organic evolution. If this is admitted, I am not much concerned with metaphysical interpretations, which are conditioned by religious attitude and personal preference.

Special creation, however, is another matter. It implies the *obviously* miraculous. My objection to it as a hypothesis is that I know of no criterion by which it can either be proved or disproved. However powerful may be the evidence for evolution, it is still possible to say, if you are so disposed: Yes, in this case you have the right kind of evidence, but it is still possible that the forms were specially created. I suppose it is. To put it briefly and bluntly there is a sense in which special creation will account for anything. That being so, in another and more important sense it will account for nothing.

Your statement that the first flying bird originated suddenly, *per saltum*, 'whether by the transformation of some other animal or otherwise, is, I think, impossible for science to determine' is open to the same objection. It does, however, raise the question of the size of the variations which normally occur in living beings. As a rule these are small, but occasionally a larger one occurs. I am not prepared to dogmatise exactly how large these freak variations

may be. Nor can I dogmatically say what proportion of evolutionary change is due to small variations, and what proportion to larger ones. Personally I think the bulk of evolutionary change comes from small variations, but that is merely an opinion.

All the same there is a limit, though I shall make no attempt to draw the line exactly. I will, however, say at once that I regard as entirely out of the question anything of the size of the formation of a bird *per saltum*. If an animal in any way like a crocodile laid an egg which hatched out to anything like an eagle, that is as near to special creation as does not matter, and I reject that form of special creation as emphatically as any other form. Indeed I should be highly sceptical if I were informed of the occurrence of a much smaller change, such as the assertion that the egg of a hedge sparrow hatched out to form a cuckoo.

There are a few details which I had better deal with now. The first are what I may call your supplementaries to Lunn's difficulties, in particular the 'innumerable transitional forms.' Sometimes we get them, as for example in the West Indies there are innumerable transitional forms between the white man and the black man. But generally speaking the characters of species are *relatively* fixed, and the reason for this relative stability is fairly clear. The form of a species tends to approximate to that best fitted to the environment and mode of life. When these remain constant, so do the species. Some forms of life have not changed appreciably since Cambrian times. Evolutionary change usually occurs either when the environment changes, or as an adaptation to a new environment. I should remark, however, that variations do occur in 'fixed' species, though they are suppressed by the action of natural selection.

About this question of feathers and flyers, I must defer the detailed discussion to the appropriate chapter. If Lunn wants to know how feathers originated that is one question, and if he wants to know how the bird became a flyer that is another, and the two questions are not necessarily connected. Of course I cannot answer either, except in a speculative way. I don't know. I have given a speculative answer to the first question, and with regard to the second I can only in a speculative way refer him to the numerous living creatures with incipient flying organs : flying squirrel, flying phalanger, etc. Granting both incipient feathers and incipient flight, it is surely no very serious difficulty that the feathers should stream line, and afterwards become specially adapted to aid and improve the flight.

You ask me what I mean by saying that some of Lunn's quotations are out of date. The one I had particularly in mind was from T. H. Huxley.:

> 'In answer to the question "what does an impartial survey of the positively ascertained truths of palaeontology testify in relation to the common doctrines of progressive modification?" I reply: "it negatives these doctrines, for it either shows no evidence for such modification or demonstrates that such modification has been very slight."'

This passage first occurs in an address to the Geological Society in 1862. It is repeated in a second address to the same society in 1870, and it is from this second address that Lunn's quotation is made. It would have been instructive if he had taken the trouble to read the address through. Had he done so he would have found a number of other relevant passages, as for example: 'There is much ground for softening the somewhat Brutus-like severity with which in 1862 I dealt with a doctrine for the truth of which I should have been glad enough to be able to find a good foundation,' and 'But when we turn to the higher vertebrate the results of recent investigation, however we may sift and criticise them, seem to me to leave a clear balance in favour of the doctrine of the evolution of living forms from one another.'[1]

To put it briefly and bluntly: the very passage Lunn quotes is itself a quotation by Huxley from his previous address, and it is quoted for the express purpose of showing that then (in 1870) it is out of date. I think it will be agreed that a statement already out of date in 1870 is still more so in 1945. Also I may be excused, when I find the quotation I am able to verify is of this character, for being a little suspicious of the others which I am not able to verify.

This well illustrates what I said in my last letter that I am more interested in your arguments than in your quotations, especially when no references are given.

Yours sincerely,

H. S. SHELTON.

DEAR SHELTON,

May I entreat you not to attribute to me statements I have not made? I did not ask you what you meant by saying that

[1] *Collected Essays*, Volume 8, pp. 303, 342, 347, 348.

some of Lunn's quotations are out-of-date. I reproduced your words, and asked you to name such quotations, as I could not find them. You refer me to Huxley's assertion, made nearly 80 years ago, that the geological record either shows no evidence for such modification or demonstrates that such modification has been very slight.

But please note (1) Lunn quoted the passage to show that Huxley accepted evolution with enthusiasm, not because the evidence was compelling, but because he was seeking for some alternative to what he described as the 'untenable separate creation theory.' If you will pardon me for saying so, this seems to be the reason why you so enthusiastically accept evolution. I say this because, although we are discussing evolution, you again and again gird at the theory of special creation. Please bear in mind that even if evolution were proved to-morrow, the fact would still remain that Huxley, Yves Delage, Watson, and others had previously accepted it for unscientific reasons, for it is unscientific to accept a theory because of dislike of the rival theory.

(2) In any case the statement of Huxley we are considering is one which, at any rate with the omission of 'very,' can be made to-day, and statements of the kind have recently been made by competent geologists and biologists, for example, Paul Lemoine, L. Merson Davies, L. Vialleton, Austin Clark ; also less than 20 years ago, by Depéret. As you object to my quoting authorities, I will refrain. But *I challenge you to cite a genetic series of fossils linking by small steps an order with another order or a family with another family.*

You have but to show such a series in order to convert Lunn to evolutionism, and to make me reconsider my attitude towards it.

You say special creation implies the obviously *miraculous*, I agree. But evolutionism implies the obviously *fabulous*, such as the gradual conversion of a reptile into a bird.

You also say 'however powerful be the evidence for evolution, it is still possible to say that the forms were specially created.' I agree, but it is equally true : 'no matter how scanty be the evidence for evolution it is possible to say that evolution is a fact.'

I am sorry, but I am compelled to challenge your statement that the patagium of the flying squirrel or of the flying phalanger is an incipient flying organ. It is nothing of the kind. It is a *gliding* organ. Please accept the correction or describe how a patagium of squirrel or phalanger could possibly be gradually transformed into the wing of a bat or of any kind of winged mammal. I am sorry

to have to contradict you so often, but if *in a debate* you make unsustainable statements you must expect to be taken up.

In conclusion, may I remind you (1) you have not yet answered Lunn's questions: What is your theory of the causes of evolution? How did the process begin? What is the mechanism which gradually transformed inorganic matter into life and which bridged the gaps between the different Families? What is the *vera causa* of evolution?

(2) You have not yet stated what you are trying to prove in this debate.

Yours sincerely,

D. DEWAR.

DEAR DEWAR,

I am sorry if I misunderstood you, or Lunn, but really I find you both a little difficult to understand. I did think when I pointed out that a quotation by Lunn 'could not now be made by competent geologists' that I was answering your question. Your explanation why Lunn made the quotation at all is welcome, as it is not clear from his exposition. With regard to your further remarks on the geologic record, we will discuss all these matters in the next two chapters.

It is really necessary, however, to comment once again on your continual repetition of the view that special creation can be regarded as an equal rival theory to evolution. We have agreed (that is Lunn and I) that a 'miraculous explanation' must not be invoked until we have exhausted all known natural causes. Whether or no you agree I do not know, but if you do not you should say so. Anyhow, you both try to get out of it, and that illustrates my contention that anti-evolutionism is confusion of thought.

Let me be quite clear on this matter from the start. If you regard special creation as an equal rival hypothesis with evolution, and think it a sufficient answer, when evidence for evolution is given, to reply that the evidence is also consistent with special creation, *I have no answer to give you.* I acknowledge from the start that there is no evidence, *and I can think of no possible evidence*, which cannot be explained by special creation, if for some extraordinary reason anyone wishes to interpret things in that way. It is the stray dog over again. I cannot prove that the stray dog was not specially created. Let me illustrate the point further by a simple analogy. If anyone likes to say that the sun rises every day, not

because the earth is rotating, but by a miraculous act of the Creator, I cannot answer that either. I could give evidence that the earth *is* rotating, but that is all I could do. Similarly all through this discussion I shall give evidence that evolution *has* occurred, but again that is all I can do. If you like instead to say that the whole process is a series of miracles, that is a personal metaphysical interpretation to which no reply is possible. I cannot then prove the case for evolution *to you*. I can only prove it for those who accept the normal fundamental metaphysical assumptions.

I have nothing further to say at present about the causes of evolution. The space at my disposal is not sufficient for me to enter into such involved and intricate discussions. Moreover, as you appear to doubt whether evolution has occurred at all, the question hardly arises for you, and there is no point in discussing it until the primary question is settled. If evolution has not occurred, there are no causes. Finally, I think my remarks on the speculative question of the remote possibility of polyphyletic evolution were reasonably clear. If they are not so, I cannot make them clearer.

Yours sincerely,

H. S. SHELTON.

DEAR SHELTON,

It is unkind of Lunn to ask you to state what you imagine to be the causes of evolution. I would not have been so cruel, because I am convinced that evolution has not occurred, and so there is no cause of it ! But Lunn does not share this conviction, in consequence his question is a very proper one, and the fact that neither you nor any one else can answer it tells heavily against the evolution theory. Seeing that you have devoted your last letter almost entirely to special creation, which we are not debating, your excuse that want of space prevents you from answering Lunn is patently a lame one.

I agree that we ought not to invoke a miraculous explanation until we have exhausted all known natural causes. I certainly shall not do so. Your remarks about special creation in your last letter ignored my challenge to you to cite a series of fossils linking by small steps an Order with another Order, or a Family with another Family, coupled with my words '*You have but to show such a series in order to convert Lunn to evolution and to make me reconsider my attitude towards it.*'

Instead of accepting this challenge you drag in the stray dog, because neither you nor any one else can cite such a series. You continually meet argument with mere assertion. You have not made the slightest attempt to show why special creation should be ruled out of court as inadmissible. You take refuge in pretentious phrases such as 'the normal fundamental metaphysical assumptions.'

In spite of this implied claim to be more conversant with the laws of thought than I am *you are guilty of a gross confusion between two very different metaphysical conceptions* to wit *an explanation and a reasonable explanation.* There is a sense in which everything could be explained by special creation even if a true linkage series of fossils were discovered, but if each link in the series were described as a special creation that would not be a *reasonable explanation.* We all admit that animals are modifiable *within limits. The only question which divides us, a strictly scientific question, is whether such modification is limited or unlimited.*

I suppose your stray dog will come into this discussion every time you are in difficulties.

There is one significant difference between you and the older evolutionists. They realised that the *onus probandi* is on the evolutionist, that it is his job to suggest a *vera causa* of evolution and to *prove* that the agent they invoke can bridge the gaps between the Families, Orders, etc. But, as their efforts have all failed, you plead limitations of space as an excuse for no attempt to explain how evolution began and how the process continues.

At the close of my first letter I put to you a very definite question, viz.; is all that you are attempting to prove *evolution within the phylum*? The plain answer to this is either 'Yes' or 'No'. If the latter is your answer, then you should state precisely what you are going to try to prove. If you do not know what you want to prove, why not state this candidly?

Yours sincerely,

D. DEWAR.

DEAR DEWAR,

I am getting tired of this preliminary fencing, and wish to get on to the details of the discussion. Indeed, so far as this chapter is concerned, I have nothing further to say. As, however, what I have said does not appear to be understood, I had better *repeat.*

First, about the causes of evolution, I have already said that

I think that *the variations that normally occur and can be observed in living creatures are all that are required.* These, though small, in course of time add up to the changes that have actually taken place. According to the environment, and the changes in the environment, and to the movement of the organisms to other environments, some variations will survive and others be eliminated. This is usually known as natural selection.

The causes of the variations are unknown. We know they occur, but we do not know why. Personally I believe in a small feature of use-inheritance ; but that is merely a personal opinion, and of no consequence whatever.

Next you want to know what I am setting out to prove. I am not setting out to prove anything. The time for that is gone. The *onus probandi* in the year 1945 is no longer on those who accept evolution. The title of this book is :—*Is Evolution Proved*, not whether I am personally able to prove it. A few people like you and Lunn appear to think that evolution is not proven. That is an unusual attitude, and I have consented to do my best to discuss the matter with you. My contention is that evolution has been proved sufficiently to convince any reasonable person. The implication that you are not a reasonable person is unfortunate, but I can't help it. Of course I may not succeed ; but if that occurred, it would be on account of my lack of skill. I regard the matter as settled for all practical purposes.

By evolution I mean the continuity of life from primitive non-cellular life up to the present time. I am not interested in your list of phyla. The point I tried to make clear was whether the evolution of life to form a definite cellular structure occurred once or more than once. The probability is once only, but I am not dogmatic about it, and, if it occurred more than once, there would be a sense in which all living beings would not necessarily be genetically connected. Polyphyletic evolution is not a very good name for this hypothetical suggestion, but I do not know a better. The answer to your question whether I am attempting 'to prove evolution within the phyla' is yes, subject to the qualification that I am not attempting to prove anything. I give you a yes, as you seem to want it, but the question is misleading.

I note your distinction between an explanation and a reasonable explanation. You did not make that clear before, and it did not occur to me that you thought special creation an unreasonable explanation in any case. Personally I think it an unreasonable

explanation *always*. Still, in the course of the subsequent discussion, if occasions arise when you think it reasonable, and not in contradiction to the principle on which we are now all three agreed, by all means say so.

We have two chapters in which to discuss the evidence from geology, and your obvious desire to anticipate does not seem to me to be sensible. Still, as you are so insistent, I will say at once that the change from Equus to Eohippus can be traced by small steps, and the change is *considerably greater than* that usually implied by linking 'a family with another family.' You must understand of course that it is only narrow specialists who regard these terms family and order as more than mere conveniences. There is no such thing in nature as a family or an order, there are merely varying degrees of difference which we do our best to classify in the most convenient way. I say this now, but I must protest against your forcing me to deal with these matters out of order.

I wish to add also that it seems to me foolish to attach such importance to this particular point, which seems to me quite a minor one, but it does happen that there is evidence which I think gives reasonable grounds for the statement I have made. I would say farther that in my opinion both you and Lunn attach too much importance to the evidence from geology. The case for evolution would be perfectly sound if no fossils had ever been found.

Yours sincerely,

H. S. SHELTON.

2

THE GEOLOGICAL RECORD (ITS VALUE)

DEAR SHELTON,

(I) The Evidence of the Fossils.

The theory of evolution must stand or fall by the evidence of the fossils. These furnish the only *direct* evidence. If this be against the theory no amount of indirect evidence can avail.

(II) How Fossils are formed.

Fossils are the remains of, or impressions made by, animals and plants in the rocks. If an animal having a skeleton, shell, or other hard parts, be buried or covered by sediment after death before the hard parts have had time to decompose, they are likely to be preserved as fossils.

In this process they frequently undergo change by the replacement of more or less of their original material by mineral matter, such as oxide of iron. Animals lacking hard parts are much less often fossilised, and, in consequence, the fossil record of them is comparatively meagre, although they in many cases leave traces in the form of impressions made in the soil before they decay. Fossils of even jelly fish have been found. Fortunately the animals having hard parts outnumber those that lack these, in consequence the crust of the earth holds untold millions of fossils. I may here say that birds and bats, owing to their power of flight are less liable than ground animals to be drowned and buried in floods, or to be buried by falling cliffs, or to be bogged, hence their fossils, although numerous, are not so abundant as those of mammals tied to the ground. Arboreal quadrupeds are less liable to fossilisation than these last.

(III) The Alleged Imperfection of the Fossil Record.

One of the most formidable objections to the evolution theory is the fact that no fossil has been discovered of an animal intermediate between creatures having a very peculiar skeleton, such as

bats, whales, dugongs, seals, frogs, turtles, pterodactyls, ichthyosauruses, etc., and the supposed ordinary quadrupedal animals, from which, according to the theory, they have evolved. If this theory be true, these intermediate forms must have existed in immense numbers in the past. Darwin devoted a whole chapter of the *Origin of Species* to the attempt to meet this difficulty. The best he could do was to express his belief that the fossil record is 'incomparably less perfect than is usually supposed.' And so far as I am aware no later evolutionist has been able to improve on Darwin's effort. *Mirabile dictu neither Darwin nor any of his followers seems to have made any attempt to ascertain to what extent animals are fossilised.* Their attitude seems to be: The evolution theory is true; therefore these intermediate forms must have existed; as none of their fossils have been found the fossil record is exceedingly imperfect. But is not this to put the cart before the horse?

(IV) The Fossil Record is far more complete than Darwin supposed it to be, and than his followers admit.

(*a*) *Every genus of animal having a skeleton or hard parts has left fossil remains.*

This last may seem to you a rash statement, but I propose to substantiate it. It is commonly said that only about one animal in a million is fossilised. I am prepared for the sake of argument to accept this. I now ask you to consider (i) the average population of a genus (ii) the average length of time during which on an average a genus persists. As to (i), in few cases can the population of a genus be less than 3 million. In the case of some genera this average is very many million. Let us take the case of a genus of which the average population is 3 millions, and which is renewed every 20 years. This means that every century in all 15 millions of individuals of that genus have existed. As to (ii), we know that quite a number of genera of animals have existed for more than 100 million years. But let us consider a comparatively short-lived genus which exists only 5 million years. To arrive at the total number of individuals that composed this genus during the period of its existence we have to calculate the number of centuries that make up 5 million years, this is 50 thousand, and by multiplying this by 15 million, i.e., the number of individuals that lived in each century, we arrive at a total of 75 thousand million, and as one of these in each million was fossilised, the genus in question left behind it 75 thousand

fossils. Thus, unless you dispute the above figures, I think you must admit that my assertion that every genus having hard parts leaves fossil remains is a very modest one. If you dispute my figures, will you please give what you consider to be the correct figures.

Let me emphasise that my figures do not mean that I assert that some 75,000 fossils of every extinct genus of animals having hard parts now exist. I am dealing with the extent to which fossils are laid down. In considering the percentage of such fossils that still exist we have to try to discover to what extent fossiliferous rocks are destroyed in course of time. Let us do this.

(*b*) *The extent to which fossils are destroyed.*

The destruction of rocks and with them the fossils they hold has been considerable, but nothing like so great as is generally believed.

The great bulk of the destruction of rocks is due to what is known as sub-aerial denudation, that is to say by the action of wind, rain and sun, and by exposure to the air. Most of the later sedimentary rocks are formed from the material derived from the denudation of earlier fossil-bearing rocks. The amount of destruction that any particular rock has suffered depends very largely on the situation in which it is laid down. I contend that :

(i) *The destruction has been comparatively small in the case of rocks laid down in the sea. Thus the geological record is not far from complete in the case of these rocks, which constitute by far the greater part of the sedimentary rocks now existing.*

It is only when, as the result of some disturbance, rocks laid down in the sea are raised above sea level that they become subjected to sub-aerial denudation. This ceases as soon as the rocks in question again become submerged. But these elevations are local affairs, and so each means merely the destruction of a number of fossils of the local fauna, and except in cases when the range of the species and genera so destroyed is limited to the affected area, the only affect of the destruction will be a diminution of the number of the fossils preserved of the species and genera affected. Thus, taking the geological record as whole, it is not far from complete in the case of marine animals having hard parts. Marine animals, be it noted, are far more numerous than land animals.

(ii) *The destruction of land rocks has been great in the case of those laid down in the Primary Epoch, and in those laid down far from the sea and at high elevations in the Secondary Epoch.*

(iii) *The destruction of land rocks laid down in low-lying coastal*

areas has not been very great in the Secondary and Tertiary Epochs, and the fossil record of the animals and plants that inhabited such localities is fairly complete.

(iv) *In the case of all land rocks laid down in the Tertiary Epoch the destruction has not been great, and, in consequence, the fossil record of Tertiary land animals is not very far from complete. For example fossils have been found of every genus of land mammal (except bats) now living in Europe.* Details of the fossils of Tertiary mammals of which I have found records are given in the tables in the appendix of this letter. These figures are rather startling. They tell us that at some stages of the Tertiary Epoch a greater number of genera lived in Europe and North America than now inhabit these continents. They dispose of the notion that the fossil record of Tertiary mammals is very incomplete. They tell us that many genera of land mammals became extinct in the Pleistocene period, also that in that period more genera of land mammals seem to have lived on those two continents than in most of the previous periods. The recent fall in numbers seems to be the result of the last ice age. The cold drove south many northern animals and eventually killed off many organisms. The warm intervals in the glacial period also caused some southern forms to migrate into Europe and North America. Fluctuations, mostly less marked, occur in the figures for the earlier stages. Some of these may indicate climatic changes, such as dry periods unfavourable for the laying down of fossils of land animals. Most of the fossils of these occur in lacustrine beds. But probably the greater part of the fluctuations is to be accounted for by the varying extent to which the various beds rich in fossils have been worked over by palaeontologists. The discovery of a new bed may result in the addition of a number of genera to the lists of those of which fossils have been found. Similarly more intensive excavations in beds that have not been completely explored for various reasons lead to lengthening of these lists. As the result of recent discoveries more than 120 new genera have been added to the lists made by me in 1932. For this I am greatly indebted to the papers of Dr. G. L. Jepson in the 'Proceedings of the American Philosophical Society,' since 1930 and to that of Dr. G. G. Simpson in 'Bulletin 169 of the United States National Museum,' also to the new 1937 edition of Dr. W. B. Scott's *A History of Land Mammals in the Western Hemisphere.*

From the above it is evident that the fossil record of the genera of Tertiary mammals is nearly, if not quite, complete. This fact

is of great importance in connection with the non-discovery of the intermediate fossils demanded by the theory of evolution.

Yours sincerely,

D. DEWAR.

APPENDIX

STATISTICS SHOWING THE EXTENT TO WHICH MAMMALS HAVE LEFT FOSSIL REMAINS IN TERTIARY AND LATER ROCKS

In order to gain some idea of the degree of completeness of the fossil record of mammals since the beginning of the Tertiary Period I have :

1. Ascertained the percentage of the genera now living of which fossils have been found, and

2. Compared the number of genera now living in Europe and North America with the number shown by the fossils to have lived at various stages of the Tertiary.

I. Percentage of living genera of mammals of which fossils have been found.

Here are the results of these enquiries :

Some years ago Levett-Yeats and I devoted much time to the working out of this percentage. The results of our labours are summarised in Tables I and II :

TABLE I

Type of Mammal.	No. of genera now living.	Percentage of such of which fossils have been found.
Land	408	60.54
Marine	41	75.61
Volant (Bats)	215	19.07

The Fossil Record of Marine Mammals

The above figures show that fossils of more than three-fourths of marine mammals (whales, sea-cows, seals, etc.) have been found. As new fossils are frequently discovered, it seems likely that, before fifty years have elapsed, fossils will have been found of every living genus. That this forecast is not unduly optimistic is suggested by the fact that in 1931, when Levett-Yeats and I made our enquiry, fossils had been recorded of 70.73 per cent of living genera, and later discoveries have since raised the above to 75.61.

The Fossil Record of Land Mammals

The above figures show that the percentage of land mammals of which fossils have been found is lower than that of marine animals. The figures

are also less conclusive, because fossils of about 42 per cent of these genera have been found only in deposits near the surface, which, being above sea-level are subject to denudation entailing the destruction of fossils. But, *per contra* (and this applies also to marine mammals), these genera are still living and in consequence fossils of them are now being laid down and this is likely to continue for many years to come; thus their possibilities of fossilisation are far from being exhausted. To overcome the difficulty presented by land mammals No. 2 of the above investigations was undertaken. Before considering this, let me give some statistics indicating that the reason why relatively fewer fossils have been found of land than of marine mammals is our lack of knowledge rather than the incompleteness of the geological record of land mammals.

TABLE II

PERCENTAGE OF GENERA LIVING IN EACH CONTINENT OF WHICH FOSSILS HAVE BEEN FOUND

Continent.	No. of genera of land mammals now inhabiting it.	Percentage of these genera of which fossils have been found.
Europe	48	100.00
North America	71	94.44
South America	86	72.09
Asia	134	72.06
Africa	145	53.79
Australia	48	45.83

As there is no known reason why mammals should be less readily fossilised in some continents than in others, the percentages in the Table are almost certainly indices of the extent to which the various continents have been explored geologically. Future discoveries will probably eventually bring the percentage of each continent up to 100 and thus demonstrate that the geological record of the Tertiary Period is complete in respect of genera of land mammals.

The Fossil Record of Volant Mammals (Bats)

So far fossils of only 19.07 per cent of the living genera of bats seem to have been recorded. This low figure, despite the fact that these animals are less liable than marine or ordinary land mammals to meet with accidents resulting in fossilisation, does not necessarily mean that the geological record is incomplete in their case. One cause of the present low figure is that most kinds of bats are confined to areas in the tropics in which very little geological exploration has been made. Fossils of all five genera of bats now living in England have been found, and this is equally true of every genus now found in Europe. It is within the bounds of possibility that fossils of every genus of bat may eventually be found.

In conclusion we have to consider the past populations of the land mammals of Europe and North America.

2. Comparison of the numbers of genera of land mammals now living in Europe and North America with those shown by the fossils to have existed in those continents at various stages of the Tertiary Epoch.

The fossils are the only means available of knowing whether or not any genus of animal was in existence in any particular period of the past. The fossil records may be said to be the census returns of prehistoric times. If we find the fossils of 100 genera in the rocks of any period, we know that 100 genera were then in existence ; there may have been many more but there cannot have been fewer. Here are some of these census returns.

TABLE III

NUMBER OF GENERA OF NON-VOLANT LAND MAMMALS KNOWN TO HAVE LIVED AT VARIOUS STAGES OF THE TERTIARY AND QUATERNARY IN EUROPE AND NORTH AMERICA

Stage	North America	Europe
NOW LIVING	72	48
Pleistocene	117	68
Upper Pliocene	52	47
Middle Pliocene	28	48
Lower Pliocene	67	88
Upper Miocene	61	82
Middle Miocene	54	59
Lower Miocene	63	52
Upper Oligocene	61	43
Middle Oligocene	66	41
Lower Oligocene	61	80
Upper Eocene	46	68
Middle Eocene	80	38
Lower Eocene	78	24
Upper Palaeocene	60	14
Middle Palaeocene	68	
Lower Palaeocene	32	

DEAR DEWAR,

My first duty is to make it clear that the choice of geology as the subject with which to begin our detailed discussion of evolutionary evidence is not mine. The editor has taken the responsibility for the choice. Consequently my letters must not be regarded as an attempt to present a logical and coherent statement of the case for evolution. It is an order that implicitly assumes that evolution has taken place, and puts in the first place the subject in which I have to meet the most important of your difficulties. As is clearly shown by Lunn's quotation from Huxley, the idea of organic evolution

became widely accepted in the scientific world when the evidence from geology was practically non-existent. Since then, as I shall show in the next chapter, very powerful evolutionary evidence has been found, but this is additional to the general evidence on which the idea was originally based.

There is some confusion of thought on this matter on both sides. I have heard it stated by people of some knowledge and considerable intelligence that the question of evolution was settled because fossils have been found which prove its occurrence. In a sense this is true. The original evolutionists were well aware of the very slight palaeontological evidence, and would gladly have welcomed confirmatory evidence from the fossils. This confirmatory evidence has been found, and so far as it goes it is very good. It is what the early evolutionists wanted and more—*for confirmation*—but it is not sufficient and complete in itself. To put it in another way—in some cases it is very good, but in a large number of cases it is still missing. The reason of course is quite obvious. The geological record is very imperfect. No ordinary sensible person expects it to be otherwise. This general question of the imperfection of the geological record I must defer to my next letter, as in this letter I have to deal with a number of arguments which I suppose are intended to show that the record is not imperfect, or at least not seriously so.

You will thus see how fundamentally I differ from your first paragraph. I had better quote it: 'The theory of evolution must stand or fall by the evidence of the fossils. These furnish the only *direct* evidence. If this be against the theory, no amount of indirect evidence can avail.' Well, well! In the first place, though there are many theories of evolution, I am not prepared to admit that there is any *theory* of evolution. Evolution is something that has occurred, explain it how you will. Nor can I agree that it *must* stand or fall by the evidence of the fossils. There is no must about it. There is no *a priori* necessity that there should be any fossils at all. And whether there are enough to be of any use in our discussion is a matter for detailed investigation. It is theoretically possible that fossil evidence might be so complete as to render further discussion superfluous. It is possible that in the future it may be so, but at present I should hardly venture to say that it is. It is again theoretically possible that the fossils might give evidence against evolution, but as a matter of fact there is no such evidence. Much evidence we should like to have is missing—that is all.

Once again, fossils do not give direct evidence. You might find a fossil of a species of fish which was ancestral to the whole of vertebrate land life, but it would not be labelled, and, if we found it, we should not know that it *was* ancestral. To know which forms are approximately in the line of descent of modern species requires very complicated inferences, and it is wholly impossible in the present state of knowledge to know whether they actually are so. Propliopithecus may be the ancestor of man and the apes, or it may not, we don't know. Piltdown man may be the ancestor of modern man, or it may not, we don't know. To disentangle the evidence given us by the vast complexity of nature is a very long process. Things are not as simple as you appear to think. It would be difficult to put together a plausible sentence more fallacious than the one with which you commence your letter.

As some of your other sentences are just as fallacious, it would take up too much space to try to deal with them. This will have to do as a sample. As a rule I have no use for wild general assertions, and cannot find space to deal with them.

Instead of that we will look at some of your detailed evidence, and particularly your tables. In Table 2 you give the percentages of living land mammals of which fossils have been found. What about it? We must remember that fossil is a relative term. The relics which remain recognisable for 100,000 years may not be sufficiently well preserved to last a million years, to say nothing of 10, 100, or 500 million years. I am not referring to the plain fact that the position of these fossils, usually in caves or river gravels, are never, or very rarely, those in which fossils are found in older formations. I know of no instance of early fossils found in what was originally a cave. And river gravels, when they appear at all, appear as conglomerates (though most conglomerates are old sea beaches) which are very poor in fossils. What is more important is that the fossils very often do not remain in the rocks at all. They get removed by underground water and other natural agents, or destroyed by metamorphosis. Those that remain (in secondary and primary formations) are lucky finds. I will deal with this more fully in my next letter.

Your third and most important table shows that a considerable number of fossils of *land mammals* in *Tertiary* formations have been found. What about it? As the evidence for evolution in Tertiary land mammals is so strong, it naturally follows that a large number *have* been found. Where many fossils have been found the evidence

for evolution is strong. Where few fossils are found, the evidence is weak—naturally. What you seem to be trying to prove is not only that the record is much better than usual, but complete—or nearly so. You give no reasons for this, or at least the only one I can find is the following: 'Future discoveries will probably eventually bring the percentage of each continent up to 100 and thus demonstrate that the geological record of the *Tertiary Period* is complete in respect of land mammals.' It is difficult to comment on this. The percentage in each continent refers to living creatures. The Tertiary Period is about 70 million years. There is no connection between fact and inference, especially as I have already pointed out that few of these fossils would be found if deposited in a similar way in earlier epochs. I may also remark that your tables have very little bearing on the geologic record as a whole. The third and most important refers to land mammals and to the last 70 million years. What is this in comparison with the 1,000 million or more of the earth's history? This comparatively brief time is merely the last period of mountain building. There were quite a number of similar periods before. Of course the last phase is better preserved than the others.

One obvious reply to your implied contention that the record is reasonably complete is that new discoveries are continually being made. You cannot make new discoveries if there is nothing to discover. The tables themselves show obvious incompleteness: 52 genera in the upper Pliocene compared with 117 in the Pleistocene in America, and in Europe 80 in the lower Oligocene and 41 in the middle Oligocene. Another point to notice is that the Pleistocene is a short period of only about a million years, in which few if any new genera would have time to evolve, whereas the average third part of the previous epochs is about 6 million years in length, giving ample time for more than one series of genera in a rapidly evolving class. Moreover, if you find all the genera, you still have not all the evidence for evolution we should like to have, or even all that has in a few special cases been found. Finally it is quite easy from an incomplete fossil, sometimes only a lower jaw, to infer that it represents a genus not previously known, but it would be very difficult indeed to interpret its evolutionary significance. To do this properly requires a fossil that is reasonably complete.

I know you attach considerable importance to these tables, and so have given them more attention than I should otherwise have done; but I think you would do well to explain quite clearly and

explicitly what you think they prove. Personally I regard them as a side-issue and must defer the main question of the value of the geologic record to my next letter.

Yours sincerely,

H. S. SHELTON.

DEAR SHELTON,

It is easy to see why you dislike the editor's decision about the order of our discussion, because it reveals very early in our debate the weakness of the evidence for evolution, but logically the decision is correct. It is the order adopted by Chambers, who forestalled Darwin, in his *Natural History of Creation* published some 15 years before Darwin's *The Origin of Species*. Chambers devotes the greater part of the first 100 pages of his book to what he calls 'the wondrous section of the earth's history which is told by geology.' Then he proceeds to interpret the successions of the fossils on the theory that the later ones evolved from the earlier. After that he adduces subsidiary facts relating to morphology, embryology and geographical distribution in support of the main evidence—that of the fossils. But his book, although it went through eleven editions, did not exercise much influence, because the only direct evidence is so unfavourable. Darwin was astute enough to see this and in consequence took care to put the geological evidence in the background. He devoted the first half of his book to the variability of animals and to trying to make his readers believe that, given time enough, the variations exhibited by animals afford natural selection unlimited material upon which to operate. Then he comes to the testimony of the fossils, and asserts that this counts for little as the geological record is exceedingly incomplete. Thus he succeeded in fooling a world only too willing to be fooled. You, in our debate, very naturally want to follow the procedure which Darwin found so successful.

You say you disagree fundamentally with my assertion that the theory of evolution must stand or fall by the evidence of the fossils as this is the only *direct* evidence. You say there is no *a priori* necessity that there should be any fossils at all. This is true, but the only hope of proving evolution is the appeal to the fossil record. Apart from this no one can know (apart from revelation) whether or not animals or plants or men existed on the earth 10,000 years ago. Were there no fossils all theories of evolution would be merely

interesting speculations which could neither be proved nor disproved. Clearly then, if the theory of evolution is to have a scientific basis, that basis must be the fossil record. This is why the statistics contained in the tables I have compiled are of great importance.

What the Statistics in Tables I, II and III prove.

You ask me to state explicitly what the statistics in the appendix to my last letter prove. The answer is that they go a long way to confirm in the case of mammals (they relate only to mammals) the conclusion, based on animal population figures, that every genus having hard parts leaves a fossil record in the rocks. Table I shows that fossils have been found of more than half the living genera of land mammals, three-fourths of those of marine mammals, and about one-fifth in the case of bats. In order to show the extent to which mammals are fossilised an unknown number, x y or z, must be added to each figure in the table to represent fossils now in the rocks that have not yet been found of genera not included in the above figures, and to allow for the fact that, as the genera are still living and fossils of them are now being laid down, the record of these genera is still being compiled. In this connection I must comment on your naïve statement: 'One obvious reply to your implied contention that the record is reasonably complete is that new discoveries are continually being made. You can't make discoveries if there is nothing to discover.' Cannot you see that you can only discover something that exists? You can only discover fossils that have been laid down. Every discovery of a fossil of a genus of which no fossil has already been recorded proves that the fossil record is more complete than it was known to be before the discovery.

Table II was compiled with the object of ascertaining as far as possible whether the reason why fossils of every genus have not been found is the incompleteness of the record, or inadequate exploration by the fossil hunter; and the data make it almost certain that in most cases the latter is the reason. Thus the table gives evidence in favour of the completeness of the record. The statistics embodied in Table III were obtained with considerable labour with the object of trying to discover the extent to which fossils are destroyed in the manner suggested by you and in any other manner. If this destruction be considerable, then the older the period the fewer should be the genera which the rocks of the period in question contain: the numbers should fall off as we read

down the table. They do not. They show irregularity, for reasons I have explained.

As the oldest known undisputed fossil was laid down about 500 million years ago according to the time-table we have agreed to accept for the sake of argument, the 70 million years of the Tertiary should be compared with the 500 million years of the fossil record, not 1,000 million, and so the Tertiary period represents about one-seventh of the period between now and the beginning of the Cambrian. Thus the question arises: since there seems to have been little destruction of the fossil record in 70 million years, is the destruction likely to have been very great in a period four times as long?

If you do not accept my contention I presume you will adduce statistics supporting your assertion that the geological record is very imperfect. I will now adduce evidence in support of my assertion that:

The Geological Record is not far from complete in respect of marine animals having hard parts.

I may here say that, apart from the late Mr. G. A. Levett-Yeats, who gave me great assistance, I believe that I am the only person who has made the least attempt to ascertain the degree of completeness or otherwise of the geological record. As the labour involved is considerable and I have not been able to devote very much time to the enquiry, I have had to confine it in the case of marine animals other than mammals to the molluscs that live and have lived in the British Isles area. Even this limited enquiry is not nearly complete, but it has gone sufficiently far to reveal a number of facts very unfavourable to all theories of evolution. The statistics I have compiled are set forth in the appendix to this letter.

Table IV shows that, in all, fossils of 74.58 per cent of the living genera of British molluscs have been found. It also shows that the percentage varies greatly with the group. The variation is due to the fact that in some groups all the genera have shells, while some members of the others lack shells, and in the case of the cephalopods (cuttle fishes) the only 'hard part' is the cuttle-bone, which is soft and easily decomposable. In the case of these last, fossils have been found of only 3 of the 11 living genera. In the opisthobranchiata (the class that includes the sea-slugs), 41 of the 57 genera lack shells or any kind of hard part. Fossils of these are almost unknown. One of the Pulmonata (snails, etc.) lacks a shell

and no fossil of this has been found. Table V shows that of the 187 genera of British molluscs having shells, fossils of 176 have been found, leaving 11 genera of which I have found no fossil record. Of these, one has no shell in the adult state, five have minute shells $\frac{1}{10}$ inch long in two cases and less than $\frac{1}{4}$ inch in the others; the shell of two of the others is as thin as paper, and one is an ecto-parasite.

Table VI shows the number of British genera which the recorded fossils *prove* to have been in existence in each of the geological periods. In order to demonstrate the significance of the figures in the table, let us consider those for the Eocene period. They show that 138 of the 187 genera now living in the British area were living in that area in the Eocene period. These 138 do not represent *all* the genera of which fossils have been found in British Eocene rocks, but only those that are still living in England. To these must be added the genera that were living in England in Eocene times but have since become extinct or have migrated to warmer climes. I have come upon records of 62 of these, and my enquiry is not yet completed. Thus we must add 62 to 138 and we find that at least 200 genera of molluscs lived in the British area in the Eocene period—*a greater number than are now living in it.*

You concede that you cannot produce a lineage series of fossils linking one Order with another, still less one Class with another Class. You appeal to the imperfection of the geological record which has resulted in the disappearance of the fossils you need to support your theory, fossils of millions of genera that must have existed if slow and gradual evolution be true. All the genera linking Order and Order, Class and Class, Phylum and Phylum have vanished. You can at best point to a few dubious forms which you label transitional.

Your theory stands or falls with the doctrine of the great imperfection of the geological record. But how can you sustain this doctrine in face of the statistics embodied in the above tables? How can you square it with the very high proportion of living genera found in fossil form? Why should all the fossils necessary to prove your theory have vanished and those necessary to demonstrate the comparative perfection of the geological record have survived?

Yours sincerely,

D. DEWAR.

APPENDIX

TABLE IV

British Molluscs

Class of Mollusc.	No. of genera now living.	No. of genera now living of which fossils have been found.	Percentage of living genera of which fossils have been found.
I. Lamellibranchiata (bivalves)	67	67	100.00
II. Gastropoda			
Polyplacophora	1	1	100.00
Prosobranchiata	77	76	98.70
Opisthobranchiata	57	11	19.30
Pulmonata	25	19	76.00
Scaphopoda	2	2	100.00
III. Cephalopoda	11	3	27.30
Total	240	179	74.58

TABLE V

British Molluscs having External Shells

Number of genera now living.	No. of these of which fossils have been found.	Percentage.
187	176	94.20

TABLE VI

Fossils of British Molluscs

Period.	Number of genera of which fossils have been found.
Pliocene	173
Eocene	138
Cretaceous	95
Jurassic	64
Triassic	41
Permian	33
Carboniferous	31
Devonian	19
Silurian	16
Ordovician	6
Cambrian	1

DEAR DEWAR,

The statement that I dislike the editor's order is hardly correct. It all depends what you want from me. If you merely want an

answer to your difficulties, one order will do as well as another. If, however, you want me to state the case for evolution in a clear and logical manner, I cannot do that unless I am allowed to choose my own order. This requires to be stated quite definitely from the start. With regard to geology, the position at present is that, as I shall show in the next chapter, there is ample evidence for evolution, but much evidence we should like to have is missing.

It is not necessary to give further attention to your tables. In my last letter I showed that your first three tables indicated quite clearly that, even for land mammals in tertiary formations, the record is seriously incomplete. Your sixth table shows the same thing for the molluscs. There is no need for me to make out new statistics. Yours suit my purpose well enough.

I think, however, that you do not realise the great difference between the record of the present period of mountain building and those of previous epochs. When there is a continuous period of mountain building, which is what has happened since the Eocene, the deposits which have been formed in seas and lakes are pushed upwards, and are available, in so far as they are accessible, and have not already been eroded away. But when they have been eroded away, *they are gone*. This is what has happened to the earlier deposits, to which this process has been applied several times. Consequently, what remain are fragments, and it would be absurd to expect that they would be continuous in time. We get, as would naturally be expected, abundance of fossils now and again, but with gaps. What surprises me is not that the record is fragmentary, but that it is as good as it is. Certainly in the earlier epochs it would be very surprising if we found anything approaching continuous series.

It is instructive to be asked to give reasons for this natural assumption of the fragmentary nature of the fossil record, especially that of earlier epochs, because it is one of those things which we are liable to take for granted. All the same, I think some very cogent reasons can be given. In the first place, I must point out that the argument that because evolutionary evidence is missing therefore the record is fragmentary is not the *petitio principii* that it might appear to be at first sight. If there were no geologic evolutionary evidence it would be arguing in a circle; but when there is very good evolutionary evidence in a few special cases, and obvious links are found in other cases when series are missing, the inference that the record is incomplete is not altogether fallacious.

Otherwise we should be faced with the extraordinary conclusion that some forms of life (as for example a large number of land mammals) have been evolved and other forms have not. To say the least, this would be peculiar. We all of us expect to find some sort of order in nature, and if we fail to find it we naturally assume that we have not sufficient evidence.

We are not, however, dependent on arguments like these. Our knowledge of the rocks gives us all the evidence we need. I think we can assume that most of the recent sedimentary rocks contain fossils of some sort. *But they do not stay there.* I do not merely mean that the rocks are destroyed by erosion, I mean that even when the rocks remain the fossils often disappear. An obvious example of this is metamorphosis, which destroys them altogether. What is still more important is the solvent action of underground water, and similar agents. It is not all sedimentaries, even those which show no obvious sign of metamorphosis, which contain fossils. Any good text-book on geology will tell you this. The usual statement, which you will find in Geikie, is that as a rule shales are fossiliferous and sandstones are not, though there are exceptions on both sides. I think we can assume with confidence that as a rule the fossiliferous sandstones are comparatively recent, and the unfossiliferous shales more ancient formations, though here again there is no absolute rule. Thus, as we go back in time, not only are there serious gaps in the sedimentary series, but many of the rocks that remain have lost their fossils. Consequently the fossils that remain, while they merely form an incomplete record in recent formations, become more and more fragmentary as we go back to earlier times. What then remains is a number of lucky finds which we have to try to piece together as best we can. Our general conclusion must be that, while the fossils that have been found are evidence, we must be exceedingly careful what inferences we make from the absence of fossils.

There is a point of definition here which needs emphasis. We need to be quite clear what we mean by the fossil record. I know of only one meaning which has relevance to the present discussion. By the fossil record I mean *the fossils that have actually been found.* Consequently, any remarks you make on the fossils that have been laid down (whatever that may mean) or those that are in existence, or even those that are available, have very little bearing on the matter. There is, for example, in your letter on p. 59 a passage in italics about a nearly complete record in rocks laid down in the sea,

which the subsequent paragraph explains as meaning so long as they stay there. I am not disposed to argue what fossils are now under the sea, as they are not available, and so not part of the fossil record. The point is that it is only by a lucky chance that a palaeozoic deposit has stayed there all this time, and now has come up conveniently for us to examine it. These lucky chances do occur, and that is why we know anything about palaeozoic fossils, but you should realise that it is an outside chance, and can only be expected to give us fragmentary information about the life of these earlier times.

Nor are we confined to these general reasons. We get cogent evidence of the fragmentary and incomplete nature of the fossil record from the fossils themselves. The following examples will illustrate my meaning :

1. The following passage occurs in your first letter in this chapter : 'Thus, taking the geological record as a whole, it is not far from complete in the marine animals having hard parts.'

I think we may take it that marine animals having hard parts include fish. Now let us look at an interesting example. On the 18th of March, 1939, an announcement was made in *Nature* of the discovery of a living fish which was supposed to have been extinct since Mesozoic times. The discoverer states that it is nearly related to the Macraphorna of Agassiz, and is more nearly related to the earlier types (Jurassic) than to the later ones. Thus, in the 150 million years or so since the Jurassic, during the formation of the most recent and complete part of the fossil record, these fish have been in existence and *not a single fossil has been found.*

2. The second relates to birds. Two specimens of the feathered reptile Archaeopteryx and one or two of the related Archaeornis have been found in the Jurassic. There is then a gap of fifty million years or so, and in the upper Cretaceous we find birds with teeth. Then there is a smaller gap and birds nearer to the modern type are found in the Eocene. Where are all these missing feathered creatures? Surely there were feathered creatures in all this time, but their fossils have not been found. A further fact about birds is Zittel's statement that 12,000 living species are known, but only 500 fossil species have been found. These figures speak for themselves.

3. The third relates to amphibia. Until recently no anura (frogs and toads) were found earlier than the Eocene. Recently specimens have been found in the Jurassic, but none in the Cretaceous.

4. The fourth relates to land mammals. Since the Eocene the record is good. But specimens which from their bony structure appear to be mammals are found in the Jurassic, and possibly even in the Triassic, but the very few specimens that have been found, some of them only lower jaws, show how fragmentary the record is. The collection in the Natural History Museum, which is said to be the second largest in the world, is very scanty indeed.

5. The fifth example is a general one, showing how difficult it is to trace a phylum to its source. Fragments of vertebrate bone have been found in the middle Ordovician, but no recognisable vertebrate skeleton till the late Silurian—a gap of 50 million years or so.

We need not multiply instances. Quite a number of cases can be found when orders or classes are found in two epochs not consecutive, but not in the intermediate epoch.

The common sense attitude is very simple. It can briefly be stated in the obvious question : where do the fossiliferous rocks come from ? Your statement that upheaval and erosion is purely a local affair is very misleading. If you mean that the oceanic mass as a whole is relatively permanent, most geologists will agree with you. But when we remember that the Himalayas now tower beyond the reach of the human climber where it once was sea, we can appreciate that upheaval is more than local. The American Geological Survey have calculated that about three quarters of the rocks subject to erosion are sedimentary, the rest being igneous or metamorphic. We must also remember that metamorphic, and to a lesser extent igneous rocks, imply a considerable destruction of earlier sedimentary rocks. This answers the question. All the later sedimentary rocks exist because an equal volume (or approximately so) of earlier sedimentary rocks have been destroyed, ground to powder, carried to the sea and re-deposited. The same applies to earlier formations. Consequently it follows that the earlier rocks that remain are only a minute proportion of what has been laid down, and this proportion becomes less and less as we go back in time. It would be most extraordinary if these deposits were continuous, or anything like it. We have a succession of fragments, a very small proportion of which have been thoroughly explored. The conclusion seems so obvious as to need no further explanation.

In dealing with your exposition I have to guard against the danger of overstating on the other side. There is one aspect that gives hope for the future. Very few even of the rocks that are now

available have been thoroughly explored. Up to the present we have been mainly confined to the outcrops, and even of these it is only parts of Europe and North America which have had a reasonably good preliminary exploration. No doubt rocks bridging present gaps will be found in future, and very slowly a much better record than now exists will be built up. Yet I greatly doubt whether it is possible in more than a few isolated instances to find such series for earlier evolution as are even now available for the ancestors of some of our existing land mammals.

Yours sincerely,

H. S. SHELTON.

DEAR SHELTON,

You say that what you mean by the fossil record is the fossils *that have actually been found* . . . and as fossils under the sea are not available they are not part of the fossil record! Clearly you have failed to distinguish between the fossil record and our knowledge of it. This explains the remark in your first letter which mystified me, that the record cannot be reasonably complete because new fossils are constantly being discovered. By the term fossil, or geological record, I mean the testimony of the rocks which consists of the fossils they now hold *plus* those dug up by man. Its completeness has nothing to do with the extent to which it has been explored by man. A fossil in the bed of an ocean or under ice in Greenland is as much a part of the record as one that will be dug up next week in England.

Please bear in mind that most of the marine rocks known to us contain much terrigenous material, and, in consequence, seem to have been formed within two or three hundred miles of the shore. Thus the fossils these rocks hold are of *organisms which lived near the land.* Further, there is evidence that most rocks devoid of terrigenous matter, e.g., chalk and some limestones, were laid down near land. Thus *the known marine fossils represent, not all oceanic plants and animals, but merely those living near the coasts.*

Here are comments on your examples of the imperfection of the fossil record:

1. The fact just mentioned disposes of the difficulty of the recent discovery of a living Coelacanth fish supposed to have become extinct 150 million years ago. Something caused this fish to move from the coastal to the open seas, so that any fossils of it that may

have been formed since are buried under the sea. That only one specimen has been caught within living memory suggests that it is not a common denizen of coastal waters. The fact that it is shown by the fossils to have been in existence 150 million years ago affords an example of the stability of species or genera.

2. I am greatly amused at your calling Archaeopteryx a feathered reptile. It is as good a bird as the duck-billed platypus is a good mammal, so why not call platypus a hairy reptile? The paucity of fossils of birds in Mesozoic or Secondary rocks is easily explained, because, as I stated in my first letter in this chapter, flying animals are less liable to fossilisation than ground animals. Moreover, birds, other than those that subsist on fish, are almost entirely directly or indirectly dependent on flowering plants, which in the Jurassic and the first half of the Cretaceous period had not reached the lowlands, at any rate outside the Arctic Circle. Before this they were confined to the uplands, where the rocks laid down in the Secondary period have been almost entirely eroded away with the fossils they contained, as I said in a former letter (p. 60).

3. This last fact accounts for the paucity and very fragmentary nature of the known fossils of Mesozoic mammals. These were confined to the uplands, and their fossils laid down there have disappeared. What remain and have been found are parts of their skeletons carried by streams to the lowlands where they were buried and became fossilised.

4. Fossils of frogs and other modern amphibia are very scarce; I doubt whether, all told, those of forty genera have been found. This may be due to their soft, slender bones being easily decomposed after burial. If the fossil record were as incomplete for other vertebrates you would have quite a good case for pleading the poverty of the record as a reason for the non-discovery of the thousands of links required to support the evolution theory.

5. The facts you mention, while not quite accurate, show, in my view, that fishes existed in the Ordovician and most of the Silurian period (also in the Cambrian), but they were then very scarce in the coastal seas. As I shall show, since the Ordovician there have been several waves of fishes invading the coastal seas.

I do not know what you mean by 'epoch,' but I am surprised at your statement that quite a number of Orders and Classes are found in two epochs, but not in the intermediate one. Please name six cases of each, merely giving the name of the Class and Order and the epoch in which their fossils are missing.

In conclusion, let me say that your examples of the imperfection of the fossil record are all of animals that are not very readily fossilised or that inhabited localities of which the fossils are not accessible or have been eroded out of existence. You have not tackled my statistics relating to molluscs which are readily fossilised. These show that in some groups fossils of 100 per cent of living genera have been found, and that the number of molluscs living in the British area was greater in the Eocene period than it is to-day. Here are some further facts. The fossil record tells us that more than 220 new families of molluscs have appeared between the Cambrian and the Eocene periods. According to you, each of these evolved from some pre-existing family. Six genera intermediate between each of these and its parent family is a low estimate (ten would be nearer the mark). This means that more than 1,200 intermediate genera of molluscs must have existed in the past, of which no fossil seems to have been found. If this large number of intermediate genera really existed, is it credible that none of their fossils should occur among the 1,000 odd genera of which fossils have been found?

The fossil statistics of the other phylum of animals having shells —the Brachiopods or Lamp-shells—are even more unfavourable to your idea of the imperfection of the fossil record. This record tells us that no new Class of Brachiopods has evolved since the Cambrian period, i.e., during the last 500 million years. The record also tells us that a greater number of Brachiopods existed in the Middle Cambrian period than are now living: 37 as opposed to 33.

Here we have yet another example of the fact that this record—so imperfect according to you—reveals a greater number of genera of Brachiopods living in the Cambrian period than in the world around us. If the record be as fragmentary as you make out, then would it not seem that the 37 Cambrian genera revealed by it indicate that, shall we say, 37,000 genera of lamp-shells lived in the Mid-Cambrian period?

Genera may be divided into two kinds: those which we know to have existed, and those whose existence in the past is essential to the theory which you affirm and I deny. How can you account for the *amazing contrast between the richness of the record in respect of the genera which are known to exist and the appalling poverty of the record in respect of the genera necessary for your theory?*

Yours sincerely,

D. DEWAR.

DEAR DEWAR,

This discussion seems to me to be petering out, and I have little more to say, having I think proved my point. The only reason for the few remarks that follow is to point out for the benefit of the general reader that most of what you say in your last letter is irrelevant to the issue we are discussing.

The object of this chapter, arranged by the editor, so far as I am able to understand it, is to argue whether the admitted gaps in the evolutionary series are due to evolution not having occurred or to deficiencies in the fossil record. *Obviously*, if this is the point under discussion, the only meaning of fossil record that has any bearing on the matter is *the fossils that have been found.* The other point—the fossils that exist but have not been found—has no bearing on the discussion. Of course, when I point out that fossils have been destroyed by erosion, by metamorphosis, by the action of underground water, and in other ways, that is relevant, because if they are destroyed they *cannot* be found. But when you speculate about what are in existence under the sea, or in other inaccessible places, or indeed in accessible places, I am not concerned to argue the matter. Speculate away if you like, but it is not incumbent on me to notice anything you have to say. I am only concerned with the fossil record in the sense of our knowledge of it. The rest is pure speculation and irrelevant.

I note your explanations of *why* our fossil record is fragmentary in certain cases. Again that is speculative, and I am not interested. The important point is that it *is* fragmentary, and, that being so, the evolutionary evidence is what it would be expected to be, that is reasonably good in a few special cases, but generally speaking, showing only a few obvious evolutionary links.

You will now, I think, understand the meaning of one of my arguments which puzzled you. You gave tables intended to show that a fossil record of land mammals *had been discovered* (at least I hope you meant that or your tables are valueless) which you thought to be reasonably complete. I pointed out that as new forms are continually being discovered it could not be complete. This, of course, is additional to the uncertainty whether many of the forms that existed are there to be discovered. The confusion is entirely yours. I always use the term fossil record in the sense of *our* record, the fossils that have been found. You vary in your use of the term, and so cause confusion. Is it allowable to suggest that it is partly because you are liable to similar confusions that you are an anti-evolutionist?

There is one other point in your letter which requires notice. You want to know why a large number of genera of Brachiopods are found in the Cambrian. I don't know. Nor do I know how many have not been found. In our fragmentary fossil record we have from time to time lucky finds. Now and again rich fossil beds are found. When you make such a lucky find, obviously you find a great variety of fossils. Why not? And what has it got to do with the question under discussion? You do occasionally get a rich find of certain types of creature which happen to be abundant at the time, and which are liable to be fossilised under the particular conditions existing when that particular rock was being formed. This gives us no information about their ancestors or descendants. Nor does it even tell us the period of time between the formation of this rock and the formation of the next one containing fossils which were formed under similar conditions, that is, of course, assuming that such a thing exists, which is by no means obvious.

Now let me summarise what I have said in this chapter. The fossil record (our record) at its best is very incomplete. In a few special cases, such as Tertiary land mammals, it is good enough to give us detailed evidence for evolution. Generally speaking, especially in earlier epochs, it is fragmentary. Then we only get occasional links. These links, though not detailed evidence, are evidence. In short, the evolutionary palaeontological evidence is very much that which from the very nature of the fossil record, it would naturally be expected to be.

Yours sincerely,

H. S. SHELTON.

P.S.—I will discuss Archaeopteryx in the next chapter.

3

THE GEOLOGICAL RECORD (ITS CONTENT)

DEAR DEWAR,

In the last chapter we discussed the value of the geological record. Although we disagree about the extent of its incompleteness, we agree that it is better in the Tertiary formations than in earlier epochs. You also in your tables showed that a considerable number of genera of land mammals have actually been found in these later deposits. Moreover it happens that in the tertiary formations the evolutionary record of the land mammals is better than that of other classes. We are fortunate in that a very considerable amount of evolutionary change has in fact taken place in land mammals during that period, whereas, in the reptiles, birds and amphibia the most fundamental changes took place earlier. The reason for these introductory remarks is to make it clear that there is no contradiction between my contention that the geologic record as a whole is very imperfect, and, what I propose to show in this chapter, that the record, such as it is, gives very powerful evolutionary evidence.

This letter, therefore, will deal only with land mammals, and I shall still further limit it by confining it to one branch. Naturally I am choosing the best example. There are others, but they are not quite so good, and I shall mention them briefly later. The branch I have chosen is the fossil equidae, that is the ancestors of the present horse genus, which now consists of varieties of the horse, the ass and the zebra, together with the recently extinct quagga. We need to emphasise from the start that the fossils of this group, after the first step or two, are related to all living equidae in exactly the same way as they are to the horse, and no distinction can be made between them.

According to Lydekker, the late Pliocene Equus stenonis was a variable species, different specimens of which showed similarities to the ass and the zebra as well as to the modern horse.[1] In other words these different species seem to have disappeared, and a single generalised species to have taken their place. Equus caballus, the modern horse, has gone. (H.249.)

[1] *The Horse and its Relatives*, p. 248 (referred to later as H.).

Behind this late Pliocene form is a valuable series. I have referred to a number of books and find them all in substantial agreement on the order and details, so will now follow that of the *Science of Life*[1] (in future referred to as S), though I am putting the facts in the reverse order to that given there. Very little earlier than Equus stenonis, and possibly contemporary with it, is a form known as Plesippus (S.349). In this form the teeth are not so well developed as in the modern species, the splint bones are longer and expanded at the tip, and there is a nodule of bone representing the fifth digit.

Previous to Plesippus, and found in deposits ranging from the late Miocene to the middle Pliocene, is a genus known as Pliohippus. In this genus the transition from the three-toed to the one-toed animal is completed. Some of the species have small side toes with hoofs, and in others the hoofs are missing, and the remnants are splints larger than in existing species. The teeth are similar to Plesippus. (S.348.)

Previous to Pliohippus, earlier in the Miocene, there seem to be two genera: Parahippus and Merychippus, both very near the direct line of descent, and to some extent overlapping in time, though Parahippus is found earlier. Merychippus always has three toes two of which never touch the ground. In the young of the species a frail separate ulna has been noted, though in the adult it is fused with the radius as in the modern genus. Important differences are found in the teeth, those of the later species being considerably longer in proportion to the width than those of the earlier species. In Parahippus the complete ring of bone round the orbit of the eye has disappeared. (H.254.) Another feature of Parahippus is that in the earlier species the cement of the teeth is very thin, and in the next earlier genus Miohippus it disappears altogether. (S.347, 348.)

Miohippus, and the next earlier genus Mesohippus, are found in the Oligocene and in the early Miocene. They are called two genera for convenience, but they seem to grade into one another. In Mesohippus the three toes all appear to touch the ground, the radius and ulna are distinct bones, though the radius is much better developed.

[1] This reference is deliberate. I have noticed in anti-evolutionary circles a tendency to depreciate this book. Where I have been able to check them, I have found its statements of fact reliable, and therefore propose to refer to it *for facts* in exactly the same way as I shall to the other numerous sources of information that are available.

Before Mesohippus, in the later Eocene, comes the genus Epihippus, and here there seems to be a small gap. But Epihippus grades without a serious break into Orohippus of the middle Eocene. In Orohippus the difference between molars and premolars is noticeable. Both genera are four-toed, but the fourth toe is smaller in Epihippus. In Orohippus the radius and ulna approach the normal mammalian type.

The last of the series is Eohippus, earlier in the Eocene, and in this form the adaptations which characterise the modern genus are not to be found. In the fore foot all five digits are present; four of them are functional, but the fifth is reduced to a splint. In the hind foot there are three functional toes, the fifth is reduced to a splint, and only the first is missing. The teeth are hardly distinguishable from those of the normal mammalian type, and the premolars are quite distinct from the molars. In the nearly related European Hyracotherium the teeth are still more primitive. Radius and ulna and tibia and fibula are normal, and the animal is adapted, not for speed on grassy plains, but for soft ground and for the eating of succulent herbage.

Anything of the nature of a complete series now breaks down in a tantalising way. Although the animal is now very like the ordinary lower Eocene land mammal, there is some doubt about the exact evolutionary steps joining these early forms. I shall say a word or two about that in my next letter. We can disregard a number of interesting related forms which occur at different points in the series, particularly hippidium and hipparion, as they are universally regarded as side branches. It is as well to add that the earlier members of the series are small. Eohippus varies between the size of a cat and that of a terrier, but this is not of great importance, as the cat and the tiger are classified in the same genus.

I think this evidence speaks for itself. What better evidence can any reasonable man expect? There are one or two small gaps, but not large enough to be of consequence. Probably they will be reduced later, and it is even possible that they have already been reduced, but the papers have not come my way. But, taking the record as it is, can there by any reasonable doubt of the genetic relationship of the whole series and of its side branches? I had better make it clear what I mean by genetic *relationship*. I mean exactly what I say, and do not dogmatise about the exact line of descent, that is which form is ancestral to which. As an example,

I could not possibly guess which of the 13 known species of Eohippus gave rise to Orohippus. For all I know the actual ancestor may not have been discovered, and there is similar uncertainty all down the line. No one claims, so far as actual ancestry is concerned, that the series is more than approximate. This question in its proper setting has its interest, but here it seems to me to be irrelevant. If you assert that the exact line has not been made out I agree; but it is only in very exceptional cases that it is possible to make out anything of the kind, and so far as evidence for evolution is concerned it is not needed.

This somewhat tedious exposition, and the further tedious exposition that follows, is necessary because of your previous letters. I have to deal with the objections raised by you, and so must devote considerable space to what I consider to be minor points. I have now in mind the following passage:

'. . . your last letter ignores my challenge to you to cite a series of fossils linking by small steps an order with another order, *or a family with another family*.' I explained at the time that I did not think this attitude reasonable, and that families and orders were not things existing in nature but merely artificial conveniences for classification. I therefore replied: 'the change from Equus to Eohippus can be traced by small steps, and the change is *considerably greater than* that usually implied by linking a family with another family.'

I do not think there can be any doubt about the small steps. I have now to discuss the degree of the change, a discussion which is to me very trivial, but which is necessary because you and Lunn attach so much importance to it. In some cases, such as the Equidae, the family is isolated, and no nearly allied family is in existence. In this series several side branches, including hipparion and hippidium are extinct, and the nearest living relatives are the tapir family, which is more nearly allied to the rhinoceros. The tapir branched off before the time of Eohippus, and consequently it would naturally be expected to differ from the horse more than does Eohippus. But there has been some degree of parallel evolution, and so it is a doubtful point. You can work out this for yourself if you are interested. But it does illustrate the point that you may occasionally find the nearest extant allied families differ as much as Equus and Eohippus. This is exceptional, and for a sound comparison we must find families that are reasonably nearly allied. Generally speaking the differences between a family and an allied

family is *very much less than* the difference between Equus and Eohippus.

Instead of choosing one for myself, I will take Lunn's example of the carnivora, the principal sub-order of which consists of the following recognised living families: cats, civets, hyenas, dogs, bears, and 'procyonidae' e.g. the racoon. I invite you to take any two of these families which are nearly related, e.g., the cats and the civets, or any *nearly related* families in any branch of land mammals, and compare their differences with the differences I shall now list between Equus and Eohippus. The only differences that can be considered are *skeletal differences* as, of course, in the fossil Eohippus we have not sufficient information about the soft parts. The principal differences between Equus and Eohippus are as follows:

(1) The skull is different, in particular the ring round the orbit of the eye is missing in Eohippus, and the muzzle, though still long, is of smaller proportional length.

(2) The teeth are entirely different. The abnormal length has entirely gone, so has the cement, so have all the special features of the horse teeth. The molars and premolars are quite distinct. The first premolar which is rudimentary in the horse is present in Eohippus. The canines, which form the 'tusks' of the horse and are rudimentary in the mare, are present and normal. In view of the importance of the teeth in classification, this in itself is more than a family difference.

(3) The horse has one functional toe and two small splints. Eohippus has four functional toes on one foot and three on the other, in each case with another toe represented by a splint, the hoofs of the horse are exceedingly well developed, whereas those of Eohippus are rudimentary.

(4) In the horse the radius and ulna are fused, and in the hind limb the functional bone is the tibia, the fibula being only a remnant. In Eohippus radius and ulna and tibia and fibula are normal. The proportionate lengths of cannon bone or metacarpal to (say) the humerus (and similarly in the hind limb) are entirely different. The limb of Eohippus could be twisted like that of an ordinary mammal, in the horse the limb works in a single plane to give strength and speed. These differences alone are more than a family difference.

(5) The back of the horse is hollow (another adaptation to speed), the back of Eohippus is arched like those of modern carnivora. (H.274.)

(6) There are a number of minor differences such as size and proportion of length of legs to body which it would be too tedious to mention.

The real question is not what are the differences, but what are the similarities. Both have a skull, a backbone, four legs and a tail. The rudimentary hoofs and vegetable feeding would I suppose classify Eohippus as an ungulate, but really there does not seem to be much more. *The whole structure of the animal is entirely different.* If you think their similarities are such as to justify their being classed in the same family it is for you to say what they are.

I would therefore ask you to make out a similar list of *skeletal* differences between the cats and the civets, or, if you do not like the example, between any two families of land mammals in the same sub-order which are regarded by comparative anatomists as being nearly allied. *Unless you can do so, my point is proved.* Personally, I think it doubtful whether you could make out a list of the same magnitude and importance for the most widely separated families of carnivora which I have mentioned.

There is nothing new, eccentric, or original, about this statement. I think all competent comparative anatomists would agree with me. Classification is for convenience only, and the 'natural family' is a contradiction in terms. When we have a continuous series it is not considered convenient to break them up into families, as the place where we make the division is purely arbitrary. It is only when people take classification to mean something different from what it does mean that a question of this kind arises.

This question of classification is, however, discussed briefly by Lydekker (H.262 seq.). Professor Osborn did not think it convenient to divide into families, and, somewhat casually I think, suggested four sub-families. Lydekker was more of a systematist, and was not satisfied with this arrangement, so he divided the series into three families, and it is by no means clear why he did not make it four. (H.263.)

I would call your attention to the difference between families in an order varying in different ways about a mean and families in a linear series. The differences in the latter case, *especially in the end members of the group* are very much greater.

I deprecate your method of conducting such a discussion as this, which should be carried out in the spirit of calm scientific thought, by 'challenges' and similar means. It happens, however, that this series is better than would naturally be expected, even for Tertiary

land mammals, and so I can meet your 'challenge.' Indeed, I have given you twice as much as you ask. On the basis of Lydekker's classification, which so far as I know is *the only one which takes account of the degree of the difference*, the family Equinae can be traced back by small steps to the family Anchitheriinae in the Oligocene, and this again by small steps to the family Hyracotheriinae in the early Eocene. Your question is answered and your 'challenge' is met.

Yours sincerely,

H. S. SHELTON.

DEAR SHELTON,

Parturiunt Montes, Nascetur Ridiculus Eohippus.

You meet my contention that no series of fossils has been found linking one family with another family by citing facts suggesting that the one-toed horse may be derived from a multi-toed horse, that is, since the family Equidae originated, it has undergone considerable diversification (call it evolution if you like), but this diversification or evolution is *within* the family. Now quite a number of zoologists who reject the evolution theory admit the possibility that all the members of any given family are derived from a common ancestor, indeed at least one, Vialleton, thinks that this probably happened, although it is as yet far from proved. Personally, I am inclined to doubt this, but am quite prepared to be convinced by evidence on the matter. One of my difficulties in believing that Equus is descended from any of the known Eocene horses is the difference in the teeth, which you have rightly emphasised. You have not described this difference. I cannot see how a low-crowned tooth of which the dentine was covered by a coat of enamel can have been *gradually* converted into a rootless high-crowned tooth of which the enamel became enfolded longitudinally in the dentine and the space between the folds filled with a new substance called cement. If you can explain how this happened I shall be grateful. Thus, even if you had proved that Equus is descended from Eohippus or other similar horse, this really has no bearing on our debate on evolution which postulates not only changes within the family but change of one family into another, one order into another, etc., for which I cannot find an iota of evidence. But I am interested because, as I have already said, I have for years been trying to discover the extent to which animals

can become modified in course of time. You evidently appreciate that the proof of what you call evolution (and I call differentiation) within the family does not help you in our discussion. And you are so perturbed by your inability to produce a series of fossils linking one family with another family that you have made statements which you would not have made in calmer moments. One of these is: 'Generally speaking the differences between a family and an allied family is *very much less than* the difference between Equus and Eohippus.' I agree that there is considerable anatomical difference between Eohippus and Equus, but this is true of the most widely-separated members of almost every family. Despite these differences there is no room for doubt that Eohippus is entitled to a place in the family Equidae. Can you name a single book written within the last thirty years in which Eohippus is not included in the horse family?

Marsh, who was the first to find a fossil of Eohippus and gave the fossil this name which means 'Dawn horse,' writes: 'In the structure of the feet and teeth the Eohippus unmistakably indicates that the direct ancestral line to the modern horse has already separated from the other perissodactyl or odd-toed ungulates.'

The great French comparative anatomist, L. Vialleton, who made a special study of the limbs of backboned animals writes: (*Membres et Ceintures des Vertébrés tétrapodes* (1924), p. 682): 'Eohippus of the lower Eocene and Mesohippus of the Oligocene, despite the feet having several toes, are easily seen by the gracefulness and length of their limbs, so different from that of other Perissodactyls, and by the form of the head and the body, to be representatives of the horse family.'

Another great French zoologist, L. Cuénot, writes of (*L'Adaptation* (1925), p. 59): 'Eohippus of the Lower Eocene of the size of a fox, but already a little horse in its general appearance ('aspect global').'

W. B. Scott, a recognised authority on Mammals, writes (*A History of Land Mammals in the Western Hemisphere* (1937), p. 412): 'Eohippus is unmistakably a horse. . . . It is horselike in all parts of its structure, though the equine likeness cannot always be expressed in a description.'

G. de Beer writes (*Embryos and Ancestors* (1940), p. 65): 'The ratio of the dimensions of a five months foetus (of Equus) corresponds to those of an adult Eohippus.'

D'Arcy Thompson writes in his classic book (*Growth and Form*

(2nd. edn., 1942), p. 1081) : 'The differences (between Eohippus and Equus in respect of the skull) are not greater than those between one human skull and another, at least if we take account of the older and more remote races ; and they are again no greater, but if anything less, than the range of variation, racial and individual, in certain other human bones, for instance, the scapula (shoulder-blade).'

From the foregoing you will see that you have exaggerated the differences between Eohippus and Equus. You have very properly given a list of what you consider the chief of these differences, but instead of giving at the same time the chief differences between two selected allied families in support of your contention that these differences are fewer, you invite me to compile such a list and say that if I *cannot do so your point is proved.* You invite me to do that which is incumbent on you, nay to do more, for you have not compiled a complete list of the Eohippus-Equus differences. For me to do this in the case of the civets and cats would involve a journey from Camberley to Regent's Park, spending there some hours in looking up facts and then using some of the limited space at my disposal in this debate in setting forth these facts. I ought therefore to decline your request, but here, offhand, are some of the ways in which the bones of civets differ from those of cats : (1) the ear bones, (2) elongated head, (3) prolonged muzzle, (4) elongated body, (5) short legs, (6) toes usually 5–5 as opposed to the invariable 5–4 of cats, (7) some civets but no cats plantigrade, (8) molar teeth more numerous, (9) number of lumbar vertebrae variable in civets, constant in cats, (10) claws less retractile. Here then are 10 differences, as opposed to 5 (one of which is invalid) listed by you. To justify your assertion that cats and civets are *much less different* than Eohippus and Equus you have to add to your list at least 10 differences.

I will now make a few comments on your list.

(1) Difference in skull. This is disposed of by the words of D'Arcy Thompson, and the fact that the orbit is open behind in some genera of the civet family and closed in others, and this is true of some species of the same genus.

(2) In most families the teeth differ much in the genera of a family, thus in that of the civets, the cheek teeth of the bear-cat are small and rounded, of the Indian civet small, compressed and sharp, while in the palm civet the teeth vary in form and are small in some genera and large in others. As regards numbers some civets

have two upper molars and others one, and this applies to some species of the same genus. I note that you omit to state that in the small Eocene horse, Paloplotherium, the first premolar is usually absent so that this has 'evolved' more than that of the horse of to-day!

(3) The feet are certainly very different, but it is doubtful if Eohippus has splint bones, and the hoof of the stoutest toe, No. III, relatively to the size of the toe, is as well developed as the hoof of Equus.

(4) The degree of fusion of the tibia and fibula is very variable. In some of the beaver family the two bones are quite separate, in others they are fused at the lower end.

(5) The back of Eohippus. Scott considers that it was arched as you describe but Lull gives it no arch at all. I am not prepared to say which view is the right one; but on account of this difference of opinion between experts this feature affords rather slender evidence of change.

As regards the fossils intermediate between Eohippus and Equus, you admit (1) an exact lineage has not been made out, (2) there are gaps in the series, 'but not large enough to be of consequence,' but you do not mention that two are gaps between sub-families! (3) Although you believe Orohippus to be descended from Eohippus 'you could not guess which of the thirteen known species of Eohippus gave rise to Orohippus' which is rather surprising, if the latter be derived from Eohippus.

I gather that your case is this: We know fossils of some 250 species of horses, most of which differ only very little from one or more other species, so that it is reasonable to believe that all the horses are derived from a common ancestor. I agree, as do most of those who reject the evolution theory, that this may have happened. Some, like Vialleton and Le Fèvre, believe this. I do not go so far, because breeders have not been able to produce anything like the changes postulated by this theory, and therefore I deem it essential to await further evidence before forming a definite opinion. I believe that almost every horse of which fossils have been found in America and Europe was an immigrant to the locality in which its earliest fossils occur. The fossils show that the horse family is composed of two well-marked types of horses: (1) those that lived in forests where the ground was soft and often marshy: these horses fed on soft leaves; and in consequence their teeth and toes were adapted to this kind of life. They make up all the three sub-families other than the Equinae. All the early fossils known belong to horses

of this type. (2) Those that lived on grassy plains where the ground is hard ; these fed on grasses impregnated with silica which rapidly wears away teeth. The feet and teeth of these horses were adapted to this kind of habitat. The earliest known fossil of this type appears in America in a middle Miocene rock. It becomes abundant in the Miocene and persists until to-day, whereas the other type became extinct in the early part of the Pliocene period. Are these horses with high-crowned teeth descendants of those having low-crowned teeth ? The known fossils do not enable us to answer this question with certainty, because there is evidence that throughout the Tertiary period there were waves of immigrant horses. The earliest known horses appear as fossils in the Lower Eocene period simultaneously, geologically speaking, in North America and Europe. As no fossils have been found in either continent which can have been the direct ancestors of these animals, it is generally believed that they were immigrant or invading animals. That the middle Eocene horses which replaced them in both America and Europe were also immigrants is proved definitely in the case of one European horse, Palaeotherium, which cannnot possibly be descended from any known earlier horse, because of its great size in comparison with these (one species had the dimensions of a pig, and another those of a rhinoceros), moreover it had only three toes on the fore-feet, as opposed to the four of all other known Eocene horses. It is also almost certain that the first three-toed horse, of which a fossil has been found in America, Mesohippus, and the first horse having high-crowned teeth, Merychippus, are also immigrants, and the probability is that this is true of most of the other newcomers. The fossils tell us that during the Tertiary period the climate in Europe and North America grew progressively cooler, for they tell us that palm trees and crocodiles lived in England in the Eocene period. They also indicate that there was no grass growing in the United States or Europe before the Miocene period. The arrival of the horses having high-crowned teeth coincides with the advent of the grasses.

There are two theories as to the origin of these grass-eating horses that appeared in America, one is that they are immigrants, the other is that they are derived from browsing animals, in consequence of the change of diet resulting from the coming of the grasses, which gradually supplanted the forests ; thus Scott writes (*op. cit.*, p. 200) : 'Because of the minute particles of silica which they contain the grasses are very abrasive and rapidly wear down

the teeth of grazing animals. In adaptation to this new source of abundant and nutritious food supplies many different animals developed a form of tooth which was fitted to compensate by growth for the loss through abrasion.'

Lest it be thought that I am overdoing the amount of migration, let me say that the whole history of plants and animals shows that all along there have been extensive migrations. 'So great,' writes Scott (*op. cit.*, p. 263) 'is the importance of migrations . . . that the history of mammals is quite unintelligible unless such wanderings be taken into account . . . mammalian migrations sometimes involved whole faunas . . . the close similarity of the Lower Eocene animals of Europe and North America is due to migrations in both directions, presumably from Central or Northern Asia.'

If, as I believe, the horses having high-crowned teeth were immigrants to America, this does not settle the question as to their origin. In my view this cannot be done until we know much more of the fossil record in the Far North and Asia.

I do not agree with you that families and orders are 'not things existing in nature but merely artificial conveniences for classification.' They are very definite things, about which there is agreement among biologists in contrast to the disagreement about genera and species. In the case of families the only differences of opinion are in the case of some very big families composed of a number of subfamilies, which some people think should be given the status of families. Every family has very distinctive marks as I have shown in the case of the Equidae. It is only when a fossil consists of a tooth or a bone, or a few bones that there is doubt as to which family it belongs.

As I have already said, the last part of this letter is really outside the scope of our debate, because the dispute is not about modification within the limits of the family. What the evolutionist has to prove is the conversion of a member of one family into a member of another family, and the evolution of new Orders, Classes and Phyla.

You say that the horses are the best example of evolution you can produce. If this be so, you have proved nothing not admitted by many of those who reject the evolution theory. There cannot be less than 2,000 families of animals living and extinct, each one of which, according to you, evolved gradually from some pre-existing family—there are over 230 families of molluscs. Yet apparently *you are unable to adduce any fossil evidence that any one of*

these evolved from any other. Much less the evolution of any Order or Class. I invite you to offer such evidence. I also invite you to name the fossil series which offer any evidence of evolution beyond the limits of the family. My point is that very few of such series have been put forth by transformists and that none of these can bear close scrutiny. If the evolution theory be true hundreds of such series must have existed.

Your bias against what you describe as 'conducting such a discussion by challenges' is understandable, but the whole point of this book is to challenge a dogma which is accepted by the public only because it is unchallenged. I submit that your theory that there is something alien to the spirit of scientific enquiry in the rigid examination and cross-examination of every statement in support of an unproved theory is the very reverse of the truth.

Yours sincerely,

D. DEWAR.

DEAR DEWAR,

Before expounding a little of the available palaeontological evidence for evolution I must give some space to your letter. In the first place I will ask you to use terms in their correct sense. I only know of one meaning for the term horse, and that is Equus caballus, the horse as usually understood. This includes a pony, but not a zebra (Equus greyvi), or an onager (Equus onager) or any of the various types of asses (Equus asinus). There is no convention, as in the case of the cats, allowing a wider use of the term. There is a legend that Julius Caesar rode an abnormal horse with three toes. That is correctly described as a multi-toed horse, *and is the only sense in which the term should be used.* Your statement, therefore, that we are considering the descent of a single-toed horse from a multi-toed horse is a gross mis-statement. So far as is known, no horses existed behind the Pleistocene, as Equus stenonis is regarded as an entirely different species. Even if you regard that as a doubtful point, there is certainly nothing behind it which can with the slightest pretence to accuracy be described as a horse. As this is a scientific discussion, it is desirable to use terms in their usual and correctly defined sense.

As I explained in my last letter, Eohippus is an entirely different animal, probably as different as is the tapir. There is no doubt about the difference. Everyone knows this, and the only point in

dispute is the degree of the difference. In trying to elucidate this, I suggested during the original correspondence that, instead of digging up quotations, you should go to the Natural History Museum and examine the specimens for yourself. You have evidently not adopted my suggestion. As I have acted on my own suggestion, your quotations do not interest me. Had you told me that Vialleton had noted subtle anatomical similarities that had escaped my attention, and said in detail what they were, I would have listened carefully. But when you merely tell me that Vialleton talks of the gracefulness of a fossil skeleton, I can only reply that he is using his imagination in a very unscientific way. And when he talks of the length of its limbs, he is talking contrary to the facts. Compared with the horse, the legs of Eohippus are shorter in proportion to the length of the body. Moreover, Eohippus was not built for speed, but was more adapted for walking on marshy ground. (H. 270, 274.)

What Prof. W. B. Scott, who has no more doubt of the occurrence of evolution than I have, and indeed who has written a book to say so, is doing in this galley I do not know. I am very suspicious of quotations by anti-evolutionists, which sometimes give a false impression by taking passages out of their context. Anyhow, as you will insist on giving quotations, which would be quite unnecessary if you would use your own eyes, the following may interest you :

'We have traced the horses back to little animals *whose structure is but remotely equine* and which are *in all respects* so unlike modern species *that hardly any palaeontologist would be bold enough to connect them.*

This is definite enough, and comes from W. B. Scott's *Theory of Evolution*, p. 107. The italics are mine. This quotation is certainly relevant to the problem we are now discussing.

Now let us come a little closer to the degree of the difference. In the same way as you put forward incorrect arguments because of your inaccurate use of terms, so in assessing differences you mistake names for things. You say you have found ten differences between cats and civets to balance my five between Equus and Eohippus, and seem to think that because ten is greater than five, therefore you have proved something. Unfortunately, however, none of your differences are of much importance. Let us take them one by one (and sometimes two by two). (1) I know nothing about the ear bones of Eohippus, and so will pass that for the time being, though I should be surprised if there were no difference. (2, 3) Civets compared with cats have elongated heads and prolonged muzzles. So has Equus compared with Eohippus. (4, 5)

Civets compared with cats have longer bodies and shorter legs. So has Eohippus compared with Equus. (6) Civets toes 5 and 5, cats toes 5 and 4. Compare this with horse 1 and 1 (in each case with two splints) and Eohippus 4 and 3 (in each case with one splint). (7) *Some* civets is not a family difference and anyhow Eohippus is plantigrade. (8) Molar teeth more numerous (how many?), Eohippus had 8 more functional teeth than has Equus. (9) Lumbar vertebrae variable. I do not know the number of vertebrae in Equus and Eohippus, but this is a very variable quantity, and I should be very much surprised if they were the same either in total or in detail. I will, however, point out a much more important difference in the shape of the vertebrae, which I did not previously mention, particularly the long dorsal protuberances. (10) The difference in the claws is balanced by the differences in the hoofs. Thus all but one of your very trivial list of differences are balanced by the *minor* differences between Equus and Eohippus. The really important differences, the whole structural change shown particularly in the extreme difference in the character of the teeth, and in the structure of the limbs, such as the radius and ulna, and the method of jointing, indeed anything of real anatomical importance, has no parallel whatever. The difference between cats and civets is a normal family difference. The difference between Equus and Eohippus is of the kind that exists between different orders, or at least sub-orders.

You really should try to grasp this point and meet it. It is your challenge. So far as the case for evolution is concerned, it does not matter in the least. I do not need this point and am not particularly interested. What does matter is that you have raised the point and should face the issue clearly. If you think I have exaggerated the difference between Equus and Eohippus you should say in what particulars.

In the same way, in speaking of families, you mistake names for things. A good example of this is your making the point that two of the gaps in the Equus-Eohippus series are between two sub-families. Does it matter where they are? The important point is the size of the gaps not their position. But it does not seem to have occurred to you that the reason for the sub-families is the gaps, and though these have been partially filled up since Osborn made his suggestion, they have not been filled up entirely. Osborn was certainly under no delusion that his sub-families were more than conveniences. There is no such thing as a family. It is

merely a conventional classification which works fairly well for living species, but breaks down entirely when we have a nearly continuous series like Equus-Eohippus. In that case you can put the family divisions where you like, and the common sense of modern palaeontologists cuts the knot by not bothering with them at all. I cannot argue this point now, as it belongs to the chapter on morphology, which, according to my scheme, would have come first. I can therefore only assert, and reserve discussion till the proper time. The point to be understood at present is that when I am discussing the degree of the change, it is merely evading the issue to talk about families.

I will bring this tedious discussion to an end with a suitable example of my meaning. Imagine you have in front of you the skeleton of a horse, the skeleton of a hare, and the fossil hyracotherium. Which modern form is the more similar to the fossil? I don't know. *With our knowledge after the event* we can use our imagination and find numerous equine similarities. But the first discoverers had not this knowledge. Hyracotherium was first examined by the foremost anatomist of the day, Professor Owen, who labelled it Hyracotherium lepinorum, implying that it reminded him of the hare. Of course he did not put it in the same family. *He had not the least idea it had anything to do with the horse.* Nor, so far as I am aware, had anyone else until the intermediate fossils began to be discovered. This seems to me conclusive. It is incredible that Owen should have been so ignorant or unobservant as to fail to notice the similarity if the forms were as similar as are even the most widely separated of living animals which are classed in the same family.

I will now pass on from this minor point and try to give some slight idea of the overwhelming evidence for evolution to be found by a study of the fossils, in spite of the fragmentary record. We will first note another lucky find in the Cretaceous. In the case of the Equidae the exact line of descent is difficult to determine because of migrations. There are no such complications in the sea-urchin series worked out by Rowe and others. Here the transition from one type to another is continuous, so continuous indeed that even the species classification breaks down. You cannot say where one species ends and another begins. You have here the 'innumerable transitional forms' you asked for in another connection. The authorities state that a difference of six species is a conservative estimate of the amount of the change, and please note also that

these species are in a linear series, which implies a very considerable change.

Now let us return to the land mammals. There are other series, not so good as the Equidae, but still of some importance, particularly the camels, the tapirs and the rhinoceroses. Prof. W. B. Scott, in his work on evolution, mentions numerous links connecting the camels and llamas with a four-toed creature in the Oligocene. Other series and links can be made out by studying the collection in the Natural History Museum with the aid of the official guide to fossil mammals (which I shall refer to as N.). It is particularly illuminating in the description of the series culminating in the elephants.* In these series, the exact line of descent is more doubtful than it is in the Equidae. We are much in the same position as we were when Hipparion was regarded as a possible ancestor of the horse. We now find it is a side branch descended from a common ancestor of the Merychippus type. All the same, Hipparion is nearly as good evolutionary evidence as if it were the direct ancestor. The same applies to these fossils related to the elephants and rhinoceroses. It does not much matter whether the fossil is directly ancestral or a side branch, the evolutionary evidence is plain.

Where the fossils do not show a sufficient number of stages to be called a series there are *always* links. I do not think an exception is to be found in any group of European or North American land mammals. The *Guide to the Fossil Mammals* (1934) supplied by the Natural History Museum mentions amongst its numerous examples the following :

(1) Ictitherium, from the lower Pliocene, 'an intermediate genus connecting the Hyaenidae with the Viverridae.' (N.24.)

(2) Cynodictis, from the Oligocene and upper Eocene, connects the Canidae and the Viverridae. (N. 25.)

(3) Cephalogale, known as bear dogs. (N.25.)

(4) The fore-runners of the ruminants showing 'the *gradual* acquisition of the typical selenodont molar teeth,' 'the beginning of the gap between the front teeth and the back teeth,' and the '*gradual* fusion of the basis of the two supporting toes.' (N.41, italics mine.)

This is sufficient. It would be tedious to multiply examples. I may give a number of others in an appendix to a later letter.

Let us now turn to the fundamental question of the interpretation of these facts. I will ask you once again to note that I am careful not to overstate, and particularly, with the example of

Hipparion in mind, to dogmatise which intermediate genus is ancestral and which is a side branch. It really does not matter from the point of view of this discussion. *Unless evolution has occurred, there is no sense in the existence of these intermediate genera.* That one might occur occasionally is possible as a matter of chance; but that they occur regularly is a definite proof of evolution.

I would here call to mind the admission I drew from you in the first chapter concerning the distinction between special creation as an explanation metaphysically possible and special creation as a *reasonable explanation.* I shall probably have occasion to bring this up against you more than once, and this is the first instance. I propose to apply this criterion to *all the existing placental land mammals.* You have in the Equidae a series which by a succession of small steps connects two animals so different that they can reasonably be described as belonging to different sub-orders (here again I am careful not to overstate). You have several series of lesser importance. Nearly always where series are missing you have connecting links. Evolution explains all this in a simple and reasonable manner. With the possible exception of man, which will be discussed in detail later, in no case is any gap anything like as great as that shown to have been bridged by small steps in the Equidae, and in the great majority of cases the gaps are very much smaller. Any alternative explanation implying special creation in any form is therefore within these limits *definitely unreasonable.*

Indeed, I do not think we need trouble any further with these placental land mammals. All those found in the lower Eocene are very much alike. In some cases it has been difficult to decide to which type of modern mammal the specimen is most nearly allied. The record becomes more scanty as we pass from the lower Eocene to the basal Eocene, but the similarities are so striking that I think we can assume that the latter are descended from the former. After (or before) this the record is fragmentary, but still secondary mammals are known and different specimens show definite similarities both to the placentals and to the marsupials. I will wait for your reply before deciding whether this obvious point requires further elaboration.

I will conclude with a remark about Vialleton's idea that evolution occurs only within the family. As I have already shown this breaks down with the Equidae, unless you are going to extend the term family to include differences much greater than those commonly understood. It also breaks down in another way, and

this is a point which seems to me to deserve your attention. The proper way to divide into families (if you really must do it at all), is to take the known animals at any period and classify them *without relation to those that follow*. If you will do this with the lower Eocene land mammals, you will find that a number of those which are related to different families of existing mammals would properly themselves be classed in a single family.

Yours sincerely,

H. S. SHELTON.

DEAR SHELTON,

You have more than once suggested that quotations which embarrass you would look very different in their context. Let me remind you that you made a similar assertion about that damning quotation from Lemoine. Lunn then showed you the fourth volume of the French encyclopaedia in which Lemoine says that evolution is 'impossible,' and you discovered for yourself that the context reinforced the quotation.

As to the meaning of the term 'horse' it may interest you that among the meanings given in the *Shorter Oxford English Dictionary* is 'In Zoology sometimes extended to all species of the genus Equus or of the family Equidae.' In fact, apart from you, I know of no zoologist who does not use the term horse to include every genus of the family. The only reason why you do not follow suit is your desperate anxiety to find a series of fossils linking one family with another.

I am well aware that 'family' is to some extent an arbitrary classification, but the different views of present-day biologists are merely as to whether a big well-defined group of animals should be deemed one big family divided into a number of sub-families, or each sub-family be given the status of a family, as in the case of flower-peckers among birds.

You ask me to use words in their correct sense. What you really mean is you want me to use a terminology which you have invented to meet the exigences of your theory.

As you ran away from the challenge about the causes of evolution so have you failed entirely to suggest any plausible hypothesis for transforming a horse having low-crowned teeth into one having high-crowned teeth ; all you are entitled to assert is that a plausible case has been made out for the descent of Equus from Eohippus.

If your assertion that the differences between Eohippus and Equus are much greater than those between the cats and the civets be correct, all you have to do to substantiate it is to set forth in deadly parallel columns the skeletal differences in each case, placing in one column the 30 odd differences between cats and civets, and in the other the differences between Eohippus and Equus. You have not done this, because you know that such lists would show up the utter futility of your contention. I have said that there is a plausible case for the descent of Equus from Eohippus because it is possible to select fossils and arrange them in series suggesting such descent. But I am amused at your unscientific contention that because, although Equus is unlike Eohippus and the fossils suggest that the former is descended from the latter, we are entitled to assume that Eohippus and the tapirs are descended from a common ancestor *in spite of the complete absence of fossils suggesting this*, or linking Eohippus and the tapirs. Your argument may be summarised as follows. A and B had a common ancestor because A is connected with B by a series of fossils. A is not more unlike C than A is unlike B Therefore A and C are descended from a common ancestor!

I had good reason to ask you, as I did, to name similar series to the horse series. The evolutionary tactics consist in producing the horse and vaguely suggesting that there are dozens of other series. There are NOT. The geological record is barren of such series. There are perhaps half-a-dozen of rather lame series (instead of the thousands there should be).

Your tactics are familiar. You are attempting with the potent aid of fashion as an ally to *sneer* creation out of court!

As regards the series (save the mark!) you produce linking one family to another, this time the labouring mountains bring forth merely hot air! You lead off by citing (characteristically without giving it a name), a sea-urchin, where 'the transition from one type to another is continuous, so continuous indeed that even the species classification breaks down.' You doubtless refer to Micraster (with which I will deal later). So far from the changes this displays being from one type to another, the whole gamut is *within the limits of the genus*!!

Then you talk of 'other series not so good as the Equidae,' viz., the camels, the tapirs and the rhinoceroses. But these are in each case a series *within a family*, the families being the Camelidae, the Tapiridae and the Rhinocerotidae. In no case do you mention a single intermediate genus. Then you talk vaguely of 'the series

culminating in the elephants.' Here again not a single member of the series is given ! Thus all your talk of the series you can produce linking one family with another, ends in your saying that series (unnamed) occur *inside* four families ! Finally, you name four genera, each of which you suggest is a link between two families. But a single genus can hardly be described as a series. In any case, so far from being intermediate, each of the three genera you cite *falls definitely within a family*, the dog family in the case of Cynodictis, and the Viverridae in the case of the other two. Finally, you talk of changes in the teeth and toes of 'forerunners of the ruminants.'

Having failed to adduce *any* evidence of changes in animals greater than those which some scientists who reject evolution believe to have taken place, you fall back on special creation. You imply that the problem of creation is a religious one and should be left to the theologians. The truth is that the question whether the various families appeared suddenly or whether it is possible to trace any of them by a true lineage series into another family is a strictly scientific one. It is you whose approach to this problem is quasi-religious rather than scientific. For what is the essence of the scientific method ? Surely research unhampered by *a priori* prejudice and a habit of weighing the evidence. You begin your research with a purely religious faith in evolution. You have already stated (see p. 31) that you do not know the *vera causa* of evolutionary change and that you are not setting out to prove anything (see p. 55). You snatch like a drowning man at any straw of evidence to support your faith and you ignore all that tells against it.

A. The scientist scrutinises the geological record, appraises it, and finds that, even in the case of the Phylum regarding which it is most complete, it has not yielded a single instance of a true lineage linking one family with another family.

B. The scientist is well aware of the fact that a type with certain characteristics of other types is not necessarily a blood relation of the latter. Therefore, transitional forms unconnected with the forms between which they are supposed to be intermediate cannot be adduced as evidence for evolution. Lunn's analogy of the fighter-bomber (p. 18) is very much to the point.

C. The scientist knows that the geological record tells against evolution. He realises that while the horse series of fossils suggests that changes within the family may be considerable, it also shows (if it be a genetic series) that a period of about one-tenth of the

length of that separating the date of the earliest Cambrian fossils from to-day has not been sufficiently long for the evolutionary process to convert one family into another family! From this it follows that the whole period covered by the fossil record is not long enough to permit one Order to evolve into another Order. This is doubtless one of the reasons that led the great French geologist Lemoine to declare that the theory of evolution is impossible. I shall revert to this matter later.

Yours sincerely,

D. DEWAR.

DEAR DEWAR,

I find it a little trying to be obliged to spend space on trivial side-issues. First let me tell you that none of your quotations embarrass me; but when they come from people who I know take the normal view about evolution, naturally I am a little suspicious. I have no such suspicion about Lemoine, although I think you exaggerate the importance of his remarks. He really did say that evolution was impossible, but I should hardly be inclined to call that damning. I take a more lenient view, and think it probable that, although he has curious ideas about evolution, and is sometimes abusive about it, he may still be a useful specialist in his particular line of investigation, whatever that may be.

You do seem to have scored a minor point about the meaning of the term horse. The *Oxford Dictionary* does say that the term is sometimes used in zoology in an extended sense. That is what I am complaining about. It seems a little foolish in zoology, when accurate terms are available, to use inaccurate terms, although I must admit I sometimes do it myself. The trouble in this case is that it has not come into general use. While most people would understand the phrase 'great striped cat of the jungle' as applied to a tiger, they would be a little puzzled by 'the striped horse of the forest' and would need to think a little before understanding that a zebra was meant. Nor would they refer to the coster's moke as the 'braying horse of the backyard.' I must, however, ask permission to modify my previous assertion. Instead of saying that it is a 'gross mis-statement' to talk of the descent of a single-toed horse from a multi-toed horse, I must substitute 'grossly misleading statement,' and trust that in acknowledging my error I am setting a good example.

I am getting a little tired of your 'challenges.' With regard to the causes of evolution, I have given you quite enough to go on with and have said it twice (see my letters p. 31 and p. 55). As my duty in this discussion is to maintain the generally accepted view that evolution has occurred, it would be out of place to spend much space on difficult and doubtful questions concerning causes, on which the scientific world is not agreed. If I have any space left at the end of the discussion I may say a little more then. In the same way I am not in the least concerned to put forward 'a plausible hypothesis for transforming a horse (?) having low-crowned teeth into one having high-crowned teeth.' While remarking in passing that you will insist in calling an animal a horse which is only one in a Pickwickian sense, I need only say that the stages in the transformation have been observed in the fossils, and that the change has been proved to occur. That is sufficient ; that is what we are discussing ; and I really cannot be troubled to elaborate theories of how and why.

Now let us try to get these horses, real and Pickwickian, out of the way. There are two minor points I should like to mention. One is the hollow and arched back. I cannot insist on the arched back of Eohippus in view of the conflict of authority. But there is no doubt whatever about the disappearance of the hollow back. If you will look at the skeleton of the horse, or even at a good picture (there is a good one in Lydekker H.16), you will see that the dorsal protuberances are long in the anterior region and reduce in size lower down the back. This is one skeletal sign of the hollow back, and is not found in Eohippus. The other minor point is the number of the vertebrae. I should be exceedingly surprised if this were the same in Equus and Eohippus, but do not know where to find the details for Eohippus, so this point must remain unsettled.

With regard to the general question as to whether the difference between Equus and Eohippus can reasonably be described as equivalent to a change 'within the family,' I have clearly shown that it cannot by the example of the cats and the civets. I have shown that all but one of the skeletal differences you mentioned for cats and civets are met by the *minor* differences between Equus and Eohippus, and that the major differences have no parallel. That is sufficient. My point is proved, and I have nothing to add.

A few further remarks are needed about Tertiary land mammals, though I have very little to add about that either. You say that besides the Equidae there are 'half a dozen of rather lame series.'

I mentioned only the camels, the elephants, the tapirs and the rhinoceroses. As I carefully remarked that they were not as good as the Equidae, you can translate this as 'rather lame' if you like. I did not suggest that there were numerous others. Where I suggested the existence of others was in the links, not in the series.

You seem to have some objections to raise to my treatment of this matter, but as so much of what you say takes the form of attributing to me arguments that I did not use, I find difficulty in making sense of them. For example, the reasons you attribute to me for assuming the common descent of horses and tapirs are certainly not mine. Your remarks about 'sneer' and 'labouring mountains and hot air' and 'tactics are familiar' can be dismissed at once as abusing plaintiff's attorney. I don't worry about the abuse, but it makes it a little difficult to discover whether there are any solid arguments mixed up with it. The principal objection seems to be that the genera I mentioned as intermediate are not so, but each 'falls definitely within a family.' To take the example of Cynodictis, the Museum guide I quoted says, 'Cynodictis and allied genera . . . connect the Canidae and the Viverridae.' Is this true or not? If it is, you may place some genera in the Canidae and others in the Viverridae, and it does not much matter where you draw the line. In that case there is no sense in the objection. If they don't connect the families, it is up to you to explain how the Museum authorities went wrong. This is a specialist question on which I am bound to accept authority, and I prefer theirs to yours. That is all the space I can spare for your last letter. If I have missed any solid arguments, I suggest you re-state them simply without rhetoric and without the abuse that makes them difficult to discover.

I will conclude this consideration of Tertiary land mammals by a repetition of the lesson to be drawn from them. We have one very good series—the Equidae—and several other series which you describe as rather lame. We have these other extinct genera, which you object to be called intermediate, but which are described by the best authority in the country as connecting existing families. If this is true, evolution within the family breaks down entirely. So I infer that for all this evidence, of which there is more than I have given, there is only one *reasonable explanation.* Obviously that explanation is evolution. This is in spite of a record (our record) which, though better than usual, is still very incomplete. The evidence for evolution in this important case is all, and indeed

more, than we should naturally expect. I can well leave the matter there.

We will now consider briefly a few earlier fossils. Obviously, as I carefully explained in the last chapter, the record (our record) becomes more fragmentary as we come to earlier times. It would therefore not be reasonable to expect series. There are a few which can be called by that name. I am informed, for example, that both the ammonites and graptolites are rather good, but cannot assert this from personal study. The point is that anything that can be called a series is exceptional, and is only to be expected in very slowly changing organisms in which a lapse of time of a few million years makes very little difference. I shall not now attempt to give a list either of the series or of the links, because some of them relate to forms which I think you will mention in what you term your difficulties. I will conclude with two more examples.

The first is the mammal-like reptiles, or reptile-like mammals (it is not always possible to say which), found in Secondary strata. Two examples worthy of mention are Scymnognathus from the upper Permian and Cynognathus from the lower Triassic (S.357). Both of these show definitely reptilian features. Both show a differentiation of teeth similar to that of mammals. Both show transitional stages between the mammalian and the reptilian forms of jaw and earbones. Our knowledge of these transitional forms of life has been considerably increased in recent years by the discoveries of Broom in Africa. It is not claimed here that series are found ; but as you have given reasons for thinking that in the case of the early mammals the record is very incomplete you are not entitled to ask for series. I am not sure that your reasons are good ones, but we agree about the fact. What *reasonable explanation* is there to account for the existence of intermediate forms between mammals and reptiles ?

The second example is the well-known Archaeopteryx which I mentioned in the last chapter. You objected to my calling it a feathered reptile. I do not insist on the description ; call it a bird if you like. The important point is that it has features belonging to both classes. As it has feathers, and the wings are attached to the body in a general way like those of birds (though there are important differences of detail), perhaps a bird is the best description. Now let us look at the reptilian features. In the first place, it has well developed teeth of the reptilian type, 13 in each jaw. Incidentally, birds with teeth are found also in the upper Cretaceous,

but not later. I would like to suggest that this throws some doubt on whether the bird digestive structures, crop, gizzard, etc., which are so characteristic, and indeed so variable, had been developed. In the second place, while the modern bird has 'pneumatic' bones which combine strength and lightness in a manner so advantageous to a flying creature, nothing of the kind has been found in Archaeopteryx. In the third place, the animal has a long reptilian tail. Of the fifty vertebrae, twenty are caudal, whereas the caudal vertebrae of the modern bird are about six, and these are specially adapted to support its peculiar fan tail. A further fact implying evolution is that there is a similarity between these tail vertebrae of Archaeopteryx and those of the modern bird embryo. A fourth important reptilian feature is the jointing of the bones of the fore limb. In the modern bird the joints of the wing bones are welded to give rigidity. This feature is not found in Archaeopteryx. The claws also can be described as reptilian, and these are not found in modern birds with the exception of the young of one South American species. There are, in addition, a few other minor features which are found in reptiles but not in modern birds.

What possible *reasonable explanation* other than evolution can there be of these extraordinary facts? You have in the last chapter agreed with me that for millions of years after Archaeopteryx feathered creatures existed of which no fossil remains have been found. You cannot therefore raise the difficulty that the earlier stages of evolution are missing. If feathered creatures of which no fossils have been found existed for millions of years after Archaeopteryx, it is not unreasonable to assume that they existed for millions of years before. If we tried to imagine a fossil proof of evolution of birds from reptiles, we could hardly suggest a better one than has been discovered. Anyone who suggests that this form was specially created is open to the Editor's retort that what was created was a tremendous lie. These retorts do indeed come home to roost.

I do not propose now to cite further examples. What is wanted is not a multiplicity of examples, but correct reasoning on those that are well known. As I explained in the last chapter, both logically and historically geology comes last not first, and all that is asked from geology is confirmation. Since the early days of the acceptance of evolution by the scientific world, when the evidence from geology was so scanty as hardly to be said to exist, this confirmation has been found in generous measure. In spite of a

fragmentary record, and of the very small number of workers in this field, further confirmation is continually being added. Certainly there are gaps, and serious gaps ; no one denies it. *But the record which we now have gives us all the confirmation we need.*

I am going to break my rule about quoting authorities on matters of opinion, because of your use of the name of Professor W. B. Scott. Professor Scott, speaking on the evolutionary significance of the geological record, makes the following remark : 'Even as matters stand, the outstanding fact is that so much has been preserved rather than that the story is so incomplete.'[1]

This agrees so well with what I said in the original correspondence about the incompleteness of the palaeontological record, and the marvel that in spite of that we have as much evidence as we have, that it is unnecessary to add anything further.

That is the substance of my case that the geological record gives ample confirmation of the conclusion drawn from other branches of science, that the forms of life existing on this earth have come into existence by evolution. I may add further details later, but what I have given is sufficient. I believe you have some difficulties to bring forward. Perhaps it would be well for us to take those next.

Yours sincerely,

H. S. SHELTON.

DEAR SHELTON,

It becomes more and more apparent that you can only sustain the case for evolution by demanding preferential treatment from the jury. In your first letter you insisted that evidence which is equally valid for evolution and special creation can reasonably be cited as evidence for evolution and against special creation. In your last letter but one you claimed the right to make me produce evidence in support of *your* theory, viz., evidence that civets differ less from cats than Eohippus does from Equus. In your last letter you (i) claim that you have the right to challenge me (i.e., about the civets) and yet complain when I challenge you. Undeterred by your grumbles, I shall continue to challenge you when you make unsustainable assertions. You (ii) claim that anatomical differences that support your views are major ones, while those that support my views are minor ones. You claim (iii) that the jury must

[1] *Theory of Evolution*, p. 86.

(a) ignore over 20 differences between cats and civets, because I offhand mentioned only 10, and (b) hold that these differences do not exist. In short, you claim a novel method of proving a point without adducing evidence in favour of it!

I am glad that you have noted the distinction between an explanation and a reasonable explanation, for my case is that evolution is not and special creation is a reasonable explanation of the phenomena presented by the animal kingdom.

May I put to you in a slightly different form questions already put to you by Lunn?

(1) *Why is it less reasonable to explain the origin of the living world by an act calling into existence a great variety of living organisms than to explain it by the creation of a micro-organism endowed with the power of evolving in the course of millions of years into a great variety of living organisms?*

(2) *Is there anything unscientific or unphilosophic in the theory that the special types of organisms owe their origin to special creation?* T. H. Huxley, at any rate, did not think so (see p. 16).

(3) *If, as you concede, there is nothing intrinsically improbable in the hypothesis of special creation, what is your reason for supposing that the Creator must start de novo with each new creation? Why should God not use certain characters in different combinations?*

Why, in other words, do you assume that Archaeopteryx must be descended from a reptile, because it has certain characters which you deem to be reptilian? Plainly the real difference between us is that my approach to this problem is inductive and yours is deductive. You know (by some private revelation) that the creative acts of God, in whom you believe, came to an end immediately life appeared on this planet, and from this first unproven premise you proceed to deduce your evolutionary philosophy. I go to nature and by an inductive process come to the conclusion that slow, gradual evolution is disproved by the geological record. Whereas you always appeal to the missing links, to the evidence which is not there, my beliefs and my theories about creation are inductions from facts.

You resent my asking you to suggest a plausible cause of evolution. But one of my many reasons for rejecting evolution is that nobody has ever suggested a plausible hypothesis to account for evolution in terms of natural agencies. And it is only when all natural explanations fail that the supernatural hypothesis may be invoked.

I am glad to establish the fact that you cannot and do not pretend to explain the process by means of which things evolved, and that you admit that there are not many series of fossils which are of any use to the evolutionist.

You ask : is the assertion of the British Museum authorities, 'Cynodictis and allied genera . . . connect the Canidae and the Viverridae' true or not ? The answer is : 'Almost certainly not.' Cynodictis and Viverra make their first appearance simultaneously in the same Upper Eocene deposit of Europe. The two genera differ considerably, as may be seen by looking at the pictures of their jaws on pp. 63 and 73 of Zittel's *Palaeontology*, Vol. III (1925). Their teeth differ in form, Cynodictis has three molars and Viverra two. The shape of the jaw differs. If one of these animals gave rise to the other, it must have been by a great mutation. The transformist has to believe that every family is derived from a different family, and, as the Cynodictinae is the sub-family of the Canidae most like the Viverridae, it is supposed to be more closely related to the latter than are any of the other sub-families Therefore, adopting the loose phraseology employed by transformists, the B.M. authorities assert that Cynodictis and other allied genera, i.e., the sub-family Cynodictinae 'connect the Canidae and the Viverridae.' This is a question-begging statement, unworthy of a scientific treatise.

To speak as you do of Scymnognathus and Cynognathus being intermediate between mammals and reptiles and Archaeopteryx as intermediate between birds and reptiles is equally incorrect. *No fossil has been found intermediate between a reptile and a mammal or between a reptile and a bird.* Every palaeontologist knows this. Thus your request for a reasonable explanation of intermediate forms between reptiles and mammals and birds is asking me to account for something that does not exist ! Every form which transformists cite as an intermediate between two Classes is the member of one Class deemed to be the least unlike a member of the other Class. Archaeopteryx is less unlike a reptile than is any other known bird. Scymnognathus and Cynognathus are less unlike mammals than are most other reptiles. To speak of Archaeopteryx as an intermediate between birds and reptiles and the two mammal-like reptiles you name as intermediates between mammals and reptiles is on a par with describing a greyhound as an intermediate between the dog family and the horse, because it is less unlike a horse than is the dachshund ! As to accounting for this, it is inevitable that

in every Class some member must be least unlike the members of another class. You make much of the supposed reptilian characters of Archaeopteryx. Let us examine those you have cited: (1) the teeth, and (2) the long vertebrated tail. Neither of these are characters that distinguish reptiles from other kinds of animal. Turtles lack teeth, and the tail of some of the flying reptiles (Pterodactyls) was as short as that of some modern birds. You cite (3) the non-pneumatic bones of Archaeopteryx; but the bones of some modern birds are not pneumatic, and those of some pterodactyls were pneumatic. Lastly you cite (4) the jointed fore-limbs of Archaeopteryx. As regards this, let me quote the following passage from Prof. Vialleton's *L'Origine des Êtres Vivants* (p. 109): 'As to the hand (of Archaeopteryx), the only reptilian features of this is that the metacarpels are not fused at the distal extremity, but this occurs in the ostrich; on the other hand, the hand could not be bent downwards as every hand is, but only sideways as in the case of true wings. What then is there truly intermediate in Archaeopteryx? It is not its perfectly developed feathers, its bipedalism entirely like that of a bird, or its head which, though provided with teeth, has the shape of that of a bird and the relative proportions of face and brain-case so different from those of reptiles of which the face greatly dominates the infinitely reduced brain-case. Archaeopteryx is a bird, as Dames and Depéret recognised. Its feathers indicate a warm-blooded animal. Its comportment was that of a flightless bird and probably its wings served as a parachute. It was a kind of ratite, an aberrant form, as one can see, but most certainly not an intermediate between *two Classes* (reptiles and birds) as it is often represented to be.' Now, Vialleton was a great zoologist. For forty years he worked at embryology, and he devoted himself in his later years to the study of the limbs and girdles of backboned animals, and as the result of his labours in connection with these he wrote his *Membres et Ceintures des Vertébrés tétrapodes*, published in 1924. As in my case, he when a young man accepted evolution, and his work in the laboratory and the museum, and mine in the field, led him and me to reject the theory. His last book, published in 1929, from which I have just quoted, went through 17 editions within two years of publication, and has done much to cause many French biologists to reject what the French call correctly *Le Transformisme* and we English incorrectly call Evolutionism.

I now come to your imagined intermediates between reptiles

and mammals. As I have already stated, in the Secondary period the reptiles were the dominant vertebrates in the lowlands, where they occupied all the niches now filled by mammals. They were much more diversified than are mammals to-day. For example, some reptiles were as small as a mouse, others were larger than any land mammal, e.g., the Dinosaur Diplodocus, which measured 87 feet from tip of snout to tip of tail. Some of these reptiles had the habits of carnivorous mammals, and had teeth adapted to devouring large quarry, hence the mammal-like teeth differentiated into incisors, canines and molars. These are the creatures which transformists imagine to be the ancestors of mammals. I use the word imagine of set purpose, for in a subsequent letter I shall show that no reptile can possibly have been gradually converted into a mammal.

This lack of transitional forms is not confined to those between reptiles and birds and reptiles and mammals; it extends to all Classes. Let us hear what L. Berg has to say about it. As Berg was a zoologist employed by the Soviet Government, which is pledged to Marxism and officially accepts Darwinism, he can scarcely have a prejudice against evolution. '*It is truly remarkable*,' he writes (*Nomogenesis* (1926), p. 347), '*that palaeontology in no ways displays transitional forms between Phyla and Classes, and, possibly, not even between Orders. . . . Formerly this circumstance was accounted for by the imperfection of the geological record, but it is none the less surprising that the deeper our knowledge penetrates into the domain of fossils the further back recede generic interrelations which, as it were, ever elude our grasp.*' Berg then points out that the alleged transitional forms have been proved to be nothing of the kind, e.g., the lung-fishes as a stage between fishes an amphibians, the Acanthodii as one between the sharks and the higher fishes, the Bennettitales as one between the Gymnosperms (pine-like trees) and the Dicotyledons (flowering plants).

Yours sincerely,

D. DEWAR.

DEAR DEWAR,

This trivial minor points about cats and civets is getting tedious, and I must acknowledge that for the first time I am in a difficulty, but not the kind of difficulty which you sometimes allege. Up to the present I have, on the strength of your reputation as an expert on Indian birds, assumed that, though like myself you are not a

professional anatomist, you had that broad general knowledge of comparative anatomy which makes it unnecessary for me to explain elementary points. Your treatment of this matter throws doubt on that assumption, and I must now be very elementary in order to explain to the general reader a biological question, which, if we had observed a proper logical order, would have already have been discussed under morphology.

First allow me to inform you that no competent comparative anatomist proceeds by the method of listing differences by simple enumeration. It is of no consequence whether you call the differences between the cats and the civets 5, 50, or 500. I will illustrate the principle by the skeletons of a cat, a tiger, and a mongoose. For the purpose of making out a list of differences which are obvious and numerous, the tiger is much more unlike the cat than is the mongoose. Every bone is different in size, and the proportions between length and diameter necessarily vary because of the difference in the weight of the animals. You would do well to read Spencer on this point, or, for a popular exposition, Professor Haldane's short essay, *On Being the Right Size*. Yet, though the differences between the cat and the tiger can be listed to make an enormous number, every comparative anatomist knows that *they are not important* and the comparatively small number of differences between the cat and the mongoose are much more significant.

This same elementary principle applies to the matter in hand. First let me tell you that there are no skeletal differences of any importance between the cats and the civets that have not been mentioned. Second, let me inform you that, as the differences between the cats and the civets (e.g., a mongoose) are important compared with those between a cat and a tiger, so are those between Equus and Eohippus more important still. Indeed, as I showed in a previous letter, the differences between Equus and Eohippus include all the important ones between a cat and a civet, but those to which there are no parallel are still more significant. I don't think any competent anatomist would disagree. I could just imagine it possible that one might argue that, although what I said was obvious on the surface, I had overlooked some subtle points. That is exceedingly unlikely, as, if there were any, they escaped the notice of Professor Owen, but in any case you have nothing to say on this matter, as, if you had, you could settle at once the question of the number of vertebrae. There is no reasonable doubt about the truth of my assertion on this matter.

Your very inexpert handling of this question makes it impossible to take your criticism of the authorities of the British Museum seriously, and the reasons you give for disagreeing are no better than your treatment of the question of the cats and civets. You say: 'if one of these animals (Cynodictis and Viverra) gave rise to the other, it must have been by a great mutation.' No one suggests that one gave rise to the other. The point is that the assertion of the existence of a common ancestor for Viverra and Cynodictis is much more obvious than that of one for modern Canidae and Viverridae, as the latter have differentiated, whereas the former are nearly allied. Moreover, it is a complete answer to people like Vialleton (according to your account, because I know nothing about him except what you tell me), who admit the common ancestor for all the Canidae, and for all the Viverridae, but deny a common ancestor to the two. But as Cynodictis is nearer to Viverra than are some modern Viverridae to each other this does not make sense, and evolution within the family only breaks down entirely. Whatever twists and turns you make, you cannot find any position about evolution which makes sense except that it has occurred and there are no known limits.

Your treatment of the mammal-like reptiles shows the same disregard for the principles of comparative anatomy. Your contention expressed in my language would be parallel evolution owing to similar conditions of life. As you do not believe in evolution, you must translate this into your own language; I can't do it for you. But a mammal-like differentiation of teeth is not needed for a carnivore. It is needed much more for a herbivore. You have only to look at the teeth of a shark to satisfy yourself on that point. Moreover, even if we admitted such an improbability, that would not account for the intermediate structure of jaws and earbones. This is a type of evolution which is exceedingly unlikely to have occurred twice (or to have been created twice for the matter of that). I need hardly say that your comparison of these similarities to those of the horse and the greyhound is even more contrary to the recognised principles of comparative anatomy than those I have already mentioned. The similarities between Archaeopteryx and a reptile are precisely those on which comparative anatomy is based; the similarities between the greyhound and the horse are not. You are not only denying modern evolutionary science, but the principles of comparative anatomy which preceded evolution. Archaeopteryx is intermediate between a bird and a reptile in the strict

anatomical sense, as you half acknowledge by your use of the fighter-bomber analogy, and that it is for convenience classified as a bird is irrelevant. You show in addition other examples of confused thought on the principles of classification ; but these matters I will defer till the proper chapter.

I must now, by the editor's request, deal with your metaphysics. I can't answer your questions because they are all leading questions. The third question, for example, begins : 'If, as you concede, there is nothing intrinsically improbable in the hypothesis of special creation.' What can I do with people who talk like that ? I don't concede anything of the kind. I must therefore say quite bluntly that I regard the hypothesis of special creation as too foolish for serious consideration ; indeed, I do not regard it as a hypothesis at all, but merely one of those peculiar confusions of thought which remove some anti-evolutionists from the class of people with whom it is possible to conduct a rational discussion. If you believe it say so, but why say that I concede it when I don't ? I have therefore to try to imagine what it is you want to know, and the following is my answer. I think the answer really amounts to what is meant by creation, and as it is quite certain that you and I mean different things, I can only say what I mean.

If you ask me how this universe came into existence, all I can say definitely is I don't know. I should probably add : I think it must have been created. You would certainly not understand my meaning. It has nothing to do with science, or with scientific explanation in the ordinary sense of the word ; it is not a hypothesis. It is a metaphysical speculation which takes us out of the region of ordinary reasoning into that of mystical intuition. St. Augustine and others must have thought on similar lines when they postulated the creation of time. My creation means just as much and just as little as does St. Augustine's creation of time. When human reasoning fails we may or may not fall back on mysticism, as expressed by the ideas of God, creation and final causes.

If you ask me how life came into existence, I should give you a very different answer. I should say, *in all probability* it originated by a gradual synthesis of complex organic compounds. If *you* like to say it was another act of creation, my reply would be : very well, keep your mysticism, I don't know enough to argue the matter.

If you ask me how Archaeopteryx came into existence, I have no doubt whatever about the answer. Archaeopteryx and the

stray dog which so annoys you are to me precisely similar. I have not the least doubt that both were born (or hatched) in the ordinary way, although there are many things about birth and reproduction which are to me (and I think to everyone) unfathomed mysteries. Similarly, though there is much I don't know about the causes, I have no doubt that both are descended by the ordinary process of reproduction and change from pre-existing forms : in the one case a reptile and in the other case the generalised early Eocene mammal. We will not trouble now about the earlier stages in both cases.

If you say that Archaeopteryx was specially created, I can only laugh and ask : in the name of God, Christianity and anything else you may happen to believe in—Why ? The suggestion is quite unnecessary, because its descent from a reptile fully explains the facts. More important still, the suggestion seems to me such a travesty of religion. I do not recognise the Christian God in your Mephistopholean Demiurge creating practical jokes. And, when you come to think of it, it would be an enormous practical joke to create a sort of bird and forget to fuse the wing bones. Your demiurge would answer fairly well as a comic Devil in a revised Faust, or book of Job, but I really cannot admit him into the study of biology. You do not even get out of it a literal interpretation of the first chapter of Genesis. If you want that, Gosse has given it to you, and I like his one big lie better than your multitude of little ones. You will, I suppose, call this sneering creation out of court, but it is not creation I regard as out of court, but the inextricable confusion of amateur theologians who mix their categories, which is a worse fault than mixing metaphors. I will try to put this very simply by an analogy. My attitude towards creation is very similar to that of Father Brown on miracles. When the question was put to him he answered : 'Yes, I believe in miracles. I believe in man-eating tigers, but I don't see them running about everywhere.' You and the editor have asked for this, and so you must put up with plain speaking. I think what I have said is what you want to know ; if it is not, I have not the least idea what you *do* what to know.

Well, so much for metaphysics, and I hope I shall not be asked to expound them again until the last chapter. This is supposed to be a scientific discussion, and the title of the discussion is : *Is Evolution Proved.* If you have any scientific objections to allege in the negative, I shall be glad to hear them, as we have had none of any consequence up to the present. You have, I believe, a

number of difficulties connected with geology. Perhaps you will start by stating them without further preliminary fencing.

Yours sincerely,

H. S. SHELTON.

DEAR SHELTON,

In your first letter in this chapter you asserted that the difference between the civet and the cat family is *very much less* than that between Equus and Eohippus. I replied that evidently you were so perturbed by your inability to produce a series of fossils linking two families that you made the above assertion, which you would not have made in calmer moments. Instead of candidly admitting (as you did in the case of my use of the term 'Horse') that in this matter your valour had overcome your discretion, you decided to try to justify your assertion, and in consequence made three unsustainable claims which I dealt with in my last letter. You now add to these three more equally unsustainable claims and make several unjustifiable assertions.

You assert that when classifying animals no competent anatomist proceeds by the method of listing differences by the method of simple enumeration. Far from this being the case, the anatomist, in classifying two animals, starts by ascertaining the differences and listing these. He then appraises the value of each of these differences, and, taking all this into consideration, classifies the animals in question.

Then you make the amazing statement: 'It is of no consequence whether you call the differences between the cats and civets 5, 50 or 500!'

Not content with this, you claim that the greater the number of differences between two animals the closer the relationship, and give as an example a cat, a tiger and a mongoose. The two former belong to the same genus Felis of the Felidae and the mongoose belongs to the Viverridae or civet family. You then assert that the differences between the cat and the tiger can be listed to make an enormous number (despite the fact that they are of the same genus) while although the differences between the cat and the mongoose are comparatively small in number, they are much more significant! But hitherto you have insisted that the differences between cats and civets are not of much importance or of no importance!

Then you drag in Sir Richard Owen, who was born five years

before Darwin, but you do not say how he helps you. Please quote, with reference, the statement of Owen on which you rely. You say you do not think any competent anatomist will disagree with your notion that cats and civets are more closely related than are Equus and Eohippus. I challenge you to quote some statements of competent anatomists to this effect.

Then you chide Vialleton because he does not accept another claim of yours, viz., the lesser includes the greater, because he admits that all the cats are derived from a common ancestor and all the Viverridae are derived from another common ancestor, but not that these two ancestors are themselves derived from a common ancestor!

You crown the above assertions by speaking of 'this trivial point about cats and civets,' but it is you who not only raised it, but have made it the basis of your attempt to produce a series of fossils showing the descent of one family from a different family!

By the way, it is odd that you should be so dogmatic about anatomy and yet admit you know nothing about Vialleton except what I have told you. As you have not heard of, much less read, his classic work, it is useless for me to ask you to name a recent comparative anatomist more competent than Vialleton. I can tell you, however, that de Beer, who is an evolutionist and a F.R.S., spoke highly of Vialleton to Lunn and commended his work on comparative anatomy to J. B. S. Haldane. Your admission illustrates the provincialism of British evolutionists.

I come now to your replies to my three questions, which were put because you evaded them when asked by the Editor. You begin by saying you cannot reply to them because they are leading questions. A leading question is one which suggests the answer the questioner desires or expects, or suggests disputed facts about which the witness is going to testify. In a court of law a man may not put such questions to a witness called to support his case, unless that witness prove hostile, when with the permission of the Court leading questions may be put. You are not a witness on my behalf, therefore I am entitled to put leading questions to you, and the Court would compel you, if the questions were put in Court, to reply to them. In fact, the three questions I have put to you are not leading ones. Not one of them suggests the answer. They are not leading questions but questions you cannot answer. That is why you again evade them. But in the case of Number 3 you have a legitimate grievance, because in putting it I attributed to you a belief you do not hold.

My reason for believing that you conceded that special creation is a hypothesis that cannot be dismissed as intrinsically impossible was based on three grounds. First, you had allowed the Editor's statement to this effect to go by default; secondly, because I credited you with as much insight as T. H. Huxley, who, as quoted by the Editor, said: 'The *a priori* arguments against . . . the possibility of creative acts appear to me to be devoid of reasonable foundation.' Thirdly, I assumed that you would admit that a philosophic theory held by a large number of men of great philosophic and scientific attainments was a sufficient ground for assuming that this hypothesis cannot be sneered out of court.

I am greatly obliged to you for having made your position crystal clear in the following words: 'I regard the hypothesis of special creation as too foolish for serious consideration; indeed, I do not regard it as a hypothesis at all, but merely one of those confusions of thought which remove some anti-evolutionists from the class of people with whom it is possible to conduct a rational discussion.' This statement of yours I find very gratifying for two reasons. First, it contrasts so well with the following pronouncement which occurs on page 365 of Vialleton's *L'Origine des Êtres vivants* (1930): 'Le mot création qui avait été bani du language biologique doit y reprendre sa place, au moins pour bien marquer le fait indubitable que le monde nous est donné comme un ensemble coordonné et par consequent voulu à quelque moment et dans quelque parties qu'on le prenne. Le mot transformisme doit être abandonné parce qu'il désigne une théorie dont l'impuissance à donner ce qu'on lui demandait est manifeste.'

Secondly, your reaction to the theory of special creation seems to me to resemble that of the proverbial bull to the proverbial red blanket. It shows that you and Prof. D. M. S. Watson belong to the same school of thought: you accept evolution, not on its merits, but because you cannot stomach special creation.

Being quite unable to answer my questions 1 and 2, which are very plainly put, you affect to be unable to understand them, and say you will try to imagine what I want to know; and then you put to yourself two questions made up by you and answer these! This part of your letter does not need my attention, but you do deal with my question as to why you suppose that the Creator must start *de novo* with each new creation, with Archaeopteryx as an illustration. You say the descent of Archaeopteryx from a reptile fully explains the existence of Archaeopteryx. This would

be a fairly satisfactory answer if you could (1) explain how feathers could have evolved gradually (which you have failed to do); (2) say positively from what Order of reptiles Archaeopteryx has been evolved (which neither you nor anyone else can do); and (3) you could produce a series of fossils linking Archaeopteryx with a reptile (which you cannot do). You therefore make the best of a bad job by saying you regard Archaeopteryx as a practical joke.

The creation of a bird of which the wings act as a parachute and the claws on the free digits of the wing enable it to grasp the branches among which it climbed is no more a practical joke than is the flying squirrel. As to why it was created, you might learn some natural history by reading Paley's *Natural Theology* as an antidote to Spencer's books on zoology. Paley, like Cuvier, appreciated the marvellous variety of the animal kingdom, and said (p. 170) this might 'induce us to believe that VARIETY itself distinct from every other reason, was a motive in the mind of the Creator or with the agents of His will.'

TESTIMONY OF THE GEOLOGICAL RECORD

In the remainder of this letter and those that follow I propose to set forth five important items of the testimony of the geological record against the evolution theory:

I. A great and diversified marine fauna appears on the scene with startling abruptness at the beginning of the Cambrian Period.

II. Every new type of animal appears suddenly in the geological record, endowed with all the attributes by which it is characterised. The changes it undergoes afterwards are comparatively insignificant. Sometimes, however, a group of animals about to become extinct undergoes considerable changes of a pathological nature before it disappears from the scene.

III. So far it has been found impossible to produce a series of fossils showing that one Family has gradually become converted into another Family, or an Order, Class or Phylum into another.

IV. While it is open to doubt whether or not the geological record furnishes good evidence of one genus having been converted into another, it certainly shows (1) that a large number of genera have persisted unchanged during long periods; (2) in the cases where the record enables us to trace far back into the past, two or more genera of a Family, their lines, instead of converging

until they meet in a common ancestor, seem to follow a parallel course.

V. While the record indicates that some species have changed into other species, it also suggests that some species are exceedingly stable and have persisted during an immense period of time.

In this letter I will deal with item No. 1.

A great and diversified fauna appears on the scene with startling abruptness at the beginning of the Cambrian Period.

Many of the Pre-Cambrian rocks which immediately precede the Cambrian rocks and underlie them are rocks in which fossils could equally well have been deposited, but not a single undisputed fossil has been found in them.

Suddenly in the Cambrian Period we find the sea full of highly organised types. We find nothing which suggests slow evolution. We find no experiments in the production of new types, no experiments, for instance, in shell making. The first shells are fully developed. We find these earliest animals as sharply differentiated into Species, Genera, Families, Orders and Phyla as they are to-day.

These earliest fossil-bearing rocks—those of the Cambrian period—are wide-spread. They occur in several places in England, in Wales, in Scotland, in many localities in Europe, in Alaska, Eastern Canada, Newfoundland, Australia, China and Siberia, and they are wide-spread in the U.S.A.

So varied are these earliest known fossils that they include members of all the great Phyla that possess hard parts, except the backboned phylum, of which the earliest known fossil does not occur earlier than the later part of the Cambrian Period. All belong to existing Phyla and Classes, some to existing Orders and Families, a few to existing genera, but only one seems to be of a species now living. These fossils, in the words of Brooks (*The Foundations of Zoology* (1899), p. 216), 'far from showing the simple unspecialised ancestors of modern animals are most intensely modern themselves in the zoological sense.' Were it possible for anyone now living to take a boat with a drag-net on a Cambrian sea, the net would catch creatures, many of which the non-expert would find it difficult to distinguish from those in a haul he would make to-day; animals like present-day mussels, limpets, lamp-shells, sponges, jelly-fishes and sea-cucumbers, but his net would contain in place of shrimps, crabs and lobsters, trilobites (creatures looking like great wood-lice), and the lamp-shells would be more numerous instead of less numerous than molluscs.

Many of the Pre-Cambrian rocks have been so metamorphosed and disturbed that any fossils they may have once contained would have been destroyed, but this does not apply to a number of these early rocks, which remain to-day just as they were deposited, beautifully preserved and eminently fitted to hold and retain fossils. It is these that have been so keenly searched in the hope of discovering fossils in them. And some enthusiasts have found in them what they believed to be fossils. But, to quote Percy E. Raymond, formerly President of the Palaeontological Society of America (*Bul. Geol. Soc. Amer.* (1935), pp. 375–92) : 'so anxious are geologists to obtain fossils from these rocks that anything which remotely resembles an organism is carefully saved and studied in great detail. Although many such objects have been described, very few have been unreservedly accepted as fossils.' Raymond, after a careful scrutiny of all these supposed fossils, accepts none of them unreservedly ; he thinks that two of them may eventually prove to be fossils, but judgment should be reserved until further specimens shall have been found.

Unless this lack of fossils in Pre-Cambrian rocks can be accounted for in a way compatible with the theory of evolution, this must remain an unverified hypothesis. I have come across about a dozen explanations of this absence of Pre-Cambrian fossils, each incompatible with all the others, and all in my opinion, untenable. What explanation have you to suggest ?

Yours sincerely,

D. DEWAR.

DEAR DEWAR,

In my first letter of this chapter, as you say, I stated that the differences between cats and civets was much less than that between Equus and Eohippus. In my subsequent letter, pp. 94–95, I showed that all but one of the significant differences between cats and civets were paralleled by the *same* differences between Equus and Eohippus, and that the most important differences between Equus and Eohippus had no parallel. *That is the proof*, and nothing that you have said since controverts it in any way. The best reference I can give you for Owen's discovery is H.269.

About your three questions, although I think I have given you the necessary information, perhaps I had better answer them directly, so far as is possible, in order to give you no possible excuse

for saying that anything you have to put forward has been evaded. The answers are as follows: (1) Because one absurdity is better than a million absurdities. (2) Yes. (3) I cannot give you any reason why God should not do anything. This is supposed to be a scientific discussion.

Huxley's opinion and my own are identical. In order to understand Huxley on this matter, it is necessary to read his works, and particularly his essay on *Possibilities and Impossibilities* (*Collected Essays*, Vol. 5, pp. 192 seq.). The following illustration may make the point a little clearer. When you see a conjurer take a rabbit out of a hat, it is a *possible* explanation (in the sense of conceivable), that the conjurer has created the rabbit. But it is highly *improbable*. It is also unscientific and unphilosophical to assume that he has done so. You seem to regard impossible and improbable as convertible terms. In Huxley's language (and in mine) they are not so.

With regard to the rest of your letter, I will first ask you for your authority for the statement that vertebrates are found in the upper Cambrian. The rest of this letter will deal with the one point you have elaborated at length, namely Pre-Cambrian fossils. With regard to that, I will say at once that your main statement of fact is not disputed. The traces of life in the Pre-Cambrian are very much less than those found in the Cambrian. There is no doubt of the existence of traces of life in the Proterozoic. In the Archeozoic there is nothing but graphite, which is probably of organic origin, as we know of no other natural source for the element carbon. But there is no doubt about the great difference between the Pre-Cambrian and the Post-Cambrian record (our record).

I cannot see that these facts are in any way surprising. I explained in the last chapter how fragmentary the record is, and how it becomes more so as we go back in time. It is, therefore, to be expected that, before a certain time, fossils shall become rare, and finally cease altogether, which is exactly what happens. The point is perfectly valid, and, for what it is worth, I concede it. You do, however, exaggerate the amount of Pre-Cambrian sedimentary rocks which are available. Any book on geology will warn you on that matter. For example, Geikie, in his text-book of geology says, 'Rocks have been claimed as Pre-Cambrian which are certainly eruptive masses of later date' (p. 882). Gregory and Barrett (*Stratigraphy*, p. 43) says: 'Rocks have been called Pre-Cambrian because they have no fossils.' Also I think due regard should be

paid to the indurated nature of the bulk of the rocks that are undoubtedly Pre-Cambrian. This implies that any fossils that might have been there are more than usually likely to have disappeared.

As we go back in the history of the earth there was a time when the animals had no hard parts. It may be that that time was not long before the Cambrian, and that the early stages of the formation of hard parts occurred in the considerable period that elapsed between the Lower Cambrian and the known Pre-Cambrian. Of that, however, we cannot be sure, and earlier fossils than those now known may be discovered at any time. There the question must be left, and it is no use speculating.

APPENDIX

(Further evolutionary evidence from the land mammals.)

1. The Eocene Miacidae, classed as Creodonts, resemble 'partly the Viverridae and partly the Canidae and Ursidae, being probably the common ancestor.' (Zittel).

2. The earliest fossil forms of the Mustelidae (pole-cats and weazels) are not markedly differentiated from the earliest Viverridae, 'from which they are probably descended.' (Zittel).

3. The early Eocene mammals are divided into creodont and condylarthrous animals, 'the exact boundaries of which are badly marked in early times.' (M. 102).

4. The upper Eocene lemurs Adapt'is combine features of existing lemurs and apes.

5. The Creodonts are not easy to separate absolutely from the existing and more especially from some of the extinct members of the Carnivora Vera. (M. 455).

6. 'The progress is so gradual that the forms enumerated and described seem to have been part of a continuous series culminating in the ground sloth of later times.' (M. 193 on Edentates).

7. Beddard thinks Allotheria a possible ancestor for both Monotremata and Marsupials. (M).

8. Arsinotherium (lower Oligocene Egypt) is allied both to the Amblypoda and the Hyracoidae. (N. 55).

9. 'Tritylodon longaevus was originally described as a mammal by Sir Richard Owen, it was later placed among the reptiles by Professor Seeley, but the most recent workers include it among the mammals.' (N. 81). Which is it? Why should some Mesozoic

mammals and reptiles be so similar on any other ground than that of evolution?

10. Palaeotherium magnum was classified by Cuvier in the Tapiridae. Modern authors have removed it from the Tapiridae to the Equidae. (N. 35). Owen regarded Hyracotherium as related to Chaeropotamus, which is now regarded as related to the Suidae (pigs), it has also been regarded as related to Lophiodon (Tapiridae), see H. 269. This illustrates the fact that the early Eocene ungulates are so nearly allied that they may be regarded as a family, and so evolution within the family really means a common ancestor for all ungulates. Items 3 and 5 also show that similar reasons may be given for regarding ungulates and carnivores in the same light. There is thus very little doubt of the common origin of all living and extinct land placentals.

Yours sincerely,

H. S. SHELTON.

DEAR SHELTON,

'I cannot,' you write, 'give you *any reason* why God should not do anything. This is supposed to be a scientific discussion.' The operative words are italicised. You decline 'reasons' but you are prodigal of assertions about the absurdity of any conception of God which you find difficulty in understanding. I quite agree that this should be a scientific discussion. But we who do approach this problem of the living world in a scientific and inductive[1] manner find ourselves confronted by people like you and Watson who accept evolution not because you have any hope of proving it to be true but because you assert (without, of course, giving 'any reason') that special creation is incredible. In effect this controversy is a controversy between science and pseudo-religiosity, between the scientific demonstration that new types appear suddenly and the pseudo-religiosity which refuses to draw scientific deductions from the clear evidence of the geological record.

The difference between us is that my beliefs are based on reasoned inductions from the evidence and you concede in effect that your creed starts from the premise *Credo quia absurdum est.* You

[1] By induction I mean arguing from the particular to the general, and by deduction arguing from the general to the particular. Your letters are deductions from the unproved premise that evolution has occurred. You began by assuming the truth of evolution. So did I, but my inductions from the particular, from the facts of nature which I observed, led me to the conclusion that these facts can be explained only by creation.

believe in God and in creation, but you prefer to believe that creation antedated the appearance of life on this planet because, 'One absurdity is better than a million absurdities.' I disagree. If my own belief rested on absurdities, whether one or many, I should not attempt to defend it against a rational opponent. I hold on the contrary that there is nothing absurd in the concept of creation or special creation, and that the question as to whether new types appeared suddenly on this planet or evolved gradually is strictly scientific and should be discussed without dragging in your emotional prejudices in favour of your particular pet absurdity. My own beliefs about God are in the main inductive, that is they are suggested to me by the available evidence and the only *a priori* assumption which I am prepared to make is that Charles Kingsley was correct when he avowed that he could not believe that God would write on the rocks 'one enormous and superfluous lie.' A God who played such practical jokes on the earnest seeker after truth, creating a world with the appearance of a planet in which a great and diversified fauna appeared suddenly, when in fact this had come into being by a slow process of evolution, would be no God for scientific men to worship.

It is true that as we pass back in time the quantity of fossiliferous rocks that have been preserved diminishes to some extent, but there is no question of the Palaeozoic fossils gradually tailing off. In the Cambrian rocks they are numerous and greatly diversified. Suddenly we come to a stage when they are *no longer found anywhere in the world.* Underlying the richly fossiliferous Cambrian rocks are vast masses of rocks in which not a single indubitable fossil has been found, rocks thousands of feet in vertical thickness and in many places identical in all their physical features with the overlying Cambrian rocks into which they fade imperceptibly, rocks beautifully preserved, undisturbed, unmetamorphosed, eminently suited to hold and retain fossils.

There is no justification for your statement : 'the considerable period that elapsed between the Lower Cambrian and the known Pre-Cambrian.' Doubtless, as in the case of all later rocks, in some places there has been a time gap between the deposition of a formation and the one overlying it, but these gaps are all *local affairs* and the result of local conditions. If the known Cambrian rocks were all confined to one locality there would be something in your explanation, but as Cambrian rocks have a world-wide distribution it is of no avail.

As regards cats and civets, you still decline to produce that long list of anatomical differences. You prefer assertion to facts. But the fates are unkind to you ; a few minutes ago, I happened to come upon the following on page 621 of Scott's latest book, already mentioned by me : The 'highly distinguished palaeontologist Dr. Max Schlosser' was 'so impressed by the isolated position of cats that he thought they must have arisen independently and separately from all other fissipeds (land carnivora) and denied any relationship with the Viverridae (civets) !'

You have not given Owen's statement on which you rely.

In reply to your query re the earliest Cambrian vertebrate fossil I refer you to W. L. Bryant's account in the Fifteenth Biennial Report, Vermont State Geologist (1926).

Your appendix consists of the efforts of a number of transformists to find ancestors for animals or groups of these. 'When one is a transformist,' writes Maurice Thomas, 'one has to assign an ancestor to another animal some creature even if it be a beast of the Apocalypse.' As I have already considerably exceeded the space allotted to me for these opening chapters I make the following sporting offer. If you will transfer to me 1,000 words of your quota, I will pull your appendix to pieces, item by item. Meanwhile in reply to your query re Tritylodon. At present we know only an incomplete skull without lower jaw. When we learn something of its skeleton we shall be able to classify this animal.

I now come to the second item of the testimony of the geological record against the evolution theory.

II. Every new type of animal appears suddenly in the record with all the attributes by which it is characterised.

Take the fishes. The earliest nearly complete skeleton known reveals that the fishes then living had typical fish bodies, tail and backbone. They show not the least resemblance to any other phylum ; so much so that there is no agreement among transformists as to the Phylum that is supposed to have given birth to vertebrates. Later new Orders of fishes appear, but no transitional forms between them and the earlier fishes. In the Upper Silurian scorpions appear suddenly, hardly distinguishable from those now living. So is it with spiders which appear in the Carboniferous, provided with complete web-spinning apparatus. Insects show the same thing. One of the earliest known is the blackbeetle so common in old houses : another is a typical dragon-fly.

So is it with the turtles. These are unique among mammals and undergo no noteworthy modification in structure since their appearance in the Trias. The extinct marine reptiles called Ichthyosauruses tell the same story. These differ as widely from other reptiles as whales do from other mammals. The same story is told by the Pterodactyls, extinct flying reptiles as different from other reptiles as bats are from other mammals. The earliest known Pterodactyls have fully-developed wings.

As regards birds, we have noted that Archaeopteryx appears unheralded in the Jurassic. All the highly specialised types of mammals, the cetacea (whales, etc.), the Sirenia (sea-cows), the Pinnipedes (seals) and the bats make their first appearance in a fully-developed condition.

Please note that each of the above Orders or sub-Orders that appears upon the scene so abruptly remains almost entirely unmodified from the time of its appearance until the present day or until it becomes extinct. Is it reasonable to assume that each group underwent tremendous modifications, nay transformations, in localities unknown to us, and, after they show themselves, remain static?

Please note also that not a single fossil has been found transitional between any of these and its supposed generalised ancestor.

In order that those who are not biologists may appreciate the magnitude of the difficulty to evolution presented by the lack of intermediate fossils, let us look into the matter of the marine reptiles and mammals which evolutionists assert are derived from land ancestors: Ichthyosauruses, turtles, whales, sea-cows and seals. *Ex hypothesi* these all evolved on the fringe of the sea, i.e. the locality of which the geological record is the most complete. Yet not a single transitional fossil has been found in the case of any of these. Take the seals, walruses and sea-lions which appear in the Miocene period. There must have been at least 10 genera intermediate between the first seal and its hypothetical dog-like ancestor, and a further 9 linking the three families of Pinnipedes now existing with the parent or first seal, making 19 intermediates in all. As fossils of more than three-fourths of the living genera of Pinnipedes have been found, fossils of 14 or 15 of these intermediate genera ought to have been discovered. Not merely 14 or 15 fossils, because several specimens ought to have been found of some of these genera. Thus, if seals gradually evolved from land animals, we might reasonably expect to have found by this 50 intermediate fossils. Fossils of

living genera of Pinnipedes have been found in England, Scotland, Norway, Holland, Belgium, France, Italy, Germany, Austria, Hungary, South Russia, in Oregon, Virginia, Maryland, New Jersey, also in Egypt, Victoria and New Zealand. In view of facts such as these, there seems no alternative but to believe, that, however the Pinnipedes originated, their origin must have been sudden. In the case of so diversified an order as the Cetacea the intermediate fossils that ought to have been found should have numbered about 300, linking the first whale with its land ancestor, and connecting with one another its three sub-orders and eight families. Not one has been found. Taking also the Sirenia, turtles and Ichthyosauruses in all some eight or nine hundred fossils of genera intermediate between them and their hypothetical land ancestors ought to have come to light.

Nor is this all. Each of these groups does not appear in the form of a single species or genus, which for a long period is the only representative of the group. Several genera of each appear practically simultaneously: 9 Ichthyosaurs and 6 Chelonia in the Trias; 5 genera of Sirenia and 6 of Cetacea appear in the Middle Eocene; 6 genera of the Pinnipedes in the Miocene. Yet not a single fossil of any of these occurs in the early Trias in the case of the two reptiles, the early Eocene in the case of the Sirenia and Cetacea, and the early Miocene in the case of the Pinnipedia. Do you believe that the fossil record is so good at the time each of these groups appear and afterwards, and so bad before the appearance of each? Do you believe that the fossil record of the Lower Miocene is bad in the case of the seals and so good in the case of the Cetacea as to yield fossils of ten genera of these?

I put it to you that the fossil record tells us plainly that the supposed ancestral forms of all the above groups exist only in the imagination of transformists. I contend that no theory of origins that demands such intermediate forms is tenable.

III. So far it has been found impossible to produce a series of fossils showing that one Family has gradually become converted into another Family, or an Order, Class or Phylum into another.

This need not detain us long, for you have not been able to produce this series despite your gallant attempts to convert Eohippus into an Eohippopotamus!

IV. While it is open to doubt whether or not the geological record furnishes good evidence of one genus having been converted into another, it certainly shows (1) that a large number of genera have persisted unchanged during long periods. (2) In cases where the record enables us to trace far back into the past two or more genera of a family, their lines, instead of converging until they meet in a common ancestor, seem to follow a parallel course.

As I am trying to discover the extent to which animals can be changed, I have endeavoured to find a fossil series showing the gradual transformation of one genus into another. So far I have not succeeded. All you can produce is one doubtful case, the Equidae, which we have already discussed in some detail.

You imply that the total lack of fossils linking genera of mammals is due to the fact that most of the rocks in which these occur cover a period of short duration, speaking geologically. But in deposits such as those of the Phosphorites of Quercy, which are so rich in fossils and cover the Upper Eocene and Lower Oligocene periods, no such series of fossils has been found. This can only mean that, either new genera do not originate gradually, or, if they be products of evolution, the process is exceedingly slow—something like from 5 to 10 million years for the evolution of a genus of land mammals. If it takes so long for a genus to evolve, the evolution of a Family would require from 50 to 100 million years and that of an Order 500 to 1,000 million years. The earliest indubitable fossil was laid down about 500 million years ago. So you see that the fossils are not exactly favourable to the theory of evolution !

As I have been able to find no record of a series linking one genus of mammal with another genus, *if you know such a series I hope you will name it.*

Long-lived Genera

Some idea of the length of time during which most genera of molluscs are known to have existed without appreciable modification may be gathered from Table VI (p. 71). Table VII (see appendix to this letter) gives some information in the case of the other Phyla. Table VIII gives a few instances where the fossils show that two or three genera of a family have existed for from 100 to 340 million years and have run a parallel course and cannot be traced to a common ancestor.

That the fossils show that many genera in coastal seas have undergone no modification for an immense period of time suggests that those inhabiting the sea far from the land are equally long-lived, and the reason why fossils of *all* living genera of molluscs have not been found in the Cambrian rocks known to us is that in the Cambrian period their habitat was the open sea, and they did not move into coastal waters until comparatively recent times. The fact that 31 genera of molluscs now living in and around the British Isles existed in the Carboniferous period, i.e. some 250 million years ago, renders it not unreasonable to suppose that these lived in the open seas 250 million years before they migrated into coastal waters.

V. While the geological record suggests that some species have changed into new species, it shows that some species are very stable and have persisted during an immense stretch of time.

The stability of species is shown by the fact that in the case of molluscs 84 per cent of the species now living have left fossils dating from the beginning of the Pliocene Period, which, according to the current method of dating rocks, was 15 million years ago. Thus while the fossil record *proves* that many species are very stable it only *suggests* that, if allowed sufficient time, a species may change into a different one.

Yours sincerely,

D. DEWAR.

APPENDIX

TABLE VII

LONG-LIVED GENERA

Phylum	Period in which earliest fossil has been found.	Date in millions of years. B.C.
Mollusca	Cambrian	500–400
Protozoa	Cambrian	500–400
Brachiopoda	Cambrian	500–400
Arthropoda	Ordovician	400–350
Vermes	Ordovician	400–350
Echinodermata	Triassic	190–150
Coelenterata	Triassic	190–150
Vertebrata	Triassic	190–150

TABLE VIII

OF LIVING GENERA
PARALLEL LINEAGES WITHIN THE FAMILY

Family.	Genus.	Period in which earliest fossil has been found.	Years ago (in millions)
Nuculidae	Nucula	Silurian	340
(Nut-shells)	Acila	Cretaceous	100
Pinnidae	Pinna	Jurassic	140
(Wing-shells)	Cyrtopinna	Jurassic	140
	Atrina	Carboniferous	250
Pectenidae	Pecten	Triassic	170
(Scallops)	Hinnites	Triassic	170
	Amusium	Jurassic	140
Mytilidae	Mytilus	Triassic	170
(Mussels)	Modiolus	Devonian	300
	Lithophagus	Carboniferous	250

DEAR DEWAR,

As the editor asks me to make this my last letter in the present chapter, perhaps I may be excused if I am a little discursive. It will be best first to say a little more about your metaphysics. You seem to find inconsistency between accepting creation in any shape or form and declining to accept your somewhat peculiar ideas on special creation. If that were so I should merely ask you to ignore anything I said on creation, as that is a somewhat vague metaphysical speculation irrelevant to the issue. When we get beyond the range of our very limited human knowledge it is a natural human reaction to speculate on creation and final causes. To a certain extent this is legitimate, but it is not legitimate within or near the bounds of ordinary knowledge. It does not seem to me allowable to fall back on creation because we find difficulty in finding the exact line of descent of living creatures. The postulation of creation ends the discussion. There is then no more to be said or known. In my view, however, we are only at the beginning of our study of the subjects implied in this discussion. This is particularly the case with palaeontology, in which very little has been thoroughly examined even of the evidence that is available, to

say nothing of the difficulty that we do not know how much has been irretrievably lost because the fossils are not there to be discovered. It is no doubt true in a sense, as you say, that whether or not forms of life appeared 'suddenly' is a scientific question, but we are nowhere near the detailed knowledge necessary to consider that at all. We have quite sufficient evidence to say quite definitely that some types, e.g. land mammals have *not* appeared suddenly, in other words have been evolved. With regard to other types—the majority of types—geology gives very little evidence. So far as geology is concerned *we don't know*. That is all that can be said, and we must proceed to other types of evidence. Your mistake lies in making inferences from ignorance. It is quite illegitimate to infer creation from ignorance, which is what you are doing all the time. It is quite conceivable that geology might give *positive* evidence against evolution, *but not a trace of such has ever been discovered.* At least, if you think you have any such evidence, you have reserved it till your last letter to which I shall not be able to reply.

That is the substance of what I have to say. The rest is elaboration, which is desirable because politeness demands that I pay a little attention to the details of your last letter. The first detailed point is the difference between the Cambrian and the Pre-Cambrian. The difference is admitted in a general way, but, when you have a legitimate point to put forward, you exaggerate. In the first place I must insist on disagreeing with you on the question of the continuity of the two series of rocks. In most cases, including the Canadian series where most of the fossils have been found, there is no doubt whatever that they are discontinuous, because they are unconformable. In a few cases, such as Australia, there is no obvious uncomformability. This does not prove continuity, it merely proves the absence of mountain building in the time between the deposition of the two series. This merely makes it more difficult to prove discontinuity, which can, however, sometimes be done. You need to study a good text-book on geology on this matter. The most that you can legitimately say is that, *in a very few cases*, the question must be left open. You are not entitled to assume continuity of deposition *in any case*. The only proof in geology for approximate continuity of deposition is similarity of fossils. Next there is no reasonable doubt about the geologic evidence for Pre-Cambrian life. I have had the opportunity of examining some of the specimens. The worm casts, for example, are very like later worm casts, and the worms are rather big. The carbonaceous

remains are very like those in later strata, attributed to algae, though they are too carbonised to enable species to be discovered. I am inclined to think that careful microscopic research might get us further even with these. The fossil remains are indubitable but not recognisable, if I may put it in that way. The main point you put forward is a good one, and quite legitimate, but I do not think you help your case by exaggerating. All through the strata unfossiliferous rocks are continually found both over and under the fossiliferous ones, there is nothing exceptional in that. Still, for what it is worth, I gladly concede to you the fact that, *so far as our present knowledge goes*, the transition from recognisable to unrecognisable fossils is a little abrupt, and I am pleased to be able to congratulate you on finding one really relevant argument.

With regard to the fossil record as a whole, the answer to all you said in your last letter is found in the last chapter. The geological record is fragmentary, and so *it is entirely illegitimate to make inferences from the absence of fossils*. If you want further evidence for this, I would suggest you read the Introduction to Zittel (1913 edition) which explains in some detail how and why the fossils which are 'laid down' do not remain. With this fragmentary record in our minds, let us look at one of your italicised passages : 'Every new type of animal appears suddenly in the record with all the attributes by which it is characterised.' I find considerable difficulty in making sense of this. What does it mean ? What is meant in the first place by the record ? If you mean our record, the fossils do not appear at all. They have to be very carefully got out of the rocks, collected and classified. If you mean by record the fossils that are in the rocks the statement is meaningless. *We don't know what fossils are in the rocks*. We only know what we have found. Obviously a new type of animal has all the attributes by which it is characterised, otherwise it would not be a new type of animal. What do you mean ? You surely do not mean that Eohippus has all the attributes of a zebra. It is really excusable if I am inclined to infer that your exposition is so vague and confused because of the confusion of ideas, and that it is because of this confusion that you are an anti-evolutionist. Now let us look at another instance when you seem to fail to follow, or even to attempt to answer, a very simple piece of reasoning. Let me once more put this question of the cats and civets in the simplest manner possible :

1. In your letter (p. 89) you give a list of the skeletal differences between cats and civets, and suggest that there are many more.

I inform you (p. 112) that there are no more *of any consequence.* You have not stated any more.

2. I show you in my letter (p. 94) that all but one of these are paralleled by the *same* differences between Equus and Eohippus, and that the most important differences between Equus and Eohippus and not paralleled by any differences between cats and civets.

What further proof does anyone want? Now let us look at your reply this time. You say on the authority of Max Schlosser that the cats are unusually isolated. Very well; I have no objection; all the better for my case.

It is also not true that I raised this tedious matter. In your letters (p. 51 and p. 53) you stated that I had only to prove this point in order to convince the editor and make you modify your position. The point is proved, but I have still to hear from you and the editor that the promised result has followed.

Your reply on the subject of Tritylodon is equally inconsequent. You say the fossil is incomplete—a skull without a lower jaw. Does that appreciably affect the value of the evidence? The point to meet is that there would be no difficulty whatever in classifying a similar incomplete skull from any ordinary mammal or reptile.

A few words are required about the fossil forms you describe, in your peculiar phraseology, as appearing suddenly. Obviously if we have a fragmentary record (our record) the fossils will 'appear suddenly.' All the same even the fossils you choose as examples show definite evolutionary evidence. You surely cannot attach any importance to the 'sudden appearance' of insects and spiders which have no hard parts. You speak of web-spinning apparatus in the spiders. Why web-spinning? Only some spiders spin webs, though nearly all spin threads. There is no evidence that the Carboniferous spiders span webs. The early insects are not differentiated like the later ones. Zittel says the differentiation between the orders has little depth. The *Encyclopaedia Britannica* says that the Paleodiptera combine the characters of several existing orders. Zittel also points out that a Devonian form is intermediate between insects and isopoda. The zeuglodon, though probably a side branch, is definitely intermediate between whales and other mammals. Some of the earliest known tortoises had teeth, like the earliest birds. Teeth must be a frightful nuisance to the anti-evolutionist. Clearly then, even in the examples you choose, there

is definite evolutionary evidence, unless you prefer the hypothesis of the Demiurge creating practical jokes.

You also supply tables showing that some genera are very persistent. Why not? A genus which is thoroughly adapted to a simple environment, especially in the sea, is naturally persistent. There is no evolutionary principle which necessitates change in particular species and genera. Conditions sometimes change, and then the genus either evolves or dies out. But there is no reason why species occupying simple and persistent environments should change. This example of a difficulty is not a difficulty at all.

I will conclude by trying to put this geological evidence as simply as possible. I think I showed fairly clearly in the last chapter that the fossil record (our record) is fragmentary. Consequently, as a rule we get only imperfect series, such as the ammonites, or links, such as Archaeopteryx, or Tritylodon, or the brittle stars. All this fits exactly, *we do get these*. When the record is better than usual we get either continuous series, such as Micraster, or overwhelming evolutionary evidence such as we find in the land mammals. In this case we are exceptionally lucky because the better record coincides with a period of rapid evolution. The evidence as a whole is exactly what is to be expected, in a few cases, like the Equidae, distinctly better. There is no positive adverse evidence whatever. No reasonable person can expect more. It is futile to make long lists of chains or links that have not been found. If the record (our record) is fragmentary, of course they have not been found.

This assumes the fragmentary nature of the fossil record (our record). It is a natural assumption both from the nature of the case, and from the detailed evidence, a little of which I gave in the last chapter. You have given no reasons whatever for thinking the natural assumption wrong. The only point of any real importance you have brought forward is that in one case (land mammals), where there is exceptionally good evolutionary evidence, a large number of genera have actually been found. Of course a large number of genera have been found, or the evolutionary evidence would not be there.

Yours sincerely,

H. S. SHELTON.

P.S. I have given you a reference to Professor Owen's discovery which I think is good enough (H. 269).

DEAR SHELTON,

You seem to think that because you infer evolution from ignorance, I infer creation from ignorance. In fact I infer creation because of the great weight of evidence against the only alternative to creation, namely evolution. The geological record (I use the term in the sense that Darwin and all geologists use it), which has yielded millions of fossils has not yielded a single fossil essential to the proof of the evolution theory, and as I shall show in my letters to follow, in the words of Maurice Thomas (*Revue des Questions Scientifiques* (1940) p. 333) : 'The whole animal kingdom rises up against the transformist conception.' The difference between us is that I rely on the fossils that have been discovered, you rely mainly on those that have not been discovered and which you imagine exist or once existed.

In my letter (p. 107) I called attention to several of your claims to preferential treatment by the jury. Your last letter enables me to notice a further claim by you, viz. you may draw inferences from the absence of fossils but it is '*entirely illegitimate*' (Italics yours) for a creationist to do so. My inference from the absence of fossils in Pre-Cambrian rocks is that animals were not in existence when those early rocks were laid down : your inference is that animals were then abundant and much diversified, because the evolution theory requires this. You say that the transition from recognisable to unrecognisable fossils is 'a little abrupt.' A little abrupt indeed ! Hundreds of thousands of well-defined fossils, in rocks in all parts of the world, fossils representing all the phyla except the vertebrates—then suddenly a great thickness of rocks in all parts of the world, in which, in your words, not a single 'recognisable fossil' has been found. To call this change a little abrupt is in keeping with your assertion that the differences between a 'family and an allied family is very much less than the difference between Equus and Eohippus.' Having completely failed to substantiate this last statement, you not only claim to have proved it but you ask what further proof does any one want ? It is *proof*, not further proof, that everyone wants, and there is only one way of furnishing proof that would convince a court of law or any sensible person and that is complete lists of the differences between cats and civets on the one hand and Eohippus and Equus on the other. You have not given these lists, but knowing that the former list is much the longer, you say (p. 112) it is of no consequence whether I call these differences 5, 50, or 500 ! Further you say

that I gave a list of these differences. But this is precisely what I refused to do. I am however prepared to do so if you will allot me 1,000 of your allowance of words in this debate that the list would entail.

It would have saved much space if you were to reply to what I said instead of to what it would be convenient to you that I should have said. You incorrectly say I raised this matter of civets and cats. It was you who did this owing to your inability to produce a series of fossils linking two families. On top of all this you affect to express surprise that you have not made the editor and me change our views. You have strengthened these by showing that no series of fossils links two families. I note that because the geological record does not give positive evidence for evolution you take comfort in the fact that it does not give *positive* evidence against evolution, i.e., because the record does not prove a negative! You affect to be unable to understand what I mean by 'fossil record' and 'types.' By 'fossil record' I mean what Darwin and all geologists mean by the term. As to types, I named ten of these of which the earliest known fossil is a fully-developed member of the group to which each belongs. You say there is no evidence that Carboniferous spiders spun webs, but there is proof that they possessed the apparatus for spinning webs.

I note that you have not accepted my offer to pull to pieces the opinions set forth in the appendix to your last letter, but you quote more opinions. I will deal with these if you allot me a further 500 words for the purpose. You do make one definite assertion, viz., the earliest known tortoises had teeth. In fact no known tortoise or turtle has teeth in the jaws: one Upper Triassic genus, Triassochelys, had some teeth, not on the jaws, but on the roof of the mouth. As to teeth being 'a frightful nuisance to the anti-evolutionist,' he is not obliged to believe that in the Tertiary period horses exchanged rooted teeth for rootless ones, that at the end of the Cretaceous period every bird lost its teeth, as did an order of reptiles at some unknown period, and that while one order of reptiles was losing its teeth another exchanged its homodont dentition for a heterodont one.

You ask: does the fact that all we know of Tritylodon is the greater part of the skull without lower jaw affect the value of its evidence? I answer 'Yes.' Practically all the skeletal differences between reptiles and mammals are in the body skeleton, limbs, and lower jaw and the bones of the inner ear, also the presence or

absence of a quadrate bone. The only known fossil of Tritylodon tells us nothing of these characters, and no other fossil is known closely resembling it that might assist us in classifying it.

I note that you have not seen Owen's paper and not even repeated a secondhand version of what he said.

In your attempt to meet the argument against evolution afforded by very long-lived genera, you say a genus which is 'thoroughly adapted to a simple environment, especially in the sea, is naturally persistent.' But surely the great bulk of marine genera fulfil this condition. So why do some remain persistent, while others, according to the evolutionist, become modified into new genera, families, orders, etc.? In any case your explanation is unsatisfactory. Arca and Leda are two genera of molluscs now living in the sea round Great Britain. The fossils show that these have persisted unchanged from the Ordovician in the case of the former and the Silurian in the case of the latter, i.e., for some 400 million years. To-day these genera occur in most parts of the world at varying depths from low water on the shore to 250 fathoms. Arca to-day occurs in such different environments as Prince Regent Inlet and the muddy waters of the Jumna in N. India about 1,000 miles from the sea. Moreover the fossils of these genera indicate that during most of the 400 million years of their existence they have had a wide distribution and so have all along been exposed to very different conditions in various parts of their range.

You declare the geological record to be very incomplete, at any rate before the Tertiary Period, but Table VI shows that of the 174 genera of molluscs having shells now living in British seas no fewer than 64, i.e., more than one-third, were existing in the Jurassic period. Yet, according to you, most of the rocks laid down in the Jurassic period have been destroyed with the fossils they contain. If two-thirds of these have been destroyed then the 64 genera of which fossils have been found, as above, represent only about one-third of the genera now living that existed in the Jurassic period; in short no British mollusc has undergone any evolution since the Jurassic period. In truth you are on the horns of a dilemma. If the geological record be very imperfect, there has been no evolution among molluscs for some 100 million years. If not very imperfect the absence of fossils intermediate between families, orders, classes, etc., is fatal to the theory.

Yours sincerely,

D. DEWAR.

4

GEOGRAPHICAL DISTRIBUTION

DEAR DEWAR,

This section on geographical distribution is best taken now because of its connection with the geological record which we have just discussed. We here find a series of facts which are of special interest in that their collection was the main life work of Alfred Russell Wallace, whose contribution to evolutionary theory was second only to that of Darwin. Indeed it was these facts that did much to convince both Darwin and Wallace that evolution had certainly occurred. Thus, though it is ancient history, it is very relevant in a discussion with one of the few competent zoologists who still remain to be convinced of the truth of this important conclusion of modern science.

There is one point which it is well to make clear from the outset. It is not all the facts of geographical distribution which are relevant. Many of them are neutral, and this applies in particular to most of the facts concerning the distribution of existing species on the continental mass. Where you have no impenetrable barriers, such as the ocean or high mountains, a species may distribute itself anywhere where climatic and other conditions are suitable, and generally speaking such facts as these are of no use in our discussion. Similarly the distribution of most species of birds and bats, to which, as they have good powers of flight, the ocean is no barrier, need not be considered. As a rule the same applies to the distribution of sea fish, which have almost unlimited powers of migration, though even here the species differences between fish on the Atlantic and Pacific coasts of America are not without evolutionary interest. Generally speaking we can put it that where migration is possible there is very little obvious evolutionary evidence, and it is only when there are barriers to migration, and have been for a considerable time (geologically speaking) that we find the evidence we need. For that reason we must concentrate our attention on islands, lakes, and other habitats where animal and vegetable life has been isolated for a considerable time.

Before doing this it is well to mention in passing how the fossils help us to explain certain facts of geographical distribution which might otherwise seem anomalous. For example, the tapirs are now confined to South America and the districts round Malaya. The camels are Asiatic species, and the nearly allied llamas are natives of South America. It might at first seem strange that these are genetically related. But the well-known fact that fossils of both families have been found in intermediate regions clears up the difficulty.

In general what I shall attempt to show is that the geographical distribution of life is in accordance with what is expected if evolution has occurred, and that, if the forms of life were specially created, this has been done in such a way as to make it appear that they have been evolved. Then, according to our agreed principles, special creation can only be regarded as an unreasonable hypothesis.

We will first consider oceanic islands which are believed either never to have been joined to the mainland, or, like New Zealand, the junction of which, if it occurred at all, was in times inconceivably remote. Australia, which has been separated from the continental mass since Mesozoic times, is nearly but not quite in this category. The higher forms of life from amphibia upwards *ex hypothesi* were evolved on the continental mass, which includes both the Old World and America, as connection at times has been made at the Behring Straits. A rise of a few hundred feet accounts for this, very little more than that required to join England to the Continent, a junction which I think you will agree has occurred in recent times. We shall expect, therefore, that the higher vertebrates will be distributed all over the continental mass, but that in the islands there will be a great dearth of them, the exceptions being such beings as birds and bats which fly, and rats and mice, and possibly dogs, which travel with man to such islands as are, or have been, inhabited.

This is exactly what occurs. The one mammal found in New Zealand was a rat. The only placentals in Australia were bats, mice and the dingo dog. Most of the islands have no indigenous mammals except bats. We need to be a little cautious about asserting the impossibility of distribution from the mainland to the islands. Drifting timber carries many forms of life and particularly eggs. Polar bears have been known to have been carried to Iceland on the ice. Eggs of some creatures (e.g. snails) have been found attached to the feet of birds. But I know no case of the

existence of vertebrates on islands which cannot reasonably be accounted for by distribution from the mainland. In this study of oceanic islands, I will particularly call your attention to the scarcity of amphibia, particularly those very common specimens the frogs and toads, the eggs of which are destroyed by sea-water, compared with lizards, the eggs of which are not so destroyed. There are, for example, no indigenous frogs in Madeira. They have since been introduced and flourish exceedingly, making night hideous with their croaking.

It is interesting also to note that very isolated island St. Helena. This island lacks even land birds (except one wader) and reptiles. On the hypothesis of special creation, it does seem strange that, with such an enormous variety of the higher forms of animal and vegetable life, none appears to have been created on the islands.

More striking even than these significant absences are the forms of life that are present. On the hypothesis of evolution we shall expect that, when islands are easily accessible from the mainland, the forms of life will be very similar on account of continual interbreeding. If there is reason to suppose that the time when such access was possible is not very remote we shall expect the differences to be slight. When there is reason to think that the connection with other bodies of land, if it occurred at all, was in times exceedingly remote, we should expect the animals and plants to evolve on their own lines and to produce special and peculiar forms. Yet, though peculiar, we shall expect them to bear some relation to those on the mainland, or at least to those existing on the mainland in past ages.

This is exactly what occurs. There is a considerable similarity in the fauna of the East Indies adjoining Malaya, including Borneo and Sumatra. But Celebes, which is surrounded by channels of deep water, is peculiar. It contains, for example, a peculiar kind of ape, a remarkable buffalo, and an extraordinary creature related to the swine family. Once again, in the chain of islands east of Java there are no considerable distances till we come to Timor, but between Bali and Lombok there is a deep water channel indicating the improbability of junction in recent geologic time. By a strange coincidence there is a striking difference in the forms of life between Bali and Lombok. In St. Helena half the insects, three-quarters of the snails and four-fifths of the flowering plants are peculiar.

The Galapagos Islands have been described frequently. Nearly all of the land birds are peculiar to the islands. Yet, though different species, they are related to the birds of South America. One or two, very widely distributed, and apparently with exceptional powers of flight, are identical. Why, if they were specially created, should there be land birds on the Galapagos but not on St. Helena, and why should they be related to those of South America rather than to those of Europe? We must not forget also that the fauna of these islands differ considerably among themselves. There are distinct species in the separate islands, but these species are more nearly related among themselves than they are to any others. How well the facts fit in with the hypothesis that these birds are the descendants of stragglers blown over from South America by storm winds at odd times during vast ages.

Everywhere in isolation peculiar forms of life are to be found, but not so peculiar that they cannot easily be accounted for by evolution from pre-existing forms. The most striking example is Australia. It is not fully known how it happened that the ordinary mammals never gained a footing in that continent. As you will probably enlarge on this lack of knowledge, I will say nothing about it till you have had a chance to speak. Whatever the reason may be, the placentals did not get a footing there, and the result has been a remarkable evolution of the marsupials. The simple marsupials found in the rocks of remote epochs have evolved and flourished exceedingly, producing vegetable feeders, flesh eaters, and even a colourable imitation of the flying squirrel. Further evidence that they have so evolved is provided by the geologic record. Tertiary formations in Australia are not common, but those that have been discovered show no remains of placental mammals, only marsupials. These marsupials are not the same as those existing to-day, but they are marsupials none the less, and belong to groups now living in Australia. As you are an expert in birds, I need not point out the peculiarities of Australian bird life. Needless to say, owing to the power of flight, species are found similar to those on the continental mass, but the great variety of birds peculiar to this area needs only to be mentioned.

The Sandwich Islands are another good example. Wallace tells us that three-quarters of the land shells belong to peculiar genera, and that one sub-family (Achatinellinae) is confined to this group of islands. Also 352 species and 99 genera of beetles are peculiar to the islands. In this case the introduction of the ancestral beetles

must have occurred a long time ago when the distribution of the nearest islands was probably very different.

Equally suggestive is the fauna of South America. This is an enormous tract of land, sometimes isolated, and sometimes as at present open to invasion from the north. In South America, as in Australia, the fossil forms are related to those now living, not identical, definitely distinct species, but of the same general type. It should be pointed out that this similarity is powerful evidence for evolution. On the hypothesis of special creation there is no reason why the present should bear this relation to the past. If the old mammals died out and a new lot were created, there is no reason why the later ones should be of the same type as the earlier ones.

Nor is it only in land animals that unusual evolution occurs in isolation. The classical example of a similar occurrence in isolated bodies of water is Lake Baikal. The fish of that lake, needless to say, are peculiar, but still more so are the crustacea. The crab and lobster type seems never to have reached this lake, but in its place, occupying the same niches in the economy of nature, is a remarkable series of Gammarids. Several hundred species are known. On the hypothesis of special creation, assuming that a special fauna is created to people an isolated lake, why should they be Gammarids? We could as easily expect types bearing no relation to species elsewhere.

I need not emphasise that plant life, like animal life, shows the results of isolation. The extraordinary variety of peculiar flora in Australia and New Zealand needs only to be mentioned. Yet, though peculiar, they belong to the same classes as the plants of the continental mass, and usually to the same orders. Another peculiarity of the flora of isolated islands is illustrated by the Azores. Not only are there many species peculiar to the islands, yet related to those of the African mainland, but the kind of species is suggestive. They are species with wind-blown seeds, and species with small seeds readily carried in the intestines of birds, and, generally speaking, species of wide distribution. There is a remarkable absence of species with large heavy fruits, the carrying of which across the ocean is much less likely. On the hypothesis of special creation we should naturally expect a proportion of this type.

This will suffice for a first letter. We can briefly sum up by saying, assuming that evolution occurs, that in isolated districts species will adapt themselves to the peculiar conditions, whether

of climate, food or enemies. We should therefore expect to find just what we do find, namely, species related to those in the neighbouring districts, but different in detail. Also, though there is no absolute rule, we should in general find, in districts isolated for a long time, greater differences in the forms of life from those on the mainland than in those isolated for a shorter time. Evolution explains all these things, and I know of no other explanation.

Yours sincerely,

H. S. SHELTON.

DEAR SHELTON,

Your statement on p. 140 betrays a complete misunderstanding of our 'agreed principles.' You are referring, I assume, to pages 10–13 of the introduction, which the reader should re-read.

The main difference between us is, as I have already pointed out, that my approach to the problem of creation is inductive. I do not pretend to your sources of illumination. I do not know enough about the Creator to say whether or not creation took place only on the mainland. I agree, however, with Charles Kingsley (and this is my only deviation from the inductive to the deductive method) that God would not write on the rocks a monstrous and superfluous lie. Where, for instance, I find that the rocks are eloquent in their testimony to the suddenness with which a great marine fauna appears, I do not believe that God would have written on the rocks a lie so strongly suggesting a great creation at the beginning of the Cambrian period, if these animals had evolved gradually.

But I never agreed that, where the evidence is equally easy to reconcile with two hypotheses, creation and evolution, God must be held to have suggested falsehood because one of these two hypotheses cannot be true. It is odd that a distinguished contributor to *Mind* should have been unable to distinguish between two such very different positions.

In your letter you mention no facts that are incompatible with special creation, but you cite some facts against evolution!

You believe that every species of each Order, and every Order within a Class, is derived from a common ancestor. If this were the case, the geographical distribution of animals should show clearly the locality in which each Order of a Class originated and how the species of each reached their present habitats. But I put

it to you that it is not possible to do so and this is a serious objection to the evolution theory.

The Amphibia are a small class formed of only three Orders and 20 Families. Here are a few of the difficulties you encounter if you expect the evolution theory to account for the present geographical distribution of the Class. The Coecilians (legless, worm-like, burrowing amphibia) occur in America from Mexico to Peru, Tropical Africa and the East Indies. How did they come to be thus distributed? One genus, Dermophis, is composed of 6 species, of which 5 inhabit America and 1 West Africa. Another genus, Uraeotyphlus, is made up of 3 species, 2 of which live in the Malabar Hills of South India and 1 in West Africa. Of the tailed amphibia, the genus Amblystoma has several species in N. America and 1 in Siam. Among the frogs, the family Liopelmidae is composed of 2 genera, one of which is found only in New Zealand and the other in the N.W. corner of the U.S.A. The Dentrobatidae is composed of 2 genera, one of which is confined to Madagascar and the other to S. America. Nearly all the species of the Cystignathidae live in Australia and Tasmania, but a few occur in America south of Mexico. In my view, these facts fit the theory of creation much better than they fit the evolution theory. If you disagree, please say where each of the above groups originated and how they reached their present habitats.

In my view, the facts of geographical distribution are unfavourable to the evolution theory because they show :

I and II. That some species and some genera are very stable.

III and IV. If ever a Family or an Order evolved from a pre-existing one, the process must have been so slow that the earth has not been in a habitable condition nearly long enough to allow a Protozoan to have evolved into a mammal.

I. *Evidence of the Stability of Species.*

The stability of many species is shown by the wide area over which they are spread. As an example, let me cite the wingless insect known as the spring-tail. The species Isotomurus palustris occurs in all parts of Europe (including the British Isles, Sicily and Sardinia), in Siberia, Nova Zembla, Spitzbergen, Bear Island, Greenland, Ellesmere Land, Canada, U.S.A., Mexico, Costa Rica, British West Indies, Cuba, Argentina, the Azores, Algeria and Mesopotamia. The individuals living in, say, Siberia, must have been isolated for a very long time from those living in the Argentine

and have been subjected to very different climatic conditions, yet there is no difference in their appearance. No one shown a springtail of this species could tell whether it lived in Cuba or Spitzbergen or any other particular locality. A very large number of species of animals of all kinds exist, of which the geographical range is great; and in many cases the range is discontinuous, e.g., that of the snake Polydontophis melanocephalus, which occurs, as far as is known, only in the Malay Peninsula and Archipelago, Comoro Islands, Madagascar and Central America.

The facts of geographical distribution show that, if new species do arise by evolution, the process is very slow. Dr. F. E. Zeuner has made a careful study of the distribution of a group of swallow-tailed butterflies living in the Malay Peninsula and Archipelago. He describes about 70 species of these, some of which are confined to one island. He believes that these are all derived from a common ancestor, and have arisen in consequence of their isolation on these islands, and, taking into consideration the times at which various parts of the area have been under the sea and re-emerged, he writes ('Systematics of the Troides and its Allies,' *Trans. Zool. Soc.* (1943), p. 174): 'One will be fairly close to the mark . . . if one accepts a period of 500,000 to one million years as the time for the evolution of a "good" species.'

Butterflies are land species. Those who have studied the geological and geographical distribution of animals have come to the conclusion that the evolution of a new marine species takes much longer than that of a land species. Thus E. Mayr writes (*Systematics and the Origin of Species* (1942), p. 223): 'Speciation in marine animals moves at a snail's pace as compared to that of terrestrial animals. The connection between the Atlantic and the Pacific oceans (at Panama or Nicaragua) was interrupted some two or three million years ago, but some of the species of fish and crustaceans are still the same on both sides of the Isthmus of Panama. . . . The palaeontology of marine animals indicates the same slow evolution as does the existence of so many bipolar species.[1] The speed of evolution should not be overrated, even in terrestrial groups. . . . Many of the insects of the mid-Tertiary amber cannot be separated specifically from living species, and even an amber fauna believed to be Cretaceous was found to be remarkably similar to living forms.'

I do not know what time-scale Mayr uses in saying the connection between the Pacific and Atlantic oceans was interrupted 2 or

[1] Species found in both the Polar Seas and nowhere else.

3 million years ago. Scott, on the strength of the mammal fossils, writes: 'In early Pliocene times the junction between the two continents (N. and S. America) was re-established and has continued to the present day.'

According to the time-scale we are using, the Pliocene period began 15 million years ago, and this would mean that the communication at Central America between the Atlantic and Pacific was cut off some 12 million years ago. The mid-Tertiary amber to which Mayr refers, on our scale, was formed about 30 million years ago.

II. *Evidence of the Stability of Genera.*

Very little work seems to have been done with a view to discovering how long it takes to change one genus into another, presumably because, if such change has actually occurred, it is so slow as to make it difficult to estimate the pace. But the data of geographical distribution show that many genera are very stable in that their geographical range is great. Thus the White-ant genus Eutermes ranges from Africa to Australia, the lizard genus Gymnodactylus occurs in South Europe, South Asia, Australia, the islands of the Pacific and Tropical America. The genus Crocodilus (crocodile) is found in Africa, South Asia, North Australia and tropical America. The genus Tapirus (tapir) occurs only in Central America and Malaya. In some cases the fossils confirm the antiquity of these wide-spread genera. Thus, fossils of Tapirus have been found in Lower Miocene deposits, proving its existence at least 30 million years ago. In the case of Crocodilus fossils have been found in the Eocene of England, showing that this genus was in existence over 50 million years ago, while doubtful remains in Upper Cretaceous rocks suggest an antiquity of about 100 million years.

III. *Evidence that casts doubt on the idea that one Family has evolved from another.*

Many of the oceanic islands have been in existence a very long time, nevertheless all the animals on them belong to a Family now living on the mainland. In other words, *no new Family has evolved on any oceanic island.* The nearest approach to a Family peculiar to such an island are the flowerpeckers of the Sandwich Islands, which are deemed to be a sub-family of the Drepanidae. Some of the 'splitters' among systematists have raised these Sandwich Islands flowerpeckers to the status of a Family, but it is improbable that

this change in nomenclature will ever be generally accepted. In any case, the fact that this group of flowerpeckers now exists only on the Sandwich Islands does not necessarily mean that it never existed on the mainland. The Sandwich Islands flowerpeckers may be descended from mainland members of the Family that found their way to the islands when the Family was living on the mainland.

As oceanic islands are isolated and, *ex hypothesi*, were originally uninhabited, they are the spots at which evolution should be most rapid, because for a time there would be little competition for food and no enemies to fear, so that variation could run riot without being subjected to the pruning-hook of natural selection. Thus the fact that no new Family seems to have evolved on any oceanic island suggests that it is not possible for one Family to become converted into a different one.

IV. *Evidence that casts doubt on the idea that one Order has evolved from another Order.*

The mammals of Australia furnish this evidence. As you have stated, they all belong to one Order—the Marsupialia. Evolutionists seek to explain this by assuming that mammals evolved on some part of the mainland far from Australia, first the marsupials, then the placental orders. After the marsupials had found their way to Australia, but before the placentals could do so, Australia became separated from the mainland. Geologists agree that Australia became separated from Asia in the Cretaceous period, i.e., about 100 million years ago ; therefore, the Australian marsupials have been separated from all other land mammals for that length of time. But, all the known fossils of marsupials, other than those found in Australia, occur in Europe and North and South America and Africa. *Not a single fossil of a marsupial has been found anywhere in Asia.*

Clearly then, if Australia derived her mammals from some other continent, that continent must have been one in the far south. As the ocean south of Australia is very deep, if Australia was ever joined to a southern continent, the severance must have taken place much more than 100 million years ago. Thus marsupials must have existed in Australia during an immense stretch of time. But—and here is the fact which I consider fatal to the evolution theory—all the Australian indigenous mammals are members of one Order. Thus an immense period of, I should say, more than 200

million years has not been long enough for the evolution of a new Order of mammals. Nor have any ruminants or bat-like or marine mammals evolved in that enormous period. If then it takes more than 200 million years for a new Order to evolve, ten times as long, or over 2,000 million years, is needed for the evolution of a new Class and more than 20,000 million for a new Phylum to have evolved. But, according to the latest views—those of Prof. Holmes—the earth has not been in existence for more than 3,000 million years, and it is open to doubt if the earth has been habitable for half this period.

This time element is *all-important*, and in considering it I have taken care not to over-state the case against evolution ; my figures are almost certainly too low.

The lowest estimate of the time taken for the evolution of a new species is 500,000 years. As the differences between two genera of a Family are at least tenfold greater than those between two species of a genus, the minimum time needed for the evolution of a genus is 500,000 × 10, or 5 million years. As the differences separating two Families of an Order are ten times as great as those that separate two genera of a Family, 50 million years are needed for the evolution of a Family, and, for similar reasons, the evolution of an Order needs 500 million, that of a Class 5,000 million, and that of a Phylum 50,000 million years. These are startling figures, but, as Mivart pointed out over 70 years ago : 'It must be borne in mind that in tracing back an animal to a remote ancestry, we pass through modifications of such rapidly increasing number and importance that a geometrical progression can alone indicate the increase of periods which such profound alterations would require for their evolution through "Natural Selection" only' (*On the Genesis of Species*, p. 138).

The gradual conversion of a Protozoan into a Mammal involves the evolution of two Phyla—a simple Metazoan, such as a jellyfish and a Vertebrate (fish)—a matter of 100,000 million years. The conversion of a fish into a mammal involves the evolution of three Classes—Amphibia, Reptilia and Mammalia—requiring 15,000 million years, thus the whole process is a matter of 115,000 million years.

I maintain, therefore, that the facts of geographical distribution are either neutral or definitely against evolution.

Yours sincerely,

D. DEWAR.

DEAR DEWAR,

I have some difficulty in discovering the relevance of much of your last letter either to my introductory letter or to geographical distribution. So far as I can discover, you have made only two objections to my thesis. The first is that geographical distribution does not give evidence of evolution greater than that which can be reasonably well described as evolution within the family. In a general way this is true. We have to consider every type of evidence for what it is worth, and the greater part of the evidence from geographical distribution does not go further than this. There are exceptions but this is the rule. The other objection is that species on the islands may be only slightly altered from extinct species on the mainland. Again this is true, they *may*. If we have evidence that it is so, it must be accepted, but I think we can assert quite definitely that it is very unlikely to be the rule.

You mention other facts, not those brought forward by me, which you think unfavourable to evolution. In more than one case, I am unable to discover what the difficulty is. In particular, I find your remarks on the time needed for evolution not only irrelevant to geographical distribution but exceedingly obscure. When the habitable earth has existed for at least a thousand millions of years that does seem time enough for anything. When in the course of 80 millions of years or so our placental land mammals have arisen from a generalised early Eocene form (or forms) in which even the difference between carnivores and herbivores seems to disappear, the question of adequate time for the earlier evolution hardly seems to arise. I think you find your difficulty in the fact that at certain times, and with certain groups, evolution takes place very slowly if at all. I must therefore emphasise very strongly that there is no evolutionary necessity for a species to change. It may or may not according to circumstances. Whether it does or not is a matter for detailed investigation. But because in particular cases evolution takes place very slowly, this is no reason why it should not take place much more rapidly in other cases. I cannot see in the time scale any difficulty at all.

You also speak of discontinuous distribution and seem to find evolutionary difficulties there. Some of these, such as the camels and tapirs, I have already explained. Others may be difficulties, but they are not evolutionary difficulties. Polydontophis melanocephalus, a *species* of snake, has a very erratic distribution. As it is a species, I cannot see the evolutionary significance of this puzzle.

After admitting a certain probability to evolution within the family, you are surely not telling me that this species has been created two or three times over. This would surely be the *reductio ad absurdum* of special creation. Snakes, by the way, get distributed by drifting timber and even in commerce, but still this peculiar puzzle is wholly irrelevant.

I don't pretend to be able to solve every conundrum concerning discontinuous distribution, and it really is not necessary that I should. The example of the camels and tapirs is sufficient to show the lines on which explanation will no doubt be made out in course of time. As, however, I can find hardly anything in your letter which needs a specific reply, I will just mention a theory which has come to the fore in recent years which, if it should be definitely established, would do much to solve many of these difficulties, and particularly similarities in the forms of life in Australia, South America and South Africa. I refer to the theory of moving continents, first brought forward by Wegener, and developed by Du Toit and others, which is assessed very fairly in Holmes' recent book on *Physical Geology*. It appears that in later Carboniferous times there was an ice age in the southern hemisphere, but nothing similar in the northern, and that this ice age cannot possibly be accounted for if the continents were in their present position. Traces are found in eastern South America, South Africa, South Australia, and Southern India, and in Southern India the ice came *from the south*. It appears probable that these continents were joined together in one mass, and that some of them (particularly southern India, which belongs geologically to the southern continent) were in very different latitudes. Since then they, and also Europe and America, have slowly drifted to their present positions. When we go back to early secondary times, therefore, we have to consider not only the possibility of changes in the level of land and sea, but the different positions of the continents themselves. Incidentally, this may throw light on the existence of coal in places like Spitzbergen. Thus the connection between Australia and Asia may not have been through Malaya at all, and Madagascar may have been connected with Asia as well as with Africa. A great many puzzles are solved if this theory be accepted. There is much to be done before this theory of wandering continents can be either established or disproved, and, though I am personally inclined to regard the theory favourably, I am not contending that it is more than a theory. It can at least be said, however, with regard to the distri-

bution of forms of life which probably occurred in these early times, that no solution can be accepted as final until this matter is settled. For the present, therefore, we must exclude the earlier geographical distribution from our discussion.

All this, however, does not affect the plain evidence I gave you in my last letter for the contention that the geographical distribution of animals and plants which can be referred to Tertiary times is in accordance with the idea of evolution, and has no other rational explanation. For what it is worth, and within the limits I have stated, the facts of geographical distribution point clearly to evolution as their explanation, and no other explanation fits the facts.

Yours sincerely,

H. S. SHELTON.

DEAR SHELTON,

The land bridges which are alleged to have connected the mainland with islands now isolated were available for all land mammals whether specially created on the mainland or evolved on the mainland. But when I drew your attention to the fact that you have 'mentioned no facts which are incompatible with special creation' you distort what I have said and represent me as admitting that 'geographical distribution gives evidence of evolution within the family.' I said no such thing. Mere assertion is unimpressive. Instead of sweeping aside some of my arguments as 'irrelevant to geographical distribution,' you should have proved that they are irrelevant. Our readers will have noted that whenever you are unable to reply to one of my points you describe it as 'exceedingly obscure.'

I note that you emphasise very strongly that there is 'no evolutionary necessity for a species to change. It may or may not do so according to circumstances.' Please do not be so distressingly vague. What are the 'circumstances' which explain the fact that the molluscs Arca and Leda have continued to exist for 400 million years, to which I invited your attention on page 138?

I note that you have not challenged my assertion that the geographical distribution of amphibia fits the theory of creation better than that of evolution.

Wegener's theory is, of course, completely 'neutral' so far as

this controversy is concerned, and it would be interesting to know why you should suppose that it will provide any answer to the difficulties of reconciling geographical distribution with the evolution theory.

Yours sincerely,

D. DEWAR.

5

MORPHOLOGY AND CLASSIFICATION

DEAR DEWAR,

Our previous letters have cleared the issue. Among those who oppose evolution there are various grades of opinion. There are still a few who think the world has existed for only about 6,000 years, and who accept the first chapter of Genesis as verbally and literally true. You have already separated yourself from these. Although you do not accept as valid the results of experiments on radio-active substances giving an age of the order of a thousand million years for geologic time, you do admit an antiquity for this earth besides which the old estimate of 6,000–8,000 years is absurdly small. You accept in a general way the record of the rocks showing vast changes in animal and vegetable life during the geologic epochs. You are also not definitely opposed to what Mr. Arnold Lunn defines as micro-evolution, for you admit that a plausible case can be made out for evolution or differentiation within the family. Undoubtedly some of the most striking proofs of evolution take us no further than this. But I shall attempt to show that many of the main lines of evidence are equally available both within and without those limits. In this chapter and in those that follow it will be important for me to emphasise this type of evidence, and I shall be able to treat more briefly evidence of evolution within the family, the probability of which is admitted at the start.

Now let us consider some of the problems of morphology and classification. In dealing with classification, I shall try to avoid any recent developments which are based on evolution, and to use only modes approved by you, that is classification which was established before evolution was generally accepted. Let us note in the first place that all known fossil forms belong to existing phyla. There would be nothing contrary to evolution if this were not true. I know of no reason why a phylum should not become extinct. Still more important is it that all existing phyla date back at least to the Ordovician, and all but the vertebrates to the Cambrian. This is very extraordinary from the point of view of any form of special creation. Why this fundamental unity underlying the enor-

mous diversity? If there has been any form of special creation it certainly has taken place on very restricted lines. Descent with modification explains. I know of no other explanation.

I wish now to emphasise how different this is from anything in human manufacture which can possibly be regarded as an analogy as, for example, the editor's fighter-bombers. If we wish to design an aeroplane we do not use the same fundamental plan which applies to a motor-car, still less that used for a submarine. But that is exactly what occurs in nature. Fish, birds and beasts have the same fundamental vertebrate structure. One type of flying animal (the bat) and several types of aquatic animals have that more specialised mammalian structure adapted to life on land. More extraordinary still is the fact that two other phyla, mainly aquatic, which have developed hard parts, have representatives on land, and one group has conquered the air.

As you are well aware, pond snails, land snails and whelks are nearly allied. So also are the lobster and the wood louse. The scorpion, the extinct sea-scorpion and the king-crab are another group. That enormously numerous flying group—the insects—instead of having the vertebrate structure like other flying creatures, or a peculiar structure of their own, are classed as arthropods, a classification which asserts a distant similarity to the lobster and the crab. Even if we search the depths of the oceans and examine the creatures living under incredible pressures, in a darkness illumined only by their own phosphorescence, we find they are still vertebrates, molluscs and crustacea.

Facts like these (though not all of them) were noted before evolution was seriously considered. Linnaeus thought every species had been created separately. To him these similarities were unaccountable marvels. They are still marvels. The more the structure of living things is studied the more marvellous it appears. But they are not quite so unaccountable. Our clue, the hypothesis of evolution, throws some light on the mystery. These similarities do not fit the idea of units of creation. Assuming species, families or even orders to have been created separately, there is no intelligible reason why one group of flying animals (the birds) should be something like reptiles, a second (the bats) something like mice, and a third (the insects) something like crabs. These similarities are not surface resemblances, but the underlying plan on which the creatures are built. Even if we search the past to find if perchance some flying creatures had a structure specially designed for flying, we find one

more group and they were reptiles. We may express this concisely by saying that animal life bears no sign of a structure specially designed for the conditions of its existence; the main structure of the groups with different environment is the same, but there is evidence of special *adaptation*. Descent with modification explains. I know of no other explanation.

This appears to me a suitable place to remark once more that I regard the common mode of classification—varieties, species, genera, orders, etc.—merely as a scientific convenience. Flora and fauna show no such clear-cut differences as we find, for example, in the chemical elements. Even if we include the modern discovery of isotopes, these isotopes, so far as is known, are classes of things absolutely identical. With living creatures it is very difficult to say exactly what a species is; indeed, unless a species is so distinct as to be placed in a genus of its own, it is almost impossible. I believe your definition of a species is a mutually fertile group. That, in addition to being arbitrary, is defective in that there are all grades between complete fertility and complete infertility. The mule, as is well known, is infertile, but a very vigorous animal. The wild cat of the Highlands, on every other ground, would be classed as a distinct species, but Miss Frances Pitt has shown that it is completely fertile with the domestic cat. If the numerous varieties of dogs were found in fossil strata, and nothing else were known about them, they would be classed in different genera. In any case, the criterion cannot be applied to fossil forms. According to you, are lions and tigers different species? Ligers and tigons have been bred, but I am not aware whether they are fertile. The point that arises here is that conditions are exactly what would be expected on the hypothesis of evolution: namely, a great variability which makes the term species a useful, but rough and inexact, description of fact.

Classification and morphology are inter-related. We will now turn to the morphology. Similarity in morphology is called homology, of which there are all grades. Sometimes the homology is almost complete. A good example is the horse and the donkey, which are classed as different species mainly because they are not completely mutually fertile. But they are homologous, bone for bone, muscle for muscle, nerve for nerve, or at least nearly so. I make this qualification which, so far as I am aware, is unnecessary, in case you have been ingenious enough to find some obscure exception. The differences that exist are in the proportions of the

structures. As you are prepared to consider the family as a possible unit of creation, you may admit this homology as a sign of common descent. But, according to your view, the striking homology, nearly as exact, between man and the chimpanzee, means nothing at all. You would even go further and admit that the homology between the hoof of the horse and the middle toe of Eohippus probably indicates genetic relationship; but the homologies between the rabbit, the cat and the hedgehog, which are considerable, mean nothing to you. It is important to note that the argument from homologous structures, if it means anything at all, does not favour your hypothetical units of creation, but can reasonably be applied over the whole range of the vertebrates.

Numberless illustrations can be given. To mention any considerable number would be merely to write a treatise on comparative anatomy. A few well-known ones will suffice. The wing of the bird is homologous with the fore-limb of the reptile. Let us imagine that birds *were* separately created, and that for some unknown reason it was necessary that they should be vertebrates like the reptiles. The fore-limb of a walking animal needs a certain flexibility which is given by joints. The wing requires one joint only; the rest of the bones, to make an efficient wing, must be as rigid as possible. We should therefore expect the wing bones to be simple and rigid. They are rigid, but not simple. The bones of the reptilian fore-limb are there, quite recognisable, but fused together. This does not look like special creation, but it is intelligible on the assumption of descent with modification. In the extinct Archaeopteryx the wings are smaller and less efficient, the fusion of the bones is not complete, some reptilian features remain. I am not asserting that the Archaeopteryx is the direct ancestor of the modern bird. It may or may not be, I do not know. But it illustrates the argument from morphology, and shows the exceeding probability that the typical bird structure has been formed from the reptiles by descent with modification.

On this subject of morphology and classification, the 14th chapter of the *Origin of Species* is well worth reading even to-day. So is the 11th chapter of the second part of Spencer's *Principles of Biology*, and the whole of Part III on evolution. These chapters will show that the writers of the last century had reasonably adequate data. I take a few examples from Spencer. The first is similar to the fusion of the wing bones of the bird. The sacrum of the mammal, which provides the rigidity required to support the limbs, is given,

not by a single bone, but by a fusion of vertebrae. In man there are five fused vertebrae, in the ostrich a much larger number. Why, if specially created, is a rigid part formed by the fusion of vertebrae? There was in Spencer's time some doubt about the number of segments in the insect, but it was recognised that the number of 'somites' is constant. What the recognised number now is I do not know, but is it not strange that the same number of somites are required for the louse and for a stick insect a foot long? 'Why should one crustacean which has an extremely complex mouth formed of many parts consequently always have fewer legs, or conversely those with many legs have simpler mouths'? Why, indeed?

The following is from Darwin: 'What can be more different than the immensely long spiral proboscis of the sphinx-moth, the curious folded one of a bee or bug, and the great jaws of a beetle? Yet all these organs serving such widely different purposes are formed by infinitely numerous modifications of an upper lip, mandibles and two pairs of maxillae.'

It is very curious that correlations run in phyla, although there is no obvious reason why they should do so. The oxygenation of the blood is one example. All through the vertebrates the active compound is haemoglobin, and is carried in corpuscles. The actual chemical constitution of the haemoglobin may vary slightly, but one common feature is the oxygenation by the agency of the element of variable valency iron. Neither the iron nor the corpuscles are necessary. In most of the molluscs and crustaceans the compound is haemocyanin in which the element of variable valency is copper, and there are no corpuscles. Indeed, in one phylum the comparatively rare element vanadium is utilised instead of iron or copper. I am aware that, in the invertebrates, the constitution of the blood presents some difficulties, which it is possible you may wish to expound later. This does not affect the fact that, in a general way, the blood composition follows the phylum classification, and in particular in all the vertebrates the method of oxygenation is the same. On any other hypothesis but community of descent, this uniformity is very difficult to explain.

The same applies to the structure and function of the alimentary canal. In organisms so different as the man and the frog, the homology of the alimentary canal is functional as well as structural. The liver in both cases forms glycogen, the pancreas in both cases forms trypsin. The same applies to the glands. The adrenalin of

the frog is chemically identical with our own. None of these things are necessary. In the arthropoda, the structure, the functions, and the chemical compounds involved in the organs which roughly correspond are different.

I would submit that the enormous mass of evidence to be found in the homologies of the animal and vegetable kingdoms is of a kind to which it would be difficult to attach too much weight.

What is the principle underlying classification? Why are some features important and others unimportant? Before evolution was an accepted principle, zoologists and botanists had worked out a classification on empirical principles. By some sort of instinct they knew that certain resemblances such as that between the dog and the Tasmanian wolf were unimportant, and that others such as the existence of warm blood and mammary glands in the whales and in the bats were important. But why? Apart from the hypothesis of evolution no clear reason can be assigned. The basis of a sound classification is genetic relationship. Unless that is assumed, no sound basis can be found. Why, on any other grounds, is the current classification valid? Why, for example, is the pouch to hold the young found only in marsupials and not in all of those of such importance? It is a very simple adaptation which might easily have developed in different classes of animals. You will reply, of course, that the marsupial pouch is correlated with other differences. Agreed, but why should there be this correlation? I know of no answer to this question except community of descent.

We must try to make this argument from homology quite clear. Similarity in a single organ proves nothing. The eye of a cuttle-fish is strikingly similar to that of a vertebrate. Some of the extinct South American Litopterna had single hoofs like the horse. The electric ray and the electric eel are not nearly related. Similar needs sometimes give rise to similar structures in creatures widely separated by descent, though in such cases the structures are seldom morphologically identical. Such resemblances are known as analogies, and it is interesting to note that the distinction between analogies and homologies is not an invention of the evolutionists, but was formulated by that well-known nineteenth-century opponent of evolution, Professor Owen.

What gives evidence of common descent is that deep-seated fundamental homology of structure and function. When, as in the vertebrates, not only are the bony structures a series of modifications of the same fundamental plan, but the same is true of the general

character of the alimentary canal, the structure and function of the liver, the pancreas and the ductless glands, the chemical identity of many of their products, the chemical composition of the blood, the structure of the brain, the lungs and the kidneys, we are impelled to search for an explanation. These structures and functions are not necessarily implied in the nature of living things, nor are they necessitated by the physico-chemical environment. This is shown quite clearly by the fact that in the cuttle-fish and in the lobster, both fairly highly developed organisms, all these features are different. Then why do they occur in such vast multitudes of organisms? I know of no explanation but descent with modification.

These facts alone, if the fossil record had been entirely destroyed, would still show the hypothesis of evolution to be exceedingly probable. They all fit in. To make evolution the obvious explanation, only two other data are needed. One is the variability of organisms. That is obvious. For that fact we need look no further than our domestic dogs. The other is sufficient time for the changes to take place. That is the fact that Linnaeus did not know. When, instead of a few thousand years, a thousand million years is a reasonable estimate for the existence of life on this earth, the enormous changes that have taken place can readily be accounted for as the sum of many inconsiderable ones.

A little later in the discussion you will bring forward a number of difficulties, and ask me how this or that structure can be accounted for by descent with modification. I am not sure that in every case I shall be able to give a satisfactory answer. What I want to emphasise here is that they are *difficulties*: problems for future research. The main trend of the evidence is clear. The structure of animal and vegetable organisms is very much what we should expect it to be if formed by descent with modification. It is not what would be expected if species, families, or even orders, had been separately created. In the general structure of animal and vegetable life I can find no evidence for 'units of creation,' or for special creation of any kind. The general trend of the evidence is clear. I wish here to submit that this general type of evidence is of the utmost value, and that very great weight should be given to evidence of this kind, which in a detailed discussion is liable to be overlooked.

In conclusion, perhaps I may be allowed to say a word or two about the evaluation of evidence. In this vital question of evolution there is no such thing as direct evidence. Certainly the evidence

from geology, important as it is, cannot rightfully be so described. It is therefore important to point out that, of all this indirect evidence, these facts of morphology are perhaps the most significant *for both sides*. The facts are plain, and have long been known, at any rate in their main outlines. Their evolutionary explanation is simple and clear. Just as the Copernican astronomy displaced the old Ptolemaic theory mainly because it gave a clear and simple explanation in place of complicated cycles and epicycles, so evolution has displaced the older explanations of the facts of morphology (if there are any that can be so called), mainly because of its clarity and simplicity. We can dispense with your special creations in the same way as we can with the angels who were supposed to carry the planets round in their orbits. Both are superfluous. On your side, also, morphology is the only place where difficulties can be alleged which need to be taken seriously in a scientific discussion. You will no doubt expound some of them shortly. No line of evidence is all-important, but I think in morphology we find the most important, and the rightful starting place. That being so, I wish to emphasise the beautiful simplicity of the evolutionary explanation of many facts which otherwise have no explanation at all.

Yours sincerely,

H. S. SHELTON.

DEAR SHELTON,

None of the facts set forth in your letter are against special creation, but some are against evolution. It is to your interpretations that I demur, and to your analogies.

The designer of an aeroplane does, in fact, use an engine constructed on the same fundamental plan as that of a car. The fighter-bomber designer adapts the fundamental design of the bomber. No analogies are perfect, else they would cease to be analogies and become identities, and no analogies are more dangerous than those which argue from a human to a Divine Creator, but within these limits the analogies from human creators tell against you.

When you were invited to state why the Creator should start *de novo* and not make use of certain characteristics in certain types you replied loftily that this is a scientific discussion, but the whole basis of your last letter is, in effect, theological, and until you can explain why the Creator should not use 'the same fundamental

plan' in the creation of different types it is clear that much of your letter is not relevant to our discussion.

I am amused at the way in which you evolutionists repeat each other. Haldane made that silly remark about 'the angels who were supposed to carry the planets round in their orbits,' but neither he nor you name the astronomers, if any, who believed this. Actually, the pre-Copernican astronomers adduced facts in favour of their thesis, which apparently is more than you are able to do in support of yours. Moreover, their arguments seem to me as convincing as those whereby you defend evolution. I accept Copernican astronomy for the same reason that I accept special creation. Both theories explain far more facts than any rival theory.

You cite the similarities that run through the vertebrate phylum as proof of the descent of all the vertebrates from a common ancestor. From this it follows that, as you admit each phylum is constructed on a fundamentally different plan, the phyla are *not* descended from a common ancestor ; in other words, that each of these was specially created.

You admit that all the phyla were in existence in the Ordovician period. This is equally true of the aquatic Classes. Thus, no new Phylum or aquatic Class of animals has evolved since the Ordovician period. Since the Ordovician only 5 new Classes have appeared, and, these being all land Classes, if they existed in the Ordovician period or earlier, cannot have left fossils in the known Ordovician or Cambrian rocks because these are all marine. Two of these land Classes—the insect and Amphibian Classes—appear in the earliest known land rocks—the Devonian—i.e., in the earliest possible period in which their fossils could be preserved. After that, only three new Classes appear—the reptiles, birds and mammals—all of which, like the amphibia, belong to the Vertebrate Phylum. Hence, if you believe that these Classes are the products of evolution you have to believe that the Vertebrata were unique among all the Phyla in throwing off new Classes long after all the other Phyla had lost the power to do so. It would be strange if this were so. But this is not a sufficient reason for denying it is so. And this is where the morphological evidence comes in ; the morphological differences between the vertebrate classes are so great that a gradual transformation of one Class into another is, I submit, incredible.

Before demonstrating this, I must notice your statement that the basis of a sound classification is genetic relationship. This is refuted by the fact that chemists, although they classify crystals and

the chemical elements into 'families,' do not invoke the principle of evolution to account for these groups.

The present classification of animals was made by zoologists who were not evolutionists, and who when making it did not take the fossils into account ; nevertheless, it has been but little modified in consequence of the inclusion in it of the thousands of fossil forms since discovered. All these (even the oldest) fit in to the original Phyla and Classes. This last fact is most unfavourable to the theory of evolution. Had this theory been true, the fossils of the earlier periods would have completely revolutionised a classification of animals made without taking into consideration extinct animals.

As animals differ from inorganic substances in that they exhibit the phenomena of variation and reproduction, you may ask : may it not be that the causes of the diversification of the animal kingdom differ from those of the diversification of inorganic things ? To this my reply is, that a close examination of the morphological characters of animals gives no grounds for supposing this to be the case, because these characters show (1) that no Class can possibly have been gradually changed into another Class, nor a Phylum into another Phylum ; (2) that it is highly improbable that all the members that compose a Class are descended from a common ancestor.

Evidence that no Class of Animals can have gradually changed into another Class, or one Phylum into another.

1. *The alleged gradual transformation of a reptile into a bird.*

Lunn, in his introduction (p. 20), raised the difficulty of the evolution of feathers, which you tried to evade in your first letter. I drew your attention to this and in your reply you said that you must defer the detailed discussion until the appropriate chapter. This is the appropriate chapter and I request you to describe how a reptile could possibly have gradually developed feathers and become transformed into a flying bird. I also request you to take any ordinary reptile, either quadrupedal or bipedal, or, if you prefer, a marine reptile, and describe the main stages by which the fore-leg became gradually changed into a feathered wing like that of a bird. Please describe the mode of locomotion of this reptile-bird at each main stage of its transformation. Please show also the way in which its skeleton was adapted to its changing habits. For my part, *I cannot conceive how this supposed half-reptile half-bird could possibly survive while it was losing the adaptive advantage of its reptilian*

ancestor and before it acquired the characters that adapt every bird so admirably to its mode of life.

You, as an evolutionist, believe that mammals, like birds, gradually evolved from reptiles, and as I believe that such a gradual transformation of a reptile into a mammal is utterly impossible I invite you to deal with :

2. *The gradual transformation of a reptile into a mammal.*

Such a transformation would involve, in addition to a number of physiological changes, and changes in the soft parts of the body, fundamental skeletal changes. I will here deal with only the changes involved in the lower jaw, and the ear. These are :

(1) In every reptile, past and present, each half of the lower jaw is composed of six bones, in all mammals it is made up of only one bone.

(2) In every reptile the lower jaw articulates with the skull, not directly as in all mammals, but through the intervention of a bone, known as the quadrate.

(3) In all reptiles the drum of the ear is connected with the tympanum by a single rod-like bone, called the columella ; in all mammals this connection is by a series of three bones—the stapes, malleus and incus, so-called because they are in shape respectively like a stirrup, a hammer and an anvil.

(4) In the mammalian ear there is a very complicated organ—the organ of Corti, that does not occur in the ear of any reptile. The most striking feature of this is its 10,000 rods or pillars (into which run some 20,000 nerve fibres) set in two rows on a base ; each rod in a row leans towards its opposite number in the other row, so that their swollen ends or heads meet, the convex head of one fitting into a concavity in the head of its opposite rod. Thus a tunnel composed of some 4,000 arches is formed. Sound waves cause these rods to vibrate.

I maintain that the above changes cannot possibly have been effected gradually and, in consequence, the theory that a reptile ever became gradually converted into a mammal is untenable. If you disagree, please describe, stage by stage, how these various changes can have taken place, the order in which they occurred, and how the organ of Corti can have started, and describe its gradual development into its present condition.

As all reptiles, past and present, seem to have fared or to fare quite well with their present type of lower jaw and hinge to the

skull and with their present auditory apparatus, please state what caused one kind of reptile to get rid of five bones on either side of its lower jaw, to change the type of its hinge with the skull and to exchange its reptilian auditory apparatus for the mammalian type. Please explain how this reptile, while its jaw- and ear-bones were being thus reorganised, contrived to eat and to hear. Moreover, how do you account for the fact that, whereas thousands of fossils of reptiles have been found, all of these have six bones on each side of the lower jaw and one ear bone; not a single fossil has been found of a reptile having 5, 4, 3, 2 or 1 bone on each side of the lower jaw, and all have one ear bone and none 2 or 3 ear bones; also why no living reptile displays any of these peculiarities.

3. *The alleged gradual transformation of a Land Mammal into a Whale.*

It is an article of the evolutionist creed that the Cetacea are descended from land quadrupeds. I maintain that the transformation of a land quadruped into a whale or a whale-like creature cannot have been effected gradually because for a very long period the quadruped about to become a whale must have had a hipbone too small to serve as a base on which the hind legs could articulate, but too large to permit the muscles that move the great tail of the whale to be attached to the backbone, in consequence the unfortunate animal could neither walk properly on land nor swim well in the sea. Twenty years ago Vialleton pointed out that such a creature cannot have existed and that it is an illusion to search for the fossil of an animal mid-way between a land quadruped on the one hand and a whale or a dugong on the other. Such a fossil will never be found. I have repeatedly challenged evolutionists to describe or make a sketch or a diagram of a possible animal mid-way between a land quadruped and one adapted to swim in the manner that cetaceans and sea-cows swim. So far, this challenge has not been accepted; accordingly, I extend it to you. I do not ask you to describe the actual ancestor, but a *possible* one between a present-day whale or a sea-cow, or a Zeuglodon on the one hand and a land quadruped on the other. If you accept my challenge I shall be delighted to criticise your description or sketch. The matter of locomotion is by no means the only difficulty involved in the supposed transformation. The mother whale gives birth to her young one and suckles it under water: both mother and young have to be specially adapted to this. The mother has a receptacle into which she can secrete milk; this receptacle is provided with

muscles, of which the contraction forces the milk into the mouth of the young one. She has also a cap round the nipple into which the snout of the youngster fits very tightly to prevent it absorbing sea water with the milk she gives it. The young one has its wind-pipe prolonged above the gullet to prevent any of the milk ejected from the mother's receptacle getting into its lungs.

Please bear in mind (1) that no gradual transition is possible between the young one being born and suckled on land or in the air and in being born and suckled under water; (2) the latter would be impossible before the above apparatus was fully developed; (3) therefore, *all the modifications of both mother and baby would have to be complete before the first baby whale could be successfully suckled under water.* Thus you have to account for the pre-adaptation of both mother and young for suckling the latter under water and the complementary adaptation of parent and young, i.e., the size and shape of the cup round the mother's nipple to the size and the shape of the youngster's snout, so as to keep out the sea-water. Do you seriously believe that these mutual pre-adaptations of mother and offspring were the chance results of the accumulation of fortuitous variations in adult and young?

Yours sincerely,

D. DEWAR.

DEAR DEWAR,

I am sorry if I seem to plagiarise from Haldane. I did read Lunn's discussion with Haldane at the time, though I have not seen it since, and possibly some of Haldane's remarks may be at the back of my mind, though I have forgotten the source. If either here or elsewhere I am using arguments or analogies previously used by Haldane, I wish to make due acknowledgment. I cannot give you any definite reference to the angels carrying the planets round in their courses; but if you will read Dante's *Banquet* (there is a good English translation) you will find an account of the point of view of those who accepted the mediaeval construction. It is a very beautiful construction, and the only reason for abandoning it was that it was not true. The comparison of your ideas with those of mediaeval philosophers, which were at least coherent and harmonious, is unduly complimentary to you. Your ideas of special creation, in addition to not being in accordance with the knowledge we have, jars exceedingly both from the religious and

aesthetic points of view. They fit neither science, nor orthodox Christianity, nor Genesis, nor common sense, and can merely be regarded as a regrettable eccentricity. As I mentioned before there is a sense in which special creation will explain anything, which is merely another way of saying that it explains nothing.

It is very trying to deal with a letter so much of which seems to me pointless. For example you keep on telling me that I admit things which I do not admit at all, or which I positively assert. As an example of the first you tell me I admit that the phyla were specially created. Of course I do not admit anything of the kind. Their relationship is obscure, and, as they were evolved before the date of recognisable fossils, and probably before the evolution of hard parts, palaeontological evidence is not available. But they are not so different as to be beyond the reach of reasonable speculation, although it *is* speculation. Here is an example from the *Cambridge Natural History*. (Fishes p. 31) 'However improbable it may appear at first sight it is possible to hold the view that Balanaglossus is related at the same time to vertebrates and to star-fishes and other echinoderms.' Brachiopods are a puzzle from the point of view of the phyla, and it is difficult to know where to put them. Peripatus is intermediate in structure between worms and arthropods. In its embryonic structure it simulates the worm. Certainly the relation between the phyla is obscure, but erratic forms show *possible* ways in which they may be genetically related. The worm form, for example, is a possible ancestor to several phyla.

You also say I 'admit' that all phyla were in existence in the Ordovician. Why admit? I believe I *asserted* it. That is good evolutionary evidence. Phyla do not come into existence in an erratic and unaccountable way, as might well happen if they were specially created. Why should the vertebrates succeed in 'throwing off new classes?' Surely it is obvious that the change from life in the sea to the full conquest of the land implies fundamental morphological changes. Classes are not 'thrown off'; they take hundreds of millions of years to evolve. You mention again the surface incredibility of the process, but I need not add anything to what I said in the first chapter on that aspect.

Now let us pay a little attention to your special difficulties:

1. I can't make sense of some of your remarks about classification. The fact that extinct organisms fit fairly well into the same classification as living organisms is good evolutionary evidence. If they did not it would be a serious difficulty.

2. You must state your question about the bird and the reptile a little more clearly. I have already given you a speculative theory of the origin of feathers. I have suggested the origin of flight by the analogy of the flying squirrel, different species of which, by the way, vary considerably in their powers. Of course I can't tell you the mode of locomotion at each stage, though I think it highly probable that the birds were descended from a bipedal reptile. It is well also to repeat that Archaeopteryx had *not* acquired many of 'the characters that adapt every bird so admirably to its mode of life.'

3. I have already pointed out that fossil forms have been discovered intermediate between mammals and reptiles in the structure of jaws and earbones.

4. The question of the *possibility* of the evolution of the whale from a land mammal is answered to a considerable extent by living forms. The manatee, for example, suckles *in* water but not *under* water, which suggests the mode of transition. Obviously the original whales would be comparatively small creatures, and most of the difficulties, which arise because of the abnormal size, are irrelevant. Seals have hind limbs under the skin (with of course a pelvis) and sea lions have small functional hind limbs, which do not interfere with the swimming. I note here also another of your undignified 'challenges.' This is a brief answer to your difficulties so far as you have stated them. I think there is some point about the swimming which you have not made clear. I should like to deal with it, but you must state a difficulty clearly before I can answer it.

I will conclude by repeating the most important point about classification. You do not seem to understand it, and certainly have made no attempt to answer it. What is the principle on which you classify living things? Why are some obvious differences such as size and shape unimportant? Why, for example, is a whale classified as a mammal and not as a fish, despite the surface resemblances to the latter? The point that I am putting to you is that by a process of very subtle insight the earlier biologists found a principle which they could not clearly enunciate or explain, and which indeed could not be clearly explained in terms of the knowledge of the time. When you say that the forms of life were classified *as if* they were genetically related, the principle is clear, and I do not think it can be expressed clearly in any other way.

Yours sincerely,

H. S. SHELTON.

DEAR SHELTON,

I am amused that you should take your views on mediaeval astronomy from a poet who makes no pretence to be writing science. I am glad that you have made so clear the contrast between your approach to ultimate problems and my own. I lack your robust confidence in your power to impose on the Deity a mode of creation which accords with your particular religious and aesthetic preference. Lacking your private sources of illumination I prefer the inductive approach to this problem. I do not handicap myself with aesthetic criteria (for the bias in favour of one fact of creation rather than several is in essence aesthetic) but go to Nature to try to discover what happened. The question at issue is whether the great groups of the animal kingdom appeared suddenly or evolved gradually. That is a strictly scientific question and those who start on their researches unhampered by *a priori* dogmas are more likely to reach the truth than those who flatly refuse to consider any evidence which conflicts with those prejudices.

I notice in your letters an increasing tendency to substitute assertion for argument, opinions for facts, and to evade discussion by loud and confident pronouncements as to the intellectual inferiority of those who disagree with you. The question whether special creation is inconsistent with science and common sense is a question which cannot be decided, as you seem to suppose, by your *ex cathedra* pronouncements. *Shelton locutus est causa finita.* I am amused that Pope Shelton should arrogate to himself the right to assert that a belief that was the traditional belief of Christendom for nearly nineteen centuries is inconsistent with 'orthodox Christianity and Genesis.'

Your refrain 'there is a sense in which special creation will explain anything' would be as impressive as it is melodious had you made the *slightest* effort to suggest a natural mechanism whereby all living things could have evolved.

You describe my challenges as 'undignified.' Why? This book arises out of a challenge to you to defend your beliefs. There is nothing undignified about issuing a challenge but there is something very undignified about evading a challenge. You have evaded the three challenges I issued in my last letter, I therefore repeat them. Before coming to them I have a few comments to make on the early part of your letter. You assert that I assert that you admit that the phyla were specially created. That is not so. I pointed out that you had made an assertion, the logical consequence of which was that all the phyla were specially created.

I had very good reasons for saying that you 'admit' that all the phyla were in existence in the Ordovician. We are arguing a case and your statement or assertion to the above effect, being very damaging to your case, is in legal parlance an admission. There is another reason; you asked my authority for saying the vertebrates existed in the Cambrian, implying that you did not admit this. In consequence, instead of basing my argument against evolution on the existence of all the phyla in the Cambrian, which you might have been inclined to contest, I used your admission by basing my argument on the existence of all the phyla in the Ordovician. Clearly some of my statements are too subtle for you.

Many thanks for your statement that Classes take 'hundreds of millions of years to evolve.' If the evolution of a Class involves hundreds of millions of years, that of a phylum involves as many thousands of millions. Now, according to Holmes (whose figures we are accepting as a basis in our discussion), 'all the evidence is in harmony with the conclusion that the earth is between 1,600 and 2,000 million years old.' But certainly all the phyla (except possibly the vertebrates) were in existence in the Cambrian period about 500 million years ago, i.e., from 1,100 to 1,500 million years after the beginning of the earth; but during a considerable portion of this period the earth was not fit to sustain life. Hence the millions of years required for the evolution of the phyla are not available.

Now for your evasions of my challenges. The first related to the gradual change of a reptile into a bird. You require me to state this a little more clearly. In fact it was quite clearly stated, but it gave you a loophole of escape of which you have failed to take advantage, for I asked you to name the stages passed through during the conversion, and this gave you the opportunity of saying you could not say, because you were not present during the supposed stages. I accordingly now modify it to: Please describe the main stages by which the fore-leg of a reptile *could* be gradually changed into a feathered wing like that of a bird. This request, while doubtless very embarrassing to you, is perfectly clear. You therefore cannot evade it by saying it is not clear, an intelligent child of twelve could understand it. While on this point I request you to name a few of 'the many characters adapting it to its mode of life' that were lacking in Archaeopteryx.

You drag into this part of the discussion the flying squirrel, although it is neither a reptile nor a bird. In so doing you ask for

trouble, since you practically compel me to issue another challenge to you, viz., to describe a series of *possible* transitional stages between the patagium of the flying squirrel and the wing of a bird. The former is a parachute, the latter an organ of locomotion. Far from being an incipient wing, the presence of a patagium is an obstacle to the development of a wing because its existence necessitates an orientation of the forelegs incompatible with flight. It is incumbent to you to show that I am wrong.

In my second challenge I asserted that a reptile cannot have been gradually converted into a mammal and, in the event of your disagreeing, I challenged you to describe, stage by stage, how the various changes involved, including the acquisition of the organ of Corti can possibly have been effected. As you have failed to do so the jury have to construe this as an admission by you that such changes did not take place gradually. You assert that Scymognathus and Cynognathus had ear bones or an ear bone, (which was it?) intermediate between the condition in reptiles and that in mammals. Please state in what way the ear bone or bones of these creatures differed from the columella of all other reptiles.

You have certainly not met my third challenge regarding the gradual conversion of a land mammal into a cetacean or a sea-cow. The question I asked was so clear that a child of fifteen could understand it, yet you say it was not clear. Apparently in your strange vocabulary the expression 'not clear to me' is a synonym for 'baffles me.' In your efforts to meet this challenge you cite seals and sea-lions as morphological intermediaries between a land mammal and a whale. In so doing you have brought trouble upon yourself, in that you compel me to issue two more challenges; further you show that you have forgotten your anatomy. As the gradual transformation of a land mammal into a seal and a seal into a whale both appear to me to be impossible, I call upon you to describe briefly the nature of these changes and say what you think caused the animal first to change its mode of locomotion on land and then in the sea, and how it contrived to exist during the period in which its legs and body were being twisted.

I note that your anxiety to show sea-lions to be intermediate morphologically between land mammals and whales has caused you to draw upon your imagination in stating that sea-lions have small functional hind limbs, which do not interfere with swimming. The truth is that the hind limbs of sea-lions are large and robust, and, far from interfering with the swimming, they

are important organs of propulsion; by their lateral motion they propel the animal through the water. As regards the origin of phyla, it is possible to imagine that the vertebrates are derived from any of the invertebrate phyla, in consequence some transformists would derive them from worms, others from echinoderms, others from arthropods. As the vertebrates are so sharply marked off from all the other phyla, one evolutionist's guess at their origin is as good or as bad as any other.

In view of statements you make in your last letter I am not surprised that you cannot make sense of my remarks on classification. You in effect say: 'Gentlemen of the jury. The earliest recognisable fossils (which were laid down some 500 million years ago) show that the animals living at that time were divided into the present phyla and classes and these groups were as sharply separated as they are to-day. They show no tendency to meet or join up. From this my opponent infers that these groups were sharply marked off at the time of their origin. This inference is a "regrettable eccentricity" on his part. The fact that the various big groups of animals were as sharply marked off from one another 500 million years ago as they are to-day is proof that they are all descended from a common ancestor. What further proof do you want? The idea of the geometricians that parallel lines never meet does not hold in evolutionary science.'

Yours sincerely,

D. DEWAR.

DEAR DEWAR,

Your controversial methods are a matter for yourself and the editor, but I must acknowledge to finding them a little wearisome. All the same, in spite of the handicap of finding your arguments 'too subtle for me' (which I think they must be as many of them do not appear to me to make sense), I will do my best to give you such reply as is within my power.

The first point is theological. We can hardly keep theology out of the discussion as it is implied in the very idea of special creation. You appear to think that special creation 'was the traditional belief of Christendom for nearly nineteen centuries.' I do not agree, it seems to me quite modern. If you will read Genesis in its literal and grammatical sense you will not find special creation there. You will find that '*in the beginning* God created the heaven

and the earth' of which creation the rest of the chapter elaborates stages, after which 'the heavens and the earth *were finished* and all the host of them.' (Authorised Version.) Science obliges us to abandon the stages, which are certainly out of order, and the details, but the main statement may be true, and in any case is outside the scope of this discussion. The point to be noted is that up to the advent of man everything was a part of the original creation and there was no *special creation* about it. Anyone who believes Genesis to be anything more than a primitive speculation on the origin of things has to try to find a figurative interpretation. Your special creation is one of these, and I cannot see that it has any advantage over any other. In any case it is quite modern, and neither specially Christian nor traditional. So far as I am personally concerned, I am not in the least interested in trying to square science and Genesis, and here I think the modern world agrees with me ; but if anyone is interested it is just as well to point out that your special creation is a somewhat lame attempt.

Now let us get to the morphology, and I shall ignore anything more in your letter which cannot strictly be so described. In my first letter I showed (I hope clearly) that the morphological resemblances between the various types of animals fitted exceedingly well into the modern idea of evolution. This I do not understand you to dispute ; but you do think there are difficulties. No doubt there are. Some of the transformations that must have taken place are exceedingly remarkable. In the absence of direct evidence it is obviously impossible to say exactly by what stages they have taken place. In some cases it is by no means easy to point out a reasonably possible order in which the changes may have occurred, and I do not pretend that I can always do even that. This has been the stock objection to evolution since the earliest days, and it is the best objection of all, and the only one I am able to take seriously. The examples are continually changing but the type remains. Personally I always used to think the change from gills to lungs the most remarkable, and could not imagine how the change could have taken place until I discovered that certain species of living fish actually breathe both by gills and by a lung. Though these fish are probably not in the direct line of descent, they do make the change credible.

That is all the reply to your letter which seems to me to be necessary. All the same I think politeness is desirable in discussion, and this requires that I pay a little attention to what you say,

however unimportant it appears to me. The fundamental mistake I think you make (and Vialleton according to your account), is to go beyond the statement that these morphological changes are remarkable and difficult to explain, which is true, to the unprovable statement that they are impossible, which is nothing more than making dogmatic statements on the basis of ignorance. I shall therefore take one or two of your examples and say a little about their possibility. I must do so, however, with the proviso that anything I say is purely guess work, and that I am not aware that anyone has any direct knowledge of the matter.

As the first example of this guessing competition, I will take the wing of the bird. I do not see what is wrong with the analogy of the flying squirrel. In the case of the bird, needless to say, the 'patagium' must have affected the fore limb only and left the hind limb free, but I really cannot see any difficulty there. A small tree-living animal would clearly find even a primitive gliding apparatus useful both in escaping enemies and in planing to the ground in search of food. Nor can I see any difficulty in a gradual development from this stage to the full flying powers of the modern bird. I would remark in passing that I think you are entirely wrong in making a hard and fast distinction between gliding and flying. Most birds glide as well as fly, and the rigid plane such as man has made is not known in nature. All flying organs are movable, and a little extra power by the movement of the primitive wings is always possible and I think usually found. At any rate I have seen observations both of flying fish and of the flying phalanger showing that they travel longer distances than can be accounted for by gliding pure and simple.

You ask me again what characters adapting the bird to its mode of life are absent in Archaeopteryx. I thought I had answered this before. The first is the absence of pneumatic bones. The second is the absence of the fan-tail, which enables the bird to hover. The third is the lack of fusion of the wing bones, which certainly would unfavourably affect the power of flight. I may add also that the presence of teeth throws doubt on the existence of much of the modern bird's remarkable digestive apparatus.

I will conclude with a few words about the whales and the seals. If you will look at the skeleton of the seal and imagine a lengthening and strengthening of the tail and obsolescence of the limbs and pelvis, you will see that the result would give you a colourable imitation of the structure of the whale. While I do not

suppose that the whale has developed in this particular way, there is nothing impossible about it, and something of the kind would be bound to occur if the seal were to increase in size beyond very narrow limits. I therefore do not see any serious difficulty in general, and if you think there is any particular difficulty perhaps you will be good enough to say what it is.

Yours sincerely,

H. S. SHELTON.

DEAR SHELTON,

The editor writes : 'You and Shelton have both been very good about accepting suggestions for the removal of occasional acrimonious remarks. Shelton rebukes me for not toning down your last letter. I could not have asked you to remove the sentences he dislikes without asking him to delete from his letter the clear suggestion that no man with any pretensions to scientific knowledge or common sense (or true Christianity !) could possibly accept special creation. The conspiracy to represent the believer in special creation as a crank to be classified with flat-earthers is one of the great obstacles to a scientific discussion of species, and I am therefore glad that Shelton's remarks provided me with a chance of bringing this particular prejudice into the open. Whether special creation is inconsistent with "science and common sense" must be decided by argument and not by assertion. I think you are both feeling the strain of the repression imposed on you by your peace-loving editor, and the brisk clash of these last two letters may have helped to clear the air, and render possible a speedy return to the urbane note which has in the main, characterised this stimulating correspondence.'

In consequence of shortage of space and the editor's request for urbanity, I refrain from commenting on the theological part of your letter. I note your complete inability to meet the morphological difficulties set out in my letter (p. 163). As regards the additional ones raised in my last letter the fact that birds glide as well as fly does not help your case. A wing can be used as a patagium, but a patagium cannot be used as a wing. An aeroplane can both glide and fly ; a glider can only glide. A patagium merely retards falling ; it can neither lift nor propel ; a wing can do all these. I find it scarcely more difficult to believe that, by a purely natural process undirected by creative intelligence, a patagium evolved into

a wing than that by this process a glider became converted into an engined aeroplane.

The length of the glide of a flying fish or a flying squirrel depends mainly on the impetus given to it before its glide, and, in the case of the squirrel, the difference between the height of its take-off place and that of its landing place. I asked in what ways Archaeopteryx was not adapted to its mode of life, not how it differed from modern birds. You have not stated how a land quadruped could be gradually converted into a seal, but you ask me to imagine a lengthening and strengthening of the tail and the obsolescence of the limbs and pelvis of a seal, and say the result will give me a colourable imitation of the structure of the whale. That is not so. The seal swims by the movement from side to side of its hind legs and tail, while the whale swims by an up-and-down movement of the great tail. Therefore the seal, in order to be converted into a whale, in addition to the transformations mentioned by you, would have to twist the hinder half on the front half of its body a complete right angle so that one side of its hind-end faced upward and the other side downward!

You have certainly adduced evidence in support of your remark: 'some of the transformations that must have taken place are exceedingly remarkable.'

The morphological evidence against Evolution may be summarised:

1. The gaps between phyla, classes and orders, are so great as to preclude the evolution of any one of them into any other.

2. The distribution of morphological characters within each group is such as to preclude all the individuals of a group being derived from a common ancestor.

3. Animals exhibit hundreds of organs that cannot have originated gradually.

In my first two letters in this chapter I have dealt almost exclusively with the first of the above kinds of evidence. I have now to demonstrate . . .

The Distribution of morphological characters within each phylum class and order, is such as to preclude all the members of any of them being derived from a common ancestor.

As this evidence is necessarily technical I will here merely say that if all members of any of these groups were derived from a common ancestor it would be easy to draw up a family tree showing

the descent of every sub-group of a group from a common ancestor, in other words, to draw up a phylogenetic classification. In consequence, after the publication of Darwin's *Origin of Species* zoologists set out light-heartedly to draw up genealogical trees for every large group of animals, and many such were drawn up ; but when these were compared it was found that no two agreed, and on examination it was seen that the reason for the disagreement was each zoologist had based his tree on the set of characters he deemed most important, and the differences between the various pedigrees were not small or unimportant but fundamental. Bashford Dean exposed these differences in the cases of the Fishes (*Fishes living and Fossil* (1896), p. 282).

Others performed the same office in regard to other classes ; Vialleton in the case of Mammals (*L'Origne des Êtres Vivants* (1829), p. 180), and I, for one, did this in the case of Birds (*Difficulties of the Evolution Theory* (1931), pp. 164–175), and will here repeat what I wrote there : 'Every attempt to classify birds phylogenetically or to draw up a genealogical tree of the class has been a failure. It is submitted that the reason is, not that evolution has taken place along strange and incomprehensible lines, but that all the birds have not evolved gradually from a common ancestor ; if all are derived from some primitive stock, each family or, at any rate, each order or sub-order, sprang into existence suddenly endowed with its main characters and has since undergone comparatively little differentiation.'

The above applies equally to orders within the Classes ; thus fully a score of genealogical trees of the Primates have been drawn up. Of this order Sir Arthur Keith writes ('Man,' *Ency. Brit.*, Vol. 14, p. 761) . . . 'from the details revealed by anatomical analysis it is plain that evolution has not proceeded in an orderly manner in shaping the bodies of the higher Primates, characters are curiously scattered.'

I invite you to draw up a genealogical tree of any order or class you choose, but I assure you that your tree will not bear scrutiny because it will represent something that has never happened !

I now come to the third type of morphological evidence.

There are thousands of structures in animals that cannot have originated gradually.

Here the issue between us is that you believe that undirected forces of nature, or the purely fortuitous influence of the

environment and natural selection can create a complex organ such as the eye or a structure such as the feather, which I flatly deny.

The editor specifically mentioned the eye and the feather in his introduction. In your letter (p. 25), you evaded this difficulty. I pointed this out in my letter (p. 44), but in yours (p. 49) you said nothing about the eye, and of feathers you said you could not answer except in a speculative way. So you have still to deal with these two matters. As you exclude from your account of the evolutionary process the intrusion of supernatural and directing activity, you are reduced to placing on pure chance a burden which chance cannot bear. Do you *really* believe that a purely *fortuitous* process, the chance and unplanned selection of variations, transformed scales into feathers? I challenge you to explain (a) the mechanism by means of which scales turned into feathers, (b) the survival value of the first embryo feathers, (c) the process whereby feathers could be co-ordinated to produce flight. Please note a slow gradual evolution in a feather direction is no use. Flight is one of those 'hit or miss' phenomena which has got to be right the first time. And, to produce the necessary power and control to lift the bird from the ground even for the smallest of flights, an intricate and complex correlation of feathers, muscles, bones, etc., is necessary. Bird flight involves just as intricate a correlation as aeroplane flight. That kind of thing cannot evolve gradually.

I have already cited the organ of Corti as a structure that cannot have evolved gradually, and asked you to account for it, which you have not done. Among the hundreds of other structures I will now ask you to account for the origin and development of the spinning apparatus of the spider and the jumping apparatus of the click-insect.

Spinnerets of the spider.

Near the hind end of the abdomen of every spider there are six pairs (four pairs in the Mygalidae) of tubercules known as spinnerets. Every one of these is perforated by many (often hundreds) of minute tubes, each connected with the duct of a silk gland and opening at the tip of the spinneret which thus has somewhat the appearance of a 'rose' of a watering-can. The silk glands (of which there are about six hundred) in the garden spider, Epeira diademata, are of five different sorts, each sort secreting a different kind of silk each of which forms a different kind of thread, for, as Shipley

and MacBride write (*Zoology* (1915), p. 263), '. . . the various lines in a spider's web differ considerably one from another in accordance with the use they are put to. The circular lines are sticky and help to catch insects for the spider's food ; the radial lines are stout and form a framework for the support of the circular lines ; the threads with which the spider binds up its captured prey differ from these, and there is still another kind of thread with which it surrounds its cocoons, and each kind of line is supplied from different sets of glands.' I maintain that until both glands and spinnerets had attained a certain degree of complexity they would be useless. If you think these evolved gradually please describe what you imagine to be their mode of origin and the stages by which they obtained their present complexity, and of what use, if any, they were before they secreted silk.

The Jumping apparatus of the Click Beetle, Agriotes.

When an Agriotes is frightened it drops to the ground. If it falls on its feet it runs for cover, but if it falls on its back it instantly draws in legs and antennae and remains motionless as if dead. Then suddenly it jumps about 3 inches into the air, if it falls on its back it repeats the above performance until it lands on its feet. The jumping apparatus is a beautiful peg and socket arrangement. The peg projects from the hind edge of the lower part of the first body segment (prosternum). The socket is in the corresponding part of the second segment (mesosternum). The peg has a slide on each side—a smooth, highly polished surface ; it also has a groove round it a little behind its point. The socket has on the inner wall a pair of slides corresponding to those on the peg, and a ridge corresponding to the groove of the peg. The socket is rather larger than the peg so that the fit is not tight. When the insect is about to jump the peg is pulled out of the socket by two muscles and this stretches a spring that connects peg and socket ; but even when the muscles relax the peg cannot get back into the socket because the ridge of the latter is inserted in the groove of the peg, acting as a stop or a catch. The beetle, however, releases the peg by a violent twitch of the body, then the spring pulls the peg into the socket with such force that the shock of the impact not only jerks the insect into the air but makes a loud click from which the beetle derives its name.

Not only would this jumping apparatus be entirely ineffective until completely developed, but, although useful, it is not indis-

pensable, as the insect when on its back is able to turn over, but with difficulty, by means of its claws and by moving its legs. Occasionally it does turn over in this way. Cuénot put one on its back 115 times and it righted itself 100 times by jumping and 15 by means of its wings and claws.

The fact that this jumping apparatus is, so to speak, a luxury, and its possession is not a matter of life or death, in my opinion, shows that it is not a product of gradual evolution, and its ingenuity almost compels the belief that it is the work of a Designer. If you disagree please say how you imagine it started and describe the stages by which it has reached its present condition. In this connection we must bear in mind that, speaking generally, the animals having a very complicated apparatus do not seem to be more successful than the members of the same family that lack it.

Yours sincerely,

D. DEWAR.

DEAR DEWAR,

I reciprocate the wishes of the editor (and I presume also your wish) to conduct this discussion without unnecessary acrimony. All the same I must protest that I said nothing about the flat earth, and in general, cannot accept the editor's interpretation of the passage to which he refers. If I might be allowed to make a suggestion, it is that you *see* things differently from the way the majority at the present day see them, and it is not wise to get annoyed if at times you are not understood. I remember some years ago taking part in a philosophical discussion with Dr. Schiller, Mr. Alfred Sidgwick and others, in the pages of *Mind*, and Mr. Sidgwick remarked that he was not surprised that controversialists often failed to understand each other, indeed he thought it surprising that they so often succeeded in doing so. He was good enough to remark that I was unusually successful in understanding him ; but I fear you would not be able to pay me the same compliment. I should also like to say that I often fail to see the slightest connection between my statements and arguments and your account of them. That is for the same reason. As an example note the following passage : 'As you exclude from your account of the evolutionary process the intrusion of supernatural and directing activity.' I thought I had carefully explained in the first chapter that I left that point entirely open.

I have very little more to say about these morphological difficulties. Let us take a few seriatim :

1. *Whales.* You mention that the tail of the whale moves in the vertical plane whereas that of the seal moves in the horizontal plane. I see no difficulty in this additional evolutionary change, still less in its taking place gradually. If there is a mechanical difficulty in its taking place when the pelvis is of considerable size, the obvious solution is that it took place after the pelvis was reduced to a smaller size. The use of this adaptation is obvious. It facilitates deep diving and quick return to the surface. It also enables the tail to be used as a weapon of offence. Incidentally is it not powerful evolutionary evidence that the whale has a pelvis at all ?

2. *Birds, Feathers and Flight.* In my first letter, in answer to the editor, I gave a speculative guess about the origin of feathers, suggesting how they were useful even in rudimentary stages. Also, about the eye, I remarked on the many grades in living animals between a primitive sensitiveness to light and the human eye. You are pleased to call that an evasion. The reader must judge. If it is an evasion I shall continue to evade, as I have no more to say.

With regard to flight, I disagree with you entirely, especially about the flying fish. Both from personal observation and accounts by others (F. T. Bullen who had unusual opportunities for observation is emphatic about this[1]) I am convinced that they *fly* not glide. There are, of course, different powers in different species. In spite of the Cambridge Natural History, which agrees with you, I regard both the distance they travel and the time they remain in the air as too great to be accounted for by gliding. I have no personal experience of other animals with restricted power of flight, but take the liberty of disagreeing with you entirely on the question of fact. It seems to me absurd to say that an animal like the flying phalanger with a movable incipient wing never moves it. How do you know ? This seems to me a definite point of disagreement. What you call gliding or parachuting, which is not the same thing, I call incipient flight, and I regard the difference as merely one of degree not of kind.

3. You make three general statements marked 1, 2, and 3. I cannot deal with general statements except by a direct contradiction. They do not appear to me to be true. Nor can I now set

[1] See Chapter 7 of the *Cruise of the Cachelot.*

forth a new classification of animals in general or of birds in particular.

4. *Spiders.* I think if you would start with the simplest known forms instead of the most complicated it would be easier to guess some sort of answer. I believe that some spiders use the threads only for covering a bunch of eggs, and do not possess the exceedingly complicated apparatus found in the garden spider. To give even a plausible guess of the origin of the spinnerets requires much more knowledge of spiders than I possess.

5. *Beetles.* I don't know enough about beetles to give even a guess how this particular organ arose by a gradual process, though it does seem to me on general principles that many gradations are possible in *any* jumping organ. With regard to the utility of the organ, it seems to me fairly obvious that, while ordinary wriggling will suffice to put the insect on its feet, the jumping would be a very valuable device for escaping from enemies.

Now let us try to get some sort of understanding of the difference between us. We are agreed that the adaptation of animals are marvellous. We are agreed that you are scoring a valid point in pointing out that there is difficulty in some cases in understanding how they were of advantage in their incipient stages. In the absence of direct evidence we do not know the stages by which they have arisen, and can only guess. In some cases the examination of other living creatures removes this difficulty by showing possible ways in which they could have arisen. In the case of flight, for example, I do not think there is any difficulty left. But even then we cannot be certain that the organ was evolved in that way. I have made a few guesses, for what they are worth, but do not lay stress on them. Where we differ is in the inferences. To my mind, if there are difficulties of classification (and there are) the only remedy is further research. Where there are difficulties in understanding how an organ can arise by a gradual process, the only remedy I can suggest is further study both of the organism and of allied organisms. There is plenty to be done. We are only at the beginning of our knowledge of natural history. So far as I am concerned, I do not think I have done too badly in this guessing competition; but it *is* a guessing competition. When, however, you say 'animals exhibit hundreds of organs which cannot have originated gradually,' I must part company. That seems to me to be dogmatising on the basis of ignorance. The difference between

us is, that I acknowledge my ignorance and you do not. The evidence for evolution is so strong that we can be confident that solutions will be found.

Yours sincerely,

H. S. SHELTON.

DEAR SHELTON,

I was under the impression that you were defending the thesis that all living forms can be accounted for by a natural process of evolution. If you regard the 'intrusion of supernatural and directing activity' as an open question, you must be prepared to concede the possibility that natural forces and processes are inadequate to explain the evolutionary process. The point is important and I hope you will clarify your position. Scientists are often in a position in which they accept a particular phenomenon but cannot explain it. The peculiarity of your position is that you are not only unable to prove how evolution occurred, but you cannot even advance a plausible hypothesis as to how it *could* have occurred. You evade every clear challenge by taking refuge in vagueness and generalities. The fact is that an incipient feather, the first signs, shall we say, of fluff on the scaly leg of a future bird could have no survival value whatever, and that it is impossible to suggest any purely natural explanation of how feathers arose or how the hit-or-miss phenomenon of flight originated.

Your favourite device when challenged to produce evidence is to appeal to the evidence which you allege has disappeared ('missing links') or to the evidence which the future is to provide. 'Keep your seats ladies and gentlemen of the jury. The witnesses whom I propose to subpoena for the defence will be brought before you in the next century or later !'

Now a scientist whose predictions are verified is naturally more impressive than one whose predictions are not. When Mendeléef propounded his periodic law, he pointed out that, if his theory were true, there must exist a number of unknown chemical elements of each of which he was able to give approximately the atomic weight. Every one of these has since been discovered. The early Darwinians predicted the discovery of fossils of pro-Aves, pro-Cetacea, pro-Chiroptera, etc. Not one of which has been discovered. More than 20 years ago Vialleton asserted that it was futile to seek for a fossil mid-way between that of a land quadruped and a

whale or a sirenian, because half-way forms could not possibly exist and propagate themselves in competition with other animals. Vialleton staked his reputation on a statement that would be upset by the discovery of any of the scores of intermediate forms that must have lived for millions of years if the Darwinian theory be true. Please note that these half-land half-marine intermediaries would live near the coast, precisely in places where most of the fossil-bearing rocks were laid down. More than a score of years have passed since Vialleton made this prediction, which, unlike those of evolutionists, has so far been fulfilled.

You ask: 'is it not powerful evolutionary evidence that the whale has a pelvis at all?' The answer is: 'Certainly not. No whale has a pelvis. The primordium which gives rise to the pelvis in most vertebrates gives origin in the whales to the two bones which stiffen the genital orifice.'

I challenged you to describe a series of possible transitional stages between the patagium of a flying squirrel and the wing of a bat; you have not accepted this challenge. At the same time I asserted 'the presence of a patagium presents an obstacle to the development of a wing, because its existence necessitates an orientation of the fore-legs incompatible with flight. I called on you to show that I am wrong. By way of reply you say that it seems to you absurd to say that an animal like the flying phalanger with a movable incipient wing never moves it. Now, the patagium of the flying phalanger is not an incipient wing, it remains rigid when the animal is moving in the air, and is folded immediately the animal alights. The patagium cannot be used as a wing, nor could it possibly be *gradually* changed into a bat's wing. You say that what I call gliding or parachuting, which is not the same thing, you call incipient flight, you regard the difference as merely one of degree. If you look up 'Parachute' in the *Oxford Dictionary*, you will find it described as 'any contrivance . . . serving to check a fall though the air, or to support something in the air, e.g., the expansible fold of skin or patagium of the flying squirrel.' The difference between a wing and a patagium is as fundamental as the difference between chalk and cheese.

I have crossed the Indian Ocean fourteen times and have spent hours watching flying-fishes in the air, and have never detected any flapping of the pectoral fins.

You say 'the evidence for evolution is so strong that we can be confident that the solutions (of all problems it presents) will be

found.' May I remind you that so far you have not produced an iota of evidence of evolution as distinguished from mere differentiation which many of those who reject evolution deem possible and believe to have occurred.

Yours sincerely,

D. DEWAR.

6

EXPERIMENTAL EVIDENCE

DEAR SHELTON,

Experimental evidence is very unfavourable to the theory of evolution. Owing to Mendel's discoveries we know that variation results from the re-combination of genes (hereditary units). We also know that if two individuals have different hereditary units these genes do not fuse into a new gene in the offspring any more than do two chemical atoms when joined fuse into a new atom. As Heribert Nilsson puts it (*Hereditas* (1935) p. 231) : 'On what does heredity depend ? It depends on the transmission of a certain heredity unit, a certain gene, to the offspring. On what does variation depend ? It depends on the regrouping of the different genes of the father and mother. . . . Variation is caused by the re-combination of the genes, not by their change. Variation is therefore restricted by the combination possibilities of the genes. And these are limited by the crossing possibilities. Then again, since individuals belonging to different species of plant or animal cannot even be paired, much less produce offspring, the combination of variations is confined to the species. Variants are formed, out-crossed and arise anew in a kaleidoscopic sequence within the species. *But the species remains the same sphere of variation.* The various species will remain like circles that do not intersect. *Species are constant.*' (The italics are his.)

Geneticists now realise that new species and new organs or structures can only arise by a mutation. Now a mutation in a gene can, so far as I can see, arise in three ways : (1) by the loss or inhibition of, or damage to a gene, (2) by the change of a gene into a different kind of gene, (3) by the origin of an entirely new gene out of nothing or, if you prefer, out of the blue. Clearly (3) would be a miracle, and so would (2) if the rearrangement of the molecules that make up a gene (which is far too small to be seen under the highest-powered microscope) were such as to produce *a new gene having the power to originate a new structure favourable to the animal of which it formed part.* Thus the only mutations available to cause evolution are those resulting from the *loss or inhibition of, or*

damage to, a gene. Breeding experiments indicate that these are the only kind of mutations that occur under domestication or in the laboratory. In other words all the breeds and mutants that have appeared are defective in some respect, although many may be useful to man. The chances of any of them surviving in nature are small. Nor is this all : *breeding operations have not resulted in the production of any new organ or type of animal*, and there is no sign of the appearance of any new kind of animal. In the case of domestic animals, after centuries of breeding sheep remain sheep, pigeons, pigeons, dogs, dogs, etc. Many of the breeds are freaks resulting from gland unbalance, such as dachshunds, fantail pigeons, etc.

The experiments of scientific breeders or geneticists tell the same story.

The animal on which geneticists have done most work is the little fruit fly Drosophila melanogaster, of which some 25 million individuals have been bred, representing nearly 1,000 generations in which about 1,000 mutations have been recorded. But these mutations are almost all clearly what may be called *loss mutations*, all are defective in some way, thus over 100 mutations of wings have been recorded, in all of which the wings are defective or reduced to stumps or absent. This is equally true of all other animals experimented on. Le Dantec compares the productions of geneticists, as the result of 35 years work, to the metamorphosis of a man at a circus, having 35 waistcoats, who, after he has removed them all, remains a man. It is for this reason that Heribert Nilsson (who is a botanical geneticist) writes (*op. cit.* p. 236, italics his) : '*The theory of evolution has not been verified by experimental investigations of the origin of species.*'

Common sense suggests that a new organ or a new kind of animal cannot result from the accumulation of losses. And so far as I am aware, no new organ or structure has been produced as the result of breeding operations. If I am wrong, you will doubtless name some new structures and state the circumstances under which these have been produced.

What makes the experimental evidence so unfavourable to the evolution theory is that *no mechanism has been discovered capable of originating any new structure or organ.* We know that the genes are arranged in series along the chromosomes, and the combined length of all the chromosomes of an animal cell is about 1/250th of an inch, and it appears to be about the same length in the case of every animal from amoeba to man. If then an amoeba-like

creature evolved into a man, as Bateson remarked over 40 years ago, man would be a simplified amoeba! As Lunn has stated in his introduction it is up to you to point out a *vera causa* capable of gradually converting an amoeba-like creature into a man.

Perhaps unwittingly, you have testified to the extent to which the experimental evidence afforded by animals is unfavourable to the evolution theory by adopting the desperate expedient of appealing in your first letter to experiments on plants as evidence for evolution and you seem to think that because some freak plants have been bred, which, on account of the duplication of their chromosomes, are infertile when crossed with the parent, you have produced some experimental evidence in favour of evolution. That being so, doubtless, in your next letter you will tell us all about it and above all show us how it helps the theory in the case of animals which differ from plants in that polyploidy is practically unknown in the higher animals and very uncommon in the lower ones.

Yours sincerely,

D. DEWAR.

DEAR DEWAR,

Your letter, which purports to be about experimental evidence, is mainly concerned with the theorising of Nilsson and others. I shall therefore begin by saying a little about the experimental evidence, and afterwards pay some attention to your letter.

Biological experiments, so far as they are relevant, fit into the general scheme of evolution very well. The only relevant aspect seems to me to be to discover whether organisms vary sufficiently to render the changes implied by evolution credible. Experiment can only answer this question to a limited extent, as it is immediately obvious that the time during which anything of the kind has taken place is infinitesimal compared with the time in which the evolutionary changes have occurred. The change from Eohippus to Equus, which is an instance of comparatively rapid evolution in nature, has taken about 60 million years. This allows at least 10 million generations, and probably a great many more. You mention 1,000 generations in the case of the Drosophila experiments. Assuming dogs to have been domesticated for 20,000 years, this might allow 10,000 generations. Obviously this is an infinitesimal number compared with that available for the natural process. If anyone fails to appreciate the very considerable change

that can be produced in a short time, the obvious answer can be given in the four simple words : visit a dog show. If the variations you can see there can be produced in 10,000 generations, what is possible in 10,000,000 generations ?

In the ordinary way I should regard this paragraph as sufficient, and the question of experimental evidence as definitely in favour of evolution. The reason for continuing this letter, and any subsequent discussion that may be needed, is that you represent a small class of people who decline to make what appear to me to be plain inferences from obvious facts. Instead of that, you, and those who agree with you, make a number of complicated and factious objections, and I must therefore devote some space to dealing with them.

The first of these objections is the contention that these inhabitants of the dog show are freaks, unhealthy, and unfitted to survive in nature. To some extent this is true, though its relevance is doubtful. We have substituted artificial selection for natural selection. But it is by no means always true. Dogs have been known to run wild and to flourish. So to a greater extent have horses, which are considerably changed from the wild ancestors so graphically portrayed in later Palaeolithic drawings. But I can give you a better example than either of these, which I have found in Prof. W. B. Scott's excellent little book on evolution. Somewhere about A.D. 1200, a few rabbits were let loose on the sandy island of Porto Santo near Madeira. The result has been what every anatomist would describe as a new species. The colour is changed. The weight is about half that of the normal rabbit. The habits have changed and the species has become nocturnal. Whether they are a new physiological species is doubtful—probably not—but the attempt to make them interbreed with the ordinary rabbit failed. What is certain is that, if their origin were unknown, they would certainly be regarded as a distinct species. In the course of a few hundred years, in adaptation to an unusual environment, a new anatomical species has been evolved under natural conditions.

The next and most important objection, about which much has been heard from anti-evolutionists in the past, is that these dogs, although their anatomical differences are so great that, if they had been found as fossils, they would certainly be classed in different genera, do not show the formation of a new *physiological* species. They can still interbreed. Huxley admitted that the case for evolution would be stronger if a new physiological species could be

formed. That is the reason for my emphasis in the first chapter of the fact that this had now been done in plants. *This particular objection can no longer be made.* I do not myself think this fact of any great importance, but it does dispose of a factious objection.

There still remains a modified form of the same objection. This method of forming new species in one step does not apply to animals, because, even when they are hermaphrodite like the earthworm, they are not self-fertilising. Such a process would require the improbable contingency of the same variation occurring simultaneously and in the same place in at least one male and one female. Nor do I think it to be the normal method of evolution in plants. We have to search for a more gradual process. This has been found in the experiments on fruit flies, which have often produced forms with reduced mutual fertility. Recently this research has been carried a step further. If you will refer to *Nature* (3rd June, 1939) you will find that two different varieties of Drosophila pseudo-obscura have been bred which when crossed produce only sterile male hybrids. This is a degree of mutual infertility similar to that of the horse and the ass. Thus even in animals the formation of a new physiological species has been achieved.

That is sufficient for the present about experimental evidence. Now a few words are necessary about Nilsson and his peculiar theory of genes. It is worse theorising than usual, and that is saying something. First of all it is well to realise that, while the chromosomes are observed objects, the genes are theoretical abstractions. All we know is that a change in a certain part of the chromosome produces a corresponding change in the structure of the animal. We hypostatise this localisation by calling the local centres genes, and this conceals our ignorance, and enables us to co-ordinate the facts to a certain extent. But it is quite illegitimate to go beyond this and dogmatise about how a mutation can occur in a gene. In the first place mutation means (or should mean) what occurs in the animal, not what is supposed to occur in the gene. With regard to Nilsson's remarks about 'as far as I can see,' the obvious comment is that he can't see anything. To damage or destroy a gene are meaningless expressions, because we don't know what genes are. Obviously also if you destroy the reproductive mechanism the cell will not reproduce. The expression 'origin of a new gene out of nothing' is nonsensical for the same reason. It would be difficult to find a better example of clotted nonsense.

Let me try to state very simply what is known on this matter. The structure of the chromosomes, in some unknown way, partially (for we do not know whether or no it is the only factor) determines the structure of the animal. In Schrodinger's phrase they form a sort of script. If you bombard these with X-rays, or ultra-violet rays, certain changes take place, and the animal shows unaccountable variations. The 'script' of these variations is often found to be localised in parts of the chromosomes. That localisation is called genes, but the concept should be used with caution, as most certainly the chromosomes as a whole are a unit. What changes, chemical or other, take place in the 'genes,' and how and why these changes affect the development of the animal, is entirely unknown. If you are interested to learn what *might* happen, considered from the physico-chemical point of view, I can recommend Schrodinger's recent work *What is Life*. This also is theorising, but at least it is theorising on the basis of a sound knowledge of physical chemistry instead of on the basis of pure ignorance. The idea is that the changes in the 'genes' are chemical, and are subject to quantum laws. It is a good idea, and does to some extent explain, but of course it is still very theoretical. At any rate it is enormously better than this destroying genes or making them out of nothing.

The moral of all this is that we are supposed to be discussing experimental evidence, which is exactly what is required by those who accept evolution. I suggest therefore that you and other anti-evolutionists attend to the experiments and leave on one side wild and foolish theorising.

Yours sincerely,

H. S. SHELTON.

DEAR SHELTON,

You have overlooked three very important points, viz., (1) the human breeder can and does effect changes in animals far more speedily than they can be effected under natural conditions, probably more than 100 times more quickly, because, *ex hypothesi* all but favourable variations that occur in nature are soon wiped out, whereas the human breeder can select any variation he fancies, irrespective of its being advantageous or otherwise to the animal, (2) the unnatural conditions under which domestic animals live are likely to induce variations that would not occur in nature. In fact the breeder often subjects the subjects of his experiments

to conditions impossible in nature, such as subjection to X-rays. It has been shown that in the case of the fly Drosophila, upon which so many experiments have been performed, by exposing it to X-rays the rate at which mutations appear is increased 150 times, and in the plant Crepis, 600 times. (3) In nature the odds are immensely in favour of a new variation being swamped by its possessor mating with individuals that do not exhibit this variation. The breeder, on the other hand, when he wants to perpetuate a variation, segregates the individual in which it appears and either crosses it with another individual that has varied in the same way, or back-crosses it with the parent and so perpetuates the desired variation. Thus, it seems an underestimate to say that the human breeder works more than a hundred times as rapidly as nature, and your ten thousand successive generations of dogs, as regards capacity to change, are equivalent to about a million in a state of nature. Therefore the fact that breeders have not bred anything approaching a new species of dog, is most unfavourable to the evolution theory.

I am surprised that you cite as evidence for evolution the Porto Santo rabbits, because I was under the impression that it is now generally realised that, as we do not know what breed of rabbit was let loose on that island, we cannot know what, if any, change, this race of rabbit has undergone on the island. It may well have been a small race. This, I presume, is the reason why no mention of the Porto Santo rabbits is made in any recent book on the subject, such as Robson and Richards *The Variation of Animals in Nature*, (1936).

I note that you have not named any new structure as resulting from breeding operations, and that you have not suggested any mechanism whereby the genes that control the development of an amoeba can have become so transformed, as to have produced, men, birds and reptiles instead of amoebas.

As you admit that the phenomena of polyploidy in plants afford little, if any, help to the evolution theory, it is not necessary for me to say any more on the matter, but our readers may be interested to hear that if tomatoes be cut down and allowed to regenerate about two per cent will regenerate a polyploid branch.

I am sure that Heribert Nilsson will be enchanted when he reads the enconiums you have lavished upon him. If you have any to spare perhaps you would like to confer a few on another geneticist, (whose work on the gipsy moth is known all over the world), Richard Goldschmidt, for the following statement (italics

his) : '*Subspecies are actually, therefore, neither incipient species nor models for the origin of species. They are more or less diversified blind alleys within the species. The decisive step in evolution, the first step toward macroevolution, the step from one species to another, requires another evolutionary method than that of sheer accumulation of micromutations.*' (*Material Basis of Evolution* (1940), p. 183.)

Although you have not dealt with all my objections, you have done so with one I did not raise, because, although it is an objection, it is one to which I do not attach much importance, viz., all the breeds of animals produced by breeders are fertile *inter se* and when crossed with the parent form. Why I deem this objection of minor importance is that all organs, including the sex organs, vary and no two individuals are alike (cf human finger-prints). I believe Paley to be right when he laid stress on the variety of the organic world and thought this might induce us to believe that '*variety* itself, distinct from every other reason, was a motive in the mind of the Creator or with the agents of His will' (*Natural Theology*, p. 170). This being so I see no reason why in any created form successive variations of the sex organs might not result in some individuals becoming unwilling or unable to cross with others. Moreover, chromosome inversions or translocations might well effect changes of the above kind. It may be that such occur in nature, but probably when this happens the viability of the individual in question is affected. This seems to have been the case with Drosophila pseudo-obscura, which you mention. I have not seen the issue of *Nature* to which you refer, but I think that you have misread it and that the races were not bred in the laboratory, but exist in nature. There are two races A and B, of which the range to some extent overlaps. When these are crossed the male but not the female offspring is sterile. In outward appearances the races are indistinguishable.

If we define a species as an interbreeding community we may say that no new animal species has been bred experimentally. As most species do not interbreed in nature the above definition is a convenient one, although it makes the species a much smaller group than that of which Linnaeus said : 'Every species was created in the beginning by the Infinite Being.' We may take it that all the individuals past and present that compose a species in the modern restricted sense are the descendants of a single pair ; but from this it does not follow that all the descendants of a single pair are for all time fertile *inter se*, or would be if crossed with the

parent form. It may well be that groups of what appear to be closely-allied species are derived from a common ancestor, but all the available evidence, experimental, taxonomic, geographical and geological, indicates that the extent of the diversification of the descendants of a single pair is very restricted, and that, if new species in the restricted sense now given to the term, have arisen in nature the process of species-formation is a very slow one, so slow as to make the transformist theory impossible, because life has not existed or the earth nearly long enough to allow of the gradual evolution of a vertebrate from a protozoan. If the origin of a new species be a very slow process that of a new chemical compound is the very reverse; it takes place with incredible rapidity. If you half fill a test tube with the colourless liquid silver nitrate, and pour into this some colourless hydrochloric acid, before you can say 'knife,' the whole of the silver in the test tube is lying at the bottom as the white powder, silver chloride. In less than a second every molecule of silver nitrate has become converted into a molecule of silver chloride. Now it is a postulate of the evolution theory that some inorganic compound was converted into a new substance called protoplasm, which is the physical basis of life. The change, if it took place as postulated by the evolution theory, was a chemical one, and so must have been sudden, because it is a law of chemistry, that when the right molecules come into contact under certain conditions a definite chemical change *invariably* takes place. Now we know all the chemical elements of which protoplasm is composed, viz., the gases Oxygen, Hydrogen, Nitrogen and the solid, Carbon. T. H. Huxley wrote ('The Physical Basis of Life' *Fortnight. Rev.* (1869), p. 129): 'The existence of the matter of life depends on the pre-existence of certain compounds, namely, carbonic acid, water and ammonia. Withdraw any one of these three and all vital phenomena come to an end. They are related to the protoplasm of the plant as the protoplasm of the plant is to that of the animal. Carbon, hydrogen, nitrogen, oxygen are all lifeless bodies. Of these carbon and oxygen unite in certain proportions and under certain conditions to give rise to carbonic acid; hydrogen and oxygen produce water; nitrogen and hydrogen give rise to ammonia. These new compounds, like the elementary bodies of which they are composed, are lifeless. But when they are brought together under certain conditions they give rise to the still more complex body, protoplasm; and this protoplasm exhibits the phenomena of life.

When the hydrogen and oxygen are mixed in certain proportions and an electric spark is passed through them they disappear, and a quantity of water, equal in weight to the sum of their weights, appears in their place. There is not the slightest parity between the passive and active powers of the water and those of the oxygen and hydrogen which have given rise to it . . . What justification is there, then, for the assumption of the existence in the living matter of a something which has no representative or correlative in the not-living matter which gave rise to it ?'

This seemed conclusive and it was thought that living matter would soon be produced by experiment in the laboratory. Haeckel went so far as to foretell the production of protoplasm for commercial purposes ! Bear in mind that the laws of chemistry are such that, whenever the right elements are brought together under given conditions, a definite chemical compound—always the same one—is invariably produced. But all experiments to produce protoplasm from the right mixture of its component elements have utterly failed. So much so, that, although evolutionists believe that life arose in the sea, if one were to give a chemist a pool of sea-water surrounded by any kind of rock or earth he required, and ask him to produce protoplasm he would laugh at the notion. Nor is this all, numerous attempts have been made to manufacture protoplasm out of dead organic matter, i.e., out of the most complicated carbon compounds known, but all have failed. Yet evolutionists believe that natural forces in the past did with *inorganic* matter, what modern chemists, with all their apparatus, knowledge, chemicals, X- and other rays, have failed to do even with *organic* compounds. Thus, *so far as it is possible to prove a negative, experimental evidence proves that the evolution theory is not true.* Nor is this all. It is almost universally held that living matter to-day is never formed from non-living matter. And the evolutionists hold that there is no evidence that this happened more than once in the past. As we have seen, a chemical compound is invariably formed when the elements that compose it are brought together under proper conditions. Therefore evolutionists have to believe that during the hundreds of millions of years that the earth has been habitable, during all the climatic changes that have taken place *only once* did the proper conditions for the formation of protoplasm come into existence, and that no chemist or other experimenter has been able to repeat such conditions. Nor is this the only difficulty the evolutionist has to overcome. If the first living protoplasm

did arise as the result of favourable chemical conditions think of the difficulties this first living matter would have to overcome. It would have to feed and reproduce itself in a world entirely mineral and without a trace of organic matter, devoid of an ounce of 'soil' in the gardner's sense; the land hard mineral rock, or barren sand, or bare mud, devoid of bacteria or other micro-organisms, the water holding only atmospheric and perhaps other gases and mineral salts. Small wonder that Sir Gowland Hopkins, in his Presidential Address to the British Association in 1933, said: 'Most biologists, I think, having agreed that life's advent was at once the most improbable and significant event in the history of the universe, are content for the present to leave the matter there.' Do you really believe that the theory of evolution dispenses with miracles?

Yours sincerely,

D. DEWAR.

DEAR DEWAR,

I have your letter (p. 191) and find in it very little that needs a reply. Your statement that by artificial breeding changes are produced more quickly than in nature is true. Your further statement that variations occur more frequently in domestic animals than they do in nature is very doubtful. As we do not know what are the reasons for variation it cannot be accepted. Moreover your statement that 'breeders have not bred anything approaching a new species of dog' is, like many others that I have noted, confused and misleading.

This latter statement depends entirely on what you mean by a species. Let us take as an example the St. Bernard and the Pekinese. Morphologically, as we should judge if we found them as Pliocene fossils, they would certainly be classed as different genera. Whether they are physiologically different species, or in other words whether they could interbreed, I do not know. As the question of the production of new physiological species has been settled by other forms of life, it is really not worth while to enquire.

I do not know why the Porto Santo rabbit is not more frequently mentioned. At any rate, *on a question of fact*, Professor W. B. Scott's authority is good enough for me, and, in view of your appreciative references to him in a previous letter, it should be good enough for you also.

With regard to new physiological species, there is no doubt whatever that they have been formed in plants. You throw some doubt on the question whether the two forms of Drosophila are natural or produced by breeding. As we have to hurry somewhat in finishing this revision, I may not be able to look up the *American Naturalist* to settle this, and so cannot press this particular point. It really doe not matter. There are a number of undoubted cases when the mutated fruit flies show very greatly reduced natural fertility with other forms bred from the same natural species, and so, if this particular objection has not been eliminated entirely, it has been reduced considerably. In any case, now the question has been settled with regard to plants, the objection has become even more factious than it was originally.

With regard to theorising, and particularly that of Nilsson, I must acknowledge to some slight prejudice. In the original correspondence you told me that he had a theory of the origin of species from the ground elements of the biological world by the law of great numbers. Consequently, while I am willing to admit that he may be a competent specialist (and do not wish to depreciate his work on the gipsy moth) I am not disposed to take the least notice of his theorising.

Anyhow you should be aware that there is no subject just now on which there has been so much wild theorising as there has about genes. You will find in Julian Huxley's *Modern Synthesis* that biologists are now beginning to realise that gene mutation is not nearly as 'atomic' as has previously been thought. Julian Huxley puts it that a gene variation is modified by the action of other genes. What I think this really amounts to is that the concept of genes is methodological, and, if used too absolutely, disguises what is also a fact—the fundamental unity of the whole chromosome mechanism.

Generally speaking, I am surprised at your undue regard or the *opinions* of individual scientific investigators. Your quotation from Richard Goldschmidt is an example. So far as this discussion is concerned, I am not interested in these opinions. If we are going to place a high regard on the *opinions* of men of science, then evolution is a fact and there is nothing to discuss. If we do not place a high regard on them, why quote them? I do not think we can do better than quote authorities for facts only, not for opinions, though I admit that sometimes it may be a little difficult to draw the line.

I do not think I need say much about X-rays. You have in the chromosome outfit a series of complicated chemical compounds

which in an absolutely unknown way determine the structure of the animal. X-rays in an equally unknown way make changes. Are these changes those which would naturally occur more slowly? Again we do not know. All this seems to me irrelevant. Personally I like Schrodinger's treatment, but this is also theorising and irrelevant. I cannot see anything in this having reasonable bearing on our discussion.

Experimental evidence—*the facts of experiment*—are just what is needed. It is of course a minor point, as the time is so short, and the number of generations so few, but it fits well. This is sufficient, and we can ignore for the present wild and premature theorising.

Yours sincerely,

H. S. SHELTON.

7

EMBRYOLOGY AND EVOLUTION

DEAR DEWAR,

The evidence for evolution is cumulative. To the various types of evidence already considered we must now add embryology. We will start by considering carefully what an embryo is. In the Protozoa there is nothing that can rightfully be so called. In all (or nearly all) multicellular organisms, however, new individuals are formed by sexual reproduction. The sexual reproduction consists of the coalescence of two cells, called male and female, and from the single cell thus formed a new individual is developed. One fact with regard to this embryo needs to be emphasised from the start. The general form of the individual which is developing is determined from the beginning. However similar the embryo may be to those of other organisms at the beginning, or during its various stages, it *is* essentially different. We can, perhaps, best put it that the embryo and the adult are *the same individual.* You, I believe, sometimes emphasise this aspect, and so we can start by taking it as common ground.

Let us now consider how an embryo would be expected to develop on the assumption of evolution, or on the contrary assumption of special creation, either of species or of larger units. It is common ground that organisms change or mutate. Let us consider how this would affect the embryo. Obviously in an obscure and subtle way (probably in the structure of the genes) the embryo is also changed. But the change may or may not be apparent to simple observation. Let us take for an example an increased length of limb. This may be seen in the embryo when the limb structure is first formed. Or it may not be apparent till the last stage of the growth of the adult. Or it may occur anywhere in between. In the second case the embryo is apparently precisely similar to that of the ancestral organism. In the third case it illustrates the well-known fact, which can also in a sense be considered as embryology, that the young of allied species resemble each other more than do the adults. In the first case the embryo gives no evidence for, or indication of, the mutation that has taken place. Consequently the

embryos may sometimes show indications of their ancestry and sometimes may not, and it is not possible *a priori* to say when they will and when they will not. As a matter of fact they usually do, but this cannot be regarded as a universal rule.

I hope up to the present we can regard this as common ground. As you admit what you describe as transformism to a certain extent, you will I think agree that it is reasonable to look for evidence in the embryo for such transformations as you are willing to admit. Vialleton, I understand, admits evolution within the family. Assuming that he regards Equus and Eohippus as in the same family, he could hardly fail to recognise as evidence of evolution the well-known fact that in the embryo horse the second and third toes are much larger in proportion to the third toe than they are in the adult. Nor could he reasonably deny that the fact that in the Merychippus colt the ulna is separate, but fused with the radius in the adult, is evidence of the descent of the species from a form with a normal radius and ulna. The important question that arises therefore is to what extent these indications are actually found. Are they found within the family only? Or is there any indication whatever of a limit within which they are found? I wish to emphasise strongly that here you have an opportunity of testing your theory of units of creation, and particularly Vialleton's suggestion that the unit of creation is the family. If your contention (or Vialleton's) is correct, we should naturally expect fundamental differences in embryological development in different units of creation, but similarity in the species contained in the same unit. If by some extraordinary chance the scientific world should be wrong (which is possible), and you right (which is less likely), it is no use looking for your units of creation in the fossils. There are not enough of them, and the suggestion of the fragmentary nature of the fossil record is one you cannot possibly answer convincingly. But you might find these units of creation indicated by the embryos, as every animal has an embryo. That at least is a reasonable suggestion of one way in which you can find plain and obvious facts which will distinguish between the normal view of evolution and your particular theory.

That is sufficient about units of creation. Now let us look at the facts. The facts are that the embryo does give many clear signs of its ancestral history, more so than could be inferred *a priori*, and that these embryological similarities extend throughout the whole of the phylum. You might possibly make out some sort of a case

for regarding the phylum as a unit of creation, but you could not do it for any smaller unit. So obvious are these facts that they have given rise to the theory that the embryo recapitulates the history of the race. This I do not accept, and regard it as an undue simplification, but the very fact that it has ever been put forward shows how strong the evidence is. One important point to notice is that, in so far as the history of the race is indicated in the embryo, and I think it is so to a considerable extent, the embryo represents not the adult form of the ancestor, but the embryo of the ancestor, which has not been modified to the same extent as the adult form. What it amounts to is that, instead of going in a straightforward way to the formation of the adult form, the embryo develops in devious ways dependent on the fact that it will naturally follow the lines of development of the ancestor unless a further mutation occurs to modify the embryo and make the development more direct. Such modifications can only be expected to occur and survive in so far as they make smaller demands on the mechanism of growth, and so have definite survival value. They do in fact often occur. The best example I can give is the difference between the embryos of the fowl and the duck, which is obvious at a very early stage. These things have to be looked for, and are what can reasonably be expected, but they do not appreciably affect the enormous similarity in the embryological development in different families, orders, and even classes.

We will now give some specific examples of the similarity of embryological development in creatures which are far apart in their classification. Some have already been mentioned. Zittel tells us that the embryo bird shows tail vertebrae similar to those found in Archaeopteryx. What possible explanation is there of this except the obvious one that the bird is descended from a form with a long reptilian tail? Peripatus has an embryological development similar to that of the worm, though its adult structure is related to that of the arthropods. This seems to me good evidence of the genetic connection between worms and arthropods.

Now let us look at an example which is found in the *Origin of Species*[1] in which Darwin quotes a well-known passage from Von Baer:

'The embryos of mammalia, of birds, lizards and snakes, probably also of chelonia, are in their earliest stages exceedingly like one another both as a whole and in the mode of development

[1] Popular edition (1900), p. 605.

of their parts ; so much so in fact that we can often distinguish the embryos only by their size. In my possession are two little embryos in spirits, whose names I have omitted to attach, and at present I am unable to say to what class they belong. They may be lizards, or small birds, or very young mammalia, so complete is the similarity in the mode of formation of the head and trunk of these animals.'

Of course I am aware that a modern embryologist by microscopic work and the method of section cutting could identify at least the class of the embryo, but this is wholly irrelevant in view of what I have said before. There is no doubt that the embryos, in spite of their similarities, are fundamentally different ; but what possible reason can there be for this extraordinary similarity in the process of development except evolution? Darwin goes on to mention the great similarity in crustacean larvae, as illustrated by the barnacle, which in appearance is very unlike a crustacean, but is shown clearly to be one by its embryological development.

It is well at first to grasp main principles, so I will not overload this letter with examples, but will next illustrate them by the well-known branchial arches, often called gill-arches. Six grooved arches, through which pass important blood vessels, are formed at an early stage in the development of all vertebrates. In the ordinary fish four of these grooves become fully cleft and form the gills. In other vertebrates their cartilages usually form other structures, much smaller in proportion than the gills in fish, though in one or two cases, so far as is known, *they disappear entirely*.

Now let us state carefully the evolutionary argument. This structure refers, of course, to the embryo fish, not to the adult fish. The structure has its uses to the embryo, otherwise it would almost certainly have been suppressed. But all sorts of unexpected small organs, such as the thryoid cartilage, are formed from these relatively large structures in the embryo.

It seems to me impossible to account for this on any other hypothesis than that of evolution. On that hypothesis, all through the millions of generations, certain structures of the fish embryo have been retained. No explanation of all vertebrates being built on the same plan will do. No straining of the idea of a plan can equate the ear bones of the mammal with the fish's jaws, or the thyroid of a mammal with the gill of a fish, or another gill of a fish with nothing at all. The early stages of the embryo have remained unchanged, while the later stages have mutated. That is a clear and intelligible explanation, and I know no other.

The extreme similarities of the early stages of the vertebrate embryos are intelligible only in the same way. To a certain extent vertebrates are built on the same plan, but not to that extent. Birds, reptiles and mammals are very dissimilar in many ways, and on any other hypothesis it is not to be expected that their early embryos will be indistinguishable to an expert like Von Baer. But this fact gives very strong support to the idea of evolution, more so indeed than we are entitled to expect on *a priori* grounds.

You asked me about the early history of the vertebrates, and from what invertebrate group they are derived. As fossil remains of soft-bodied organisms are so rare, it is only by an extraordinary chance that geology can give us any help at all. But embryology helps a little. The first sign of the vertebrate column is the notochord—a gelatinous cylinder which forms in the place afterwards occupied by vertebrae. It happens that living creatures exist having no vertebrae, but possessing this dorsal notochord. Amphioxus is the best example. We therefore confidently assert the genetic connection of vertebrates with creatures possessing a notochord, and the combined division is called the Chordata.

At the other end of the scale gastrulation, that is the forming of a double layer of cells from a hollow sphere by its pushing inwards like a punctured rubber ball, is very suggestive. This method of forming the double layer does not appear to have any particular advantage over alternative methods, indeed in special cases it has been displaced by others, but it is undoubtedly the normal method. It is reasonable to infer that a free swimming gastrula form was one of the stages in the development of the higher forms of life. Some existing creatures still retain this structure.

The intermediate stages are obscure. There is some indication of segmentation. The skull is not, as would naturally be expected, formed from a single embryological unit. The gill arches, six in number in most vertebrates, are more numerous in amphioxus, and the Elasmobranch fish show additional vestigial pouches which do not develop. So I think we should look for a segmented creature. The difficulty is the dorsal nervous system. All known segmented invertebrates, so far as I know, have a ventral nervous system. Possibly further discoveries in invertebrate embryology may throw light on the problem.

I will conclude with a few more well-known instances where the embryo has not kept step with the adult, and shows features referring to the ancestral organism.

1. Embryos of some toothless whales have teeth, and the narwhal has a complete foetal dentition. The nostrils (blow holes) of foetal whales are further forward in the embryo, that is nearer the normal mammalian type.

2. The heart of the embryo vertebrate is simple, similar to that of the adult fish. Once again this refers to the embryo fish, not to the adult. One more embryonic feature has remained constant.

3. No less than three successive excretory organs develop in the higher vertebrates. The second, which is the one that persists in the adult fish, is formed in the embryo of higher vertebrates but later on disappears.

4. In tailless organisms such as the primates, the embryo has a pronounced tail.

5. The immature flat fish have eyes on both sides of the head in the normal vertebrate manner. At a later stage of development the eyes come slowly over so that both are on one side.

These special similarities could be multiplied indefinitely, but perhaps the strongest evidence is the extreme general similarity of the early stages of vertebrate embryos, and the way in which the young animals of all orders are usually nearer the normal type than are the adults. The facts in general are beyond dispute. What puzzles me is that anyone acquainted with these facts can fail to appreciate that they add a quota to the overwhelming evidence in favour of evolution. Certainly in embryological development I can find no trace of your units of creation, and am still waiting for some intelligible evidence that these mythical units actually exist.

Yours sincerely,

H. S. SHELTON.

DEAR SHELTON,

Almost the only common ground in the matter of embryonic development is that we agree that in nearly all multicellular organisms every individual begins existence as a fertilised ovum, and the general form of the future individual is determined from the beginning. From this point there is absolute opposition between your views and mine. My method is inductive, yours is deductive. I start off with no preconceptions ; I observe and study the phenomena with the object of trying to probe the marvels of embryonic development and to discover by what contrivances a highly complicated organism inheriting the characteristics of both parents can

be developed in a few weeks from a fertilised ovum of microscopic size. My studies have convinced me that from start to finish the process is the result of intelligent activity and that the difficulties are overcome by a series of beautiful devices. I find, *inter alia*, that the phenomena of embryonic development exhibit two outstanding features and that, when once these are grasped, the reasons for many of the phenomena become clear. These features are :

1. The structural changes through which an animal passes in its embryonic development follow the shortest and most direct route possible to the adult state, compatible with the immediate necessities of life. Among these necessities are an unceasing supply of nutriment and oxygen and the means of ridding itself of carbonic acid and other waste products of the chemical changes that take place within it.

2. The fertilised ovum is endowed with the power of developing at a very early period the tissue-producing cells or primordia of each of the major organs and structures that occur in any member of the phylum or class to which the animal belongs, even the primordia of structures which that particular individual will not need in the adult state : in the case of these the development of the primordia capable of producing them is early checked ; thus the embryo of every higher animal exhibits the primordia of both the male and female generative organs, but in normal circumstances only those of one sex attain maturity.

You start off with the assumption that every animal is descended from a very simple ancestor, and you are intrigued by Haeckel's 'theory that every embryo recapitulates the history of the race.' It is true that you regard this as 'an undue simplification,' but you say 'the very fact that it has ever been put forward shows how strong the evidence is.' For this reason you expect to find in a developing embryo conditions or states that represent 'not the adult form of the ancestor but the embryo of the ancestor.'

You say 'what it amounts to is that instead of going in a straightforward way to the formation of the adult form, the embryo develops in devious ways dependent on the fact (*sic*) that it will naturally follow the lines of development of the ancestor unless a further mutation occurs to modify the embryo and make the development more direct.' Then you say that you regard 'the theory that the embryo recapitulates the history of the race,' as 'an undue simplification.' Apparently you mean that, but for chance mutations that occasionally occur which check deviations, the embryo

recapitulates the history of the race. Then you say 'embryos may sometimes show indications of their ancestry and sometimes may not, and it is not possible *a priori* to say when they will and when they will not.'

Your theory has the advantage of being beautifully easy to defend. For example you say: 'Embryos of some toothless whales have teeth,' and you appear to regard this as a recapitulation of a toothed ancestor; but if I point out that, although you believe living birds are derived from toothed ancestors no living bird shows in the embryo any traces of teeth, you can reply 'their ancestors mutated in such a way that the teeth-stage of development has disappeared!' No one can disprove this, but I can say that your theory involves rather strange pranks on the part of the unknown forces that cause evolution, because the only two living monotremes (egg-laying mammals), both of which occur only in Australasia, lack teeth, but the embryo of one of them—Ornithorhynchus (duck-billed platypus) has teeth, while that of the Echidna (spiny anteater) has no traces of teeth. I assert that teeth occur in the embryos of toothless whales and the platypus because they are necessary to the embryo of these animals, but they are absent in those of the other two because they are not required. A massive jaw cannot develop properly in the embryo unless the jaw has embedded in it teeth on which to mould it, and that is why they occur in all toothless mammals having massive jaws, and in none that have slender, bird-like jaws.

You say I 'might possibly make out some sort of a case for regarding the phylum as a unit of creation, but I could not do it for a smaller unit.' You have to make this admission regarding the phylum because every phylum is constructed on a plan fundamentally different from that of every other phylum; in consequence, in each case embryonic development follows a different course from an early stage of development. But the first steps are the same in every embryo, i.e., the stages by which the fertilised egg develops into a multicellular embryo, because these stages must inevitably be passed through. Mechanical necessity demands it. Every fertilised cell, including its nucleus splits up into two equal parts, thus a two-celled embryo is formed. Then each of these again divides to form a four-celled embryo, this is followed by 16, 32, 64, 128, etc. stages; and all these cells are so arranged that no cell is completely surrounded by other cells; this ensures that every cell obtains the requisite oxygen and nourishment and has a means of

ridding itself of waste products.[1] This is why all embryos generally resemble one another in the earliest stages, but a time soon comes when the further development of each embryo differs from that of embryos of animals of a different phylum. Take the case of the vertebrate embryo. As all vertebrates are constructed on the same plan—the vertebrate plan—which entails bilateral symmetry, a dorsal central nervous system, a dorsal endo-skeleton (notochord or backbone), a head and a tail, and never more than two pairs of limbs, all vertebrate embryos, whether fishes, amphibia, reptiles, birds or mammals, develop in the same way until the general outline of the vertebrate plan is established. This again is a mechanical necessity if the embryo is to reach the adult state in the most direct manner. But, just as the embryos of different phyla develop differently, so do those of different classes within the phylum diverge in development after the stage at which the general outline of the phylum is established, and in the case of the vertebrates, the divergence begins directly the general outline of the vertebrate body is completed and when the features peculiar to any class begin to develop, in this case those peculiar to the fishes. This happens at the stage when the visceral or pharyngeal arches are formed. You incorrectly describe them as 'branchial arches, often called gill-arches.' These have no branchial functions. They form part of the wall of the pharynx and give passage to the blood-vessels carrying blood from the ventral to the dorsal part of the body. A heart and a blood system is indispensable to the developing embryo at a very early stage, long before a complicated heart could be developed, in consequence the early embryonic heart is a very simple one. Two tubes running parallel are formed which fuse into one to form a single tube, then the wall in front thickens, and the thickened part becomes separated by valves from the thinner part behind. The heart is now an efficient pumping organ composed of two communicating chambers : a posterior one, the auricle, that receives blood from the body, and an anterior one, the ventricle, which by the contraction of its thick wall, expels the blood into the arteries, the back-flow being prevented by the valves between the two chambers. The blood passes from the heart into a ventral vessel, which bifurcates, and each branch of this ventral aorta pushes forward until it reaches the first or foremost visceral arch, through which it runs until it enters the dorsal aorta. No sooner has this connection been established when in quick succession four more vessels are formed behind

[1] The formation of a gastrula is one of these devices.

that running through the first arch, these run through the other four arches, and so the ever-growing need of the head for blood is satisfied. Thus these arches and blood-vessels are indispensable to every vertebrate embryo at one stage of existence. They are a beautiful device for giving the head region the necessary blood, and I cannot think of any other practicable, much less better, method of securing this. Therefore I maintain that it is absurd to assert that these arches are formed because all higher animals are descended from fishes. Far from giving evidence for evolution these arches and their subsequent fate in the embryos of fishes on the one hand and those of air-breathing vertebrates on the other is very powerful evidence against the notion that a fish was ever gradually changed into an amphibian. As this is a matter of paramount importance, I am dealing with it in detail, although it makes a large inroad into my allotment of words.

In the fish embryo the tissue between each of the arches becomes absorbed and thus are formed the gill slits, and these grow wider. Meanwhile gills begin to grow on arches 3, 4, 5 and 6 and the blood-vessel in each of these arches divides longitudinally into two vessels that run parallel, one of which is connected with the main dorsal blood-vessel and one with the main ventral one. Then a network of tiny blood-vessels is formed in each arch which puts the parallel blood-vessels into communication and extends into the growing gills. By this arrangement the blood that comes from the heart in a venous or impure condition flows into the gills where it is aerated ; then it passes into the dorsal blood-vessel, thence to all parts of the body. None of the above events happen in the embryos of air-breathing vertebrates.

Now, you believe that some fish gradually turned into an amphibian and later one of these became changed into a reptile. This means that all the gill apparatus described above gradually disappeared, as did the third and fourth gill-arches of the fish, lock, stock and barrel, for *no trace of them occurs in any adult amphibian, reptile or bird*, but later they reappear from the blue to form the thyroid cartilage (Adam's apple) and the epiglottis (the leaf-like cartilage that forms a lid for the windpipe) in mammals. Yet not a hint of the above enormous changes is shown in the embryonic development of any amphibian, reptile, bird or mammal—the embryo develops as if fishes never existed. Do you believe that embryology lies to this extent? Do you seriously believe that, after they had done their work in affording a path to blood-vessels

required by the embryo in its early stages, these arches not only performed no later function, but completely disappeared. Then suddenly after millions of years they not only persisted instead of being absorbed but actually gave rise to *two entirely new organs*—Adam's apple and the epiglottis? Do you believe that these two organs originated gradually? Can you say of what use they were in their incipient stages?

If you really hold that the heart and blood-vessels develop in air-breathing animals in the way in which they do because these animals evolved from fishes, please say how you think the heart and blood-vessels of a vertebrate that never had a fish ancestor, but was first created as an air-breather, ought to have developed. As on this matter you and I differ *toto caelo*, I must ask you to support your views by describing *in detail* the process of the origin and development of its embryonic blood system.

The above is not the only absurdity of the evolutionary interpretation of the embryonic visceral arches. To repeat what I said on page 50 of *The Difficulties of the Evolution Theory* (1931): 'In no known fish do all the so-called gill arches give rise to gills. . . . In Elasmobranchs the mandibular arch forms the jaws of the adult. If the evolution theory be true that can only mean that . . . some unknown vertebrate lost its original mouth and used its first gill-arch as one!'

I hope that you will tell me and the jury how this happened, and why.

You cite as evidence for evolution and recapitulation the fact that 'no less than three successive excretory organs develop in the higher vertebrates.' I assert that these successive kidneys are an absolute necessity to every higher embryo, evolution or no evolution. As a kidney is a *sine qua non* of existence at a very early stage of embryonic development, long before a complicated one has time to develop, a very simple kidney, the pronephros is first formed. This consists of a row of two or three nephridia on each side of the body. These nephridia are minute tubes, one end of which opens into the body-cavity and the other into a common duct running along the length of the body leading to the exterior. Each nephridium comes into contact with a bunch of tiny blood-vessels known as a glomerulus. The waste products of the body are taken up from the blood by these nephridia and pass into the common duct and so out of the embryo. As the embryo grows, new nephridia arise behind the first ones. These are of more complicated nature and

so are described as a second kidney, the mesonephros. As these increase in number the simple pronephros disappears. A kidney of the mesonephros type suffices to carry off the excretory products of a comparatively simple animal, it therefore persists in fishes as the permanent kidney. But a mesonephros is inadequate for the needs of more complicated animals, in consequence a far more elaborate kidney, known as the metanephros, is developed behind the mesonephros. When at last this new kidney is ready to function, the nephridia of the mesonephros become absorbed, but their common duct persists and is used to convey male genital products. Thus, in my view, these three successive kidneys are a necessary and beautiful device for meeting the needs of the embryo while the final type of kidney is being developed ; and as this is a vital point of difference between us, I think that, if you do not agree with me, you should show exactly how you think the kidney of a specially-created air-breathing animal ought to have developed in the embryo. Please give at least as complete details as I have done.

To return to the visceral arches. You say, 'All sorts of unexpected organs, such as the thyroid cartilage, are formed from these relatively large structures in the embryo.' Why you should expect all these arches, after having completed their embryonic functions, to disappear without being utilised is beyond my comprehension. *Inter alia* they form the lower jaw, Meckel's cartilage, the bones and other parts of the ear, as well as the thyroid cartilage. All these organs develop at the place where they are required.

You say 'in tailless organisms such as the Primates, the embryo has a pronounced tail,' and suggest that this is evidence that 'tailless' vertebrates are descended from tailed ancestors. By tailless I presume you mean those animals, such as anthropoid apes, of which the tail does not stick out behind the body. I know of no tailless vertebrate. Under the vertebrate plan of construction the hind limbs and girdle invariably arise from a part of the vertebral column some way from the hind end, and the part behind this girdle forms the tail. This, like every other organ, takes different forms and is put to various uses, and during embryonic development it becomes modified to become longer or shorter to suit the needs of its possessor. It may vary in length considerably in different species of the same genus, thus in Felis it may be composed of as many as 29 or as few as 13 vertebrae.

As the primordia of the eyes in every vertebrate are on each side of the head, those of flat-fish are necessarily formed there.

But organs in the embryonic and larval stages very often change their original position. In such cases the adjacent parts of the embryo afford facilities to the moving organ ; in flat-fish a part of the bar of cartilage above the eye about to migrate to the other side of the body becomes absorbed so as to allow an easy passage to the eye. This gives the process the appearance of being intelligently directed.

In citing the eye as evidence for evolution you are treading on dangerous ground, because the eye develops in the vertebrate embryo in such a manner that until almost completely developed it could not function as an organ of sight. This fact is very unfavourable to your idea of the gradual evolution of the eye from a patch of integument sensitive to light.

Your reference to amphioxus and the notochord put me in mind of a drowning man grasping at straws to save himself. As amphioxus and many fishes thrive quite well with a cartilaginous notochord or backbone, it is up to you as an evolutionist to explain why, if all the primitive vertebrates had such a spinal stiffening, any went to the pains of replacing it by bony structure ! If you think the change took place gradually, please describe the stages, and say what caused the change ?

In the vertebrate embryo the backbone necessarily begins as a cartilaginous rod because, while the embryo is developing every part of it needs a continuous supply of nourishment, and the nature of bone is such that the presence of numerous small blood-vessels is necessary for its development. These are not necessary for the growth of membrane or cartilage, which, in consequence can be formed in the embryo at a time when the production of bone is not possible. Moreover, cartilage can grow interstitially, bone cannot. As soon as the embryo is sufficiently advanced to enable blood-vessels to form, ossification begins. As ossification is a slow process, in order to expediate it, the ossification of every vertebra and every bone in the body is effected from several centres.

Yours sincerely,

D. DEWAR.

DEAR DEWAR,

There is not much in your last letter that calls for a reply. I note that you think your method is inductive. I suppose you mean something ; but I have not the least idea what you do mean.

Once again your arguments are too subtle for me. So indeed are your interpretations of my letters. As has happened often before, I fail to recognise my own statements under the disguise of your travesties. Why, for example, you think I expect 'all these arches after having completed their embryonic function to disappear without being utilised' is beyond my comprehension. I don't. My point is exactly the opposite. The thing that seems to me remarkable is that occasionally they *do* disappear, and others, instead of forming considerable structures, form insignificant ones such as the thyroid cartilage. I regard this passage, and much of the same kind that occurs from time to time, as natural. The arguments for evolution are so plain that you need to shut your eyes to them or they would be convincing.

What are you trying to say all through this letter? You certainly have not made any attempt to answer my arguments. You say that 'the process (of embryological development?) is the result of intelligent activity.' What do you mean? If you mean that the process has been intelligently designed, of course I cannot answer that; but it looks as if you meant that the embryo is intelligent.

Again you say: 'the structural changes through which an animal passes in its embryonic development follow the shortest and most direct route possible to the adult stage compatible with etc., etc.' But they don't. And how do you know what is possible? The example of the branchial arches (or whatever else you like to call them) is plain evidence to the contrary. You say something about the development of the blood-vessels. Have you noted how they start with a bilateral symmetry, how this is modified and a number on one side disappear, and how finally a new approach to bilateral symmetry is formed? This enables me to answer your question. If there had been no evolution I should have expected a *direct* development to the blood-vessels of the adult form. Similarly, I should have expected only one kidney to be formed, and this to develop gradually, as required by the needs of the organism, to the adult form.

'The fertilised ovum is endowed with the power of developing at a very early period the tissue-producing cells or primordia of the major organs or structures—even the primordia of structures which that particular individual will not need in the adult stage.'

Exactly. That is what I have been pointing out as evidence for evolution. Most birds have only one functional ovary and oviduct,

but two are developed, and one remains as a vestige in the adult. Why? Does the embryo bird need two ovaries and oviducts? Is this the simplest and most direct route towards the adult stage? The obvious explanation is that the ancestor of the bird had the normal two ovaries and oviducts and that, in the subsequent evolution, the embryo has not kept step with the adult. What other explanation is there? Similarly the adult snake has only one functional lung, but two are developed in the embryo, and one remains as a vestige in the adult. The evolutionary explanation is obvious. What other explanation is there?

You say that although the embryos of toothless whales have teeth, those of birds have not. How do you know? If you will look at a modern book on embryology (such as that of Sir Graham Kerr), you will find this mentioned as a doubtful point. On the other hand, Prof. W. B. Scott in his book on evolution says quite definitely that the embryo teeth are there. I am not competent to settle this disputed point, nor I think are you, but at least you should not dogmatise on the basis of ignorance. It might perhaps have occurred to you that the rudiments of teeth are more difficult to discover in a small jaw, like that of the birds commonly used in embryological study, than in that of the whale. It is not necessary, however, for me to trouble about doubtful points. The undisputed ones are quite good enough.

You make some remarks about tails and find fault with me for describing man and the apes as tailless vertebrates. Of course, I am as well aware as you are that we and the apes have vestigial tails. That is good evidence for evolution. But I cannot understand what it is you are trying to tell me. In man and the apes the tail is much more prominent in the embryo than it is in the adult. Similarly, the tail of the embryo bird, as Zittel pointed out, is remarkably similar to that of the adult archaeopteryx. Why? On the assumption that all these forms are descended from animals with considerable tails this feature is intelligible. It is evidence for evolution. I cannot see that you have given us the glimmer of an alternative explanation.

I cannot find anything further in your letter that requires an answer. The development of the embryo in general, *and particularly the striking similarity of the embryos of different classes*, contain unmistakable evidence for evolution. Sufficient examples have been given. Your answer seems to me to cloud the discussion with a mass of detail, none of which contains any facts which do not fit

well into the evolutionary explanation. I suppose, if you have no case, that is as good a method as any other. Let us therefore bring the discussion back to the point where I left it in my last letter.

You (and even more, Vialleton) admit what you call transformism to a certain extent. What happens to the embryo when this occurs? Does it keep step with the adult? Or does it give evidence for such degree of 'transformism' as you are willing to admit? Finally, if the embryo develops directly to the adult, as you appear to contend, it is fairly obvious that (shall we say) a snake, a bird and a rabbit will develop entirely differently. Do they develop in such an entirely different way as would be expected from their adult forms? Also, if as you suggest there are units of creation, whether species or larger units, should we not rightfully expect that the different modes of development of the embryo will give us some idea what these units are? Can you find any such indications in the structure of the embryos?

Yours sincerely,

H. S. SHELTON.

DEAR SHELTON,

You say that there is not much in my last letter that calls for a reply. This is rather surprising in view of the five vital questions I put to you, questions going to the very root of embryonic development:

(1) Regarding the origin and development of 'Adam's apple' and the epiglottis.

(2) A request to make good your assertion that the development of the heart and blood-vessels (described by me in detail) takes a devious course, by describing how these should have developed had mammals not descended from fishes.

(3) As according to your theory some vertebrate lost its original mouth and used its first arch to make a new mouth, I asked you to tell us how this happened, and why?

(4) A request to you to make good your assertion that the development of the mammalian kidney (which I described) does not follow the most direct way, by describing, in at least as great detail as I have done, how the kidney ought to have developed had mammals not had fish ancestors.

(5) If you believe the earliest vertebrates had only a cartilaginous spinal stiffening, I asked you why this became replaced by a bony

one, and, if the change took place gradually, to describe the stages and say what caused the change.

You have completely ignored Nos. 1, 3 and 5 of the above and have disposed of Nos. 2 and 4 in eight lines !

You try to show that my feature No. 2 of embryonic development favours the theory of evolution. You ask does the embryo bird need two ovaries and oviducts ? The answer is : No, it needs them no more than a male mammal needs female organs or a female male organs ; nevertheless, they occur in the embryo. On your interpretation this means that the earliest mammal was an hermaphrodite. Do you seriously believe this ? You are doubtless aware that no known mammal, bird or reptile exhibits the phenomenon of hermaphroditism. Do you believe that the mammae on the breasts of men are relics of a golden age in which the men suckled the babies ? Nor is this the only difficulty you have to meet. Every reptile has two aortae, while all birds have only one —the left, and all mammals also have only one—the right. If your interpretation be correct, then in the past some reptile lost its right aorta and became a bird, while another lost its left aorta and became a mammal. Do you really believe that these strange things happened, and that gradually by the accumulation of small fortuitous variations ? Can you suggest any reason for the complete change in the system of blood circulation ?

I put it to you that you have utterly misunderstood the principles that govern embryonic development. Although the idea of any plan or design in the animal world is very distasteful to many people, there is no getting away from the fact that every phylum is constructed on a definite plan. This imposes the idea of a law or rule. That there are laws of organisation in the animal world is shown by the fact that everyone of all the millions of species belongs definitely to one or other of about ten types, each designed on a different plan. These types are the phyla ; below these are about sixty smaller types—the classes. One of the most striking features of the organic world is that it presents such a variety of different expressions of each of these types. In the vertebrate phylum there are the fishes, amphibia, reptiles, birds and mammals. In the cases of the fishes and birds there are about 15,000 expressions (species) of each. If any member of any type ever became changed into another—for instance, an amphibian into a reptile or a bird into a reptile—the conversion must have been sudden—a special creation.

It is not easy to explain embryonic development in non-technical terms, but at the risk of being tedious I will try to convince the jury of the fallacy of your views. Let us take the tail. In the developing embryo, after the original cell has become sub-divided into more than a thousand cells, these arrange themselves in every vertebrate embryo into three basic layers known as the ectoderm, mesoderm and endoderm which enfold each other. The embryo is, so to speak, built up round the notochord—the cartilaginous precursor of the backbone. The nerve tube is the first product formed by the ectoderm, and the primitive vertebrae immediately below it are that of the axial mesoderm. These proto-vertebrae are formed all along the whole length of the body. Thus the embryo at this stage exhibits a chain of segments, the function of which is to produce vertebrae, muscles and nerve fibres. In no living or known extinct animal is the body composed of a number of exactly similar segments, not even the earthworm, hence this condition does not represent any possible ancestor. These segments or metameric primordia are indispensable to the embryo and are purely embryonic structures. Although in outward appearance all are alike, each develops on lines peculiar to itself. The members of this series of primordia behind the spot at which the hind limbs bud out from the embryo give rise to the tail of the adult, be it long or short. According to Keith, the human embryo displays from eight to eleven of these incipient tail vertebrae. This is about the number found in *every* vertebrate embryo, whether the adult tail as in man and the anthropoids, is composed of four or five vertebrae or of 40 or more as in some long-tailed mammals. In some embryos the number of vertebrae increases during development, in others it remains constant, in others it diminishes, as in man. The human tail begins to diminish when the embryo is from 8 to 9 mm. long and by the time the embryo is 25 mm. long the whole of the vertebrated part of the tail has been withdrawn into the body. The vertebrae of this part fuse to form the os coccyx which soon becomes bent forward and serves for the attachment of certain important muscles. The fact that the tail is thus curved enables man to sit down without discomfort and probably affords the additional support for the viscera needed on account of man's upright posture. Notice that in its earliest state the tail of man is as long as that of a long-tailed animal. If man be descended from a long-tailed ancestor, it is reasonable to suppose that this organ would have in the course of time gradually become shorter at its inception, but

this does not happen. As you assert that man in his embryonic development recapitulates various stages of tailed ancestors, please explain why, in the whale (which you suppose to be descended from ancestors having well-developed hind-legs), the embryo does not exhibit at an early stage well-developed legs which later shrivel up? Why this difference in the two cases.

Here is another question for you. If the human embryonic tail exists only because man is derived from a tailed ancestor, how do you account for the fact that the human alimentary canal in its earliest condition extends in front of the mouth region and behind the vent, i.e., at that time there is a pre-oral and a post-anal gut? Do you seriously suggest that this is a relic of an ancestor which possessed a length of disgestion tube in front of the mouth and behind the anus? Clearly both the embryonic tail and the pre-oral and the post-anal gut of man appear in obedience to the laws that govern the development of every embryo.

To the question, why does the tail develop in this way, why does it not develop as legs do by being budded from the body, the reply is that, as the legs require nourishment during development, they have to be budded not at the extreme hind end of the body but a little way from this, and it is more simple to make use of the vertebrae existing behind the limbs than to scrap these and form new bones behind them. In this connection let me point out that there is no such thing as waste in nature: it is not wasteful to develop structures that are subsequently absorbed, or for an animal to lay millions of eggs of which only a few reach maturity. In each case the material employed is subsequently used, again and again.

As to the paired lungs of snake embryos and the paired ovaries of bird embryos, this involves no loss of time because these are developed simultaneously.

You are not quite accurate in what you say about Vialleton and me. Neither of us admit any degree of what we call transformism. The sub-title of Vialleton's last book is *The Transformist Illusion*. We both believe that animals are capable of limited modification. My views are thus expressed (*More Difficulties of the Evolution Theory* (1938), p. 178): 'I submit that this expression (i.e., "evolution within the family") reveals a defect in Biological terminology. It is unfortunate that the word used to describe the supposed gradual transformation of an amoeba into an elephant is also employed to describe the insignificant modifications which

animals undergo when subjected to new conditions of life. It would make for clarity and precision if these minor changes were described as differentiation rather than as evolution.

'This would not prevent the evolutionist from trying to show that differentiation is the first stage of evolution.'

Vialleton makes a similar distinction, but his terminology, being applicable to French-speaking people, is different. What I suggest should be called evolution he terms transformism, and he uses the word evolution to describe what I call differentiation. He, like me, laments the confusion in France between transformism and evolution, and in places he calls the latter Diversification.

A snake, a bird, and a rabbit do develop entirely differently after their embryos have reached the stage in development when divergence becomes necessary in order to form in the most direct way the characters in which they differ. So long as they are developing the basic features that characterise the vertebrate phylum they, of course, do not develop differently. Only one straight line is possible connecting two points. The small differences that distinguish species within the family are the last to develop.

In reply to your question whether there are any indications in the structure of embryos to show what the units of creation are, there are such indications. Here are three :

(1) The phenomena of embryonic development demonstrate that, if a member of one phylum ever became converted into a member of any other phylum, the conversion must have been sudden, by a change in the direction of the development during the period of segmentation or very shortly after.

(2) If within a phylum any member of a Class became transformed into a member of another Class, the transformation must have been effected *per saltum* by a change in the embryo ; for instance, in a vertebrate at or before the time of the appearance of the visceral arches. This precludes the idea that any air-breathing animal had a fish ancestor.

(3) If within the Class any member of any order ever became transformed into a member of another order, the transformation must have been sudden and effected by a change in the direction of embryonic development at an early stage ; for example, if a whale be descended from a land quadruped, the event that caused this must have been a change in the direction of the development of the embryo at or earlier than the stage of development when the primordia of the limbs are buds as broad as they are long.

Let me make it clear I do not say that such changes have been effected. We have no means of knowing, but if they have, they must have been effected as above, and so would be what I call special creations.

Dr. de Beer, who is an evolutionist, shows (*Embryology and Evolution*, pp. 86, 90, 104) there is no logical justification in regarding an embryological stage as evidence of such a stage representing an adult ancestor. Equally well might a present adult stage represent an embryological stage of an ancestor.

In conclusion, if I be wrong in saying that no embryo bird exhibits teeth, all you had to do to put me right was to name birds that do show this, or a single bird, and say who discovered this and where the record of the discovery is to be found. You have chosen, without giving references or quoting any statement, to say that modern books on embryology state that it is doubtful if such teeth occur and that W. B. Scott in a book (unnamed) on evolution says quite definitely that these teeth are there. Then you suggest that these supposed teeth are so minute as to be very difficult to see, apparently even under the highest powered microscope! I have not been able to find a single authentic record of such teeth. For this reason please let me have the reference to such a record. I am aware that it used to be thought that teeth occurred in parrots, but it was shown that these were not teeth, but skin papillae, like those under the hoof of the horse, of which the function is to nourish and replace the wear and tear of the beak. I also read in a popular book that someone is supposed to have discovered embryonic teeth in some kind of tern. No reference or details were given, and I have not been able to confirm this. As Scott specialises in mammals it looks as though in his little book on evolution he is relying on hearsay. But let me know what he says and above all give me the name of the bird or birds (with full references to the original record), said to have such teeth. This is a matter of some importance. If these teeth do occur, it becomes important to discover why such an unusual feature is exhibited by the bird or birds in question. Is it not strange that these teeth are not cited along with those of the embryos of whales and platypus as evidence of evolution?

Please pardon me for misunderstanding your remarks about the fate of the visceral arches. As the initial size of these arches is conditioned by that of the blood vessels they contain and as there are no large structures in the head or neck region where these arches occur, your statement, as you have since expounded it,

is to me more incomprehensible than it was before you enlightened me. Perhaps you will name five pairs of comparatively large organs or structures which you would have expected to be produced from these arches.

Yours sincerely,

D. DEWAR.

DEAR DEWAR,

Once again a brief reply is all that is needed. I will consider points 1 to 5 at the beginning of your letter. You say you have put to me five vital questions, but I cannot see that they are in any way vital. Let us take them seriatim :

(1) Regarding the origin and development of the Adam's apple and the epiglottis, I find a very complicated argument in the form of a question. Had you put it as an argument it might have been intelligible and worth an answer. As you put it in the form of a question, it is much too involved to reply in the form of an answer, and I am obliged to ignore it.

(2) *The Blood-Vessels.* I don't know how they would have developed if mammals had not descended from fishes, but they would certainly have developed more directly than they do.

(3) I never said anything about a vertebrate having lost its mouth. I believe the first arch forms the *lower jaw*, not the mouth. The earliest fishes we have discovered were jawless, and so probably also were the ancestors of modern fishes. Of course, I cannot tell you in detail the stages by which the jaw was developed.

(4) *Kidneys.* I cannot in any circumstances undertake to elucidate how an organ *ought* to have developed, and certainly not in any detail. It should be obvious, however, that one kidney is simpler than three.

(5) *The Vertebrae.* It is surely obvious that a mobile creature, especially one that grows to any considerable size, needs some reasonably rigid organ to which the muscles can be attached, and that a vertebrate column is in that way better than a gelatinous notochord. Needless to say, I cannot tell you the exact stages by which the vertebral column has developed. Anyhow, this is not an embryological question.

I note your comparison between the two oviducts of the birds and the male and female organs in the embryo. But surely, as every embryo has descended from both male and female ancestors,

this does give some sort of explanation why both organs are found in the embryo. In the same way, the only explanation I know why the two oviducts have developed in the birds when only one is functional is that they are descended from ancestors which had the normal two.

You ask for a reference for my assertion that the presence of teeth in the bird embryo is a doubtful point. On p. 330 of Sir Graham Kerr's text-book of embryology you will find the following passage: 'In modern birds a transitory epidermal thickening has been interpreted as the vestige of a dental lamina, but this evidence is not convincing. Careful researches in this direction in the less highly specialised birds are highly desirable.' There may be a slight shade of difference between embryo teeth and a dental lamina, but it does not seem to me to be significant. Prof. W. B. Scott's treatment will be found on pp. 70–71 of his little book on evolution. I said quite clearly in my last letter that this is a doubtful point, and so lay no stress on it. I merely objected to your dogmatism on an unsettled question.

I cannot make sense of your remarks about the paired lungs of snakes and the oviducts of birds. You say: 'this involves no loss of time as they are developed simultaneously.' What has this to do with the matter? The argument is that these organs are there because they are the remnants of organs which were functional in the remote ancestors, and have not been entirely eliminated. That is the evolutionary explanation and I know of no other.

I have really no more to say. In my first letter I put forward a number of arguments intended to show that the embryo gives many indications of its ancestry. I also pointed out with great care that if by any chance the scientific world was wrong, and there were such things as units of creation, it would be possible to find in the embryo indications of what those units are. Instead of answering you appear to me to have clouded the discussion with a mass of detail. I decline to be drawn into a detailed discussion of minor points. The argument of my first letter is there, and the reader must judge whether you have given any adequate answer.

Yours sincerely,

H. S. SHELTON.

8

ARE THERE NASCENT AND VESTIGIAL ORGANS?

DEAR SHELTON,

If the evolution theory be true and evolution still going on, the majority of animals should exhibit some nascent and some vestigial organs or structures, i.e., organs in an incipient or partly developed condition—organs that are to be of future use, and organs that are undergoing degeneration, because, owing to changed conditions of existence, they are no longer required. *The number of nascent organs should be about equal to that of vestigial ones.* But, as I said on p. 25 of my *Difficulties of the Evolution Theory* (1931) : 'Although the anatomy of thousands of species of animals has been carefully studied, it is impossible to adduce in any species a single structure which is indubitably or even probably in a nascent condition.' This statement was not challenged by Dr. Morley Davies in his *Evolution and its Modern Critics* (1937), which was 'primarily designed to be a reply' to my book.

The absence of nascent organs is a formidable objection to the evolution theory. Darwin recognised this and, characteristically, passed it over in a few words, saying 'it is difficult to distinguish between rudimentary and nascent organs' (*Origin*, 6th Ed., p. 298). He suggested in a half-hearted way three structures which he thought might be deemed to be in a nascent condition. As it can be shown that these are not nascent organs, evolutionists from A. R. Wallace onwards have with one accord kept complete silence on the subject of nascent organs. Although a chapter of my book is headed 'Nascent and Vestigial Organs,' the word 'nascent' does not occur in Dr. Davies' reply, but he devotes several pages to vestigial organs. The word 'nascent' is taboo in transformist society!

This, as in the case of most taboos, has not been imposed without reason. The existence of vestigial structures is regarded by many as evidence of evolution, and, if nascent structures existed side by side with them, these vestiges would be good evidence for evolution ; but the *presence of vestiges coupled with the total lack of nascent structures and organs is evidence against evolution and in favour of*

special creation. The existence of nascent organs is as essential to the evolution theory as is that of fossils of transitional forms. Even as the rocks have not yielded the latter, so the bodies of living animals fail to display the former.

If each type of animal be an independent creation, we should expect it to be endowed at the time of its creation with all the organs necessary for its existence in the conditions prevailing at that time. It might well happen that subsequently conditions changed and in consequence an organ originally useful ceased to be so and then disappeared or became vestigial. This seems to have happened in the case of some, at any rate, of the insects living on wind-swept islands. For example, many on Madeira lack wings or have wings too small to permi them to fly ; the insects on one of the stormiest spots on the earth, Kerguelen Island, are all incapable of flight. In the laboratory cultures of the winged fly, Drosophila, an individual is occasionally born that lacks wings, or in which wings are replaced by knobs or are very small. These 'sports' seem to be the consequence of one or more of the genes that control the development of the wings being inhibited in some way from fulfilling their normal function. Doubtless such freaks are born in nature. Ordinarily their inability to fly results in their being killed before they produce offspring, but on a wind-swept island such freaks being less liable than individuals having well-developed wings to be swept out to sea and drowned, may have lived to produce descendants, many of which have inherited the ill-developed wings, and in some cases these forms may have displaced the winged ones. But it is only in exceptional circumstances that these defective individuals would be preserved, and this explains why so few vestigial structures occur in animals. Let me here say that fully 90 per cent of what are commonly described as vestigial structures are nothing of the kind. In the mistaken belief that vestiges afford evidence for evolution and because such evidence is so difficult to obtain, many transformists describe as vestiges (1) muscles, tendons, bones, etc., which are less developed in a species than they are in most species ; (2) structures of which the use is not known ; (3) what Vialleton well calls embryonic remains.

A few transformists even go so far as to cite as useless vestiges, structures of which the use is known, but not to them on account of their ignorance of anatomy. As examples of such let me cite Dr. Julian Huxley, and Messrs. H. G. and G. P. Wells, who in their *The Science of Life* have inserted a section headed 'Man's

Body: A Museum of Evolution' (pp. 410–14), in which they solemnly assure their readers that such important structures as the body hairs of man and the fleshy fold in the corner of his eye are useless vestiges, 'each one of them a stumbling block to the believer in special creation but an ally to the Evolutionist.' There is perhaps some excuse for this display of ignorance in the case of the eye, but the uses of the human body-hairs are fully described in every good text-book of human anatomy.

As already stated, embryonic remains are either structures of use only in embryonic life, or structures resulting from the laws of embryonic development, so ill understood by many transformists. Examples are the organ of Rosenmüller in female mammals, the mammae of male mammals and the right ovary and oviduct of birds.

In conclusion, I may say that some transformists realise how rare vestigial structures are. Thus E. S. Goodrich, late Regius Professor of Zoology at Oxford, writes (*Ency. Brit.*, Vol 8, p. 926): 'It is doubtful whether any really useless parts are ever preserved for long unless they are insignificant, and many of the so-called useless organs are now known to fulfil important functions.'

Some structures exist of which the uses are not at present known; but it is rash to assert that these are useless. It may well be that the assertion, 'the number of useless organs is the measure of our ignorance,' will eventually be found to be correct.

Yours sincerely,

D. DEWAR.

DEAR DEWAR,

I think the difficulty about nascent organs is artificial and due to confusion of thought. There is a sense in which a great many organs can be described as nascent, and there is another sense in which there is no such thing as a nascent organ. Let me try to explain this as clearly as I can.

The sense in which nascent organs do not exist is the teleological sense. Except by some extraordinary chance (which is just possible but never to be assumed) no organ will develop unless, even in a rudimentary condition, it has survival value of some sort. As an example, I carefully explained in the first chapter that I thought incipient feathers were useful as an adaptation ensuring warmth *even in their rudimentary stages*. Similarly, while there are many grades

of complexity in the organ of sight, each one is useful to the organism. These organs are nascent in the sense that they are crude compared with the corresponding organs of other creatures. They are nascent in the sense that they are capable of further development, and may possibly develop further if there is a change in the conditions of life; but there is no necessity for them to do so. Similarly, as I explained in the chapter on morphology, the flying fish and numerous other creatures have nascent flying organs. They are reasonably well adapted in each case to the conditions of life of the animal, and, as a rule, there would be no advantage in their further development. Should, however, conditions change, as for example if there were no birds or bats in that particular neighbourhood, we should expect them to develop into efficient flying organs.

To summarise this briefly: the whole animal world is full of nascent organs in the sense of organs capable of further development. Obviously, as we cannot forecast the future, it is impossible to say which are nascent in the sense that they *will* develop further. There are no organs which are nascent in the sense that they *must* develop further. I hope this clears up your difficulty, and that this particular case of confusion can be eliminated from the discussion.

Now let us consider vestigial organs. You admit their existence to some extent, as for example the vestigial wings of insects on oceanic islands. I wish you would avoid question-begging epithets such as calling this plain adaptation to unusual conditions the survival of freaks. As, however, you agree that the wingless insects on oceanic islands are probably descended from winged ancestors, by what process of reasoning do you distinguish these from other cases of the same kind? For example, a considerable number of beetles have rudimentary wings. So have the whole of the order Aphaniptera (fleas). In these and many other cases the insects are wingless but show plain traces of the rudiments of wings. If you are going to admit that all these are descended from winged ancestors, that is evolution, and if you do not admit it where do you draw the line?

You refer me to that excellent book the *Science of Life*, and I have read again the section you mention, which seems to me a good one. May I say in passing that it would be more fitting, and more modest, if you would modify your tone both about the book and the authors. Julian Huxley ranks high as a biologist. G. P. Wells is a thoroughly competent biologist of the younger generation, and even H. G. Wells, who is the least competent as a biologist, took first class honours in zoology in his degree examination, and

is the author of a text-book of biology which is still used. Mistakes are possible, even probable, in so extensive a work, though I do not remember finding any myself, or having any pointed out to me by you. With regard to the points you mention, you speak of 'this display of ignorance in the case of the eye.' Perhaps you will be good enough to enlighten their ignorance and mine. I know that this particular fold is more liable to inflammation than any other part of the eye structure, but I do not know its use. Moreover, with regard to the hairs on the human body, I have studied this to some extent, though I do not claim to be an expert, and have come to the conclusion that they have no use, or none of any consequence. Perhaps you will enlighten us in that matter also, and at the same time you might be good enough to tell me your interpretation of the 'goose flesh' effect, if you do not agree with the obvious one given in the *Science of Life*.

All this, however, is a side issue. I am not impressed by your quotation from Prof. Goodrich, as there are so many insignificant vestigial organs that it is quite superfluous to trouble about any others. All the same, I agree that it is difficult to be sure whether or no there is some use in many vestigial organs. It does not seem to me to make much difference. A vestigial organ adapted to some minor use is nearly as good evidence for evolution as it would be if it were useless.

Yours sincerely,

H. S. SHELTON.

DEAR SHELTON,

When I assert that no nascent organ or structure occurs in any animal I mean that no animal possesses in any incipient or very incomplete condition any organ or structure not found in a very well-developed or complete state in *any* animal. In this connection I will repeat what I wrote on p. 51 of *More Difficulties of the Evolution Theory* (1898): 'Consider the significance of this absence of nascent organs. According to the evolution theory all multicellular animals are derived from one-celled ancestors, which exhibit nothing that can be called an organ in the strict sense. Consider now the vast number of organs and structures which are supposed to have evolved in the descendants of these organ-less ancestors; every differentiated cell, bone, cartilage, muscle, tendon, nerve, blood-vessel, ganglion, hair, feather, scale, spine, shell, spur, antler, horn,

hoof, claw, nail, tooth, tusk, antenna, appendage, every internal organ from the blood corpuscles to the stomach and liver. Every type of each of the above organs, according to the evolution theory, must have at one time existed in a nascent condition. Now consider the million or so existing species of animals, all of which are supposed to be in a state of flux, evolving. If these species be really evolving, the majority ought to exhibit nascent structures in all states of development, from unrecognisable excrescences to structures almost ready for use. Not a single one seems to exist! I know of only four explanations of this lack of nascent organs. (1) Every type has been separately created with all its organs. (2) New structures arise, in animals not gradually but *per saltum*. (3) Evolution no longer operates; it is a thing of the past. (4) That the capacity of evolving has ceased in the vast majority of animals, but is the possession of a few of them. Now, of the above, (1) and (2) are creationist and (3) and (4) are evolutionist explanations.

Such nascent new organs are quite easy to imagine, e.g., partly developed eyes in the buttocks of herbivorous animals, or glands of which the use is not apparent but which are on the way to becoming glands manufacturing a fluid that blinds an animal into whose eye it is ejected, and together with this the half-developed apparatus for squirting the fluid, or an incipient or half-developed fly-whisk on the neck of an animal to which flies are troublesome. The tail, which is often an effective fly whisk, does not reach to the front part of the body.

While the fact that no new organ in a nascent condition has been found in any of the thousands of species of any living animal is precisely what all theories of special creation demand it is fatal to a theory of gradual evolution.

You remind me that you had explained incipient feathers as an adaptation to insuring warmth, a bird equivalent for a fur coat. It is as easy to imagine a fur coat accidentally possessing the power to lift its owner off the ground as to imagine a furry feathery down fortuitously developing into the intricate and co-ordinated structure necessary to lift a bird off the ground.

As to the alleged vestigial organs, a house-fly that hatched out devoid of wings would be a freak whether the event happened in Teddington or on a windswept island. If an individual of an insect species characterised by wings laid an egg that hatched out into a flea, I should say that that flea had been specially created, just as I would of a bird that emerged from an egg laid by a lizard. From

the fact that the primordium that gives rise to wings in most groups of insects gives rise to halteres or stumps in some groups, it does not follow that these are derived from winged ancestors. You seem to find it difficult to distinguish between your opinions and established facts. Thus you say there are 'many insignificant vestigial organs,' but you have not furnished any proof of the existence of any of them. Then you say 'a vestigial organ adapted to some minor use is nearly as good evidence for evolution as it would be if it were useless.' This is not so ; any structure of any use whatever is quite compatible with special creation. But I challenge you to name such an organ and state its present use, its early form and the different use to which it was put formerly and the stages by which it lost the latter and acquired the former ; and please state what caused this change of function.

You ask me to enlighten your ignorance and that of Dr. Julian Huxley and Messrs. H. G. and G. P. Wells as to the use of the semi-lunar fold of the human eye. In the human eye this fold acts as a scoop which picks up foreign particles that enter the eye, with the result that they become formed into a sticky mass in the corner of the eye where it causes neither irritation nor damage and can easily be removed by the finger. The moment a foreign particle gets on to the eyeball the eyelids close tightly and tears are shed and thus a kind of cistern is formed so that the front of the eye is freely bathed and the offending foreign matter floats in the tears. A special muscle sweeps both tears and foreign matter to the inner corner of the eye and, past the minute orifice of the lachrymal ducts which are raised by the flood of tears from the surface of the eyeball, to the concave membranous edge of the fold which scoops it up and passes it on to the *caruncula lachrimalis*—a small patch of modified skin covered with very fine hairs and provided with sebaceous glands which secrete a fatty substance that envelops the offending particle.[1]

As to the use of the human body hairs, let me here repeat what I wrote in the article 'The Man from Monkey Myth' which appeared in the *Nineteenth Century* for April 1944, and has since been reproduced in pamphlet form : 'Because human hairs neither keep man warm nor provide a mat to which young babies can cling, Darwin's followers imagine that they are useless structures. The learned authors of *The Science of Life*—H. G., G. P. Wells and

[1] See E. P. Stibbe's paper (*Jour. of Anatomy*, Vol. LXII, pp. 154–175. There is a drawing of the apparatus in Gray's *Anatomy* (9th ed., p. 591).

Julian Huxley—make the following pronouncement (p. 410): "The body hair of men and women is purely vestigial. It no longer serves to prevent us losing heat. And yet each of these tens of thousands of useless hairs possesses a useless muscle by means of which it can be quite uselessly raised." The truth is that these hairs have an important function. Each is embedded in a follicle into which opens the duct of at least one sebaceous gland secreting an oily fluid necessary to keep the skin in good condition. These hairs and the muscles attached to them—the arrectores pilorum—have a two-fold function. The muscles, which are situated on the side of the hair toward which it slopes, on contraction diminish the obliquity of the hair follicle and render the hair more erect, and, at the same time, compress the sebaceous glands and expel their contents (Cunningham, *Text Book of Anatomy* (1902), p. 733). The presence of the hair and its movements also prevent the mouth of the follicle from becoming blocked with sebaceous matter. Follicles that have lost their hair sometimes become blocked and this may result in the formation of a sebaceous cyst.

As to the 'goose flesh' effect, the erection of the hairs forces much of the oily contents of the connected sebaceous glands out on to the skin and so this thin layer of oil diminishes the loss of body heat by radiation.

You describe *The Science of Life* as 'that excellent book.' This may be so, if regarded as a piece of evolutionary propaganda, but I do not commend it as a scientific or a semi-scientific book. There are a number of reasons for mistrusting it, of which the most important is the authors' suppression of many facts against evolution. In places the authors do not think straight, and on one occasion they improved upon a quotation in inverted commas. As to the former point they follow J. B. S. Haldane in confusing the *fact* of natural selection which weeds out defective organisms with the *power* of natural selection to produce evolutionary changes. Thus Haldane cites in an article called 'Darwinism Today' the case of a wood in which white-trunked birches were almost entirely ousted by pines, with the result that the whitish moths that showed up against the pines were gradually exterminated. Only a Darwinist could believe that the fact that a well-camouflaged moth has more chance of surviving than a poorly-camouflaged one has any bearing on the power of natural selection to produce new types. At the end of the process which Haldane describes the dark moths are still dark, and the whitish moths, though fewer in number, are still whitish.

The authors of *The Science of Life* introduced into their book this story and touched up the Haldane quotation so that it looked a little more Darwinian : 'The assertion is still sometimes made that no one has seen natural selection at work *in the production of new characters.*' The words in italics are not in Haldane's statement. Lunn, who reviewed *The Science of Life* quoted this sentence and asked where were the new characters that had been produced, whereupon Haldane promptly demanded an apology from Lunn for making him say something silly, and Lunn had the pleasure of apologising to one Darwinist for having assumed that he had been correctly quoted by another Darwinian ! The whole story is told in the preface to the second edition of Lunn's *Flight from Reason* —a book which I commend to you—and as he justly observes, it would take a great deal of natural selection to turn Haldane's moths into an argument for Darwinism. In later editions of *The Science of Life* the offending interpolation has been omitted.

I have criticised *The Science of Life* only as regards its zoology. Others have done so in the case of the section on psychical phenomena, for G. N. M. Tyrell writes (*Science and Psychical Phenomena*, p. 150) : 'The authors omit references to practically all the serious evidence. . . . The reader of this chapter is left with no idea of what are the true facts of the case. . . . It is not science ; it is special pleading.'

I do not think that my criticism of Dr. Julian Huxley is less respectful than yours about Vialleton ; moreover I justify my criticism, and no one can say that of yours.

Yours sincerely,

D. DEWAR.

DEAR DEWAR,

Your last letter is more interesting and more puzzling than most. Your definition of a nascent organ as one which has never at any time and in any animal developed further is new and strange, also it seems to me pointless. So also is your contention that protozoa have nothing that can be called an organ. Some protozoa are very complicated, as for example Paramecium, or even Vorticella. I think these examples answer your question about nascent organs. The bristles of Paramecium and the organ of attachment of Vorticella can well be called nascent organs. These organs of protozoa have not developed further, as other creatures which have

similar but more complicated organs are probably not genetically related. The reason is probably that more complex protozoa would not survive in competition with the better equipped multicellular organisms.

Generally speaking, however, *any* organ is likely to develop when conditions are suitable, and when the further development has survival value. You are not asking for a nascent organ in the ordinary sense of the word, but for a *unique* nascent organ. Apart from these and other protozoa I know of none, and I know of no reason, evolutionary or otherwise, why any should exist. It is possible that they do, but it is immaterial. This seems to me to be another example of the fallacy which I have often found in your exposition—namely your delusion that there is any necessity for a species to change, or evolve further. Evolution is a well established scientific term which describes the changes which have taken place, not a metaphysical one implying that they must take place. Consequently in scientific work we describe the organs that exist, and try to trace their development, both embryological and phylogenetic ; but we must not infer that organs of which we know nothing ought to exist, which appears to be your contention. This argument of yours appears to me to be confusion of thought, pure and simple, and all your complicated explanations (evolutionary or otherwise) of the absence of what we have not discovered is entirely superfluous.

I am not satisfied with your explanation of the use of the human body hairs, although some anatomists would support you. I do not think this contention can be maintained. In the first place hairs are not necessary for the working of the sebaceous glands. In certain parts of the body where this oiling effect is really required we have the sebaceous glands, *but no hairs*. In the second place on the palm of the hand and on the soles of the feet there are neither, and I do not notice any greater tendency to dryness of the skin there than elsewhere. In fact I think the facts contradict you. I find that the back of the hand which has hairs and glands is more liable to get dry than the palm which has none.

Once again, about the goose flesh, I very much doubt your interpretation. Is the skin more oily after goose flesh? I have never noticed it, and doubt it. My own view is that, so far as books on anatomy support you, this is a relic from pre-evolutionary times when it was thought to be necessary to find a use for everything. Sebaceous glands are necessary for the hair but the hair is not necessary for the sebaceous glands. So far as both remain on the

body they seem to me a vestige which is harmful rather than useful. Even on the hair of the head, people who are subject to scurf find the glands a nuisance, and the way they give rise to pimples and cysts is sometimes worse than a nuisance. I greatly doubt whether they serve any useful purpose, or, if there is any useful vestigial effect, whether it is not overbalanced by their harmful qualities. These organs appear to me to be useless, to a small degree harmful, but not sufficiently so to have been eliminated by natural selection. As a vestigial organ which has not yet been eliminated all this apparatus makes sense, but your suggestion that the hair is required to enable the sebaceous glands to oil the skin does not seem to me to be sound.

I need say very little further about the *Science of Life*. To speak of evolutionary propaganda in the present state of scientific knowledge and opinion is foolish. All except a very few of those competent to judge now accept evolution, and a book of this kind which did not explain why would be of very little use. My own criticism of the book is that I should prefer that part to be fuller. It is certainly not out of proportion to the rest of the book.

I have no wish to speak disrespectfully about Vialleton. I do not know what he has done to advance science (except write a text-book) and you have not told me; but I am willing to believe he has done something valuable, even though I do not know what it is. When I do not agree with him, in particular his contention that the family is the unit of creation (is this right?), of course I must say so. That he takes what appears to me to be an eccentric view is not a recommendation; but I have no desire to depreciate the few who support you (to some extent) more than is necessary.

All this is a side issue. So to a considerable extent is the question of the utility of vestigial organs. It is of course exceedingly difficult to prove that an organ is useless, and not very important. Probably some vestigial organs are useless and some are not, and it is not easy to draw the line. As you appear to agree that some are useless, as for example the second ovary of birds, and the second lung of snakes, mentioned in the last chapter, and some rudiments of the wings of insects mentioned in this, there is really no point in arguing about the usefulness or otherwise in doubtful cases. With regard to these I wish to make it quite clear that your explanation that these organs arise from the primordia in the embryo is no explanation at all. Of course they do. But why should these primordia be there if the organs are not needed? Once again here is a clear

proof of evolution. Otherwise you come back once again to the creation of lies or practical jokes.

I will conclude with a few more examples of vestigial organs :

1. Vestiges of hind limbs in some snakes, though not in all.

2. Vestiges of leaves in the butcher's broom and other plants where the normal function of leaves has been taken over by expanded stems.

3. Rudimentary stamens in such plants as Scrophularia (figwort).

4. The teeth under the skin in Platypus.

5. The few hairs on the muzzle of the whale.

6. The muscles for moving the human ear and the scalp, which are functional in a few abnormal people. Generally speaking, if time allowed, it would be very interesting to refer to Weidersheim who enumerated no less than 180 vestigial organs in the body of man. (S.410.)

I think this will do. As in other chapters it is not necessary to multiply examples. The whole animal world is full of organs which are important and functional in some species, but useless or converted to minor uses in allied species. Equally common is the morphological identity of organs with different uses as for example the stings of some insects with the ovipositors of others. Some vestigial organs are useless, others have different functions, in many cases the point is doubtful. It does not matter much. Whether useful or useless they show plain evidence of evolutionary change.

Yours sincerely,

H. S. SHELTON.

DEAR SHELTON,

As before when unable to meet my objections you describe them as incomprehensible or muddled, or confused. On this occasion you go one better ; they are 'confusion of thought, pure and simple.'

If the evolution theory be true almost every structure and organ in every multicellular animal must at one time have been what you describe as a *unique* nascent organ. Had we been present when some reptile was turning into a bird we should have seen on various parts of the body curious excrescences and wondered what they were. They would in fact be nascent feathers. Later these would assume what you might describe as the fur-coat stage of evolution ;

a little later we should see these structures growing very large on the forelimbs, and wonder what they were. So to-day we ought to see new organs developing, we ought to see clear evidence that many of the species around us are developing into something else. We should, in other words, see 'all nature in confusion' as Darwin admitted in the passage already quoted (p. 21).

Of what kind of organs do you imagine the 'bristles' of Paramecium and the organ of attachment of Vorticella to be the nascent state? As to vestigial organs once again you demonstrate the complete lack of objectivity in your controversial standards. When unable to explain a phenomenon in terms of evolution you complain loudly that it is unreasonable to dogmatise on ignorance, but you seem to expect me to give a perfectly satisfactory explanation of every alleged vestigial organ in terms of special creation. I have already said that the existence of vestigial structures is consistent with special creation (see page 223).

Here are my comments on the examples you now quote.

1. The hind limbs of some snakes assist them when climbing and hanging from trees.

2. Teeth in Platypus. I have already pointed out their use (see page 206).

3. Muscles that move the ears in man. These serve to stretch the epicranial aponeurosis, and provide facilities for the increased blood-supply to the external ear, thereby diminishing the risk of the ear getting frost-bitten. They also serve as a store-house for glycogen. Without some musculature in its structure the nutrition of the outer ear might be seriously impaired.

4. Bristles on the snout of the whale. What evidence have you that the whale descended from an ancestor of which the body was covered by hair? How do you know that these bristles are useless?

I am amused at your citing certain structures in plants as 'vestiges.' Do you believe that flowering plants recapitulate ancestral history in their embryonic development?

As to evolutionary propaganda, please tell me why practically every modern book on zoology sets forth only facts that do not appear to be against evolution and ignores all that appear to be against it?

In conclusion, whereas you make no pretence to offer any explanation of hit-or-miss phenomena, such as the transformation of fur-coat feathers into flight-producing feathers, I can offer an explanation of most of the structures which you describe as vestigial.

I note that your faith in evolution is such that rather than believe that man's body hairs are useful, you hold that these thousands of complicated structures, with the follicles that produce them and the muscles that work them, are not only quite useless but have persisted for thousands of years. Prodigious !

Yours sincerely,

D. DEWAR.

DEAR DEWAR,

I still think there is considerable confusion of thought in your interpretation of nascent organs. For example, in the early stage of the development of feathers, we could hardly be in doubt what the structures were. Obviously they would be divided scales. Nor is it true that most nascent organs would be unique. The sensitiveness to light of the anterior segment of the earthworm can well be described as a nascent organ, and I am not aware that it has developed to any considerable extent in any of the worms ; but it is certainly not unique. More elaborate organs of sight have developed independently in different phyla, but it is only the first of these (whichever it was) that could be described as unique. Poison organs have developed independently in insects, spiders, fish, reptiles and mammals (I think the skunk apparatus can be described as a poison organ), and even the throwing of the poison is found also in the spitting cobra. Similar needs give rise to similar structures but only the first can be described as unique. At the present time, when such a vast variety of organs have developed in the course of a thousand million years, it would be extraordinary if we could find anything absolutely new. Certainly it is not a reasonable argument against evolution that we have not done so.

How could the bristles of Paramecium develop ? There are many possibilities. They might develop into efficient swimming organs, or into poison tentacles. All sorts of things are possible. This, however, does not interest me. Evolution is something that *has* occurred and I am not interested to discuss what *might* occur.

The question of the utility of vestigial organs is a minor one. I am not prepared either to accept your dogmatic assertions about their uses, or even to dogmatise whether or no they are useful, or to what extent. This is a matter for future careful research. Anyhow it is not important. The existence of these vestiges is undoubted,

and is clearly explained by evolution. The only alternative explanation is the creation of lies or practical jokes.

I cannot make sense of your remarks about vestiges in plants. Most flowering plants have functional leaves. In a few species the functions of leaves are accomplished by stems, but the vestigial leaves are still there. The evolutionary explanation is that the plants in question are descended from normal plants with functional leaves. What has embryology to do with the matter?

The question of evolutionary propaganda arose out of your remarks about the *Science of Life*. As you now shift your ground to text-books on zoology, that criticism is now inapplicable. With regard to your further question why text-books 'ignore all that appear to be against it' (evolution) I can only say that, so far as I know they do not. At any rate you have had very considerable space to state those facts, and I do not yet know what they are. I cannot at the moment call to mind any of the facts you have mentioned, which you think are against evolution, which are not to be found in the appropriate text-book. You really should not make these general accusations. They are the sign of a bad case, and I do not think general accusations, which you have not substantiated in detail, help you in any way.

Yours sincerely,

H. S. SHELTON.

DEAR SHELTON,

If feathers evolved gradually, whether as the result of the fraying of reptilian scales or as new excrescences from the skin, they must have been in a nascent condition for a long period. In the earliest stages of their development they would not be recognisable as feathers, but later they would be before they were of use to keep their possessor warm or for flight. So with the spinnerets of spiders and other structures which we now see fully adapted to their purpose, and which, according to you, evolved from something else.

As I was a Magistrate and a Judge in India for many years and later practised at the English Bar I appreciate the importance of backing up accusations by evidence. My accusation that the case *against* evolution is virtually suppressed in modern text-books is easily decided. Please refer to what I have said about nascent organs in my first letter in this chapter. I shall in one of my letters on Man mention some suppressions of the discoveries of fossils.

How many of the books that assert that whales, sea-cows and bats are descended from quadruped ancestors, tell their readers that not a single fossil has been found of the Pro-Cetacea, Pro-Sirenia and Pro-Chiroptera which the evolution theory postulates?

You profess a great admiration for *The Science of Life*. You can discover for yourself how many of the facts and arguments in my letters are found in that book. It is for our readers, and not for you, to decide whether those facts and those arguments are suppressed because they do not deserve an answer or because they cannot be answered.

Perhaps you will quote it, if *The Science of Life* provides any adequate answer to Darwin's difficulty which you have twice evaded. We should expect, if evolution be true, to find 'all nature in confusion,' organs in every state of development from nascent organs to perfected structures. Why don't we?

As to Paramecium, if by 'bristles' you mean its cilia, these are already efficient swimming organs found in a large group of the Infusoria. If you mean its 'Trichocysts,' these are more efficient than poison tentacles would be because they can shoot out poison threads!

Yours sincerely,

D. DEWAR.

DEAR DEWAR,

You truly say that in Paramecium, a unicellular organism, there are to be found a variety of adaptations, particularly, in a sense, swimming and poison organs. No doubt they are adapted to its very simple mode of life, otherwise the creature would not have survived. All the same they are crude compared with the organs of multicellular animals. They thus can well be described as nascent organs which have not developed further in this genetic series. I would remark also that, as so many such organs are found in the protozoa, it is not reasonable *now* to expect to find nascent organs which have not developed further in some species. I cannot see anything in this argument but confusion pure and simple.

With regard to the books dealing with evolution, you have again shifted your ground. You now say that *arguments* have been suppressed. You really cannot expect any book to trouble much about your arguments, except one like this, and that of Dr. Morley Davies, which are written for the purpose.

The suppression of relevant *facts* is another matter, and that you have no good ground for alleging, and weaken your case, such as it is, by so doing. The books on palaeontology, such as Zittel, are quite clear about what fossils have been found, and I do not think they exaggerate in any way. With regard to the disputed human skulls, the only recent book I know which deals with the matter fully enough to trouble with them, namely that of Sir Arthur Keith, discusses them at considerable length. I know nothing of this alleged suppression of facts and strongly disapprove of these vague accusations.

Yours sincerely,

H. S. SHELTON.

9

SOME INSTINCTS AND HABITS OF ANIMALS

DEAR SHELTON,

One of the many reasons why I reject evolution is the fact that thousands of animals have instincts and habits, which, in my view, cannot have evolved or developed gradually. Each of these is what Arnold Lunn aptly describes as 'a hit or miss phenomenon—an action that has to be got right the first time.'

As I have done much bird-watching I will begin by describing the nest-building of the Indian sunbird, and that of the common house-martin, because I have watched the construction of the nest of the former at all stages, and every one in England may easily watch the latter at work.

I may say, in parenthesis, that in my *More Difficulties of the Evolution Theory* I have said something about even more wonderful nesting habits—that of the Indian tailor-bird of making a nest by tacking leaves together, that of the oriole of slinging its nest to branches in the manner in which a hammock or shrimping-net is slung to its supporting frame, and that of the weaver-bird of constructing a hanging nest made of plaited grasses.

I maintain that the nest-building habit of the sunbird (Arachnecthra asiatica) cannot possibly have originated gradually, because the nest in an incipient condition, or, indeed, complete save for the finishing touches, would not only be useless as a nest, but a positive death-trap. *The nest of the first sunbird must have been almost as perfect as it is to-day.* In shape and size it resembles a pear, and pear-like it often hangs from the branch of a tree. More usually it is suspended from a branch of a bush. But the bird is catholic in her choice of sites. The nest is often suspended from a rafter in a verandah or a bungalow. I once saw a nest attached to the pendant of an electric light bulb, and another attached to a creeper some way down a well. As regards material for the nest, all is grist that comes to the mill of this little craftsman or rather craftswoman, for the hen alone builds : bits of grass, straw, bark, cocoons, lichen, hair, thorns, dried wood, and even scraps of paper and bits of rag. The materials used are not woven together ; they are attached to one another by

cobweb, and by means of this the nest is fastened to its support, possibly assisted by some other sticky substance. The nest has at one side an entrance hole, over which a little porch is constructed. While sitting on the eggs the hen often looks out through the entrance hole with her chin resting on the lower edge. Having chosen the point from which the nest will hang, the bird begins by winding some cobweb round this by means of her bill. Then she brings bits of dried grass, etc., to the cobweb to which they stick; then the attachment is strengthened by further applications of cobweb. Thus the nest is held together by cobweb. By the end of the first day the upper part of the nest has been formed. On the second day the skeleton outline of the whole nest is completed. During the three following days the exterior is strengthened and the egg-chamber is lined with silky cotton-seed or the like. When the nest is nearly complete the bird is at great pains to adapt the interior to the size of her body by repeatedly turning herself round inside. Usually two (rarely three) eggs are laid; thus, after the eggs are hatched, the nest has to support the weight of three or even four birds. I have never come across a nest unequal to the strain. Clearly a hanging nest such as this would be useless as a nursery until almost as complete and strong as it actually is. If you do not agree that the habit of making a nest of this kind must have originated *per saltum*, please show *any way* in which the habit of building can have arisen gradually, and describe a possible evolutionary series of nests leading from a scrape or hole in the ground to the sunbird's nest as it is to-day.

Here is Gilbert White's description of the process of constructing the nest of the common house-martin (*Natural History of Selborne*, letter XVI to Daines Barrington): 'The crust or shell of this nest seems to be formed of such dirt or loam as comes most conveniently to hand, and is tempered and wrought together with little bits of broken straws to render it tough and tenacious. As this bird often builds against a perpendicular wall without any projecting ridge under, it requires the utmost efforts to get the first foundation firmly fixed so that it may safely carry the superstructure. On this occasion the bird not only clings with its claws, but partly supports itself by strongly inclining its tail against the wall, making that a fulcrum: and thus steadied it works and plasters the materials into the face of the brick or stone. But then, that this work may not, while it is soft and green, pull itself down by its own weight, the provident architect has prudence and forbearance enough not to

advance her work too fast ; but, by building only in the morning and by dedicating the rest of the day to food and amusement, gives it sufficient time to dry and harden. About half an inch seems to be a sufficient layer for a day. . . . By this method in about ten or twelve days is formed a hemispheric nest with a small aperture towards the top, strong, compact and warm : and perfectly fitted for all the purposes for which it is intended.'

A nest of this type, equally with that of a sunbird, would be a death-trap until sufficiently large and strong to hold and bear the weight of mother and chicks. If you think this habit can have been acquired gradually, the least you can do is to describe its growth either from the habit of building a nest of a different type or from no nest.

Needless to say that habits that cannot have originated gradually are not confined to birds. They are to be found in a great many different kinds of animals. The many books of Henri Fabre are filled with accounts of such habits in insects. I will not notice any here, but will notice the habits of the spider that lives in a diving bell of its own construction. The water-spider, which inhabits our pools and sluggish streams, is, like all spiders, an air-breather, but it builds under water a dwelling place in which the young are reared, and these, like the adults, breathe air. This home has the shape of a diving-bell : its walls are of silk, manufactured by the spider. It is moored to submerged leaves or to stones on the floor of a pool by silken threads which the spider spins. Before it can be inhabited it has to be filled with air brought to it by the spider. The body hairs of this creature are so constructed as to entangle bubbles of air ; this applies particularly to the hairs of the abdomen which are long and hooked at the tip. Thus the spider when it goes underwater is enveloped in air, and this causes it to look as if clothed in silver, hence its name Argyroneta, the silvery-spinner. The lower part of the nest is connected with a surface weed by a strong silken cord, spun by the spider, along which it runs between its home and the surface. After the bell is completed the spider makes many journeys from it to the surface where it collects air which it takes below and liberates under the nest, so that this replaces the water there. This process continues until the water in the nest is all replaced by air, and then the habitation is ready for occupation, and the family is reared in it and kept as dry as if the nest had been above water. By the same process used-up air is taken to the surface and replaced by oxygenated air. I assert

that this habit cannot have been acquired by an ordinary land-spider by a gradual process of evolution. The ordinary spider cannot survive long under water. On being submerged it immediately curls up and is drowned if not rescued within a few minutes. Thus the spider could not have begun to operate under water until it had become fully adapted to this, and this adaptation would have been useless as such until nearly complète. And I find it difficult to see of what use it can have been above water while it was developing. Having acquired the adaptation, you presumably have to imagine that another set of haphazard variations caused the spider thus equipped to take to entering water. Even so your spider has a very long way to go before it constructed its nest under water. I cannot think of a habit half-way between that of rearing the young in the air above water and of rearing them in a receptacle under water. So far as I can see, the change of habit, if it occurred, must have been *per saltum*. Do you seriously suggest that this change of habit was the consequence of the accumulation of a number of haphazard variations, that had no effect on the habits of the spider until all terminated simultaneously at the point when one set of variations caused the spider to take to making a silken sheet of the proper size and shape impermeable to water, another set caused the spider to construct silken guy-ropes, a third set to attach the ends of these to stones or other objects under water, a fourth set caused the spider to crawl under the sheet and let loose air there, and to repeat the process until the sheet became a diving-bell, a fifth caused the spider to oxygenate the air in the bell when necessary, and a sixth set caused it to decide to change its old practice of rearing the young above water to that of rearing them in the bell? Do you suggest that this concatenation of hundreds of variations was such that it caused the spider to discard the mode of life of its ancestors and exchanged this *successfully* for quite a different mode of life? If these be not your views I am sure that our readers will welcome your exposition of them.

Yours sincerely,

D. DEWAR.

DEAR DEWAR,

I must congratulate you this time on sending me a really good letter, in substance, form and tone. Up to the present I have found very little that you have said that appears to me to be even decently

plausible ; but this time you have stated problems that are beyond my power to answer to my own satisfaction. The instincts of animals, and particularly of insects and spiders, as a whole are certainly an unsolved problem. Moreover, to give even plausible suggestions as to the stages by which these instincts have arisen requires the knowledge of a specialist (or rather of a committee of specialists in different orders of animals), which, needless to say, I do not possess. I am not a specialist in the sections of natural history dealing with birds, insects and spiders, and consequently anything I may say on these matters, in addition to being speculative, must also be of a very amateur kind.

At the same time I think a few comments are allowable. The first is about your dogmatism. The whole world of science and of nature is full of unsolved problems. No progress at all would be possible if men of science were to adopt your attitude and say that because a problem was difficult and unsolved, even so much so that we cannot see the lines on which a solution is possible, therefore it is insoluble. Consequently to say that any of these instincts or methods based on instincts 'cannot have arisen gradually' is an unwarranted piece of dogmatism. Nor is it reasonable to speak of a nest 'in its incipient condition.' Obviously a nest at all stages, though it may vary in complexity, must have been good enough for the purpose, and any variation that may have occurred must be from some other form of nest which was reasonably well adapted to the same or to slightly different conditions. We do not know by what stages these instincts have developed, but I think it is a sufficient answer in some cases to say that they *must* have been modified from some previous condition.

One of your examples is that of the house-martin. A very short time ago there were no houses, no bricks, no convenient stones such as those which are found in stone houses, and no mortar, which makes a very convenient point of attachment. The pieces of broken straw, again, did not exist before human cultivation. What corresponds roughly to straw undoubtedly exists in nature, but it is different in character and texture. Clearly therefore the method of nest building *must* have been modified in comparatively recent times. Unless you are contending that the house-martin was specially created after the time when man began to build brick and stone buildings, you must admit that instinct and method are not fixed quantities given to the creature at its creation, but variable and subject to modification. It might throw some light on this

question if we knew whether this particular species builds nests elsewhere than in human buildings, and if so where and how. Possibly this is known, but I do not know it. Anyhow in this case there can be no doubt about the modification of the methods of nest building.

Once again I wish you to realise one other consequence of this non-possumus attitude. The instincts and methods of allied species, especially of birds, vary greatly. You must therefore here be contending for the special creation of each separate species. What therefore becomes of your friend Vialleton's contention that the unit of creation is the family? Your arguments are just as valid against him as they are against me. This indeterminateness of what was created puts you in the position of merely postulating special creation whenever you are in a difficulty. That is quite easy but somewhat futile. I will give you an example to show that this occurs to others beside yourself, and the light in which it appears to the working specialist who finds difficulties. The following is from Professor Hawkins recent presidential address to the geological society. (*Quarterly Journal Geological Society*, 1943.)

'The teleological doctrine of special creation affords the only easy explanation. However we have to realise that such a tenet affords no real explanation at all but is merely a restatement of the facts in an incomprehensible jargon.'

There are difficulties in many branches of natural history. When you find them you can if you like say special creation, but it really does not mean anything unless you reduce it to particulars. There is a sense in which special creation is intelligible, but in order for it to be so you must express it in some sort of exact and intelligible form. Unless you have some clear idea of what you mean, and some notion of what was created and when and where, it is not a scientific hypothesis, but merely, as Prof. Hawkins says, a piece of incomprehensible jargon. As an example of this confusion, in our discussion on geology, you found fault with my examples because you thought they did not go beyond the family. Now you contend for special creation of species and unknowingly acknowledge that my arguments were very relevant indeed.

Perhaps I had better say a few words about the water-spider, although I do not know enough about spiders to discuss them at any length. As a suggestion it is perhaps worth considering whether a previous form of the spider's nest may not have been attached to leaves, etc., on the surface of the water, and the power of the spider

to carry air for its own breathing might have arisen gradually in course of diving for prey or in escape from enemies. Alternatively, the nest might have been attached to the body of the spider, as indeed is the case with some land spiders. All sorts of suggestions are possible, but in any case they are pure guess-work, more indeed of a guessing competition than are the puzzles of morphology.

That is one more guess, and if I thought it worth while no doubt I could give a few more. I must acknowledge, however, that it is a valid point to make (though it proves too much) to point out that many instincts, especially of insects and spiders, are unexplained. The only remedy I can suggest is further study and research.

Yours sincerely,

H. S. SHELTON.

DEAR SHELTON,

Apparently in order to prevent your admission that I have stated problems which are beyond your power to answer to your own satisfaction being an anti-climax in this debate you make a number of assertions calculated to impart a little liveliness into this chapter.

You complain of my dogmatism because I named nesting habits of two birds, which 'in my view cannot have developed gradually,' and asked you, if you disagree, to show that I am wrong. Far from attempting this you say they '*must* (italics yours) have been modified from some previous condition.' Is not your assertion the more dogmatic? What you describe as my dogmatism I prefer to describe as my sceptical reaction to your dogmatism. My position is this: many instincts cannot have evolved gradually, and it is up to you to produce a plausible hypothesis as to how these hit-or-miss phenomena could have evolved gradually.

I have never asserted that, because a problem is very difficult and unsolved, it is insoluble. I do assert that you ought not to accept evolution until you can suggest a plausible hypothesis as to how the host of hit-and-miss phenomena can have evolved gradually.

As regards nests in an incipient condition you confuse the fact that every sunbird's nest in the earliest stages of its construction is in that condition with the problem of the inception of the habit of constructing a nest of the sunbird type and the difficulty of conceiving

of a series of completed nests starting from no nest and ending with a sunbird's nest.

You assert that 'the instincts and methods of allied species, especially of birds, vary greatly,' and that I 'must therefore be contending for the special creation of every species.' To this I reply that, as a general rule, to which there are many exceptions, the nesting instincts and habits of allied birds are very similar. All the 16 species of Indian sunbirds make the same kind of nest. All the kingfishers (there are 18 different species in India) nest in holes in banks dug by them: all the woodpeckers (there are 48 species in India) make nests by boring holes in trees, except Micropternus, which feeds largely on ants and bores its nest hole in an ants' nest attached to a tree. I have never contended that every species was specially created; I have told you plainly that for years I have been trying to discover the extent to which animals vary. I have never suggested that instincts are not variable. Indeed to my mind one of the greatest objections to all theories of evolution by the accumulation of fortuitous variations is the *adaptability* of most instinctive actions. Take the tailor-bird. The nest of this bird, depicted in the frontispiece to my *More Difficulties of the Evolution Theory*, is attached to only one pinna of a huge Stercula leaf. This is uncommon. Most often a whole leaf or two smaller ones are made use of, but there are records of nests in which three or four, and even as many as eight, leaves have been utilised. In each case the procedure is a little different. Do you seriously believe that the accumulation of fortuitous small variations can not only have taught the bird to build its nest, but to adapt its methods of work to different conditions? I most certainly do not. I put it to you that the theory that all things, including life itself, have developed out of salts in the sea is philosophically unsound, since you cannot get plus out of minus, or the mind of a Shakespeare or even the nest-building instinct of a sunbird, out of lifeless material. This instinct must have been implanted in this tiny bird by its Creator. It is you who assert that instinct can be accounted for by a purely natural process of evolution. That is your dogma. Now anybody who enunciates a dogma can fairly be challenged to show that that dogma does not involve absurdities. It is reasonable to ask you to produce a plausible theory to account for the phenomena in terms of your dogma. This you refuse to do.

You have hinted at what you think might be possible ways in which the spider's diving-bell might have originated. If however

you go into details you will soon find you are up against formidable difficulties. If this habit were the only one the evolutionist has to account for he might plead that an event against the happening of which the odds were billions to one had come off once, that the accumulations of fortuitous variations had accomplished this. But there are hundreds of these habits and the apparatus for carrying them out which you have to account for. It would seem that, except for the wheel, almost every mechanical device invented by man is used by some animals. Do you really believe that all these habits and utensils are the work of natural forces?

Nor is this the only difficulty you are up against. Speaking generally the most successful species, or, if you prefer, those of which the range is most extensive, are not those endowed with the most wonderful instincts. Take for example the toads. Most species take no care whatever of their eggs. The female lays them in clusters in water, and the male ejects his sperm over the cluster. No further notice is taken of these eggs.

Some species of toads however take great care of their eggs. One of these is the midwife toad (Alytes obstetricans). 'The male of the midwife toad,' writes E. L. Grant Watson (*More Enigmas of Natural History*, p. 7), 'seizes the female round the waist, after the usual manner of toads and frogs. With appropriate movements of his toes, he stimulates her to stretch out her legs, then places his own hind legs between them and bends up his knees at an angle, thus forming a kind of receptacle into which the eggs are laid. These eggs are yellow and are threaded together by sticky, elastic threads. Two to four layers of about ten eggs are laid. At the moment that the eggs are laid, the male shifts his hold on the female's waist to an embrace nearer to her head, and a little later stretches out his body in the act of fecundating the eggs. Again after a few minutes interval, he attaches the strings of eggs to his own legs, passing his feet through the egg-cocoon, and holding the gelatinous strings against his abdomen, so that the egg-mass bulges out round the posterior end of his body, he then retires to a safe retreat, where he hides during the day time. At night he comes out to feed, and on these nightly walks the eggs are dampened by the dew. After three weeks he takes to the water, still carrying the eggs with him. By this time the tadpoles have hatched: they bite their way through the envelope of jelly which has held them together. Not till the last of the tadpoles is free of the egg-strings does the father disentangle himself of what remains.'

Now the genus Bufo which takes no care of its eggs ranges over all the temperate and warm parts of the world except Madagascar and the Australasian area, while the midwife frog genus is confined to Western Europe. Nor is this all: fossils of Bufo have been found in Lower Oligocene rocks, showing on our agreed time-scale that the genus has been in existence for over 40 million years. If then this toad has lived all this time without taking any care of the eggs, why should the 'midwife' habit have arisen? Here is Joseph Needham's naïve reply (*Biochemistry and Morphogenesis* (1942), p. 33): 'In the course of time it was found that the protection of the eggs could best be carried out not merely by the continued presence of the parent organisms but by their actually carrying the eggs about with them. This is what happens in the case of the toad Alytes obstetricans.' This carrying of the eggs occurs in several species of toads, frogs and fishes. There is a toad found in Brazil which carries the eggs on its back and its skin grows round each egg so as to enclose it in a receptacle provided with a lid. Nor is this all, the male has a bladder-like pouch which projects, while the eggs are being laid, from the cloaca, and directs them backwards on to the female's back. Do you think that this habit together with the special apparatus needed developed gradually? In a considerable number of species of frogs and fishes the eggs are carried in the mouth of one or other of the parents until the young emerge. For example take the aft-topsail fish—Felichthys felis—found off the Atlantic coast of America. As the eggs are laid, over 50 in number, the male takes them into his mouth and carries them there 65 days until they hatch out and the young that emerge attain a length of about 3 inches. As it is difficult to see how the male can feed when carrying the eggs in his mouth, it would seem that he accumulates a reserve of tissue upon which he can feed while the mouth acts as a nest.

I am obliged to you for calling my attention to Professor Hawkins' assertion. This, like the oft-quoted pronouncement of D. M. S. Watson, goes far to explain the fact that so many people accept evolution in face of what seem to me to be a host of insuperable difficulties. You, Hawkins, Watson, Julian Huxley and many others are unable to see any plan or design in nature. Does it not occur to you that others may be justified in saying that they do see plan and design throughout nature? Does it not occur to you that you may be behaving like a colour-blind man who denies the existence of colour?

You complain that I say that in the present state of knowledge it

is impossible to know what the units of creation are, and that I postulate special creation whenever I am in difficulty. Well, there are a great many phenomena in the living world which neither you nor any one else can prove to be the result of the accumulation of fortuitous variations. This being so, to assert, as you do, that these phenomena *are* the result of fortuitous variations seems to me to be most unscientific, and is a proposition that ought to be challenged. I contend that, so far from the belief in special creation being unscientific, it is the *denial* of that possibility that is unscientific. G. Fano, remarking that 'the mind of many biologists is set, so that they will not tolerate the discussion of any force that is not physical or chemical,' truly observes that 'the immensity of our ignorance when compared with the very little that we know does not authorise such dogmatic obstinacy in us. It would be more scientific to assume a humbler attitude and be less assertive in our formulae.' (*Brain and Heart*, Eng. trans. (1926).)

Yours sincerely,

D. DEWAR.

DEAR DEWAR,

I have your last letter and find great difficulty in understanding it. I have said quite plainly that you have stated problems which are beyond my power to answer to my own satisfaction. That is quite easy. Anyone can do that about almost anything. Nor can I see anything dogmatic in saying that the house-martin could not have built nests in houses before there were any houses, and consequently it *must* have built them somewhere else. Still less am I unable to see that it is not allowable to assert evolution unless I can give a 'plausible hypothesis as to how these hit-or-miss phenomena can have evolved gradually.' I do not know how or where the house-martin built its nest before there were houses. On general principles I should surmise that the nests were of the same character, possibly built on cliffs or rocks, but that is a guess. I am not fond of guessing, and prefer to say that I do not know. It is something like the problem of what was the habitat of the clothes moth before there were any clothes. I should guess the furs of dead animals; but again it is only a guess. Incidentally it is only by an extended use of the term that a change of habit like this can be described as evolution, and I am inclined to regard this matter as beyond the scope of our discussion.

With regard to the manner of building of allied species of birds you say: 'as a general rule to which there are many exceptions the nesting instincts of allied species are very similar.' That is sufficient for my purpose. Unless you are contending for the special creation of species in the case of the many exceptions, the problem is yours as well as mine, and so irrelevant. Moreover, even when the nests are similar, there are considerable variations, and, if you think there is some well-defined limit to these variations, it is for you to say how and why.

You not only admit adaptability but assert it. 'Indeed to my mind one of the greatest objections to all theories of evolution by the accumulation of fortuitous variations is the adaptability of most instinctive actions.' But why? You are speaking here about birds. The adaptability is not nearly so great in insects. The bird has a brain—small compared with ours and even smaller than that of the average mammal of the same size—but still not altogether rudimentary. I see no reason whatever why it should not have some sort of intelligence sufficient to enable it to make minor variations in nest building. What is the difficulty?

I wish you would not drag in fortuitous variations and 'developed out of the salts of the sea' in sections where they do not properly belong. Fortuitous variations means nothing more nor less than variations of which the causes are unknown; we are not discussing the origin of life in a chapter on instincts; the mind of Shakespeare is equally irrelevant. I have no dogmas, or at least I have stated none. My contention is merely that the evidence for evolution is so overwhelming that any well-informed person not hopelessly prejudiced must accept it. I am not dealing with the causes, except that I may say a little in the last chapter if space allows. I really do not think it necessary to argue whether 'this instinct must have been implanted in this tiny bird by its Creator.' You seemingly intend it as an argument for special creation, but it is equally available for Deistic or Theistic evolution. Nor am I discussing whether or no there is design in nature. I have of course my own views on these speculative questions, but I shall not complicate the discussion by expounding them now. Anyhow they are speculative.

The only question which now concerns me is the common phylogenitic descent of the animal world, especially birds, spiders and insects. That these creatures have remarkable instincts is undoubtedly true. I understood that you were attempting to show that such instincts were incompatible with common phylogenetic

descent, and in particular that they could not have arisen by gradual modification. You have now however strongly asserted their adaptability, which is exactly my contention, and so the point, whatever it may have been, is lost. I always do my best to meet your objections, so far as I am able to understand them, but I fear in this matter our points of view are so different that mutual understanding is impossible. Certainly all through the book you have often failed to understand me, though I have done my best to be clear. Often, also, as here, I fail to understand exactly what it is you are trying to prove. When that is so, the best we can do is to pass on to the next section.

Yours sincerely,

H. S. SHELTON.

10

THE ORIGIN OF MAN

DEAR SHELTON,

Man is unique among living organisms. If we take his mental qualities into consideration we have to place him in a separate kingdom, as Geoffroy St. Hilaire did. If we consider only his anatomical characters it is an open question as to whether he should be placed among the Primates, because, as Prof. F. E. Parsons says : 'there is a greater gap between the musculature of man and that of the other Primates than there is between many different orders.' (*Ency. Brit.*, vol. 15, p. 990.)

The mental gap between man and the rest of the organic world is immense. As Max Müller puts it, one great difference 'consists in language as the outward manifestation of what the Greeks meant by *logos*.' (*Brit. Assn. Rep.* (1889).) Mivart pointed out that in all the accounts of evolutionists of the so-called origin of language the essential element of reason is 'quietly smuggled in as a matter of course.' He illustrates this by Darwin's suggestion that 'the wisest of the Pithecoids was able to *think* of a device for the information of his fellows.' Mivart's comment on this assertion by Darwin is : 'but it is just this first step which is ignored by those who desire lightly to span the gulf between brute and man.' (*Origin of the Human Reason*, p. 385.)

Nor is this all. To quote Thomas Dwight, Parkman Professor of Anatomy at Harvard (*Thoughts of a Catholic Anatomist* (1927)) :

'Not so strong of arm, not so swift of foot, without well-developed hairy hide, or large teeth, or strong claws, he (man) seems as a mere animal an exceedingly unfortunate one, very unfit for the struggle for existence in that imaginary period of half-fledgedness between brute and man.'

If we suppose that all these deficiencies overtook man after he had acquired his great brain, we are up against the difficulty that we have to suppose that all the men living suffered these losses simultaneously, otherwise those who suffered them would have been overcome by their better equipped fellows.

Another difficulty is that to-day we see a huge gap as regards

mental capacity between all races of man on the one hand, and the anthropoid apes and all other animals on the other. If the great brain had been acquired gradually, is it conceivable that none of the long line of intermediate individuals should have left descendants now living? This objection applies equally to the lack of present-day intermediates between every highly-specialised animal, such as a bat, a whale and a turtle, and its assumed generalised ancestors. As man, to quote Vialleton, is 'as far separated from his supposed simian relatives as bats and whales are from other animals' (*Membres et Ceintures*, p. 281), clearly the gradual evolution of man from a non-human ancestor is as impossible as that of a whale from a land mammal.

The fossils lend no support to this supposed evolution. They tell us that men and apes in the past were more diversified than they are to-day—a fact not in accord with evolutionism. They also tell us that in the past big-brained and small-brained men lived contemporaneously; that the skull capacity of a human fossil is no criterion of its antiquity. No one dares to point to any fossil and say of it that while not human it is of a species ancestral to modern man.

I am constrained to say that the fossil evidence has been so manipulated by evolutionists that the writings of the great majority of them do not give an unbiassed account of the facts. The fossils unfavourable to the evolution theory are almost universally ignored in modern books. One of these is the Calaveras skull found in 1866 in the U.S.A. in a deposit of Pliocene or possibly Miocene date. This seems to be the oldest known fossil of a man of modern type. But, with the honourable exception of Sir Arthur Keith's *The Antiquity of Man* (1925), the last book I have come across in which this fossil is mentioned is S. Laing's *Human Origins* (1892). This skull Keith (*op. cit.*, vol. 2, p. 471) calls 'the "bogey" which haunts the student of early man—repelling some, and fascinating some and taxing the powers of belief of every expert to breaking point.' The only reason for rejecting this fossil or rather its antiquity is that it does not fit in with the preconceptions of evolutionists! As Prof. Holmes put it (*Smithsonian Report*, 1899) to suppose that man can have remained unchanged through so vast a period is hardly less than admitting a miracle. Keith's comment on this assertion is (*op. cit.*, p. 473): 'It is equally difficult to believe that so many men should have been mistaken as to what they saw and found.'

The second fossil generally ignored consists of skeletal remains

of modern human type found in a Pliocene deposit at Castenedolo in Italy by Prof. Raggazoni in 1860. The well-known zoologist Sergi, who visited the site and saw the fossils and deposit, is satisfied that they are of Pliocene date. But, with the exception of Keith's book and Johnson's *The Bible and the Early History of Mankind* (1943), the latest book in which I have found mention of the Castenedolo fossils is Keane's *Ethnology* which appeared in 1901, although, as de Quatrefages and Keith, remark, but for the fact that these fossils do not fit in with the theories formed on the origin of man, no one would have dreamed of doubting them, much less rejecting them.

It is legitimate for transformists to disbelieve their antiquity, but it is not legitimate for writers to ignore them in their books, especially those written to instruct the public. The reason of this almost universal rejection by transformists of these early fossils is that their acceptance would destroy completely the claim of the much-vaunted Pithecanthropus fossils from Java and the Sinanthropus fossils from China which are paraded as possible ancestors of modern man, because these two fossils are of much later date than the Castenedolo and Calaveras fossils. In fact there are several more fossils of men of modern type of earlier date than Pithecanthropus and Sinanthropus, which are largely ignored. As I work inductively and not deductively and am not hampered by preconceptions, I accept the evidence of the rocks and realise that the known fossils tell us nothing whatever about the origin of man. Until such time as fossils of Pithecanthropus or Sinanthropus are found in Miocene deposits, I consider it pure waste of time to regard the creatures represented by the above fossils as possible ancestors of man. I look for ancestors of any animal in deposits laid down *before* those containing the earliest known fossils of the animal in question.

Quite apart from the fact that no mechanism is known capable of endowing an ordinary mammal with man's mental powers, there are morphological reasons that render the gradual evolution of man highly improbable.

It is impossible to explain by any existing theory of evolutionary mechanism, such as natural selection, the gradual transformation or evolution into a man of an animal having any of the characters which I shall cite; in other words no ancestor of man can have possessed any of these characters. These characters are:

(1) A hairy coat to which the young could cling, thus allowing the mother full use of all four limbs for locomotion.

(2) Quadrupedal gait.
(3) An opposable great toe.

Let us consider these. The passages that follow within inverted commas are from an article by me in *The Nineteenth Century and After* April, 1944) :

The Hairy Coat

'The main function of the body hair of apes and monkeys is to provide a kind of mat to which the young clings when carried by the mother, allowing her full use of all four limbs for brachiation or other form of locomotion. The young New World monkey hangs on to the back hair of the mother ; young Old World monkeys and apes cling to the hair of the mother's underparts. Le Vaillant records that he shot, in British Guiana, a monkey carrying a young one on its back. The youngster, which was not injured by the shot, continued to cling to its mother's dead body while this was being taken to the camp. In order to tear it away Le Vaillant had to get the help of a negro. When disentangled the young one made a dart for a peruke on a wooden block. It embraced the peruke with all four hands and could not be induced to quit it for four weeks.

'Now consider the case of a species of ape of which the body hair grew gradually shorter and finer. The shorter the hair became the more difficult it would be for the young to hang on and the greater would be the mortality resulting from them falling to the ground when the mother was moving fast ; and *ex hypothesi* Natural Selection would prevent the shortest-haired females rearing young, for, said Darwin (*Origin of Species*, p. 63) : "We may be sure that any variation in the least degree injurious would be rigidly destroyed." The only way in which the unfortunate species of which the body hair was becoming progressively shorter could avoid extinction would be for mothers to take to using one of their limbs to hold the young one. As this would allow only three limbs for locomotion, the mothers when fleeing from enemies would be sorely handicapped and so be eliminated by Natural Selection.'

Quadrupedal Gait

The supposition that man is descended from a quadrupedal ancestor is, I submit, unsustainable, man's upright posture and gait mark him off very sharply from all other types. Darwin did not

appreciate this. As he did not trouble to ascertain whether or not his imagined transformations were mechanically possible, the change from quadrupedal to bipedal gait presented no difficulty to him.

'He wrote (*Descent of Man*, p. 78): "We see . . . in existing monkeys a manner of progression between that of a quadruped and a biped." This is not so. Monkeys are quadrupedal, but, as they spend most of their time in trees, they are more agile, more supple than creatures which rarely leave the ground. Hence those who derive man from a quadruped naturally assert that this ancestor was a tree-dweller, be it ape, tarsier or lemur. They have to get man's ancestor up a tree. How it got there, how it became transformed from a ground- to a tree-dweller, they make no attempt to explain. Darwin starts off with an ape living in the trees and then makes it descend to the ground. Having got it back to *terra firma*, Darwin has to get it on its hind legs. Accordingly he writes (*op. cit.*, p. 76): "as it became less arboreal . . . its habitual manner of progression would have been modified; and thus it would have been rendered more strictly quadrupedal or bipedal. . . . Man alone became a biped; and we can, I think, partly see how he has come to assume his erect attitude. . . . Man could not have attained his present dominant position . . . without the use of his hands. . . . But the hands and arms could not have become perfect enough to have manufactured weapons or to have hurled stones, as long as they were habitually used for locomotion. . . . From these causes alone it would have been an advantage to man to become a biped. . . . To gain this advantage the feet have been rendered flat; and the great toe has been peculiarly modified, though this has entailed the almost complete loss of its power of prehension."

'What a picture Darwin draws of this prohominid, which, with commendable foresight and noble self-denial, abstains from using its fore-limbs for locomotion, and suffers agonies in its gallant efforts to balance itself and walk on its hind legs! How its spine, hip-, leg- and foot-bones, to say nothing of the great toes, must have ached while they were being reconditioned to adapt themselves to erect posture! Nor did these aches and pains entirely cease when, at last, the erect position was acquired. Dr. John Murphy solemnly assures us (*Primitive Man*, p. 76): "When the upright posture was new to the precursor of man, the necessity of frequent rests from it would be greatly felt."

'Even Natural Selection must have been moved to pity by the plight of this prohominid and so refrained from destroying it; otherwise, according to our evolutionists, man would never have come into being.[1]

'There is, as Vialleton puts it (*op. cit.*, p. 284), "absolute opposition between the attitude and the locomotion of man and those of the apes." No amount of wishful thinking or special pleading can dispose of this fact. He criticises a picture drawn by T. H. Huxley, showing a series of skeletons of anthropoid apes and man, all upright or almost so, differing only in size, the dimensions of the cranium and the arms, and a slight inclination of the spinal column. "This drawing," he writes (*Membres et Ceintures des Vertébrés tétrapodes*, p. 640), "which dissembles the contrast between anthropoids and man, has done much to impress on the minds of the incompetent the notion of perfect continuity between these two groups; it is one of the most striking examples of the schematism so often employed in support of transformist ideas." '

The Opposable Great Toe

'We have now to consider the supposed loss of the power of opposing the great toe. The corresponding toe of an ape may be compared to one of the blades of a pair of scissors, the other blade being represented by the remaining toes, these last being bound together by a band of fibres known as the transverse ligament. In man this ligament embraces the great toe as well as the other four, thus the human foot, as compared with that of the ape, is like a pair of scissors so tied that it cannot be opened. The hind limb of the ape is an efficient grasping organ, which the human foot is not.[2] Now, the transverse ligament must either embrace the great toe, or not embrace it; no intermediate condition is possible. If, then, man be derived from an animal having the great toe opposable, this non-opposability of his great toe must have arisen suddenly, *per saltum*, as a sport. As this would have imposed a great handicap in the struggle for existence, the Darwinist seems compelled to

[1] In addition to the handicap imposed by the change of gait, the incipient hominid would have suffered from the shortening and weakening of the arms. Baumann's dynanometer tests showed that a male chimpanzee is 4.4 times and a female chimpanzee 3.6 times as strong as a physically developed fit young man.

[2] In all anthropoid apes and a few monkeys the foot is a more efficient grasping organ than the hand. Hartmann, who objected to their feet being called hind hands, had to describe them as prehensile feet.

believe that after a definite date almost every individual had this disability, because, had only a few suffered from it, they would have been, in Darwin's words "rigidly destroyed." '

In other words, the blind forces of nature must have caused thousands of prohominids simultaneously to lose this opposability.

The theory of evolution is supposed to obviate the necessity for miracles. In fact it merely substitutes for the miracles of creation phantasies which even transformists compare to miracles. Indeed some of them speak of these phantasies as miracles, although apparently they do not deem them to be caused by supernatural agency. Thus W. Beebe writes (*The Bird*, p. 97) : 'The idea of miraculous change which is supposed to be the exclusive prerogative of fairy-tales, is a common phenomenon of evolution.'[1]

Here I must comment on Darwin's statement : 'With some savages, however, the foot has not altogether lost its prehensile power, as shown by their manner of climbing trees, and of using them (*sic*) in other ways.'

Notice the question-begging word 'lost.' Darwin here shows that his knowledge of anatomy was not profound. He did not appreciate the fundamental difference between the opposability of the big-toe in apes and in some humans.

'As Wood Jones points out, in *Man's Place among the Mammals*, "the human mobility of the big toe is effected by movement at the metatarsal-phalangeal joint, whereas in the monkey and ape the movement is largely at the saddle-shaped tarso-metatarsal joint." In less technical language, as the transverse ligament in man binds together the bones of the sole of the foot, the toes jointed on these are capable of a little independent movement varying in extent with the individual, just as the fingers of the hand are. In apes the big toe and the sole bone on which it is hinged can move at the joint with the ankle.'

Yours sincerely,

D. DEWAR.

[1] Professor J. Lefèvre writes (*Manuel Critique de Biologie* (1938), p. 35) : 'Grâce à Haeckel le transformisme est à son apogée. Il a repandu partout sa foi ; la parole ardente des maîtres entraine irrésistiblement les élèves. Dans leur leçons chargées de la mystique nouvelle, il n'est question que d'animaux se battant, s'allongeant, se ramassant, se tordant, se retournant, se pliant, redressant leurs bras, s'ornant d'appendices, se créant des organes, se fabriquant des tentacules et des yeux, se transformant les uns dans les autres se différenciant et se perfectionnant à volonté : prodiges plus merveilleux et beaucoup plus miraculeux que l'idée créatrice elle-même.'

APPENDIX

EVOLUTION THEORIES REGARDING THE ORIGIN OF MAN

CLASS I

MAN EVOLVED FROM A LEMUR-LIKE ANCESTOR

Group I

All races of men are derived from the same species of anthropoid ape (Monophyletic Origin).

(a) From an unknown kind of ape (Darwin).
(b) From a Dryopithecus-like ape (Haeckel).
(c) From a Sivapithecus-like ape (Pilgrim).
(d) From an Australopithecus-like ape (Broom).
(e) From a gorilloid ape (Morton, Schultz).
(f) From a brachiating ape (Keith, later view).

Group II

Different races of men are derived from different kinds of apes (Polyphyletic origin).

White man and chimpanzee derived from one kind of ape ; Negroes and gorillas from a second kind of ape ; Mongolians and orangs from a third kind of ape (Ardt, Kurz, Crookshank).

Group III

Man and anthropoid apes are derived from a common ancestor and are separate from all other Primates.

(a) From a Simian of later date than Propliopithecus (W. K. Gregory (later opinion), Elliot Smith (later view), Buxton).

(b) From Propliopithecus (W. K. Gregory (earlier view), Leakey).

(c) Man branched off from the Lemur stem before the appearance of Propliopithecus (Keith).

Group IV

Man descended from a Lemuroid without passing through an ape stage (Haacke).

CLASS II

MAN EVOLVED FROM A TARSIOID ANCESTOR

Group I

Man passed through an anthropoid stage (Elliot Smith (earlier view), Hooten).

Group II

Man did not pass through an anthropoid stage (Wood Jones).

CLASS III

Man evolved from a New World (Platyrhine) monkey (Hubrecht).

CLASS IV

Man evolved from a Prototarsioid (Le Gros Clark).

CLASS V

Man evolved from an upright ancestor early in the Tertiary Period (Osborn).

CLASS VI

The human stock branched off from the main Primate stock before the Lemurs and Tarsiers evolved.

This is Sera's theory. According to him the Order of Primates (from which he excludes Lemurs) is formed of six independent branches :

(1) represented by the Polynesians but by no anthropoid ape ;
(2) represented by the Mongols and the Orang ;
(3) represented by the Caucasians and the Persians but no anthropoids ;
(4) represented by the Europeans and the fossil ape Dryopithecus ;
(5) represented by Negroes, the chimpanzee and most gibbons ;
(6) represented by the Bushmen, the Andamanese, the gorilla and the syndactylous gibbons.

CLASS VII

Man branched off from the other Vertebrates as far back as Palaeozoic times ; the group to which he belongs had opposable thumbs and great toes. This group rose higher in the scale than all others because of its big brain. One branch of this stock gave rise to the Australian aborigines and bushmen, a second gave rise to Neanderthal man, the Negroes, the gorilla and the chimpanzee, a third branch gave birth to the Gibbons and Pithecanthropus, and the fourth developed into all the other types of man and the orang. This is Klaatsch's theory.

CLASS VIII

Man is derived from the most primitive mammal, and his line did not pass through a lemur- or tarsier- or anthropoid-stage. This is Westenhofer's theory.

CLASS IX

The six types of man cited by Sera above, each evolved independently of one another from different lumps of amorphous matter in the sea, which independently formed themselves into multicellular animals, each of which, by processes like those that occur in the development of embryos, followed by changes such as occur in larvae, eventually developed each into a different type of man. This is Sergi's theory.

DEAR DEWAR,

There is no doubt in one sense about the uniqueness of man. The question we have to discuss, however, is not the surface incredibility of his descent from other animals, but whether or no it has occurred. This is not to be decided by the casual thoughts of a Catholic anatomist, nor by the controversies of the last century between Darwin and Mivart, but by the evidence *taken as a whole.*

This I shall try to deal with in the present letter, and perhaps I may be allowed to reserve a few minor details till my next letter. I need not, for example, discuss your speculative objections to the evolution of a ground ape with bipedal gait, because, as I shall show later, Broom has proved that such an animal actually existed. Also I do not accept two of your fossil skulls as genuine, but instead of discussing that now I shall merely mention fossil forms that are universally accepted, and not trouble about those that are disputed. Nor shall I trouble about the exact line of descent of man from other primates, as it is first of all desirable to give evidence that man *has* so descended, although the details are not known.

What is the alternative? I think in this case there can be no doubt what it is. The anti-evolutionist in the category man must include all men now living or recently extinct, whether Veddahs, Australian and Tasmanian aborigines, pigmies, negroes, Chinese, or Europeans. These must be claimed as *one* special creation. I do not think you will dispute this, and it is important to emphasise it because some of the difficulties in tracing the descent of man from other primates are also found in the tracing of the genetic descent of the various types of men now living. We have therefore a fairly clear-cut issue. Man (by which is meant all men now living), is either *one* special creation, or, like the rest of living beings, a product of evolution, that is directly descended from other forms of life. Which is the correct answer to the problem?

On this point I must confess to a certain amount of sympathy with the prejudices of yourself and your fellow anti-evolutionists. The Christian Church in the past 80 years has adapted itself fairly well to organic evolution as a whole. Rome, for example, which in such matters is very cautious, but often more enlightened than other sects, allows full liberty on the general question of evolution, but is suspicious about the origin of man. There is no definite disciplinary decree, but Catholics are warned to walk warily. The doctrine of the Fall is somewhat fundamental, and is not easy to square with the gradual evolution of man from the lower animals. Moreover, theology apart, there is something a little derogatory to our ideas of human dignity in the suggestion that we and the apes are actual blood relations, even though separated by a million generations. There is, of course, another side, but I doubt whether even St. Francis, when he called the birds and the beasts his brothers, would have been altogether pleased if he had known that his statement was literally true.

One other general question needs to be mentioned. We keep theology out of our discussion as much as possible, but it is useless to attempt to conceal that the only reason any sensible man has for trying to find ingenious anti-evolutionary arguments is theological. Even there, so far as the general theory is concerned, there seems little point in it, but I must admit some point in the case of man. I wish to urge therefore that if there are any anti-evolutionary arguments worthy of notice, it is here that they should be brought forward. If a good scientific case cannot be made out for the special creation of man, it is not worth while to trouble about other alleged special creations. The whole anti-evolutionary case becomes not only obscurantist, but pointless.

I will grant you at the start that the faculties of man are important. No one can fail to be impressed by the difference between the mental powers of man and the apes. Even in the case of the average European, concerning whose mental capabilities I am at times inclined to be somewhat cynical, the difference is enormous, and when we consider the powers of a Beethoven, a Shakespeare, or a Newton, the contrast is unimaginable. I will however remark that I find it nearly as incredible that a human genius is a blood relation of the lowly Australian native, and of the still more lowly extinct Tasmanian, a genetic relationship on which both are agreed. How the change has come about, I do not know. I only know that it has taken place, and so we will proceed to the evidence.

First of all, to take the argument from morphology, man is a vertebrate and a mammal. He possesses hair, the female has mammary glands, and the young are produced in the normal mammalian manner. More than that, man is a Primate. He belongs to the same order as the great apes—the gorilla, the chimpanzee, the orang-outang—and has a similarity a little less striking to the gibbon. Whether you like to class him as a separate genus, family, or suborder, does not concern me. Personally, I think a separate family does full justice to the anatomical differences, but am not greatly interested in an academic discussion of this kind. I feel much the same as you do about the difference between Eohippus and Equus, and would merely remark that I consider the morphological differences between these two types greater than that between man and the apes.

What does require emphasis is the striking homology (or if you prefer it morphological resemblance) between man and the apes. With inconsiderable exceptions, every structure in the one can be

paralleled by a similar structure in the other—bone for bone, muscle for muscle, nerve for nerve. Even in the brain, in which the difference between man and the apes is most pronounced, every lobe and main suture in the one finds its counterpart in the other. In the brain, as in other organs, the differences are in size and proportions. The most important structures I know found in man but absent in the apes are an extra vertebra in the rudimentary tail and the muscle (peroneus tertius) raising the outer border of the foot. It will not take you much space to give a full list, if you think it worth while ; they are of very little importance.

There are, of course, important anatomical differences of proportion in the adaptation of man to walking on the ground, instead of living in the trees. You can make a list of these also if you wish. Also, except on the head, the fur has disappeared, and become a useless rudiment of scattered hairs. Science and common sense indicate that there are important differences, which is why I agreed to classify man as a separate family.

Embryologically also man is a mammal and a primate. In view of the detailed discussion in the 7th chapter I need not deal with this in any detail. Embryology gives no support whatever to the suggestion that man is a special creation. Not only does the embryo show all the ordinary vertebrate traits, but it shows special similarity to the embryos of the apes. In particular, in primates only, including man, the 'formation of the embryo remains in abeyance until the membranes are developed' whereas 'in mammals generally the embryo is distinctly in process of formation before the double membranes grow up and envelop it.'[1]

In addition, the very young of man and the great apes are more similar than the adults. The human baby is less human. The anthropoid baby is less simian. As is well known, a very young baby has a pronounced grip, and can support its own weight. This power is quickly lost, but it does seem to refer to the ancestral history—to the period when the infant must grip tightly to the mother's fur, *or to some other object*, or die. Another similar interesting fact not so well known is that a newly-born infant, if put in warm water, will swim naturally, with a sort of dog paddle ; indeed I doubt whether the Kanaka babies, whose parents continually frequent the warm seas, ever need to be taught to swim. This swimming power of the infant is also quickly lost.

Man is a museum of vestigial organs. We have already dis-

[1] *The Human Body*, by Sir Arthur Keith, p. 110.

cussed at sufficient length the body hair. Sir Arthur Keith mentions also a number of simian muscles, some present in all human beings, and some present in exceptional human beings only. Among those occasionally found are the levator clavicula (lifter of the collar bone), latissimo andyloidens (the apes' climbing muscle) and the palmaris longus, a tendon passing from the middle of the wrist into the palm. This tendon is well developed in the monkey, vestigial in the apes, and in man sometimes vestigial and sometimes absent. Numerous other vestiges are described in any good book on human anatomy. These facts illustrate the extraordinary morphological resemblance between man and other mammals, and particularly the apes, which is so detailed and so striking that only very cogent reasons on the other side can throw doubt on the obvious conclusion.

We must now consider human fossils and archaeology. The first point that emerges is the antiquity of man. We both of us need to trace the living races of man to a single source, and this in itself necessitates a vast antiquity. In later palaeolithic times skeletons of Cro-Magnon man have been found, tall, large-brained, possibly with Caucasian affinities. In caves a very little later are found statuettes, showing a somewhat Epsteinian representation of a race which appears to be very like the modern Bushmen, so even in palaeolithic times there were pronounced races of Homo sapiens. What was the original Homo sapiens like? How and when did the Caucasian and other white races, the negroes, the Bushmen, the pigmies, the Mongolians, the Tasmanian aborigines branch off? I do not know, nor I think do you; certainly we must go very far back into palaeolithic times. We should hear less of these truly genetic series if it were thoroughly understood that it is at present impossible to construct one for Homo sapiens.

The differences between the various races of man fade into insignificance beside the great variety of extinct man-like forms of which we have knowledge. When we go back (say) 50,000 years we have not only Homo sapiens but an entirely different species known as Homo neanderthalensis. This type had a large, but very flat brain, and peculiar teeth which differed from those of the apes even more than do our own. Whether it was a true physiological species we do not know; probably it was. Anyhow it has died out, and no trace of its enormous overhanging eye-ridges or peculiar teeth have been found in any modern race of man. Yet this peculiar type made fairly good flint implements (Mousterian) and buried its dead.

This raises a problem. Were these peculiar creatures men? Have we to look for a common ancestor for these and modern man? To the evolutionist, of course, it is obvious that there was a common ancestor, but how do the advocates of special creation regard this question? We shall probably have to go back to Pliocene times to find the common ancestor. The Lady of Lloyds, the Boskop skull of South Africa, and the type found in a cave in Galilee appear to be variants—similar though not identical—of this peculiar Homo Neanderthalensis.

A similar puzzle is found in the extraordinary massive jaw found at Heidelberg. The creature appears to have lived in the second interglacial. The jaw is simian, but the teeth are human, and some authorities think they resemble the Neanderthal teeth and so indicate relationship.

Neanderthal man is not our ancestor. He occurred too late in time; but quite recently, at Swanscombe, a skull has been found that is very modern in appearance. There are certain differences from modern man, in particular the jaw is slightly more simian and the skull thicker, but this specimen probably does not differ from modern man more than the Caucasian differs from the Australian. This skull is associated with Acheulean implements which, in Europe at any rate, always precede the Mousterian. Types very similar to modern man therefore both precede and succeed Neanderthal man in Europe. There appears to have been a struggle for survival, and we have survived.

The Piltdown skull discovered some time previously is also nearer to modern man than the Neanderthal, though much further from it than the Swanscombe skull. It is probably much earlier than the Swanscombe skull, and belongs to a race from which modern man may well have developed.

There is considerable difficulty about the dating. With the Swanscombe skull we are very fortunate in finding a type of implement which can be dated, relatively if not absolutely. With the Piltdown skull, and those I am about to describe, the dating is uncertain, both relatively and absolutely.

Two other species of Hominidae have been discovered in the east. The first is Pithecanthropus erectus. When it was first discovered it was doubtful whether it was an ape, a man, or even a monstrosity. One or two recent discoveries prove it to be a true species. The brain capacity is definitely intermediate between man and the ape. The thigh bone indicates that it was human in having

the erect posture. It has not been found associated with implements, but there is no reason to think it was incapable of making them. Of another extraordinary type, called Sinanthropus, several specimens have been found in a cave near Pekin. This is also human in being erect, and its brain capacity is very similar to that of Pithecanthropus. The two appear to be different species, but closely allied. Associated with Sinanthropus have been found primitive flint implements, which cannot be equated with any known European kind. There are also indications in the cave which show that fire was used. Both of these appear to be Pleistocene, and possibly not at the beginning of that period, so they can be regarded as survivals existing contemporaneously with more advanced types in Europe and Africa.

It should be added that the late Mr. Reid Moir for many years carried out researches in the red crag near Ipswich, and found crude implements which are now coming to be recognised as artificial. The cruder the implements, the more difficult it is to decide whether they are natural or artificial, but I think, so far at least as the later ones are concerned, he has proved his case. These deposits are certainly Pliocene, and implement-making creatures lived in that time, though no bones have as yet been discovered to tell us how nearly human the creatures were.

This is the story up to the present. The common ancestor of the present races of men is lost in the mists of palaeolithic time. Homo sapiens is the survivor of a number of species of man-like beings of which Homo neanderthalensis and Sinanthropus and possibly others also had sufficient intelligence to shape crude artificial flints. There were a number of species with brains greater than those of the apes, who were sufficiently human to walk erect, with all the anatomical peculiarities that this involves. Of these species, Piltdown man may well be our own ancestor, though this we cannot say with certainty.

Man has therefore lost his isolation. As the gorilla, the chimpanzee and the orang are different species of apes, so Heidelberg man, Piltdown man, Neanderthal man, Sinanthropus, Pithecanthropus, and Homo sapiens are all of the broadly human type, differing from the apes in erect posture and in quantity of brain. Somewhere in the remote past, probably somewhere about the beginning of the Pliocene, all these types meet. From the evolutionary point of view this is clear. These various types of Hominidae

wandered over the earth, but eventually, by slaughter or in other ways, Homo sapiens has displaced the others and inherited the earth.

It seems to me impossible to square these facts with the special creation of man. Which of these forms belong to the human creation? If you include Neanderthal man and the owner of the Heidelberg jaw, you must go back into the Pliocene to find a common ancestor. If you include Sinanthropus, there is no reason to exclude Pithecanthropus, as these types are not dissimilar. If you exclude both, then man-like forms with the erect posture, and sufficiently intelligent to chip flints and use fire, were apes. The problem of the faculties of man vanishes if you include them in the human creation, for no one will suppose that these lowly creatures had powers resembling those of modern men. An example of the confusion to which anti-evolutionists are liable in explaining these facts occurred in a discussion I had recently with Colonel Merson Davies, who is, I believe, with the exception of yourself, the only competent specialist in this country who advocates special creation. I asked him, if man was a special creation, which of these forms were men and which were apes. He replied that all were men except Pithecanthropus, which he called an ape. When, however, he had to print a reply, he abandoned this entirely. He thought it possible that the different breeds of dogs, like the lice in Exodus, were specially created. Inferentially (though he did not definitely say so) the same would apply to the Hominidae. He added that 'circumstantial evidence—indicating man's resemblance to the Trinity—is always more decisive than the details of bodily form.' (*Transactions Victoria Institute LXXI*, p. 141 seq.)

This sort of confusion, coming from a man who really is a competent geologist, does show how absurd in the light of modern knowledge is the contention that man is a special creation. I do not suppose that you would support Colonel Davies in all particulars, all the same it would interest me to know your opinion concerning which of these forms are men and which are apes.

Important recent discoveries take us a little further. In your last letter you expressed serious doubts whether an ape could acquire the erect stature and yet maintain its existence in competition with other forms. This doubt has now been solved. Quite recently Broom has found a number of curious fossils in South Africa, for the full account of which we shall have to wait a little longer. One of these he calls Plesianthropus, and the other Paranthropus robustus. The latter, which appears to be very like the

ground ape we have been searching for, has a brain of ape-like dimensions, only about 650 c.c. capacity, compared with 1,000 for Pithecanthropus and 1,300 to 1,500 for man. The skull was of a more human type than that of any other known anthropoid. Associated with the skull is a humerus very similar to that of man. We may therefore reasonably infer that the feet only were used for walking, and that the creature had assumed the erect posture.[1]

Broom thinks that the deposit in which the specimen was found is early Pleistocene, in which case it is too late to be the ancestor of man, and, like Pithecanthropus, must be regarded as a survival. We must wait for a fuller account before we can be sure of minor points like these. In any case these fossil forms are evidence of the existence of a number of creatures distantly related to man who were in brain ape-like but were also adapted for bipedal walking on the ground. Thus it appears that one of your many theoretical difficulties about what you think can or cannot happen is definitely answered because we have positive evidence that it actually has happened.

As in the other branches of Palaeontology, so in these relics of man-like forms, the links between man and the apes are not a truly genetic series in the sense that they are actual ancestors of man; but they are clearly genetic in that they must all be traced to a common ancestor. They break down the isolation of man. Anthropoid forms are much less likely to be found fossilised than are many other kinds of animals, and in view of this it is surprising how many have been found. Though the main stem has not been found, or if it has we do not know enough to identify it, sufficient branches are known to leave little doubt about the conclusion. The evidence of the fossils confirms that from morphology, embryology and vestigial structures. No theory of special creation will fit the facts. The known facts point clearly to the evolution of man from lower forms of life.

Yours sincerely,

H. S. SHELTON.

DEAR SHELTON,

You claim that the case must be decided on the evidence *taken as a whole*, but you reject the Castenedolo and Calaveras fossils,

[1] There are a number of notices of this discovery in the journal *Nature*, one of the earliest of which is November 19th., 1938.

which according to you ought not to have been in existence in the Pliocene period. May I ask if you reject on similar grounds the Kanam and Foxhall jaws, the Galley Hill and Clichy skeletons and the Wadjak skulls?

In my last letter I asserted, giving reasons, that man cannot have been derived from an ancestor having any of the following characters:

1. A hairy coat to which the young could cling.
2. Quadrupedal gait.
3. An opposable great toe.

Although your letter is a long one you have not attempted to controvert any of these assertions.

The 'serious doubts' I expressed were whether a quadruped could maintain itself in competition with other animals *while it was being gradually changed into a biped.* Once bipedalism had been fully acquired this handicap would cease, so that if Plesianthropus was erect, his erectness would not be a handicap. What you require to meet my argument are fossils of *transitional forms between bipeds and quadrupeds.* But was this ape bipedal? Is it not premature to assert this, seeing that we know nothing of its skeleton except an upper-arm bone?

I do not believe that many share your view that there is nearly as much difference between the mental capacity of a genius and that of an Australian aborigine. The latter, whatever his limitations, can talk, kindle fires and make tools. He has a language and worships God. Your whole case for the evolution of man is based on the illusion that similarities in structure necessarily denote blood-relationship. But these similarities between man and anthropoid ape are not nearly so great as you imagine. You have not listed them or the differences, but invite me to do so, as you did in the case of the differences between cats and civets. You seem to think that the creationist must, but the evolutionist need not, produce evidence! If these similarities were nearly as great as you imagine would such evolutionists as Wood Jones, Hubrecht, Osborn, Sera, Sergi, Klaatsch and Westenhofer have denied that man is a near relative of the anthropoid apes? Is their disbelief entirely without foundation? Here is one of the morphological reasons for this, as stated by Wood Jones (*Structure and Function as seen in the Foot* (1944), p. 16): 'That man in his evolution passed from a leg-longer-than-arm stage to an arm-longer-than-leg phase and then

back to his final and primitive leg-longer-than-arm state is a very unlikely phylogenetic story indeed.'

Here is an embryological reason for the disbelief in your theory of the origin of man : 'It was expected,' writes Keith (*The Human Body* (1912), p. 94) 'that the embryo would recapitulate the features of all its ancestors from the lowest to the highest forms in the animal kingdom. Now that the appearances of the embryo at all ages are known, the general feeling is one of disappointment : the human embryo at no stage is anthropoid in appearance.' Note Keith's 'disappointment' at this fact. His statement is clearly not the result of wishful thinking.

As you believe man to be descended from some kind of ape, you praise highly such a book as *The Science of Life*. In this production we read (p. 419) of a purely hypothetical creature thus described by its authors : 'Four-footed, tailed and hairy it took to the Eocene forests : it grew into lemur, into monkey, into ape : and finally ape turned into man-ape and man-ape grew to man.' This whole pedigree is purely a product of the imagination !

You reject the Castenedolo and other fossils because these do not fit your theory and you devote most of your letter to discussing such creatures as Pithecanthropus, Sinanthropus, Plesianthropus and Paranthropus, *all of which lived long after the earliest fossils of man of modern type were laid down*, and of none of which anything approaching a complete skeleton is known. Indeed, apart from the skull, jaw and teeth, we know nothing of the skeleton of any of the above, except what *may* be a thigh bone of Pithecanthropus and what *may* be the upper-arm bone of Plesianthropus !

This searching for missing links among comparatively recent fossils is what the French anthropologist Boule describes as Pithecanthropomorphism, the imaginative mythology of evolutionists !

The consequences of this vain search afford amusing reading. An imperfect molar tooth was found in a Pliocene deposit at Nebraska which Osborn identified as a tooth of an ape that had evolved a long way in the direction of man. This fossil he named Hesperopithecus, and Elliot Smith actually induced the editor of the *Illustrated London News* to print a full-page picture of Mr. and Mrs. Hesperopithecus. But, alas, the tooth, on closer examination, proved to be that of a kind of pig ! Another candidate for the missing-link club was the Java ape-man, Pithecanthropus, found by Dubois. Dubois, in order to secure its entry into the club, fooled the scientists for over twenty years by withholding from them the

fact that he had discovered in Java, not far from where he found Pithecanthropus, skulls of two very large-brained men !

Another candidate for the club that has had quite a good run is Sinanthropus, the Pekin Man. On page 32 of Mr. H. G. Wells' book *The Work, Wealth and Happiness of Mankind*, this creature is described as an ape-man just below the tool-using level. On page 53 we are informed that as the book was going to press 'Shaped tools well above the eolith level . . . have been found in close association with the Sinanthropus remains.' Within 20 pages Sinanthropus evolved from an ape-man into a skilled craftsman ! Now, the bones of Sinanthropus were found in a cave mixed with those of many kinds of animals : all these bones were much broken and gnawed as if by hyaenas. The limb bones have been broken by some instrument in order to get at the marrow, and the skulls of Sinanthropus have had holes bored in them in order to get at the brain. So the question arises : was Sinanthropus a cannibal who made tools and fires or was it the victim of a human being who preyed upon it ? The latter is quite likely as fossil bones of men of comparatively modern type have been found in China not far from those of Sinanthropus. Boule, writes ('Le Sinanthrope' in *L'Anthropologie* (1937), p. 21) : 'It seems to me rash to deem Sinanthropus the monarch of Choukoutien since he appears in the deposits in which he is found in the aspect of common game like the animals associated with him.' Few beings can have had more fuss made over them than Pithecanthropus. His discoverer, Dubois, had a picture made of him dressed up in the style of a 'masher' of those days. 'People,' wrote G. K. Chesterton, 'talked of Pithecanthropus as of Pitt or Fox or Napoleon. Popular histories published portraits of him like the portraits of Charles the First and George the Fourth. A detailed drawing was reproduced, carefully shaded, to show the very hairs of his head were all numbered. No uninformed person looking at its carefully-lined face would imagine for a moment that this was the portrait of a thigh-bone : of a few teeth and a fragment of a cranium.' Sinanthropus has not been lionised to this extent, but Black, Elliot Smith and Weidenreich have done their best for it. It, or rather the fragments of it which have been found, have been photographed more than some celebrities have. Of the twelve scraps of its jaws 81 photos have been published, the photo of each piece having been taken from several aspects.

In their zeal to demonstrate their simian ancestry few can refrain from making too much of every new fossil found. In the

words of H. H. Woollard (*Science Progress* (1938), p. 18): 'The notion of a gradual emergence of man . . . has exerted ever a seductive influence on the minds of anatomists, few of them being able to contemplate any other view consistently for long. This is shown very clearly by their behaviour whenever a new fossil has been discovered. The discoverer has been unable to resist the temptation of asserting that his fossil, if ape-like, presented all sorts of human characters, and, if human and clearly modern in character, that it possessed all sorts of simian characters, more or less hidden and elucidated only by minute examination.'

All this of course applies only to fossils that appear to support current theory. The fate of those which do not is thus described by E. A. Hooten: 'Heretical and non-conforming fossil men were banished to the limbo of dark museum cupboards, forgotten or even destroyed.' (*Apes, Men and Morons* (1938), p. 107.) The most heretical of these men are those of Castenedolo and Calaveras, and so you naturally disown them, for, in the words of Woollard, 'This discovery, that recent man has a vast antiquity, in fact greater than any other extinct variety, most anatomists have always tried to get round or minimise.' (*op. cit.*, p. 23.)

I note your assertion that some animals not only learned to talk but acquired language, yet you make no attempt whatever to show how this could come about. Nor have you even tried to meet the objections raised by Dwight. What you call his 'casual remarks' do not settle the question of man's origin nor do your *ipsedixits*, but the facts cited by Dwight are serious objections to your views, yet you have not met them. Far from taking the evidence *as a whole* you have ignored the greater part of it!

In conclusion your remark 'man is a museum of vestigial organs' will impress only those who have no specialist knowledge of the subject. To cite muscles as examples of vestiges is to disregard the fact that muscles are among the most variable structures. A number of small muscles occur in some men and not in others, and this is true of apes. When a given muscle is large in Jones and small in Smith, to call Smith's muscle vestigial is an abuse of language.

Monkeys, being better climbers than anthropoid apes, have more developed climbing muscles, but to speak of these muscles as vestigial in anthropoids and cite this as proof of evolution is to use a premise to prove a conclusion and then use the conclusion to prove the premise—a perfect example of 'confusion of thought, pure and simple!'

But *nascent structures* are your desperate need. If man be evolving his body should exhibit several of these. Not only does it not, but it shows very little power of adaptation to climatic extremes. If the evolution theory were true, the Eskimos should either have acquired a thick layer of subcutaneous blubber or have been as hairy as a musk-ox. The acquisition of a thick hair coat would have entailed merely a lengthening and thickening of man's thousands of body hairs. Facts such as these suggest that the evolution theory is just moonshine !

The words of Reinke spoken in 1902 are not out-of-date : 'The only statement, consistent with her dignity, that Science can make, is to say that she knows nothing about the origin of man.'

Yours sincerely,

D. DEWAR.

DEAR DEWAR,

It is usual in this controversy for the second letter in a chapter to be an answer to the first. This time I did not follow the usual custom, and my reasons for not doing so have no doubt by now been communicated to you by the editor. In particular I said quite definitely that I intended to reserve 'a few minor details to my next letter.' Amongst these details are the disputed skulls. We will therefore discuss the skulls and skeletons first.

The great difficulty in deciding whether or no a skull belongs to the deposit in which it is found is that we do not know whether it is a burial. It may seem eccentric to bury a skull ; but as we know that existing tribes have curious customs about skulls it cannot be ruled out as too improbable. This is why the Castenedolo skull is almost universally rejected. In 1888, as you will find described in Sir Arthur Keith's book on the *Antiquity of Man*, a skeleton with a similar skull found in a similar position was undoubtedly a burial. Consequently reasonable caution demands that the previous discovery should not be accepted. The same objection applies to the Calaveras skull. In addition there is the further objection that the remains of ancient man are comparatively rare in America, so much so as to render it probable that he did not exist in that continent until a date considerably later than that in which he flourished in the Old World. Probability is the guide in all these matters, and the genuineness of these finds is exceedingly improbable.

Now let us consider probabilities. It is generally accepted that

Neanderthal man was displaced by modern man so abruptly that he can be marked out of the direct line of descent. The first thing therefore to decide is what sort of man made the Chellean and Acheulean implements. The recently discovered imperfect Swanscombe skull throws some light on this. As it is incomplete we cannot be too dogmatic. It is apparently very like that of modern man, but we must be cautious in being too definite. There is also a significant difference in the greater thickness of the skull bones. The date is almost certainly Acheulean. This discovery gives a greater probability to the genuineness of a number of other discoveries which Sir Arthur Keith has put in his 'suspense account.' In particular, though there are of course doubts, I am inclined to accept the Galley Hill skeleton. This shows an even greater significant difference in the thickness of the skull (10-12 mm.), that is nearly double the thickness of the modern skull. The lower jaw, also, according to Sir Arthur Keith, shows primitive features. I am inclined to regard this great thickness as indicating antiquity, and to accept the Galley Hill skeleton as representative of Chellean man. It appears that Neanderthal man was an interloper, and that both before and after him in Europe the dominant type was much nearer to modern man.

When did the ancestor of man cease to be sufficiently like him to be classed in genus Homo? No one knows with any certainty. Personally I am much inclined to the idea of putting Piltdown man very near the direct line of descent. The skull was found in a secondary deposit with remains of both Pliocene and Pleistocene fragments. The general view is to date the skull in the Pleistocene, in which case it probably represents a side branch. But why should it not be Pliocene? In that case it might well be ancestral to man, and the maker of the implements found by the late Mr. Reid Moir in the Pliocene crag. Of course this is merely a guess. Anyhow, however you may interpret this discovery, such a form does show *by its very existence* the great variety of the Hominidae, and inferentially that man is part of the animal world and descended from other primates.

Now a few words about man's more remote ancestors. You think he could not have had a hairy coat to which the young could cling. Most certainly he had a hairy coat. Hair is a dominant character of the class mammalia, and this is confirmed by the vestigial hairs on the body of modern man. Whether the young could cling to it is doubtful. We do not know when this particular

adaptation of the modern monkey arose. For all we know it may have been long after the human stem diverged. The same applies to the opposable big toe. This m[illegible] not have developed in the apes after man had branched off. I cannot understand your difficulty about the quadrupedal gait. That of the modern gorilla can hardly correctly be described as either bipedal or quadrupedal. It is intermediate. I see no reason whatever why the adaptations necessary to acquire a truly bipedal gait should not be acquired gradually by a forest animal which extended its hunting and food-gathering habits to the plains outside the forest.

I do not think it would serve a useful purpose to discuss your numerous quotations. The two skulls I have mentioned must almost certainly be rejected, and it makes very little difference whether the other doubtful ones are accepted or not. Beyond the very barest outlines no one knows the exact line of the descent of man, and it is of course easy to accentuate this doubt by collecting all the eccentric theories you can find, as you have done in the appendix to your first letter. Some of them are merely foolish, as for example Sera's theory and Crookshank's. What Crookshank has done to be quoted as an authority I do not know. But most of the theories vary round the general line laid down by Sir Arthur Keith, and personally I do not see much wrong with the diagram you will find on the frontispiece of his *Antiquity of Man*. Of course I should be inclined to alter a few minor details, such as the position of Eanthropus (Piltdown), but all this is guessing. I know of no facts against Keith's suggestion that the Hominidae and the apes branched off sometime in the Oligocene, and whether a little earlier or a little later does not matter. Broom's discoveries, when we have full particulars, will have to be correlated with all this. I note your opinion that the discovery of two ground apes is no proof of their descent from arboreal apes. Perhaps not. If you believe in special creation, I suppose a few special creations more or less hardly matter. All the same I should be interested to know your opinion on the question I asked in my last letter—which are men and which are apes? If you class such forms as Pithecanthropus and Sinanthropus as men, then you have admitted such a considerable degree of evolutionary change that we are entitled to assume the rest. If on the other hand you regard them as apes you are postulating a number of special creations of lies or practical jokes such as no reasonable person is likely to accept.

I think I have replied to most of the important points in your

two letters. If I have missed anything important, please tell me, and I will try again. Your letter contains so many quotations that it is a little difficult to disentangle the points that need a reply.

I am quite aware that the discovery that man is descended from other forms of life is a revolution in human thought. Nevertheless it is a conclusion which is for all practical purposes settled, and the modern world, including the religious organisations, must now try to adjust their ideas to this discovery, as they have at an earlier date to the Copernican theory of the Earth and Sun. It is their plain duty, and I trust they will do so thoroughly and without undue delay. In that connection it is as well to remark that modern man in the Pliocene, which you regard as proved, is almost as great a departure from traditional ideas. Certainly Hominidae did exist in the Pliocene, and there is little doubt that they were sufficiently intelligent to make crude implements, but the exact difference between their bodily form and that of modern man is not yet known.

Yours sincerely,

H. S. SHELTON.

DEAR SHELTON,

It is quite easy to discover whether or not any fossil is an intrusive burial. You cannot dig a hole in the ground and then fill it up again in such a way as not to reveal that this digging has occurred. The Castenedolo skull was unearthed by Prof. Ragazzoni, a professional zoologist, while searching for Pliocene shells, where dating is of great importance. Raggazoni took the greatest care to satisfy himself that the skull had not been buried by men; and other experts, including Sergi, who inspected the site were also satisfied on this point. Moreover the skull is not a complete one, nor were all the pieces found together, some pieces were found a little way from the main part of the skull. Further, some years later were found in the same stratum, about two paces away from the first find, numerous scattered fragments of the skeletons of two children. Here again the overlying stratum was intact. So to talk about intrusive burials is ridiculous.

The skeleton you mention that had been buried by man was in a different stratum, at a level three feet higher than that in which the earlier described fossils were found. The reason why so

many evolutionists reject the Castenedolo fossils is not because they might have been intrusive burials but because they do not fit in with preconceived theories.

The author of a book written for the public on the evolution of man is entitled to say that he rejects these fossils, but it is reprehensible to omit all mention of them when discussing the evidence of the fossils. This omission on the part of scores of biologists testifies to the truth of my statement that modern text-books habitually omit to mention facts unfavourable to the evolution theory.

You say 'the remains of ancient man are comparatively rare in America.' In fact they are rather numerous, but, owing to the influence of Hrdlicka, most of them are rejected. 'The extent,' writes Prof. Hooten (*Apes, Men and Morons* (1938), p. 51), 'to which prejudice enters into over-conservative estimates of the cultural or evolutionary span of man is well illustrated by the present controversy over the antiquity of man in the New World. . . . One of the most brilliant and progressive archaeologists said : "It would be a pity to have new evidence come to light which would overthrow all the admirable scientific work of the past indicating the recent arrival in the New World of the American-Indian." This man is one of the most indomitable assailants of the new evidences of man's early arrival in the New World. . . . It now begins to appear however that the personal heroism of one Dutch boy is likely to prove insufficient to stop the increasing trickles of fossil men through the geological defences. Dr. Hrdlicka is already forced to use not only one finger but all of his capable digits to plug the holes, but still they come.'

That Hooten is not exaggerating is shown by the following extracts from a paper on the Folsom culture (*An. Rep. Smith. Inst.* (1938), p. 531) : 'At several places in New Mexico and Colorado implements have been found in association with bones of extinct animals and in deposits suggestive of geological antiquity.' A find of man-made implements at Colorado associated with an extinct bison 'was received sceptically in most quarters, and in several instances there was a definitely hostile attitude toward suggestions that the discoveries might be of importance and worthy of further investigation.' (p. 533.)

These and other finds were examined on the spot by a committee of experts who reported that there was no question of their authenticity. 'One implement (a point) was still embedded in the matrix

between two bison ribs,' 'the associations could not be questioned nor explained away by any of the customary arguments against the authenticity of such an occurrence.' 'In spite of the convincing nature of the discovery most of the anthropologists continued to doubt the validity of the discovery.'[1]

You in 1945 not only doubt the authenticity of the Castenedolo skull but deny it, and devote several pages to discussing comparatively recent fossils as bearing on the origin of man!

'The gait of the modern gorilla,' you assert, 'can hardly be described as either bipedal or quadrupedal. It is intermediate.' I have dealt with this in *Man · A Special Creation* (p. 30). Here is a very recent pronouncement on this matter by Prof. E. Wood Jones (*Structure and Function as seen in the Foot* (1944), p. 13): 'The Gorilla . . . although it has taken to a partly terrestrial life has never succeeded in attaining to orthograde bipedal progression. No matter how much its admirers may praise its attempts at walking upright, the Gorilla remains a thoroughly quadrupedal mammal when walking on the ground.'

You ask for my opinion as to whether Pithecanthropus and Sinanthropus were apes or men. The proper time to ask for this will be after fossils have been discovered of their backbones, shoulder-blades, ribs, breast-bones, the long-bone of the fore- and hind-limbs and the bones of the wrist, ankle, hand and foot, i.e., when our knowledge of their skeletons is complete or almost so.

In reply to your 'special creation of lies or practical jokes,' let me remind you that your task in this controversy is to prove evolution, not to sneer at special creation. Having failed to perform your task you fall back on the argument from authority. Your views are old-fashioned. Forty years ago it might have been reasonable to dismiss anti-evolution with a sniff and a sneer, but the tide is turning against evolution, as is obvious from the catena of sceptical passages cited by Lunn in the Introduction. The authors of these represent but a fraction of the biologists who do not accept evolution. If you will refer to page 696 of Vialleton's *Membres et Ceintures des Vertébrés tétrapodes* (1924) you will find the following passage: 'Critics of evolution have multiplied to such an extent that it is impossible even to list them here. It must suffice in order to give some idea of them to refer to the short résumés given by Diamare

[1] Here are some figures of finds of ancient human remains in America, taken from N. C. Nelson's *The Antiquity of Man in America.* (*Ann. Rep. Smithson. Inst.* (1935).
Finds of Human Artefacts 89 (34 accompanied by animal fossils).
Finds of Human Fossils 54 (18 accompanied by animal fossils).

in *Studie Senesi*, vol. XXIX (1912) and Carazzi in *Il Dogma dell' Evoluzione* (1920).'

As a scientist I am not impressed by the appeal to authority. Long is the list of errors which at one time or another all but a small minority of scientists have supported. 'In my youth,' wrote Horace Walpole in 1774 (*Letters*, XIII, 172), 'philosophers were eager to ascribe every uncommon discovery to the deluge; now it is the fashion to solve every appearance by conflagrations. . . . I am a great sceptic about human reasonings; they predominate only for a time like other mortal fashions, and are so often exploded after the mode has passed that I hold them little serious though they call themselves wisdom. . . . How many have I lived to see established and confuted.'

And Walpole gives a long list of scientific problems of his own time, all of which have been quietly dropped since he died. Is it likely that the concept of evolution will not meet with the general fate of hasty generalisations?

Yours sincerely,

D. DEWAR.

DEAR DEWAR,

Let us first deal with these fossil skulls. It is not as easy as you think to decide whether or no a disputed skull is a burial. When they are found accidentally, as usually happens, it may well be that the evidence of the disturbance of the ground has been destroyed beforehand. More important still, the evidence has sometimes entirely disappeared, that is the ground has been eroded down to the place of burial, and the fragments may or may not have been redistributed. A very good example of this difficulty is the Galley Hill skeleton. In that case the only evidence that it was a burial was the completeness and arrangement of the skeleton. There was no disturbance of the earth, and, as Sir Arthur Keith remarks, (*Antiquity of Man*, p. 185) 'as at Halling we must search for an old land surface' from which the burial took place. He comes to the conclusion that this is to be found in a stratum of gravel of Chellean age. The absence of disturbance of the ground proves only the antiquity of the burial. It does not prove that it was not a burial. In this particular case it makes very little difference. In the very improbable event of the skeleton not being a burial its date is thrown back to the Strepyan period. All the same there is a general agree-

ment that the skeleton was a burial and the only disputed point is whether it is of Chellean age or later.

It is this rigorous line of reasoning that I miss in the case of the two disputed skulls. The Italian one was found so long ago that such careful examination of all the possibilities was hardly to be expected. I know very little about the American skull, or about the date when man first appeared in America. The latter point is not important.

What does all this controversy amount to? What conclusion follows if the experts are wrong and these skulls are of Pliocene age? It really has very little to do with evolution, and the prejudice against them is not evolutionary. It makes very little difference if man very like modern man did exist in the Pliocene contemporaneously with a large number of other Hominidae. The other Hominidae are still there and give definite evidence for evolution. There is nothing very exceptional in a species remaining practically unchanged for two or three million years. Modern man (Homo sapiens) must then have developed earlier. It does not matter; there is plenty of time. It is not a question of the occurrence or otherwise of evolution. It is the minor conclusions of anthropology that are upset, and this is the reason for any prejudice against the so-called discoveries that may exist. While I wish to make it perfectly clear that I take the normal view of these skulls, and reject them entirely, I must emphasise that it really has very little to do with the subject of our discussion. Still less has the date at which man first appeared in America.

I have a little more to say about Man's vestigial organs. You appear to doubt their existence, and certainly you dispute their significance. First of all I have to return to a point which was discussed in a previous chapter (p. 228). Dr. T. W. Letchworth, the well-known ophthalmic surgeon, informs me that in his opinion, the third lid or caruncle of the eye, has no function whatever. Your suggestion that it acts as a scoop is, of course, somewhat difficult either to prove or disprove. In addition to this we have already discussed the body hairs. The tail also should not be forgotten, nor the embryonic wagging muscles, which afterwards disappear or are diverted to other uses. There appears to be no known function either for the pineal gland, or for the vermiform appendix. The organ of Jacobson is almost certainly vestigial, but this is not a good example because it is not easy to say what the original function really was. On the subject of the branchial clefts,

it should be noted that these sometimes give rise to pathological cysts which are thought to refer to the original organ in the fish. The pyramidalis muscle, which is exceedingly variable, and certainly useless as a separate muscle, is another good example. This refers back to the time before the placentals branched off from the marsupials. I think these are sufficient. It is therefore not necessary to look up the researches of Weidersheim (S.410) who listed no less than 180 organs which he regarded as vestigial. The existence of vestigial organs in man cannot reasonably be doubted, and, as was mentioned before, in which cases they are absolutely useless, and in which cases diverted to minor uses, is of very little importance.

I don't think I have mentioned special creation too often, and you certainly have mentioned it fairly frequently. As it is the only alternative to evolution that has ever been suggested, it does follow that, to the extent to which special creation is absurd, evolution must have occurred—even though there may be difficulties. And certainly it is you and not I who have used the argument from authority. If you will compare the number of your quotations on matters of opinion with mine I think you will find that obvious. Of course the overwhelming mass of scientific opinion is in favour of evolution, and there is no point in disguising that, but I think, so far as space allows, and so far as I have not been distracted by the necessity for dealing with your minor objections, I have given you plenty of positive arguments.

The general question of evolution, however, must be left to the next chapter. This chapter is concerned with man. It seems to me abundantly clear that all the lines of evidence, morphological, embryological, palaeontological, and these minor forms of evidence provided by vestigial organs all point with certainty to the conclusion that man belongs to the animal kingdom and that he cannot be regarded as an exception to the rule that all these forms of life have come into existence by evolution, that is by continuous modifications of pre-existing forms, which evolution dates back to the dawn of life on this planet.

Yours sincerely,

H. S. SHELTON.

DEAR SHELTON,

The reaction of evolutionists to the Galley Hill skeleton is evidence of their determination to distort facts in order to fit them into their preconceived theories. The point is, of course, that the

Galley Hill skeleton, which is very similar to that of modern man, has been found in a stratum (early Pleistocene) far older than those in which Sinanthropus and Pithecanthropus occur. As these creatures are supposed to be intermediate between man and his simian ancestors, it is most inconsiderate of Galley Hill man to make his appearance several thousand years before his alleged ancestors. It is incumbent on those who deem these creatures ancestors of man to show that the Galley Hill skeleton was not contemporary with the strata in which it was found. In consequence they assert that the Galley Hill man died on a much younger land surface, from which he was buried so that his body was placed in strata laid down in the early part of the Pleistocene, i.e., many years before he died. It is supposed that in course of time the younger strata became eroded away, so that their surface became level first with the upper part of the skeleton and then with the bottom of its grave, and this process was so contrived that the skeleton was not swept away, or destroyed by man, beast or anything else while the earth around it was being swept away or during the long time during which it lay exposed. After a time the surface on which the skeleton lay subsided and the course of the river changed so that it flowed over the skeleton, without washing it away, and eventually covered it with 8 feet of sand, gravel and loam. Thus was the skeleton preserved and all traces of its burial removed. The fantastic farrago of coincidence and improbability whereby the Galley Hill skeleton is extracted from the strata for which his passport has not been visaed by the evolutionists, is necessary because the only alternative is that man has undergone, as Keith remarks, 'only minor changes in structure for some 4,000 generations.' (*Antiquity of Man*, p. 265.) Several other human fossils have received similar treatment.

I do not think that Dr. Letchworth can have spoken of 'the third eye-lid or caruncle of the eye,' because these are not one and the same organ : the homologue in man of the third eyelid of birds and reptiles is the semi-lunar fold or plica semilumaris. This fold lies between the caruncula and the eyeball, and it is this and not the caruncula which is supposed to be vestigial. The view that this is a useless vestige presents difficulties. The third eyelid of birds and reptiles is a purely membranous structure, but the semilunar fold possesses between its two membranous layers a thin plate of cartilage, i.e., something lacking in the third eyelid of birds and reptiles. As you believe mammals to be descended from reptiles,

and since birds and reptiles get along quite well with their kind of third eyelid, please say what you imagine caused some reptile to change the nature of its third eyelid and to acquire this plate of cartilage, and how the change can have been effected *gradually* and without causing its possessor inconvenience. Do you suppose that all the changes necessary to convert a reptilian eye into a mammalian one took place at the same time as the drastic changes in the bones of the jaw and the skull? Changes which I asserted in my letter (p.164) could not have been effected gradually. In that letter I asked you, if you disagree, to 'describe stage by stage how these various changes can have taken place, the order in which they occurred, and how the organ of Corti can have started, and describe its gradual development into its present condition.' You have done none of these things. I invite you to do so now, also to describe how the eye of a reptile with its appurtenances can possibly have been gradually converted into the human eye. These are vital matters, because your whole case rests on your ability to show that I am mistaken in this matter.

You say the tail should not be forgotten, nor the embryonic wagging muscles. To this I add 'nor the embryonic postanal or tail gut.' Do you believe that the existence of this last means that man had an ancestor possessing a length of gut extending behind the vent into the tail?

The pyramidales muscles are not useless vestiges. They are the tensors of the linea alba, and when they are absent the lower part of the rectus muscles becomes proportionately increased in size.

There are a few structures in the body of man of which we do not know the use; but to assert that these have no use is tantamount to asserting that our knowledge of human anatomy and physiology is complete. I put it to you that such an assertion is, to say the best of it, premature.

Yours sincerely,

D. DEWAR.

DEAR DEWAR,

I have referred to Dr. Letchworth again on the structure of the eye, and he tells me that he regards it as a doubtful point whether the caruncle properly belongs to the semi-lunar fold or to the conjunctiva. In any case it is an immaterial minor point. He has also pointed out that you have overlooked the very important fact

that the nictating lid is functional in a number of mammals, as for example the cat and the dog, and that in these animals the organ is supported by cartilage. Consequently your remarks on the structure in reptiles are wide of the mark. It would be an interesting evolutionary study to follow this up, and find out in which mammals the semi-lunar fold is functional and in which it is vestigial; but we are neither of us able to find definite information about this. It should be noted also that the existence of cartilage in the human vestigial organ, like so many other vestigial characters, is variable. It is often absent in white people, but much commoner in negroes. This and similar matters you will find set forth at length in Testut's book on anatomy, a book which is now to some extent out of date, but I think reliable on this point.[1]

I cannot now enter into a detailed discussion of the organ of Corti, or of vestigial organs in general. It is, however, necessary to dissent strongly from your statement that there is anything particularly vital in all this. The existence of vestigial organs in man, as in other animals, is excellent evolutionary evidence. But it matters very little when these organs are useless, and when they are diverted to minor uses. Nor is it material if it is sometimes difficult to explain the origin of some particular organ, whether vestigial or otherwise. The principal difference between us during the whole discussion has been in the assessment of the comparative value of the various types of evidence. You continually seem to me to magnify minor points out of all proportion to their real significance.

The same criticism applies to your treatment of the fossil Hominidae. You make a great deal too much of this Galley Hill skeleton. As I explained (p. 274) Sir Arthur Keith puts it in his 'suspense account,' and I personally am inclined to regard it as a genuine representative of Chellean man. In that case all your remarks about a burial are irrelevant. As I explained in my last letter, if it is not a burial it is Strepyan instead of Chellean, which is of no consequence whatever. The suggestion that the skeleton was a burial is based on the extreme improbability that a complete skeleton would be laid down in a gravel deposit. I should add also that it is by no means certain that, in the course of hundreds of thousands of years, any traces of disturbance of ground would remain. The percolation of water in a shallow deposit would undoubtedly affect the distribution of the earth. But, so far as this skeleton is concerned, it really does not matter.

[1] Traite d'Anatomie Humaine (Octave Dohn, 1894), Tome III, p. 116.

What it amounts to is that there are two sections of opinion about the evolutionary descent of man. The older one thought the change was comparatively rapid. The later one, represented by Sir Arthur Keith, thinks it comparatively slow. Both sections are 'evolutionists' (the term is now almost meaningless). Owing to the discovery of the Swanscombe skull, opinion is now coming round to Sir Arthur Keith's view. But it really does not concern you. When we have a minor dispute between two sets of people, both of whom are thoroughly convinced of the evolutionary descent of man, it makes very little difference to the fundamental weakness of your case which one is right.

To return to the main question, I had better state once again that the exact line of descent of man from other primates is not known. It is not definitely claimed that any of the fossil Hominidae are ancestral to modern man, and my suggestion that Piltdown man may be so is a speculation on which I lay very little stress. If that is what you are contending for it is admitted ; otherwise I see very little point in your remarks. The very existence of these forms, whether ancestral or side branches, is indubitable evidence of the evolutionary origin of man. There are many things unknown, and there is need for fuller investigation, but the fundamental fact of the origin of man from other primates seems to me to be thoroughly well established.

Yours sincerely,

H. S. SHELTON.

II

SUMMARY AND CONCLUSION

DEAR DEWAR,

It is now my duty to open this final chapter of general comments and summary. I have to gather together a few loose ends of thought which have been left a little ragged in the chapters on special aspects, and you have now an opportunity to expound those general aspects of design in nature, which you have brought forward previously, in my opinion in inappropriate places. All through the book there has been very little difference of opinion about the facts, and I wish to compliment you once again on your wide knowledge of the relevant facts. Our difference is in interpretation. You interpret the facts in an entirely different way from that of the scientific world as a whole. You are reopening a question which the scientific world is inclined to regard as closed. I have made it quite clear that I think it is closed, but all the same, as modern specialists are inclined to be a little too narrow in their outlook, I think there is something to be gained by a new discussion of main principles in the light of modern knowledge.

The difference between us is a little difficult to describe. You *see* things differently. There is very little in this world that cannot be interpreted in more than one way if we are so disposed. Moreover I do not think you quite realise how differently you do see things. All through the book, when you think you are putting forward scientific arguments, you have appeared to me merely to be expounding an unusual metaphysical attitude. As you were the first to mention the idea of the flat earth (or to be exact the editor in one of your letters) I will illustrate by showing where the analogy holds and what are the differences. You and these exponents are alike in seeing things differently from the ordinary man of science and I do not think you are entitled to be too annoyed at the comparison. It is not a point in your favour that you are so intolerant. My own point of view is much more tolerant, and, if the question is raised, my attitude is : well, if it comes to that, how *do* you know that the earth is round ? The question is not too easy to answer. Mr. Bernard Shaw relates the story of an occasion

when he took the chair at a meeting when the flat earth exponent wiped the floor with his amateur opponents, and he (Shaw) summed up by saying that, so far as could be discovered from the course of the discussion, the shape of the earth was cylindrical. I may mention in passing that several of the common arguments are distinctly unconvincing. In particular it is not a sound reason that you can go round the earth using either a magnetic compass or the angle of the Pole Star. The latter implies an unusual estimate of the distance of the stars, and the flat earth theory fails to explain why the Pole Star disappears at the Equator, but you have to go fairly deeply into the matter before you find the absurdity.

The difference between you is one of degree rather than of kind ; but *your* problem is much more complex. In the last resort we can bring the flat earth exponent up against definite measurements, though it takes careful analysis to do that well. In your case the facts lie in the remote past and this is not possible. Consequently you are worthy of a detailed discussion whereas they are not. Moreover, as there are unsound arguments for the rotundity of the earth, so there are unsound arguments for evolution, and you serve a useful purpose in pointing them out. It is well to examine the usual reasons for believing in evolution and to eliminate those that are unsound.

All the same the essence of the whole matter is not scientific reasoning but the way of seeing things, or, otherwise expressed, the groundwork of metaphysical assumptions. My own standpoint is well illustrated by that famous couplet of Pope :

'Remember Man 'the Universal Cause
Acts not by partial but by general laws.'

You wish to say that instincts are implanted in the animals by the Creator. Perhaps they were, but how ? You wish to emphasise that the animal and vegetable world was created. Perhaps it was, but how ? The intervention of Divine Creative Power is not to be found in crude and easy ways. You feel that there is design, and I am not inclined to argue about this, but all the same miracles, in the sense of exceptions to the ordinary working of nature, are not so cheap and common that because you find a fossil in the rocks, and cannot find any probable immediate ancestors, you are entitled to assume that it was specially created. That is the metaphysical difference that separates us. When we agree that we must not

assume the supernatural until we have exhausted natural causes, I mean it and you do not. After agreeing, you continually come back and tell me that this or that is consistent with special creation. Of course it is if you like to look at things that way. It is so much simpler. Mathematicians have been trying with only moderate success to work out under what conditions the matter of the planets can have been drawn out of the Sun. 'If you like to say the earth was specially created, you settle the question at once. The same applies to difficulties of evolutionary descent. Assume special creation, and the difficulties vanish. We do not know the exact stages by which the reptile changed to a bird. If you like to say the bird was specially created there is nothing to investigate. By what stages did the tailor bird reach its present method of nest building, or the water spider its method of rearing its young? No one knows, and we can only make guesses, and not very good guesses at that. If you like to say there were no stages, that these species were specially created with instincts complete, no one can prove you wrong. It is just the way you see things. The convincing value of evidence depends on the metaphysical assumptions you start with. Of course there are difficulties in the study of evolution, and it is quite easy to say they are insoluble. Indeed, in view of the complexity of the subject, I am surprised that the difficulties you have found are not more formidable than they have proved to be.

I fear that we have found great difficulty in understanding one another. Certainly all through the book my statements and arguments and your account of them have been very different things. You may possibly allege the same of me, that is for you to say. With regard to the way in which my statements seem to appeal to you, I will take one trivial example, and one important one of substance.

The trivial example is found in your letter (p. 245) in which you say: 'apparently in order to prevent your admission that I have stated problems beyond your power to answer to your own satisfaction from being an anti-climax,' etc. What is it all about? *Of course* you can state problems beyond my power to answer. Similarly when I have occasion to mention polyploidy in plants there is nothing desperate about it. It is a particular answer to a particular question. The important misunderstanding of substance refers to embryology. I must emphasise once again that I do not accept the recapitulation theory of the embryo. The embryo shows unmistakable indications of its ancestry, both immediate and

remote, but does not in my opinion recapitulate its ancestry. One consequence of this misunderstanding was that a very important point was not dealt with by you at all. You admit 'transformism' to an undefined extent. The important query therefore is : what happens to the embryo when the transformism you admit actually occurs ? Can the embryo be expected to give any indications of the units of creation—if they exist ? If not why not ? As you did not exercise your right to send a final letter in the chapter on embryology, you might find it convenient to deal with this point in your next letter. In a matter so complex as evolution the difficulty of mutual understanding is very great, and I think that both of us, and the reader, would do well to make due allowances.

I will now illustrate by one or two examples the useful purpose I think you serve by attacking evolution, namely to force us to re-examine the usual arguments, and to eliminate those that are unsound. The recapitulation theory of the embryo is one, and I will pay you the compliment to acknowledge that your previous writings have had some effect in bringing to my notice that it *is* unsound. Another important point is the utility or otherwise of vestigial organs. I think this is a point which calls for further detailed research. Because an organ is vestigial, and inconsiderable in size, we are perhaps a little prone to jump to the conclusion that it is useless. Another very important point that has emerged is that it is not sufficiently realised that the geological record (our record) is very incomplete. It is nearly as necessary to emphasise this now as it was in Darwin's time. Since then many fossils have been found which give valuable evidence for evolution. All the same there is no doubt that there still remain many gaps, even in the ancestry of recently evolved species, which we should like to fill up, and in the earlier evolution of palaeozoic times the record is mostly gaps. Fuller geological research is badly needed, and it is to be hoped that, when we settle down once more to the ways of peace, that further study will be given. We are also faced with the further question what part of the record is irretrievably lost in that it has been destroyed, and we have not as yet sufficient knowledge to estimate this at all accurately.

There is one other point which should be mentioned before I pass on to a brief summary of the evidence. We live in an age of specialists. Conclusions from scientific work are liable to be partial, and to receive publicity before they have been thoroughly examined in all their bearings. There is ample room for competent lay

criticism of the conclusions of specialists. Even in this question of evolution very unsound social inferences have sometimes been made. In particular an inaccurate appreciation of the meaning of 'survival of the fittest' has given rise to social doctrines which are very unacceptable. The gospel of ruthless force, which has done something to precipitate the present war, is only an extreme instance of similar fallacies which are found elsewhere. 'My view of your attitude is not that you are in principle wrong in criticising scientific conclusions, but that you are barking up the wrong tree. You are attacking the result of scientific investigation which, above all others, is thoroughly well founded both in science and in philosophy. In this particular case, theological prejudice, which I think you represent to some extent, has received support from a modern attitude of scepticism about everything in general. A generation ago it was fashionable at Oxford to talk anti-evolutionary nonsense, much as Communism is fashionable now, and the tutors smiled indulgently because it exercised the brains of the clever young fellows. I am inclined to think that some of these French scientists are very similar. In so complicated a subject as the evolution of the forms of life it is not difficult to find grounds for a kind of vague scepticism.

It now remains for me to try to summarise the positive evidence for evolution *as a fact*. A brief discussion of the causes I shall defer to my next letter. What are the grounds on which we can assert that the fact of evolution is thoroughly established, so much so that a very important section of people think further argument superfluous?

In the first place organisms are variable. I think we are agreed, for example, that dogs are one species, in spite of Col. Merson Davies' apparent disagreement. Also you abandon the breeding species as the unit of creation. You remark (p. 193) 'it does not follow that all the descendants of a single pair (*sic*) are for all time fertile *inter se*.' So far so good. In view of all the ink that has been spilt on the inability of experiment to produce new physiological species, this is very gratifying. But you think there is a limit somewhere. If it is neither the physiological species, nor the morphological genus (which is exceeded by the dogs) what can it be? Needless to say the evolutionary answer is that this limit does not exist. All species are variable. Why should there be a limit? Of course species sometimes fail to produce the variations necessary for survival and die out, but this is hardly relevant. Any change however great, even from an amoeba to an elephant, can in theory

be broken up into infinitesimal stages, and why is this not possible in nature? Of course at first sight it looks incredible, but so do many other well-founded conclusions of science. This admitted variability in all species is the first important fact.

The next point is the available time. The rate of change, even when comparatively rapid, is slow. The evolution of modern mammals from their primitive Eocene ancestors is an example of fairly rapid evolution, and that took about 60 million years. Still I think the 500 million years since the lower Cambrian are sufficient for all the changes that have to be postulated since then, especially as we must remember that rapidity of evolution should be measured not in years but in generations, and the generations become more frequent as the animals become simpler. Before the lower Cambrian there is a possibility of at least another 500 million years of simpler life. Graphite is found in the Archaean, and we know of no inorganic source for graphite. I believe you think there is some sort of time difficulty, but I have not been able to understand what it is. I think it will be generally agreed that the enormous period since the earth became habitable for life is sufficient.

As we have the necessary variability and the necessary time, we have now to consider the positive evidence that what is theoretically possible has actually occurred. Here I wish to emphasise most strongly that the evidence is the *total evidence*. It is not morphology, nor embryology, nor geology, nor vestigial organs, *it is the synthesis of them all*. Here is the principal point of difference between us. As an example in your letter (p. 51) you state that Huxley amongst others accepted evolution for unscientific reasons. Really this is a little amusing. What you mean is that Huxley accepted and advocated evolution although he frankly admitted at the time that the evidence from geology was practically non-existent. The certainty of a conclusion is relative, and the evidence for evolution is much stronger now that good geological evidence has been discovered. It is, however, your reasoning that is fallacious, not that of Huxley. Your fallacy consists in overweighting one type of evidence and ignoring the rest. It is quite allowable to come to a conclusion when one type of evidence is missing, if the other evidence is sufficiently strong. As an example, Tycho Brahe opposed the Copernican theory because he could not find any trace of annual parallax. We have found since then that the annual parallax of the nearest star is less than a second of arc, so of course he could not. But surely you are not going to tell us that Copernicus, Galileo and

Newton accepted the Copernican theory for unscientific reasons because this very important direct confirmation was missing, and the theory implied distances for the stars which must then have appeared hopelessly incredible. The evidence for such conclusions is the total evidence and no single type should be overweighted, much less should any type be ignored. Now what is this total evidence?

First there is morphology. We do not find all sorts of phyla in different geological periods appearing and disappearing, as might well happen if they were specially created. This is in accordance with evolution and no other explanation explains. All known fossils belong to existing phyla. Similarly the creatures living in different habitats—the land, the sea, the air, the depths of the ocean—are all related in the main features of their structure. Evolution explains. On the hypothesis of special creation I should expect the inhabitants of the depths of the ocean to belong to different phyla. The very mode of classification which has come down to us from pre-evolutionary times makes sense if we assume descent with modification, but it is a hopeless tangle on any other assumption. Why should man and the apes, the dog and the cat, be so like and yet so unlike. Evolution explains, I know of no other explanation.

On this subject of morphology you can and have raised difficulties—and they exist. I do not pretend that in every case I can solve them. The particular difficulties that are fashionable vary from time to time. But what they really amount to is always that it is not easy to imagine the stages by which some particular adaptation has arisen. Two that were fashionable in the old days were the eye and the lungs. But the difficulty of the eye is considerably reduced now we know the many stages found in species now living (S. 1022 seq.). And we now hear very little about the lungs since the existence of the lung fish has become generally known. You have made a great point of the vertical motion of the tail of the whale, and I think I have answered that reasonably well. I should like to add to my answer by showing how this difficulty tells against you. I am now a little handicapped by the closure of the Natural History Museum, so that I cannot look once more at the actual skeleton, but I think the picture you will find in the Cambridge Natural History sufficient. You will see that the vertebral column in the region of the tail twists round so that the dorsal processes become horizontal. This is an obvious *adaptation*. On any

ther hypothesis I should expect these structures to be in their roper place without twisting.

A still more obvious example is the seal. I cannot tell you in letail how the hind limbs acquired their present position, though think the structure of the sea lions throws some light on it. But he evolutionary evidence consists in the fact that the creature has ind limbs at all. The bones of the hind limbs—quite large hind imbs—extend backwards on either side of the tail, and further ack than the tail. What is the animal doing with hind limbs? Most other sea mammals have strong tail bones which accomplish he same purpose and the hind limbs are vestigial. You objected o the minute structure of the whale being called a pelvis, but urely you cannot but acknowledge that these bones of the seal are orrectly described as hind limbs. Here is an obvious adaptation ; our very difficulties tell against you. This is evolutionary evidence hich leaps to the eye, and is convincing to all except those who re blind because they will not see. At the same time I wish to be ompletely honest and straightforward and will acknowledge that, hile some morphological difficulties are easily answered, and thers tell against you, it is always possible to bring forward others o which the solution is not clear. That is a small point in your avour, which should be given due weight, but not undue weight.

The second branch of evidence for evolution is geographical istribution, concerning which I have very little to add to what I aid in the appropriate chapter. It is, of course, much more limited han that from morphology. It is only in the case of distribution hich has occurred in recent geological epochs that the evidence is lear, because we then have a good idea where the forms in- abiting continental and oceanic islands came from. The absence f mammals on islands such as New Zealand, and of placentals in ustralia, is very important. On any other hypothesis but that of volution we should expect to find them there, as when they are ntroduced they flourish exceedingly. The forms that exist on the slands are similar to those on the mainland, but also different, and he degree of the difference corresponds fairly well with the time hich we have reason to think has elapsed since the island was part f or nearer the mainland. Your brief last letter in this section calls or some comment (p. 152). You say I have not challenged your ssertion that the distribution 'of amphibia fits the theory of reation better than that of evolution.' But I have. You also say : Wegener's theory is of course completely neutral.' That is where

you make the mistake. Distribution of modern mammals, and of other species which are recently evolved, is related to that of the nearest mainland. Here the evidence is clear. Distribution of amphibia, which are a very ancient class, is in a different category. If, as seems probable, this distribution occurred when the continents were gathered together in one mass, anything may happen, and such similarities as occur between South America and Australia, or South Africa, or even India, are naturally explained. Your difficulties vanish, or at least they cease to have any bearing on the main question of evolution. The distribution which has occurred in geologically recent times is clear evidence for evolution. The earlier geographical distribution (e.g., of amphibia) is confused and difficult, and cannot therefore be cited as evidence. But equally, it cannot be cited as evidence against evolution. Your difficulties become difficulties of detail, and have no bearing on the main question.

I have already dealt with one aspect of the evidence from embryology. We have now to consider the positive evidence it gives for evolution. This can be briefly stated in the assertion that the development of the embryo to the adult is indirect, and that it shows many indications of its ancestry, near and remote. I will first of all answer the question you asked me in in your letter p. 283. You ask if the post-anal gut means that man is descended from ancestors which had a gut extending into the tail. That is a possible interpretation, and in no way absurd, and is rendered even more probable by the fact that in primitive types such as the dog-fish the feature is more prominent than in mammals. But it is speculative, and so is not as a rule cited as evolutionary evidence. It is quite possible that the vertebrates arose from some worm-like form, and that this is an evolutionary relic, but it is not certain. I do, however, cite this as definite evidence against your assertion that the embryo develops directly to the adult, whatever qualifications you may like to make.

What, therefore, are the facts which *do* give plain evidence of ancestry? I must refer you to my first letter in the chapter on embryology (p. 154), and I do not think any additions are necessary. The development of three toes in the horse embryo is as good an example as any. If the embryo developed directly there would be only one toe. If on the other hand there is any metaphysical reason why the 'primordia' of the typical organism should develop even if not required in the adult, there should be five toes.

The development of the three toes is explained by evolutionary descent, and I know of no other explanation. I regard such features as the development of two lungs in snakes, and two ovaries and oviducts in birds, when the adult requires only one, as absolutely conclusive.

The evidence from vestigial organs also seems to me to need no further elaboration, and, as I have said before, I regard your remarks on nascent organs as merely confused thought. In the ordinary sense the whole world is full of nascent organs, that is of organs which are crude and undeveloped compared with similar organs in other species. That these are crude organs, and not entirely different organs, is powerful evidence of the genetic connection of widely different forms of life.

Finally we come to the evidence from palaeontology. I must first pay some attention to your last letter in the appropriate chapter (p. 136). It is necessary to emphasise once again that the evolutionary evidence consists of *the fossils that have been found.* Your statement about 'a further claim by you, viz., you may draw inferences from the absence of fossils' is entirely without foundation. I make no inferences from the absence of fossils, though I have tried to explain it. My whole contention is that *both sides* must be very careful what inferences they make from the absence of fossils. As an example, neither birds, mammals, nor even reptiles, are found in the Carboniferous coal measures. It is exceedingly improbable that any such existed, and the discovery of either birds or mammals would seriously upset evolutionary theory, but I should hesitate to assert that they did not exist merely on the ground that the fossils have not been found. My reason for certainty on this point is the whole structure of evolutionary theory, not merely the absence of certain fossils in certain strata.

A few more remarks are needed about your tables. With regard to most of them which deal with land mammals, I have nothing to add. In your reply you call my attention to your table on molluscs, and point out as an example that 64 genera have been found in the Jurassic. What about it? A few good fossiliferous rocks representing only a million out of the 50 million years of the Jurassic could easily give that number, and the evidence for evolution in the Jurassic would be negligible. Of course, the Jurassic is not so arranged, but so far as your evidence is concerned it well might be. But the finding of fossils of primary and secondary creatures is exceedingly erratic, and shows clearly the fragmentary nature of

the record. Here is one more example taken from Prof. Hawkins' address previously mentioned. 'Paleodiscus is known by some dozens of specimens from a diminutive quarry in the Lower Ludlow flag—*but nowhere else.*' Does this not illustrate what I have been telling you all the time—that fossils are lucky finds, and only represent fragments of the vast abundance of life that has existed for perhaps a thousand million years.' I think I can infer from this absence of fossils the fragmentary nature of the record, but I don't infer anything about the genetic relationship of paleodiscus.

There is one other type of evidence which should be mentioned here because it belongs both to geology and to embryology, and so is likely to be overlooked in both sections. I will give two quotations from Zittel's *Introduction*: 'The Palaeozoic Bellinuridae found their counterpart in the larvas of the common Limulus,' and 'fossil crinoids resemble the young of the living genus Antedon.' Thus palaeontology and embryology interlock to give powerful evolutionary evidence. Why should there be these remarkable similarities except on the ground of genetic relationship?

I have been trying all along to grant you such minor points as are in your favour, and so it is as well to say that the absence of *recognisable* fossils in pre-Cambrian rocks is one of them. The discovery of some record previous to that of the Lower Cambrian would be exceedingly welcome. This, of course, is merely an instance of the fragmentary nature of the fossil record, but it is a very striking one. I must, however, emphasise once again that, while in most cases there is plain evidence of the lapse of time between the Cambrian and these earlier rocks (especially where traces of life have been found), there is no evidence whatever of continuity of deposition in Cambrian and Pre-Cambrian anywhere, nor is such evidence possible in the present state of our knowledge. The absence of recognisable pre-Cambrian fossils is a minor point in your favour, but it is not permissible to exaggerate.

With regard to the evidence from geology as a whole, that is the evidence from the fossils that have been found, I do not think I need add anything to what I have given in the appropriate chapter. Of course, there is much more. One friend has remarked that I might have made more of the Ammonites. I might. But I have only given illustrative examples, and I think they are sufficient. It is not in dispute that much evidence we should like to have is missing. This, of course, is because of the fragmentary nature of the record, which has been sufficiently discussed. The important

point to emphasise once more is that all this must be regarded as *confirmation*. It is not part of the original evidence on which evolution was accepted by the scientific world in the time of Darwin and his contemporaries. There was then no palaeontological evidence of any importance. Now there is a considerable volume of it, and it is slowly increasing in every decade. It is like the discovery of annual parallax, which did not take place till the time of Bessel in 1834. And the present position when only some of the ancestral forms of living beings have been discovered is very like the present position in astronomy when only some of the stars are near enough to have a measurable parallax. The evidence for evolution is good enough without the geological support, just as that for the Copernican theory was good enough before the discovery of annual parallax. But the geological evidence has been found, which for confirmation is more than ample.

Needless to say, we expect much more to be discovered. How much can be discovered, and how much is lost because it has been irretrievably destroyed, is an open question which is much too speculative to deal with here. The position at present is that much positive evidence has been discovered, and that all that has been discovered, though it does not always give positive evidence, fits well into the general scheme. Some evidence, like the discovery of Archaeopteryx, is so plain that it is convincing to all who approach the matter with common sense and an open mind. If anyone says, as you appear to do, that they will not accept evolution until a great many more gaps have been filled, there is no more to be said. I think the common sense of the majority will decide against you.

In conclusion, I must emphasise once more that the evidence for evolution is the *total evidence*. It is not allowable to take one section and magnify it at the expense of the rest. When Morphology, Embryology and Palaeontology all point in the same direction, when all this is supported by the facts of geographical distribution, and by the peculiarities of vestigial organs, when no evidence of any kind which can be called definitely adverse is found anywhere, and when the best an opponent can say is that much desirable palaeontological evidence is missing, or that there are a number of morphological difficulties to which no good answer has been given, the conclusion is proved as well as is possible in a matter so complex. Also, as the question is so complex, it is possible for anyone to say, as you do, that he will interpret all this in another way. When all

is said, the general metaphysical outlook of the individual is the ultimate factor in decision. In this case, unlike the Copernican theory, there are no definite quantitative measurements which can be brought up against you. It is only relatively and in the mass that you can be brought up against absurdities. All I can hope to do is to show that to the ordinary person who reasons in the ordinary way the evidence for evolution is so strong as to leave no reasonable doubt. If you like to reason in an extraordinary way, and to express unreasonable doubts, there is very little to be said. The evidence is all indirect, it is to be found in many branches of science, and it needs careful co-ordination to appreciate it in all its bearings. It is the only known reasonable and probable explanation of the facts of life.

Yours sincerely,

H. S. SHELTON.

APPENDIX (On Cats and Civets)

The question of the exact degree of difference between Equus and Eohippus. and its relation to the normal differences which separate animals of allied families, as for example cats and civets, needs clearing up. Personally I have little doubt that I have proved my case ; but the discussion has been a little confused, and so a final summary is desirable here. I put it in an appendix because to me it is a minor point and hardly worth discussing. Nor do I wish to bore readers who are already satisfied that my case is proved. My reason for troubling with it at all is that it is much more important to you and to the editor than it is to me.

This question was first raised, in my opinion very inappropriately, in the first chapter. In your letter (p. 51) there is the following passage, strongly emphasised by being put in italics : 'I challenge you to cite a genetic series of fossils linking by small steps an order with another order, or a family with another family. You have but to show such a series in order to convert Lunn to evolutionism, and to make me reconsider my attitude towards it.'

The first obvious comment is that, as it would naturally be supposed that you and the editor know what the geological record will and will not do, it is a somewhat cheap method of controversy to challenge me to produce evidence a little more complete than that which you think actually exists, and usually such a method can well be ignored as a rhetorical device. Unfortunately for your

'challenge,' however, you had forgotten what family and order meant, and were obsessed by the elementary fallacy of regarding them as representing fixed instead of variable values. Therefore, as you persisted, and (again in italics) repeated the same remark on p. 56, I replied in the following words: 'As you are so persistent, I will say at once that the change from Equus to Eohippus can be traced by small steps, and the change is *considerably greater than* that usually implied by linking a family with another family.'

It was of course open to you to say that this was not what you meant, and that you were not asking for evidence that exists, but for evidence that you knew did not exist (though it well may exist in the future with further discoveries). You did not, however, take this line and so we agreed (1) That the change from Equus to Eohippus could be traced by small steps. (2) That the cats and civets were a fair example of different allied families. It therefore followed that if I could prove that the difference between Equus and Eohippus was as great as (it is actually considerably greater) that between cats and civets, I should meet your 'challenge.'

The discussion continued in the chapter on geology, but instead of meeting the point directly you argued about mountains and mice, which of us ought to give the differences between cats and civets, whether the differences could best be assessed by numbering them, and whether I ought to allot some of my space to you at a time when you were better off for space than I was. Therefore, to bring the discussion back again to actuality, I will now repeat the differences between cats and civets on the one hand, and Equus and Eohippus on the other.

The differences between cats and civets are mostly those of soft parts, and so irrelevant, as they cannot be used in the comparison. With regard to the skeletal differences, I think it will be generally agreed that those given in the *Cambridge Natural History* are sufficient. Anything not given in so full a treatment of vertebrate anatomy can be regarded as minor differences such as could be found in different species of the same genus. If you include those you might make up a portentous list. It is possible to tell whether a single bone belongs to Homo sapiens or Homo neanderthalensis, notwithstanding the fact that both are universally classified in the same genus. We must therefore only consider the major differences which distinguish families and orders, not the minor differences which distinguish species. These, as listed in the *Cambridge Natural History* are as follows:

1. In the Felidae the auditory bulla is inflated, there is an internal septum, the paraoccipital processes are flattened against the bullae, there is no alisphenoid canal. All these distinguish the Felidae from the Viverridae, except that in the Viverridae the alisphenoid canal is *sometimes* present.

2. The head and body of the civets are more elongated than in the cats.

These two are the *important* differences which separate *all* cats from *all* civets. You can of course make these up to any number you like by going into further detail, and by saying the same thing twice over, as for example if the head and body are more elongated it naturally follows that the legs are relatively shorter; but this is all that really matters.

It happens, however, that the civets vary considerably among themselves. They have several sub-families. In short they may be described as a rag bag of creatures which have changed from the primitive carnivora less than other families. Therefore to list the differences between *some* cats and *some* civets a few additions must be made as follows:

3. On each side of each jaw the cats have either 3 or 2 premolars, and only one molar. Civets *usually* have 4 premolars and 2 molars. This is not universal because the sub-family Cryptoproctinae have 3 premolars and one molar, like some of the cats.

4. The claws of civets are less retractile (could this be noticed in a fossil?) but this is not a family difference because the same feature is found in the cheetah, which belongs to the Felidae.

5. The cats have toes 5 and 4, *so have some civets*, but most civets have toes 5 and 5.

6. In *some* civets more of the foot is on the ground than is the case with the cats.

7. There is a slight variable difference in the number of vertebrae.

There is nothing else of any considerable anatomical importance.

To give a list of differences between Equus and Eohippus is not so easy as I know of no suitable book of reference, and the Natural History Museum is closed so that I cannot examine the specimens for myself, but I have found the following which, though probably by no means complete, are more than sufficient.

1. Although the skull of Eohippus is more like that of Equus than is any other part of the skeleton, it has important differences, in particular the muzzle is relatively shorter, the diastema is very

slight and the ring of bone round the orbit of the eye is absent. This balances 1 and part of 2.

2. The ratio between the body length and leg length is greater in Eohippus than in Equus. This balances the rest of 2. The family differences are already accounted for.

3. The hoofs of Eohippus are rudimentary and the animal walks with the toes on the ground instead of on the tips of the toes. This balances 4 and 6.

4. The slight difference in the teeth (3) (which does not always exist) must be compared not only with the presence in Eohippus of an extra premolar and a normal canine but with a fundamental difference in the character of the teeth so great that you asked me to try to explain how it was possible for one to be derived from the other. Here there is no comparison, the Equus-Eohippus difference is of the kind which separates different orders.

5. I have not been able to count the vertebrae of Eohippus but I feel quite certain that there are significant differences (7). Anyhow the *character* of the vertebrae, which is much more important, has entirely changed. The long dorsal spines are absent. The hollow back has absolutely disappeared. Whether Eohippus had an arched back is a minor point which can be left open. Once again the difference is of the kind which separates orders rather than families.

6. There is no difference in importance between the limbs of cats and civets except the relative length compared with the body and an extra toe in *some civets*. This fades into insignificance in comparison between the limb differences of Equus and Eohippus. Equus has one functional toe on each foot compared with 4 and 3 in Eohippus. The radius and ulna and tibia and fibula in Eohippus are normal, whereas in Equus in each limb one bone only is functional and the other is rudimentary or fused with the main bone. The proportions between the different bones of the limbs are entirely different. The adaptation for speed of Equus is not there at all. Once again the differences are such as separate orders rather than families.

I think that is sufficient. You can if you like take for comparison the most widely separated species in the families of cats and civets (which is not what is usually understood by the family difference) and you find nothing in any way comparable with the difference between Equus and Eohippus.

Personally, as I have said before, I think it unreasonable to

attach so much importance to this minor point. But you and the editor actually do so. It is therefore perhaps allowable to ask whether the editor is yet converted to evolutionism, and whether you have seen reason to reconsider your attitude towards it.

H. S. SHELTON.

DEAR SHELTON,

There is a slight difference between those who assert that the earth is round and those who hold the theory of organic evolution in that the former *can*, and the latter, including yourself, *cannot*, successfully meet objections to the theory they support.

The title of this book is *Is Evolution Proved?* I maintain that I have adduced scores of facts which are inconsistent with the theory that all living things have evolved from lifeless matter by a purely mindless process. I maintain that you have adduced no facts which are in the least difficult to reconcile with special creation. But special creation is not the issue. Your task is to justify the acceptance of evolution as a theory resting on an assured foundation as, say, Copernican Astronomy. What you have done is to show that you dislike the theory of special creation, and, like T. H. Huxley, accept evolution because the idea appeals to you. Having failed to substantiate your claim, you conclude your letter by saying that evolution is 'the only known reasonable and probable explanation of the facts of life.' I venture to suggest that some who read your letters may consider they do not bear out your assertions. For example, the editor, who asks me to say in regard to your final question as to whether he is converted to evolutionism by your arguments, that the effect of this book has been still further to reinforce his conviction that, in the words of the great botanist Reinke, 'the only statement, consistent with her dignity, that science can make, is to say that she knows nothing about the origin of man.'

Much of your letter merely consists in asserting what it is your business to prove, i.e., that morphology, embryology, palaeontology, all point in the same direction. They do, but not in the direction you wish them to point.

In places you controvert not what I have said but what you would like me to have said. Thus, you suggest that because I find a fossil and cannot find its immediate ancestor I assume it to have been specially created. I will deal with this shortly. Meanwhile, I repeat my view, that the great number and variety of fossils which

occur in Cambrian rocks and the lack of undisputed fossils in any of the earlier rocks, point strongly to a great creation of living forms at the beginning of the Cambrian period. You, despite this evidence, assert that animals existed before the Cambrian period, and you deem your attitude scientific and mine unscientific !

I notice that you are making full use of your most powerful ally, fashion, in that you devote the first part of your last letter to showing that I am out of fashion. This is true as regards the British Empire and the U.S.A., where the idea of special creation is unfashionable, not because any evidence has been adduced against it, but because it does not appeal to the 'modern mind.' It is deemed quite *démodé*. Admittedly to men of the present age—and *only* to them—the idea of a Creator suddenly producing a bird out of nothing is faintly ridiculous. But the essence of a scientific approach to a subject is the refusal to be handicapped by a fashionable prejudice. So great is the power of the fashion set by Darwin that it would be professional suicide for a British or an American biologist to declare himself in favour of special creation. School text-books therefore treat evolution as proved. The B.B.C. refuses to allow any of its speakers to attack the sacred dogma. Small wonder, then, that the professional biologists who doubt evolution keep their doubts to themselves or express these in cautious language, as may be seen in the passages quoted by Lunn in the Introduction to our debate. Thus it has come to pass that 'the man in the street' is firmly persuaded that a dogma declared to be 'impossible' by leading French biologists is proved beyond all possible doubt. The firm grip of fashion makes it impossible to persuade a professional British biologist to defend evolution in public debate.

Despite this initial advantage, you have been in difficulties from the beginning of this debate because you cannot, as Lunn puts it, 'get plus out of minus, or a quality in an effect which is not present in its cause or causes.' It does violence to the basis of logical thought to maintain that the brain of Shakespeare, our reaction to beauty and the vision of the mystic were all present in the mist, mud, sand and seas of the primeval planet.

The great merit of this form of debate, in contrast to *ex parte* statements, is that the evolutionist cannot get away with vague generalities. Popular biologists, such as Dr. Julian Huxley, write as if natural selection, which, by hypothesis, can only select and cannot create, were an adequate substitution for a Creator. He writes (*Evening Standard*, October 12th, 1927) : 'In the past of

geology the slow, wasteful and blind forces of natural selection have created the marvellous living mechanism of ant, bee, bird, horse and man out of mere living slime.'

How can selection create? It can only select from that which is already created; with as much truth might a man, who has just drowned three out of a litter of four kittens, hold up the fourth and say: 'See! I have created this!'

You are, as Lunn has remarked, extremely familiar with the literature of evolution and you are clearly a skilled controversialist. It is, therefore, not lack of debating ability but the weakness of your case that has forced you to try to rig the rules of procedure in your favour:

(a) You claim that evidence equally valid for evolution and for special creation must be deemed evidence for evolution against special creation.

(b) You call upon me to disprove statements which it is your business to prove, such as the differences between Eohippus and Equus are much greater than those between the cats and the civets.

(c) You quote from authorities favourable to your thesis, but deprecate my doing so (p. 40).

(d) It is permissible for you, but undignified for me, to issue challenges.

(e) You must be free to make deductions from the absence of fossils, but it is wrong for me to do so. Thus you are certain that mammals did not exist in the Carboniferous period because no fossils of them have been found in Carboniferous rocks, but you would consider it illegitimate for me to ascribe this non-discovery of their fossils to the fragmentary nature of the geological record.

(f) Finally you, in effect, claim the right to adapt the available evidence to the demands of evolutionary dogma. As examples of this I may mention your treatment of the Galley Hill skeleton and the Castenedolo and Calaveras skulls (p. 279).

Again, the weakness of your case is evident in the evasiveness of your replies to some challenges and your complete failure to reply to others. Here are a few cases.

You have offered no explanation of:

(1) The origin of the Organ of Corti in the mammalian ear.

(2) The spinnerets of the spider.

(3) The jumping apparatus of the click beetle.

(4) The steps by which a reptile can have been converted into a mammal.

You have offered a pitifully unconvincing account of the steps by which

(1) A reptile can possibly have become converted into a bird.

(2) A land mammal can possibly have changed into a whale.

The one supremely important fact which emerges from this is that you have completely failed to put forward any reasonable explanation of how living things could possibly have evolved by a *blind, undirected and purely fortuitous process* from the lifeless inorganic matter of which the primeval planet was composed.

I will now summarise the reasons, given in my previous letters, why I reject evolution :

1. *Palaeontological* (Chapters II and III).

The rocks have not yielded any of the fossils indispensable for the proof of evolution, apart from mere differentiation in which many creationists believe.

I have given statistics showing that animals are fossilised to such an extent as to render futile the plea that the non-discovery of these indispensable fossils is the poverty of the geological record. You have not disputed these statistics. You have, however, asked me to state the significance of the fact that fossils of 64 of the 187 shelled molluscs now living in the British area have been found in Jurassic rocks. The 64 molluscs include such familiar animals as cuttle-fish, squids, oysters, mussels, cockles, winkles, limpets, scallops, and pond- and river-snails. None of these genera has undergone any appreciable change for 150 million years. All of them particularly the cuttle-fish, are very complicated animals. *Inter alia*, the cuttle-fish has eyes almost as complicated as those of a mammal, eyes provided with a lens and an iris. If this and the other 63 complicated animals have undergone no change for 150 million years, how many billion years would have been required to allow of their gradual evolution from specks of protoplasm ?

As none of these (they constitute 35 per cent of the British molluscs) have undergone any evolution for 150 million years, it is not unreasonable to suppose that this is equally true of the other 123 of which fossils have not been found in Jurassic rocks and to believe that the reasons why no fossils of them have yet been found in those rocks are: (1) the rocks that are accessible to us have not been fully explored, (2) the Jurassic rocks that held their fossils have been destroyed in the course of time, (3) these animals formerly lived in the sea far from land, so that the rocks containing

their early fossils are not accessible to us. Thus the known fossils strongly suggest that none of the molluscs have undergone evolution.

In reply to your assertion that because I find a fossil and cannot find an immediate ancestor I assume it to have been specially created, let me say that, of the above 64 Jurassic molluscs, no fewer than 22 make their earliest appearance in Jurassic rocks. These include the cuttle-fish, squid, winkle, cowrie and the pond- and river-snails. This is capable of three interpretations, viz. : (1) All migrated in the Jurassic period to the locality in which their earliest known fossils were laid down, or (2) each evolved on this spot, or (3) all were specially created on this spot. I believe that the first is the correct explanation, and that some change in the conditions of life caused several kinds of molluscs to migrate into either coastal waters or to ponds or rivers near the sea. I believe that most evolutionists accept this explanation. The only difference between them and me is that they believe that these immigrant forms evolved in the places from which they migrated, while I believe that they were created in those or other places. As none of them has evolved since its earliest known fossil was laid down, I submit that my belief is the more reasonable. The evolutionists have to believe that these molluscs ceased to evolve from the moment that their earliest known fossils were laid down.

Between the Cambrian and the Eocene periods 220 families of molluscs make their first appearance in the rocks. If each of these gradually evolved from an earlier family, 220 series of genera linking the families must have existed. If each of these series consisted of ten genera, then 2,200 intermediate genera must have existed, and fossils of over 2,000 of them should have been found if the geological record be nearly perfect. In fact, not one has been found !

The fossils of the Brachiopods (lamp-shells) tell the same story, as do those of the mammals. As regards the latter, the tables of their fossils (p. 62) show that 100 per cent of the genera of land mammals now living in Europe have been found, and this is equally true of the whales, sea-cows, seals, etc. The evolutionist asserts that each of these groups is descended from a land ancestor. If this be the case, then, as I have shown (p. 128), at least 350 fossils of genera intermediate between these marine mammals and their hypothetical land ancestors ought to have been found. In fact, not one has been found.

I closed my letter (p. 78) with the following question, in italics : How can you account for the amazing contrast between the

richness of the fossil record in respect of genera which are known to exist and the appalling poverty of the record in respect of the genera necessary for your theory? Your case is really based on fossils which have not been found!

My second reason for disbelieving evolution is:

2. *Facts Revealed by the Geographical Distribution of Animals* (Chapter IV).

In this chapter I have shown that the geographical distribution of butterflies suggests that a period of from one half to one million years is needed for the evolution of a land species (p. 146), and much longer for a marine species (p. 146). I have shown that no oceanic island has been in existence sufficiently long for the evolution of a new family (p. 147), also that, despite the immense period during which Australia has been cut off from the rest of the land, no new order of mammals has evolved in Australia. Finally, I have shown that the earth has not been habitable nearly long enough for the gradual conversion of a protozoan into a mammal (p. 149).

My third reason for the rejection of evolution is:

3. *Certain Morphological Facts* (Chapter V).

You tell me that you do not claim to be able to solve every morphological difficulty I have raised. I put it to you that you have failed to solve almost every such difficulty. On p. 304 of this letter I have mentioned six of these difficulties. Let me add a seventh: the request I made to you to draw up a phylogenetic tree of any Class or Order linking all the animals composing it (p. 177).

If you think you have met any of the above seven points, please give me an adequate reference to your reply.

As regards the gradual change of a reptile into a mammal, I pressed you to reply to this on p. 171. I put it to you that the changes you would have to describe are so impossible that a description of them would subject you to ridicule. This being so, if you do not state how you suppose this happened, I will in my next letter reproduce what a well-known evolutionist has said on this subject.

I take exception to your description of the hind part of the vertebral column of the whale. It does not 'twist round so that the dorsal processes become horizontal.' The vertebrae in the tail differ from those in front in being smaller, wider transversely and

flatter, and movable one upon another. To say that they have *become* different is to beg the question. The established fact is that they *are* different. Doubtless they are adapted to swimming, but to say that they have *become* adapted is to make a statement unsupported by any evidence, an assertion, moreover, that imposes on you the burden of showing how such adaptation can have been the result of the accumulation of small variations. *How did the ancestors of the whale contrive to swim while in the transitional condition?*

The hind legs of the seal are its chief swimming organ; in the sea-lion the fore limbs play this role. The mode of locomotion of these two animals is quite different, both in the sea and on land. Sea-lions are not in an intermediate stage between a land quadruped and a seal. Please explain why two kinds of land quadrupeds ceased to use their hind legs in the normal manner with the result that one turned into a whale and the other into a sea-cow, and (2) why a third land quadruped ceased to use its hind legs in the usual way and changed into a seal, and (3) why a fourth ceased to use its hind legs in the usual way and became a sea-lion. Please describe the intermediate stages in each of these three types of transformation.

In this and in other chapters the fallacy recurs that morphological resemblances necessarily denote genetic relationship. Lunn has dealt with this matter. An illustration of this is your mention of some similarities between the larvae of modern animals and adults of ancient ones, and your inquiry why should these exist except on the ground of genetic relationship. The answer is: in view of the fact that every animal has to have feeding, breeding, breathing, excretory, etc., organs, and the existence of a million species all constructed on one of a few general plans, it is inevitable that some members of an Order resemble those of another Order. Such resemblances occur between very widely separated animals.

My fourth reason for rejecting evolution is:

4. *Experimental Evidence is Against It* (Chapter VI).

I have pointed out how little breeders have been able to modify animals, and I asked you to name any *new* structure any breeder had produced. You have not done so.

I asserted that we know of no apparatus whereby a protozoon could be converted into a reptile or a mammal by the accumulation of small variations, and asked you, if you disagree, to name such a mechanism. You have not done so.

I asserted that the experimental evidence is fatal to the evolution theory because scientists, with all their knowledge and apparatus, have not been able to convert the most complicated organic substances, much less inorganic matter, into living matter. In consequence, it is futile to believe that blind, unintelligent natural forces can not only have converted inorganic into living matter, but endowed some living organisms with a high degree of intelligence. Far from making any comment on this, you replied that you had found very little in my letter that needed reply, and that the experimental evidence is 'just what is needed' !

My fifth reason for rejecting evolution is :

5. *Embryology Does Not Support It* (Chapter VII).

You believe that Equus is descended from Eohippus, that had four toes on the front and three on the hind foot. You say that the horse embryo exhibits three toes on each foot because it is descended from a three-toed ancestor and its embryo recapitulates ancestral history. In that case, why has the horse embryo not four toes on the front foot? Like nearly all transformists, you invoke recapitulation when it suits you and reject it at other times. I say that three rays develop in the foot of a horse embryo because there are three bones in the adult foot, the foot bone and the two splint bones.

You cite as embryological evidence for evolution your opinion that 'instead of going in a straightforward way to the formation of the adult form, the embryo develops in a devious way dependent on the lines of development of the ancestor.' As examples of this you mention the development of 'the well-known branchial arches' and the fact that 'no less than three successive excretory organs develop in higher embryos.' You deem these facts evidence that the higher vertebrates have evolved from fishes. I assured you that there is no justification for the opinion you hold and that *developing embryos take the most direct route, and that every stage in their development is dictated by physiological and mechanical necessities*, and I described how the heart, blood-vessels and kidneys develop, and I challenged you to show how their development could be more direct than it is. In reply to my challenge you wrote : 'I don't know how they would have developed if mammals had not descended from fishes, but they would certainly have developed more directly than they do.' As to the kidney you wrote : 'I cannot in any circumstances undertake to elucidate how an organ ought to have developed.'

Despite your failure in the chapter on Embryology to substantiate your claim that the development of the embryo is not direct, you in your last letter cite as positive evidence for evolution 'the development of the embryo to the adult is not direct' !

You cite as embryological evidence for evolution that the visceral arches, instead of forming 'considerable structures,' form 'insignificant ones, such as the thyroid cartilage.' As there are no large organs in the region of the visceral arches, I asked you to name five pairs of considerable organs you would expect these arches to have produced. This question you have ignored.

Every stage in the development of every embryo can be satisfactorily explained independently of all idea of the recapitulation of ancestral stages, by the facts : (1) as von Baer demonstrated long ago, embryonic development invariably proceeds from the general to the particular and from the simple to the complex ; (2) the resemblances in the embryonic development of a fish and a mammal occur only in the earliest stages, i.e., those in which the features common to all vertebrates are developing—head, body, tail, backbone, two pairs of limbs, etc. After this the resemblance ceases. Thus there is no warrant for basing any argument for descent on these resemblances.

If you rely on embryology for evidence of evolution, you must either regard its testimony as false in some cases or abandon your theory that man and the apes are near blood relations, because at no stage in its development does the human embryo resemble that of an ape. I have quoted Keith on this matter (p. 270). Here is a later pronouncement : 'Man in all ontogenetic (i.e., embryonic) stages shows characters . . . which is impossible to admit could have been developed from any stage that had once assumed "simian" characters' (Wood Jones, *op cit.*, 1944, p. 30). Here you have two of your witnesses contradicting one another. Morphology says man and apes are cousins, Embryology says they are not ! Which of them do you accept ?

My sixth reason for rejecting evolution is :

6. *No Animal Exhibits an Organ in a Nascent State* (Chapter VII).

As creationist I do not expect to find nascent organs in animals, but I do expect to find that practically every structure in every animal has some use and that is why it exists. You, as an evolutionist, expect to find in every animal a number of useless structures (you suggest 180 in man !). When I point out that what you regard

as a useless structure is useful you dispute this, as in the case of the body-hairs of man and the semi-lunar fold in his eye. As regards the latter, I may here say that the fact that this has a layer of cartilage in other mammals as well as in man does not account for the existence of this layer in mammals in view of the fact that birds and reptiles thrive without it.

Your attitude is that, when you cannot see a use for any structure, that structure is useless. This attitude is rather surprising in view of the fact that many structures formerly thought to be useless are now known to have most important functions. As late as 1890 the ductless glands were regarded as useless vestiges. To-day we know that most of them are absolutely indispensable structures. Of these it may be said that the stone the evolutionists rejected is become the head of the corner.

I regard as quite unsuccessful your attempt to meet my objection to the evolution theory that nascent organs do not exist.

My seventh reason for rejecting evolution is :

7. *Many Instincts and Habits Cannot have Evolved Gradually* (Chapter IX).

As examples of the products of what Lunn calls hit-or-miss instincts, I have cited the instincts to construct nests of the kind made by the sunbird and the house-martin, which, until almost as perfect as they are to-day, would be veritable death-traps. I asked you to describe an evolutionary series of nests leading from a scrape in the ground to one of these nests. You have not done this. The fact that hundreds of animals have instincts or habits that cannot have originated gradually is, in my view, a fatal objection to the evolution theory.

My eighth objection to the evolution theory is :

8. *Man Cannot have Originated Gradually* (Chapter X).

You have not attempted to show how the blind forces of nature can have endowed man with his intelligence, power of speech and sense of beauty. As to the evolution of man's body, the appendix to my letter p. 259 shows that many different views are held regarding man's supposed animal ancestor. This diversity of opinion exists because there are serious objections to *any* known kind of animal having been gradually transformed into a man. Man occupies a very isolated position. I have given reasons, which you have not gainsaid, why man cannot have been derived gradually

from an ancestor having a hairy coat to which the young could cling, or quadrupedal gait, or an opposable great toe. If man be derived from an ancestor having these attributes he must have come into being *per saltum* as the result of a huge mutation and thus be a special creation as I understand the term.

In your last letter on Man (p. 284) you say that I make a great deal too much of the Galley Hill skeleton and 'the suggestion that the skeleton was a burial is based on the extreme improbability that the complete skeleton would be laid down in a gravel deposit.' But the skeleton was not laid down in a gravel deposit but in a bed of loam. 'There can,' writes Sir Arthur Keith (*Antiquity of Man*, p. 257), 'be no doubt that the skeleton lay in the lower bed of loam —the one under the Chellean gravel.'

The Castenedolo skull was found on the side of a hill in a stratum, the Pliocene age of which is beyond dispute. The skull was surrounded by and impregnated with clay containing Pliocene fossils of molluscs. In order to have buried this skull from the surface the sorrowing relatives had to dig through a few inches of surface soil, then through a stratum of clay over six feet deep, then through a second stratum of clay some two feet thick, then through four feet of yellow sand containing Pliocene shells, and finally about two feet into a stratum of blue clay to the resting place of the skull. Having dug this very deep grave, the diggers must have refilled the hole they had dug in such a way that none of the strata through which they dug or the fossils they removed show the least trace of having ever been disturbed. If you can believe that all this happened, I can only say : 'If you could believe that you could believe anything,' as the great Duke of Wellington said to the man who greeted him with the words, 'Mr. Smith, I believe.' The fact that five or six other human fossils are treated by evolutionists as they treat the Galley Hill skeleton and the Castenedolo skull shows to what lengths some people will go in support of their beliefs.

You rightly remark that the existence of man of modern type in the Pliocene period does not prove that man is not descended from some animal, but it does prove that so far no fossil has been found of even a probable ancestor of man. It is this manipulation of the fossil evidence to which I take strong exception.

When analysed it is found that none of the facts adduced in support of evolution show more than evolution or differentiation within the family which is accepted by many who believe in special creation.

It may be that some of the facts you have adduced are easier to reconcile with evolution than with special creation, but I contend that the arguments against evolution are far stronger than those in its favour. Indeed, the difficulties of the evolution theory are so immense that it seems to me that we must either accept special creation or, as does the French zoologist Cuénot, resign ourselves to crying 'Ignoramus !'

I shall have achieved my purpose if I have convinced readers that the foisting of evolution on school children and undergraduates is a crime against Science, for, as T. H. Huxley said, 'an assertion which outstrips the evidence is not only a blunder but a crime.'

APPENDIX

I am glad that you have at last adduced some evidence in support of your claim that the differences between Eohippus and Equus are *much greater* than those between the cats and the civets.

Your list of the differences between the last two is not nearly complete, e.g., in respect of the ear you have not mentioned the carotid foramen in the inner side of the bulla or the constriction of the latter, and the shape of the auditory canal. You have not mentioned the various differences in the palate or those in the jaw. Nor have you stated that in some of the civets the ring of bone round the ear is not closed. You have dealt with differences in the number of the teeth but not in their form, and in order to help your case you have included Cryptoprocta that occurs only in Madagascar among the civets, but some authorities think it should be classed in a family by itself.

Nor is this all. In the case of the Equidae you compare one genus, Eohippus, with another genus, Equus, and list *all the differences* between them that you can find ; but in the case of the cats and the civets you compare a group of genera of the former with a group of genera of the latter and list *none* of the differences except those between every genus of one group and every genus of the other. You then compare these lists, which are not comparable. What you have done is on a par with excluding from a list of differences between the British race and the Japanese race such features as height, weight, colour of hair and of skin because some Japs are as heavy and as tall as some Englishmen, and some Englishmen have hair as black and skin as yellow as some Japanese !

As you have selected in the case of the horse the two genera

which display the greatest differences, you should have selected for comparison in the case of the cats and civets the genus of civets differing most from the cats and that of cats differing most from the civets. So far as I am aware no one has done this in the case of the cats and civets, but Mivart has listed the characters of the civet genus Viverrus which are not found in any genus of cat (P.Z.S. (1882), p. 147). He lists 46 such characters of which 38 relate to skeletal parts. If you add to these 38 differences, say another 12, for differences between the extreme genera, you get a total of 50, and to support your contention it is therefore up to you to name about 75 differences between Eohippus and Equus.

Our readers will doubtless realise that your attempts to magnify the differences between Eohippus and Equus are made on account of your inability to produce a series of fossils linking one family with another and that up to the present there is nothing approaching **proof** that any family has given rise to another family.

Yours sincerely,

D. DEWAR.

DEAR DEWAR,

I have looked through your last letter to find if there are any particular points which need attention and it is difficult to know which to choose. I need say very little more about the Galley Hill skeleton and the Castenedolo skull. I regard the former as probably Chellean and possibly Strepyan. Whether or no it is a burial is of very little consequence. The Castenedolo skull is, I believe, almost universally rejected. But in any case it does not affect the evolutionary descent of man. There is no evolutionary reason why a species should not remain practically unchanged for a million years or so. In fact, they very often do. I can only suggest that anyone interested in this matter would do well to read Keith's *Antiquity of Man.*

In your appendix you have certainly succeeded in confusing the minor question of cats and civets beyond my powers of disentanglement in such brief space as I can give to it. I cannot, however, refrain from pointing out that you seem to be unaware how the points you raise tell against you. You speak of the Cryptoprocta as possibly being a separate family. If that is so, it is a sufficient answer to your challenge to compare the difference

between cats and Cryptoprocta with that between Equus and Eohippus. As this genus is unusually like the cats, this is much better for me than the difference between cats and civets generally. I am a little disappointed with your treatment of this matter. It is a plain morphological point concerning which you have not the excuse of an unusual metaphysical attitude. Anyhow, I have dealt with the matter sufficiently fully in my last letter, and so I shall now leave the point to the judgment of the reader.

I find a similar confusion in much of your last letter. It is necessary to warn the reader to refer to my statements before accepting your account of them. One flagrant example is found on page 304, where you tell me that I infer the absence of mammals in the Carboniferous from the fact that fossils have not been found. If, however, you will look at page 295 of my last letter you will find I tell you I do not. It is a little difficult to argue with an opponent who knows what you mean so much better than you know yourself.

I am sorry you think it necessary again to air your grievance when I am doing my best to remedy it—if it exists. Anyhow, you have now a public debate. Whether you have made out a case is for the reader to judge. What you describe as the cautious language of biologists does not seem to me to be anything of the kind. The causes of evolution are obscure, and many biologists say this quite plainly. This explains a number of Lunn's quotations—and yours. With regard to the *fact* of the occurrence of evolution, I cannot find in your exposition any serious difficulty of principle. Of course, there are difficulties of detail. I do not know how the organ of Corti has evolved, and do not see that I am called upon to try to guess. The same applies to most of your 'challenges.' I therefore propose not to answer your last letter, but to regard it as your statement of the anti-evolutionary case, intended to balance my last letter (p. 286). Instead of doing that, I will say a little about this very involved and difficult question—the causes of evolution.

This discussion is now in order. Before dealing with it, it was necessary for me first to make it quite clear that evolution had taken place. Whether I have succeeded in doing this is for the reader to judge. I must now, however, for the sake of argument, assume that the case for evolution has been sufficiently expounded, and that there is no reasonable doubt that it has occurred, otherwise there would be little point in what follows. It would not be sensible to expound the probable and possible causes of something

that did not exist. I will start by quoting the brief statement I made in the first chapter (p. 55) :

'I think the variations that normally occur and can be observed in living creatures are all that is required. These, though small, in course of time add up to the changes that have actually taken place. According to environment, and to the changes in the environment, and to the movement of the organisms to other environments, some variations will survive and others be eliminated. This is usually known as natural selection. The causes of the variations are unknown. We know they occur but we do not know why. Personally, I believe in a small factor of use inheritance, but this is merely a personal opinion.'

We must note first that there is more in this statement than meets the eye. It definitely dissociates me from thinkers like De Vries, who postulates a series of enormous variations. I do not deny the possibility that variations larger than usual may at times occur. If they occur they would be propagated according to Mendelian laws, and so would not be swamped. If of survival value they would survive, otherwise they would disappear. They must not, of course, in any case, in animals, be of such a size as to imply physiological infertility, otherwise they would automatically be eliminated. My objection to regarding large variations as a considerable factor in evolution is merely that it seems to me reasonable to use known causes so far as they will serve us, and I think these are sufficient. Moreover, neither Micraster, nor the Equus-Eohippus series, which are our best examples of observed evolutionary changes, give any foundation for the postulation of unusually large evolutionary variations.

With regard to small variations, there is of course no doubt whatever about their existence. No two individuals are exactly alike. Here, of course, we are faced with the serious complication that we do not know exactly what differences are inherited and what are not. Differences like those between the larger and smaller peas in the same pod are not inherited. But in matters of this kind I should like to deprecate undue dogmatism. So far as *first order* observations are concerned, we can point out a fairly clear distinction between inheritable and non-inheritable variations. But nature does not proceed only by processes which can be unravelled by rough first order observations, and, in millions of generations, effects that are not immediately observable may be important. The present genetic researches on comparatively large variations are

only the beginning of the study of heredity. All the same, so far as first order results are concerned, we have to note and accept the difference between inheritable and non-inheritable variations.

Now let us consider the variations which are studied genetically. Why do they occur? They are universally referred to the chromosome outfit. This is a fact of nature which can well be described as miraculous. We have what appears to be a series of enormous molecules, probably of the proteid type but much more complicated. Very little is known about their molecular constitution. They are arranged in a definite order, and are different for different species, and presumably also have minor differences within the species. This ultramicroscopic series of molecules in some extraordinary and unknown way determines the structure of an elephant, or of a human being. It seems to me to require emphasis that no one has the least idea why this occurs at all. This basal fact should make us very modest in our attempt to unravel the causes of evolution. Why are there variations? The first point that occurs to me is that we are putting the wrong question. Every organic chemist knows the instability of chemical compounds much less complex than those we have to consider—to mention only one aspect there is the possibility of tautomerism, and possible variations in the tautomeric balance. It can be assumed that, as a certain series of these marvellous complexes determine a vast and imposing structure, variations in the complex will imply variations in the resulting structure, though the causal nexus is wholly beyond our comprehension. It therefore seems to me that the question we ought to ask is not why there are variations, but why there are so few of them. Variation does not seem to me to need explanation at all; what does need explanation is the extraordinary stability. On these theoretical grounds De Vries' suggestion seems exceedingly plausible, and it is on grounds of observation, not on theoretical principle, that we reject it. I am inclined to suggest that this stability is the result of countless generations of natural selection in the early stages of the development of life. Without a relative stability, the close approximation of the characteristics of species to the structure fitted to their environment would be impossible. Unduly variable species would be eliminated. Similarly, if the stability became absolute, and the variations necessary for survival did not occur, again the species would be eliminated. It seems to me that the relative stability, and the small amount of instability still remaining, can well be regarded as products of natural selection; but even then it

is the stability rather than the variability that needs explanation. I think we need no elaborate explanation of variability, as it is plainly inherent in the very nature of the chromosome outfit.

I have no space, nor do I think it necessary, to discuss at length fruit flies, genes, and Mendelian laws. It is well, however, to raise once more a point I emphasised earlier in the discussion. The workers on fruit flies and other species have done valuable work in showing the localisation of the causes of certain variations in definite parts of the chromosomes. This has been described as the discovery of genes, and the description is a very good one within certain limits. All the same, we should not forget that we have not the least idea what happens to the so-called gene, why its alteration should produce variations, and, more important still, we do not know, except in a few obvious cases, what other changes in the body of the animal are correlated with those we have observed. Valuable as this work is, it rests on the roughest empirical basis. For this reason I wish to urge theorists to be a little less dogmatic in their treatment of the concept of genes. In particular, I think it important to call attention to what is often overlooked, but what would at once be admitted by any clear thinker—the fundamental unity of the whole chromosome outfit.

This antithesis is a very old one, and was apparent in the first stages of evolutionary discussion in the contrast between Darwin's pangenesis and Spencer's physiological units. The present outlook of many contemporary biologists has a considerable resemblance to the pangenesis theory, with the difference that the genes are not supposed to come from all over the body, but are there already. This fits many of the facts very well. There certainly is localisation. The aspect I am trying to add to it is that of the physiological unit. This is not, as Spencer thought, intermediate between the molecule and the whole cell, but is the cell, or at least the nuclear part of it. When we remember this aspect, we tend to be cautious in exaggerating such localisation as we have observed, and are better prepared to look for correlated variations, which, needless to say, do occur, and have been observed. It is desirable to emphasise that correlated variations are a necessary assumption, and that without doubt a change which is described as an alteration in the gene for a wing (for example) must in subtle ways imply a change in the whole organism. Once again, as it is not variations but their rarity that most needs explanation, so it is not correlated variations that need explanation, but rather that there are not more of them, and

that they are not more obvious than they actually are. It is the relative localisation (for obviously it can only be relative) that calls for explanation, and once again I think we can refer this fact to the action of millions of years of natural selection, because obviously this localisation does enable natural selection to work in a more specific manner.

This theoretical statement, which cannot help but be somewhat involved, seems to me to throw some light on the question raised by the editor in the introduction, which you have repeated in one form or another all through the discussion. Obviously, natural selection can only select from what is there; we all know that, and I do not think we need reminding. The question that arises, therefore, is whether or no we can rightfully expect variations to occur? There can be no doubt that they actually do occur. But it makes a difference whether we naturally expect them, or whether we have to regard them as something extraordinary. I have attempted to show that they are naturally to be expected, and that what needs explanation is not their occurrence but their rarity. The real problem that is at present entirely beyond our understanding is the nexus which connects the constitution of the chromosome outfit and the structure of the organism. All the same, though we do not understand it at all, there can be no doubt whatever that it exists. Granting this correlation, which cannot be denied, the real problem is not to explain variations, but to explain why the chromosome structure is as stable as it is.

Up to the present, obviously, we cannot postulate anything more than random variations, in the sense that we have not the least idea what sort of variations are likely to occur. We must however, be very clear what we mean by random variations. The randomness of the variations is merely an expression of our own profound ignorance. We have not the least idea how and why the chromosome outfit determines the structure of the organism, nor of the nature of the molecular changes that occur in it. We cannot, therefore, correlate all these unknowns and say what variations result from what changes. All the same, I think we are entitled to assume that there is nothing random in the nature of things, and that, in some way not at all understood, the molecular changes in the chromosomes (or, if you like, the genes) do absolutely determine changes in the structure of the organism. This makes it clear that all this talk about blind chance, and getting plus out of minus, is not very sensible. The blind chance is merely a clumsy way of

expressing our own ignorance, and plus and minus are not concepts which have any bearing on the question whatever. These things which happen are entirely unexplained, and we shall do well to leave it at that.

Let us now pass on to another point, and that is whether there are any gleams of knowledge modifying our profound ignorance, and whether we can reasonably expect to be able to see or to anticipate any direction in the changes that take place. Needless to say, as we advance step by step, each further advance becomes more and more speculative. All the same, I think to a certain extent we can give a positive answer to this question. There are certain correlations which have been observed, and which I think are worthy of mention. One of these has already been noted in this discussion, and that is that we have had examples of how in certain cases changes which arise first in the adult seem to work their way back in time to the young, and even to the embryo. There are several of these in the Equus-Eohippus series. In Merichippus, for example, the cement in the teeth is deposited later than in Equus (S. 348), in Merichippus also the milk teeth have no cement (H. 253), whereas in Equus the milk teeth have cement like the permanent teeth. In Merichippus the colt has a small separate ulna, which in the adult fuses with the radius. In Equus the fusion is also found in the colt. In the later Merichippus specimens the premolars are like the molars, but only those of the adult. The milk teeth are of a simpler pattern. In Pliohippus the molar-like premolars are in the milk teeth also. These facts are a somewhat slender basis, but all the same I do think it worth considering whether we have not here a general tendency. Of course, natural selection will account for this to some extent, but I am inclined to suggest that there is more in it than that. Let us assume, as we are bound to assume, that a certain chemical change in the chromosomes is the cause of the variations. It is reasonably possible that such a change will spread to similar molecules, and so eventually a change in the organism, which originally appears at one place or at one time only, may become more general. There does seem to be a certain theoretical foundation for the expectation that a partial change of this kind will spread.

A few words are now desirable about the much-discussed problem of use inheritance. This has been a disputed question since the early days of evolutionary theory, and is not yet settled. Needless to say, I cannot hope to settle it now. At the same time, I do wish

to deprecate undue dogmatism on this point. Here we are concerned, not with first order phenomena of which we know a little, but with second order phenomena of which we know practically nothing. Consequently experiments and observations are inconclusive. All the same, I think that both observations and theory give some ground for the Neo-Lamarckian view. A recent book by Professor Jones, of Liverpool, entitled *Habit and Heritage*, states some facts which are difficult to explain on any other hypothesis. One of these is the arrangement of the parting of the embryonic hair of certain mammals in the exact places where it is convenient for the animal to do its toilet with its fore and hind feet respectively. This is too trivial a feature to be the result of natural selection. It could hardly affect the survival of the animal if the embryonic parting were arranged differently, or indeed if there were no natural parting at all. Use inheritance explains, and I cannot see any other reasonable explanation. This shows the kind of feature which gives some evidence, and also the difficulty of adducing evidence. When such an adaptation has definite survival value, the opponent will always say it is a result of natural selection, and obviously most adaptations of the kind will be of this character. Consequently, it is exceedingly difficult to mention any facts for which an alternative explanation cannot be suggested, and observations and experiments at the present time will naturally be inconclusive. All the same, I think there are a considerable number pointing in the same direction.

The next aspect of this question is whether, on genetic theory, we can imagine a reasonable ground for expecting such a result. I do not think this is impossible. If we take as our starting point (as we must) the correlation between the structure of the chromosomes and the bodily structure of the organism, a very natural question is whether the correlation is reversible. No one knows, but it does seem probable. Would a change in the organism by use or disuse (use more probable than disuse) produce a corresponding change in the chromosomes of the undifferentiated cells in its immediate neighbourhood. We don't know, but it seems probable. If so, can this change spread sufficiently to work back into the genetic cells? It does not do so always or regularly, that is obvious from experiment. But does it do so at all? I think this is a factor which could be interpreted by statistical laws. All that is necessary for the suggestion of use inheritance is that it should do so *occasionally*. I think, contrary to the general impression, that, even working on

modern gene theory, we may still regard use-inheritance as a theoretical possibility.

The conclusion I would draw from all this is that those like myself who incline to the theory of use inheritance have some foundation both in observation and in theory for their views. It cannot at present be either definitely proved or disproved in view of all the possible alternative explanations. This can only be done if and when first order effects are more fully understood, and second order effects can then become the subject of intensive investigation. In the meantime, all that I am definitely asserting is that use-inheritance, while not definitely proved, is very far from being disproved, and that it is quite reasonable to hold it provisionally as a probability, while recognising that others may take a different view.

The last question that needs brief consideration is the general one of design. This is a metaphysical question, and I think one fundamental error which underlies all your reasoning is that you do not clearly recognise it as such. My own view here is that it is not a question with which science can deal at all. Obviously the crude theory of design advocated by Paley has gone. Adaptations are not absolute, but only relative, and they are continually changing. Variation and natural selection, which we can see at work, take the place in rational human thought which Paley sought to fill with particular Divine dispensations which we can only imagine. Anyhow, this idea of particular design was an uncomfortable theory. If we wish to apply design to particulars, we can find just as much design in a malaria parasite, or in a tapeworm, or even in the germs of typhoid or plague, as we can in a water spider or a human being. I do not think this will do, anyhow. In the light of our present knowledge, it can well be swept away like the cobwebs in a long deserted building.

There still, however, remains the metaphysical idea of design in general. Is the whole universe, the whole scheme of things, the product of design? What has evolution to say on this matter? This question has been raised by you and the editor from time to time with a number of question-begging epithets, and I think it requires an answer. The answer is perfectly clear and explicit. *On this matter evolution has nothing whatever to say.* I have no patience with these question-begging epithets, atheistic evolution or deistic evolution, or theistic evolution (whatever the difference between these last two may be). Evolution is none of these things. Modern science (natural science) is not concerned with these problems.

We know as a matter of fact (I hope I have shown this conclusively), that evolution has taken place, that the multitudinous forms of life are genetically connected. To a certain extent, but to a very limited extent, we can unravel a few proximate causes. That is all. No doubt in the future much more will be unravelled. But we know nothing in science of final causes. To bring this metaphysical question into scientific investigations is merely a nuisance and an irrelevance. You are quite right in asserting that your idea of special creation does postulate a *vera causa* in the mediaeval metaphysical sense, whereas modern evolutionary science is entirely unable to do anything of the kind. If this admission is of any use to you, you are welcome to it.

All the same, though evolution is neither atheistic nor theistic, it is possible to interpret it either atheistically or theistically according to your particular religious and metaphysical turn of mind. If you are inclined to see design in nature, you will have no difficulty in finding it there. If you like to assume the agnostic attitude and say that this concept of design is merely an anthropomorphic interpretation of something that is wholly beyond your comprehension that is equally rational. These questions of theism, agnosticism and atheism belong to religion and to metaphysics, not to science. I feel a little disinclined to enter into the matter at all. Personally, I incline to the theistic interpretation, but I wish to make it clear that this is merely a personal metaphysical attitude and has very little to do with the subject of this book. Questions of this kind must be decided, so far as decision is possible, on other grounds, and I have written a book dealing with them at length, and do not care to repeat or summarise here.

Where we differ in our interpretation of this matter is that I maintain a much greater separation between physical causes and metaphysical interpretation. I see the miracles in common things ; but it does not assist scientific investigation to emphasise this aspect. The real miracle (in the sense of being entirely beyond our understanding) is the overwhelming fact that these microscopic chromosomes do in some unknown way determine such vast and unaccountable changes. We have no idea how this happens ; but it is clear before our eyes whenever we study an embryo. All the same, we assume that we have a natural process, and we proceed to try to unravel it—with indifferent success. Realising this as I do, I am not tempted to look beyond natural processes to account for the evolution of living things.

I think this will do. In any case I have no space to elaborate the topic further. I can at least assure you that I have no desire to evade these speculative questions. Few things are more congenial to me than theorising on larger scientific issues. At the same time I wish to make it clear that the evidence for evolution is definite and positive, and that theorising about the causes is in a different category. The evidence I have given for evolution throughout the discussion is in the main such as would be universally accepted. The theorising, to a much greater extent, is personal and individual.

Yours sincerely,
H. S. SHELTON.

DEAR SHELTON,

My attempt to condense my last letter because of shortage of space has been the occasion of your taking exception to my remark : 'you are certain that mammals did not exist in the Carboniferous period because no fossils of them have been found in Carboniferous rocks.' I should have added : 'and because the existence of mammals in the Carboniferous period would seriously upset the evolution theory.' Your attitude is : the fact that no recognisable fossils have been found in Pre-Cambrian rocks does not mean that animals did not exist when these rocks were formed, because the evolution theory asserts they did exist then. But the fact that no fossils of mammals have been found in rocks of the Carboniferous period means that mammals were not then in existence because the evolution theory demands this. Thus you make all the negative evidence support your view !

In this correspondence you have evaded all attempts to show how certain instincts of the hit or miss variety could conceivably have originated by any process of slow evolution. I never asked you to produce a theory which you could demonstrate to be true. but I did ask you to produce a plausible guess which you could demonstrate not to be intrinsically absurd.

In your letter you have confused two very different things. I am well aware that many phenomena may be inexplicable because we do not possess the necessary data, but it is one thing to be able to *prove* a particular explanation and quite another thing to be unable to suggest any plausible explanation as to how a particular fact can be reconciled with your theory.

As in the course of your remarks on the causes of evolution you admit that modern evolutionary science is entirely unable to postulate a *vera causa* of evolution and that I will have no difficulty in finding design in nature, all that I need say on this subject is: (1) no amount of shuffling or rearranging of genes and chromosomes or parts of these can do more than effect changes within the type; it is fantastic to imagine that such could result in the production of an elephant, a fish or a mollusc from an organism lacking eyes, ears, legs, heart, blood-vessels, liver, spleen, lungs, bones, muscles, etc. Not only is there no known mechanism for accomplishing this feat, but, as Dr. R. E. D. Clark writes (*Trans. Vict. Inst.* (1943), p. 75): 'The more carefully the matter is considered the clearer it becomes that if in past ages complex organisms ever did evolve from simpler ones, the process took place contrary to the laws of nature and must have involved what may rightly be termed the miraculous.'

(2) You are quite out of date in asserting that the theory of design advocated by Paley has gone and can 'well be swept away like the cobwebs of a deserted building.' The theory to-day is very much to the fore. Even Prof. J. B. S. Haldane, F.R.S., contributed to *The Rationalist Annual*, 1944, an article entitled 'The Argument from Design,' in which he admitted that it is by no means proved that the whole course of evolution from single-celled organisms to oaks, daisies, ants and men can be explained by animals adapting themselves to their environment without any conscious planning by themselves or anyone else. There are real difficulties in the evolution of such an organ as the eye, where many parts must vary together to produce an improvement.

I note your admission that there are more differences between cats and civets than you stated, and that you have not added to your list of differences between Eohippus and Equus.

This correspondence shows clearly that your belief in evolution is based, not on evidence, but on dogma. As Davies and I wrote (*Obsessions of Biologists* (1945), p. 2): 'When we examine the often conflicting and mutually incoherent arguments offered for the belief in organic evolution, we find that the whole case is based on the dogma of continuity, and also that there are certain propositions regarding which many transformists are now so positive that they abandon scientific method and exhibit clear marks of obsession whenever the same are approached.' We noticed seven of these obsessions: (1) That Man is descended from an ape. (2) That

Man is descended from a fish. (3) That Evolution is a higher concept than Creation. (4) That Special Creation is incredible. (5) That belief in Special Creation is unscientific. (6) That evolution eliminates miracles. (7) That there are no competent critics of evolution. You seem to subscribe to all these except Nos. 4 and 7. In addition to these you have exhibited in this correspondence 3 further obsessions: (1) That every animal exhibits a large number of vestigial structures. (2) That belief in Special Creation is the consequence of confused thought. (3) That the anatomical differences between Eohippus and Equus are enormous. As to (1) formerly zoologists tried to discover the use of structures of which the use was unknown; to-day they try to show that useful structures are useless! When I point out the use of the human body-hairs, you say this 'is a relic from pre-evolutionary times when it was thought necessary to find a use for everything!' So you really believe that man during millions of years, has been unable to rid himself of these thousands of useless hairs, muscles and sebaceous glands. In support of your contention you make some misleading statements, e.g., 'hairs are not necessary for the working of sebaceous glands, because in certain parts of the body in which this oiling effect is really required we have sebaceous glands, but no hairs.' You do not say that in the only parts of the body, i.e., the nipples and another tiny area, where these glands occur without hairs the glands are of different design, being much larger and their ducts open direct on the surface of the skin! Similarly when you point out that the skin is not dry on the palms of the hands and soles of the feet, where there are neither hairs nor sebaceous glands, you do not tell our readers that here the sweat glands are far more numerous than anywhere else on the body; according to Krause there are 2,800 sweat glands on each square inch of the palm and nearly as many on the sole. Clearly then the body-hairs of man are useful.

As to your denial of the use of the semi-lunar fold of the human eye, and your assertion that its equivalent is functional in many mammals, and it would be interesting to find out in which mammals it is useful and in which it is vestigial, I put it to you that this fold or its equivalent is useful in whatever form it occurs, and it is absurd to believe that in many species having a nictating membrane that moves over the eyeball, this structure has become gradually smaller and less movable so that we see it in all stages of degeneration.

(2) As evidence of your obsession that belief in special creation

and anti-evolutionism are the result of confused thought. I cite your remark (p. 52) : 'Anti-evolutionism is confusion of thought,' and (p. 114) : 'I must say quite bluntly that I regard the hypothesis, as too silly for serious consideration, indeed I do not regard it as a hypothesis at all, but merely one of those confusions of thought which removes some anti-evolutionists from the class of people with whom it is possible to conduct a rational discussion.' See also p. 133.

(3) That you are obsessed by the notion that the anatomical differences between Eohippus and Equus are enormous is shown by your assertion (p. 95) 'The difference between Equus and Eohippus is of the kind that exists between different orders or at least sub-orders.' Two causes have contributed to this obsession : (a) your reliance on views expressed by authorities many years ago. when our knowledge of the fossils was much less than it is to-day, and (b) your reliance on views expressed by writers of popular books on evolution.

As to (a) you make much of the fact that Owen over 70 years ago named the Eocene horse fossil found near Herne Bay Hyracotherium lepinorum, not recognising it as a horse. At that time no fossils of Eocene horses had been found in America. All that Owen had to go upon when naming this fossil was a much-mutilated skull with both the front and hind parts missing ! Owen, knowing no other part of the animal, deemed it to be a creature intermediate between a pig and a hyrax or coney. Gaudry, however, thought it was related to the apes ! About the same time as the discovery of this fossil two molar teeth were found at Kyson in Suffolk, which Owen deemed to be those of a monkey, and so he named this fossil Macacus eocoenus. But on further examination he found that the molars were like those of Hyracotherium, accordingly he changed the name of this fossil to Hyracotherium cuniculus—the rabbit-like Hyracotherium. As the teeth of Hyracotherium are so unlike those of the modern horse, it is not surprising that Owen never thought of looking among the horses for relations of this little animal of which nothing of the skeleton was known. As soon as a more or less complete skeleton of Eohippus was found, it was at once seen that Hyracotherium is a horse. (b) I regret to have to say that many zoologists when popularising the theory of evolution, in order to provide strong evidence and simplify matters, make statements they would not think of making in a scientific book. Professor W. B. Scott's *Theory of Evolution* (1917) is an excellent example. You have

been badly misled by this book; in the first place you have been misled by his assertion in it of the difference between Eohippus and Equus, which, as I have shown (p. 88), he contradicts in his later and more scientific book. He has again misled you by his assertion (p. 70) that 'rudimentary teeth occur in certain embryonic birds which are re-absorbed and disappear before hatching.' Notice he *names no bird nor does he quote any authority!* He was probably casting about for evidence for evolution and remembered having heard somewhere that someone is supposed to have found teeth in a bird embryo, and, without troubling to verify this, he inserted this in his book. Even better evidence that Scott's book is pure evolution propaganda, is the following statement (p. 81), the blood precipitation tests 'come to as near to giving a definite demonstration of the theory (of evolution) as we are likely to find.' This sentence alone condemns the book. One might have expected something better in Scott's *Introduction to Geology*, as he was a professor of that Science, but the passages you quoted in your last letter are unworthy of a place in a scientific book.

The Alleged Transformation of a Reptile into a Mammal.

As you have not told our readers the strange story of how a reptile turned into a mammal I will summarise the version of it given by Dr. R. Broom in *The Mammal-like Reptiles of South Africa* (1932) (p. 320 et seq.) When reading what follows the reader should bear in mind what I said about the differences between reptiles and mammals on p. 164.

'The changes,' writes Broom, 'that converted them or one of them (i.e., an Ictidosaurian reptile) into a mammal may have been a change in diet. The snapping jaw had to be converted into a masticatory jaw, and as the quadrate became more or less fixed to the squamosal (i.e. the bone of the skull on which the quadrate articulates) it kept with it the articular and other little bones of the jaw, and the dentary became comparatively free and formed a new hinge with the squamosal. The small bones, no longer moving with the jaw, became modified as parts of the auditory apparatus. . . . The changes by which the articular became the malleus, and the angular became the tympanic (the bone encircling the ear to which the ear-drum is attached in mammals), in my opinion originated after the small bones had left the jaw, and can be fairly easily imagined.'

In less technical language Broom's story is: Some reptile

scrapped the original hinge of its lower jaw and replaced it by a new one attached to another part of the skull. Then five of the bones on each side of the lower jaw broke away from the biggest bone. The jaw bone to which the hinge was originally attached, after being set free, forced its way into the middle part of the ear, dragging with it three of the lower jaw bones, which with the quadrate and the reptilian middle-ear bones, formed themselves into a completely new outfit. The rest of the lower jaw bones, having no work to do, vanished ! While all this was going on, the Organ of Corti, peculiar to mammals and their essential organ of hearing, developed in the middle ear. Dr. Broom does not suggest how this organ arose, nor describe its gradual development. Nor does he say how the incipient mammals contrived to eat while the jaw was being re-hinged, or to hear while the middle and inner ears were being reconstructed !

The above changes relate only to the skeletal parts of the head and jaw and are insignificant in comparison with the transformations supposed to have taken place in other parts of the body. Let me mention some of these. In addition to the change in the third eyelid, already noticed, the muscle that focuses the eye lost its stripes, and the blood supply of the iris became greatly modified. The mode of locomotion became revolutionised, the thorax became reorganised, the hip bone underwent changes, and the ankle joint shifted to the root of the toes from its original position between the two rows of ankle bones. The whole breathing apparatus was re-modelled, which involved the origin of a new organ—the diaphragm. The transformists who do not believe that this structure can have been developed gradually, deem the mammals to be derived directly from amphibia. A mechanism developed for keeping the temperature of the blood constant, the left aorta was scrapped and the red blood-corpuscles lost their nuclei. The integument became changed beyond recognition ; the scales disappeared and their place was taken by new structures—hairs ; the skin acquired two extra layers ; and three new types of gland sprang into being—the sebaceous, the sweat and the mammary glands. In the wall of the intestine the longitudinal muscles changed places with the circular ones. The chemical changes that take place within the body changed so that the waste products became mainly urea instead of uric acid. In effect the reptile became transformed into an entirely different kind of animal—a new class of animal. If these changes took place as the result of the accumulation of small variations, the

process must have occupied many millions of years, and myriads of intermediate animals must have lived, but not a single fossil has been found of any of them.

The differences just mentioned nearly all relate to the anatomy of the mammals and the reptiles ; those in the minute structure of their tissues are equally marked. As Vialleton points out (*L'Origine des Êtres vivants. L'Illusion transformiste*, *p.* 358) : 'all the tissues of mammals have their own peculiar characters and there are no transitions between these and those of other vertebrates.'

Despite all this the great majority of evolutionists believe that mammals gradually evolved from reptiles. This is asserted dogmatically in all text-books, taught in universities and schools, and proclaimed by the B.B.C. In the broadcasts to schools the children are told that they are descended from fishes, through reptiles and apes, and near-men, which are described in detail accompanied by the grunts and uncouth sounds made by these imaginary beings. Is it then surprising that our magistrates lament the increase of crimes by juveniles ?

A. N. Field truly remarks in his brilliant little book *Why Colleges Breed Communists* : 'Verily the evidential standards of modern evolutionist science represent probably the lowest point in intellectual degeneration reached by civilised man in the past two thousand years.'

Yours sincerely,

D. DEWAR.

DEAR DEWAR,

This is my final letter and very little remains to be done except to say that I have found the discussion interesting in many ways, and am very pleased to have been able to argue the matter with you. I greatly dislike anyone saying that their arguments have been ignored, and that their case has not been fairly considered, and I think it will be agreed that I have tried to understand what you have to say, and to meet it in so far as I have been able to understand it.

The title of this book is : *Is Evolution Proved?* It does not seem to me that there is any reasonable doubt about the answer. The problem that interests me more is what we mean by proved. What kind of proof is possible in a case of this kind ? That is a philosophical question which cannot be properly treated in a controversial discussion, and which I cannot deal with here. All I can say

is that I think the proof in a general way is perfectly sound so far as is possible in a case of this kind. When the events concerning which we make assertions occurred in the remote past, it is only indirect evidence that is available.

I have endeavoured to show that this indirect evidence is of the kind and quantity which would naturally be expected. This is dealt with in detail throughout the book, and is summarised in my first letter in this chapter. I do not now propose, nor do I think it necessary, to add anything to that. You and the editor have been very insistent that a discussion of the causes of evolution is a necessary complement. I do not agree about the necessity, but I have done my best to meet you in my last letter—p. 314. Needless to say this is very brief and inadequate, but it does do something to meet your request. It is desirable to repeat —what I said at the time—that this section is theoretical and personal, and not part of the evidence for evolution. Still, considered in that light, I think the points I have brought forward are worthy of consideration.

It is of course not claimed that the evidence for evolution is complete. Obviously it is not. Every fossil discovered which combines characters which were previously only known in different families or orders is additional evidence. Every research in morphology, physiology, or embryology, which shows similarities not previously known between different families or orders is additional evidence. This is continually accumulating from year to year. Of course there are gaps in the geological record, and morphological puzzles. With regard to the latter, I have done a little plausible guessing in the appropriate chapters, and you are not satisfied because I have not done more. I think I have done enough, and in any case am of the opinion that plausible guessing is a futile process, and I have only consented to do it at all because you and the editor attach importance to it. But to continue, and particularly to do anything of the kind in this last chapter, would be tantamount to admitting that it is of some consequence, which most emphatically I do not. With regard to the general evidence for evolution it is of course open for you to say—as in effect you have said—that the evidence is not sufficient *for you*. There is no answer to that, and I shall not attempt to give one.

The discussion has shown quite clearly, what should have been obvious from the outset, that the only conceivable alternative to evolution is special creation. It has become more and more apparent

to me as the discussion proceeded that this is not a scientific question, but belongs to the sphere of private theology. It has not been suggested that there is any direct evidence that a particular form has been specially created at a particular time. Even Colonel Merson Davies, who believes that some lice were specially created quite recently, as recorded in Exodus, would hardly contend that the evidence was sufficient to establish it as a scientific fact. It is recorded in Exodus, and that is sufficient for him. If, on the other hand, you assert that the *direct* evidence for evolution is very slight, which is more or less true, and that, like the evidence for evolution, the evidence for special creation is indirect, you are in an unassailable position. No one can prove that special creation has not occurred, as I showed at the start by an example which greatly annoyed you, and which I will not repeat.

In asserting special creation you are perfectly safe from disproof. Put in special creations when and where you like, and no one can disprove you. As a matter of personal curiosity, I should have liked to have been informed what special creations you thought necessary in the case of the Hominidae ; but that is merely personal curiosity. You allege that it is reasonable to postulate special creations in some cases but not in others. There I disagree and cannot see reasonable ground for postulating them anywhere, though they are more plausible in some cases than in others. If there were agreement what special creations were necessary, we might take you more seriously, but I have not been able to find anything sufficiently definite, much less agreement. Vialleton, according to your account, regards the family as the unit of creation ; you are not sure ; and Davies thinks several special creations probable in the case of the dogs. If you want special creations no one can disprove you, but I cannot see any reason, in science or in theology, why you should want them. That is a fundamental difference between us, and I see no way of getting round it.

The main question of course is not special creation, but to what extent evolution is proved. I have attempted to show that the proof is as complete as can reasonably be expected in the present state of our knowledge, and that it is proved beyond reasonable doubt. To what extent I have succeeded is for the reader to judge.

Yours sincerely,

H. S. SHELTON.

5, Ferry Road, Teddington.

DEAR SHELTON,

The editor writes :- 'Shelton writes "the only conceivable alternative to evolution is special creation." He is wrong. The origin of species may be one of the innumerable insoluble problems (see p. 15). I am only one of many who believe that the difficulties both of evolution and of special creation are so great that the only scientific attitude is to suspend one's judgment. My own lines of research did not encourage me to claim to have solved insoluble problems, perhaps because irresponsible dogmatism about avalanches might lead to more fatal results than irresponsible dogmatism about Archaeopteryx. "No one," writes Shelton, "can prove that special creation has not occurred." He implies that no evidence would shake the faith of the special creationist. The same might be said with more justice of the evolutionist. In fact Prof. D. M. S. Watson has said it ! And it is charitable to assume that Shelton has *forgotten* that you and I have both defined the kind of evidence which we should both regard as completely fatal to special creation (p. 53).

'Influenced as I am by the mental fashion of the day my bias is in favour of evolution and I should accept evolution as proved but for the following facts : (a) the difficulty of reconciling the suddenness with which new types appear in the geological record with any theory of slow mindless evolution, (b) the impossibility of tracing any family into another family by means of a true lineage series of fossils, (c) the impossibility of reconciling the alleged imperfection of the geological record with your statistics which suggest that most genera are represented in that record (pp. 61–71), and, finally, (d) the fact that no evolutionist has produced a plausible *guess*, much less a theory supported by evidence, to suggest how a purely natural process could have evolved from the mud, sand, mists and seas of the primeval planet the brain that conceived Beethoven's Ninth Symphony and the reactions to the beauty of music, of art and of nature.

'Evolution cannot be *proved* by sneering at Fundamentalism. I do not go so far as Lemoine, former honorary Director of the National Museum of Natural History in Paris and editor of the section of the French Encyclopaedia dealing with Evolution, that "evolution is impossible" (see p. 10), but in view of the growing scepticism among scientists (see p. 9) it seems to me that it is a crime against science to allow evolution to be represented in most schools as a proved and demonstrated fact. In a minority of schools

pupils are told the arguments for and against evolution, and are left to make up their own minds. Those who agree that in this respect at least the attitude of the minority is more scientific will be the first to welcome this book, the first of its kind, the first point by point discussion of the case for and against evolution. It seems to me an ideal book to be put into the hands of an intelligent boy or girl. You are both masters of your subject. You have both had a scientific training. You are both skilled advocates and I must congratulate you both on your success in presenting your respective arguments in a fashion which should appeal not only to specialists but also to the public which your editor is anxious to reach, the public consisting of intelligent laymen.'

. . . . . .

The inclusion of the above remarks by the editor leaves little more than a page for the rest of this letter. I join in the editor's congratulations on the skill with which you have presented your case. What has made such demands on your skill is that you, as an advocate, have not only to try to prevent our readers from discovering that there is *no evidence whatever that any major group of animals or plants has ever evolved from another*, but you have also to induce them to believe that such evolution has in fact occurred despite the fact that all the facts, as opposed to opinions and theories, it is possible to adduce, indicate at most (what most scientific creationists believe or think probable), that many organisms have in course of time undergone considerable modification *within the limits of the family*.

You have worked these meagre facts valiantly. You believe they show that all the horses are derived from a common ancestor. This being so, you assert that it is mere obstinacy not to believe that all horses, lions, whales, bats, elephants, etc., are not descended from a common ancestor. And to stop at this point is illogical. The sensible man believes that *all* plants and animals are derived from a common ancestor. It is the fashionable belief. Only cranks refuse to join in the general acclamation of evolution. Special creation is a doctrine for fools and muddleheads. You tell our readers that in biology, the ordinary rules of evidence do not apply. The evolutionist has not to prove evolution ; he who rejects it has the task of proving that it has *not* occurred. As to the evidence adduced against evolution, this ought to be brushed aside as useless, because, if accepted, it means that special creation is true, which

no sensible man can possibly believe. We have the choice between accepting evolution or nonsense !

So far as I am aware you are the only evolutionist who has had the courage to debate evolution with a trained biologist. My experience is that, although many scientific biologists proclaim evolution loudly through the medium of the Press and the B.B.C., none of these gentlemen is willing to debate it with another biologist.[1] You, unlike such biologists, have the courage of your convictions, and you deserve the thanks of our readers for giving them an opportunity of becoming acquainted with many facts about evolution that find no place in biological text-books.

Yours sincerely,

DOUGLAS DEWAR.

Almora,
Park Avenue,
Camberley,
Surrey,
October 25th, 1945.

[1] See *Evolutionists Under Fire*, by L. Merson Davies and me. (Copies obtainable from either of us.)

GLOSSARY

(Compiled by H. S. Shelton)

Note.—A number of technical terms, particularly the names of the geologic epochs, are explained in the charts. Others are explained in the Introduction and in the text. This glossary deals with a few others.

ACHEULEAN. See Palaeolithic.

ADRENALIN. A substance manufactured by the suprarenal glands. It has the property of constricting the arteries and stopping the flow of the blood. For this reason it is used with a local anaesthetic in dentistry.

ALGAE. The simplest forms of green plants. The smallest consist of one cell only and are microscopic. Others are multicellular, and some, especially seaweeds, attain a considerable size. All the same they are simple in structure, and in particular the differentiation into roots, stems, flowers, and leaves found in the higher plants does not exist.

ALISPHENOID CANAL. A narrow passage situated in the sphenoid bone at the base of the skull. The sphenoid bone is a wedge-shaped bone situated between the temporal and occipital bones. It separates the upper and back parts of the cavity of the nose from the cavity of the brain. The name is derived from the Greek and means wedge-shaped.

ANDROMEDA. A constellation south of Cassiopeia, which is easily identified by the beginner. The Great Nebula referred to is in the north-eastern section of the constellation, about 16 degrees south and slightly east of Beta Cassiopeia, the bright eastern end star of the five.

AMMONITES. Fossil cephalopoda related to the modern cuttle fish. In the fossils, with other features, ink bags have been found.

ARTHROPODS. A phylum of animals including insects, spiders and crustacea. They are distinguished from other invertebrates by containing about 20 segments which are well differentiated also by the possession of jointed limbs.

AUDITORY BULLA. A portion of the temporal bone of the skull connected with the ear. It covers the bone which contains the ear capsule.

BALANOGLOSSUS. A worm-like creature which lives in the mud of shallow seas. There are a number of species, the length of which varies from an inch to two or three yards.

BRACHIOPODS. A well-known fossil which is in appearance something like a bivalve mollusc (e.g. the oyster), but differs in that the two halves of the shell contain the upper and lower parts of the body respectively, instead of, as in the mollusc, the right and left sides. There is considerable doubt about the classification of this form.

CANINES. The teeth next to the incisors, so called because they are well-developed in the dog, and in carnivora generally. The full complement of mammalian teeth is 44, though in many species some of them are missing. These 44 are of course 22 in each jaw and are similar on

each side of the jaw. They are classified as follows. The three front ones are known as incisors, the next as the canine, the next four premolars and the back three molars—11 in all.

CARPALS. A set of bones in the wrist. The bones in the human arm are known as follows : (1) from the shoulder to the elbow—humerus ; (2) from the elbow to the wrist—radius and ulna ; (3) in the wrist and hand (with some other small bones we need not mention): (a) carpals, (b) metacarpals, (c) phalanges or digits (last three joints). The corresponding bones in the leg are (a) femur, (b) tibia and fibula, (c) tarsals, metatarsals and digits. In other vertebrate animals the bones are similarly named but their sizes and proportions vary, and often some of the bones are missing. In the horse, concerning which the discussion of these bones occurs in this book, the lower bones, such as are there, are greatly enlarged. The knee and hock in the horse correspond to the ankle and wrist in human beings.

CARUNCLE. A small pale red excrescence in the inner corner of the eye.

CEPHALOPODS. A class of molluscs (q.v.) which includes the cuttle-fish.

CHELLEAN. See Palaeolithic.

CHELONIA. An order of reptiles including the turtles and tortoises.

CHROMOSOMES. Rod or thread-like structures in the cell nucleus, so-called because they are more easily stained than the surrounding protoplasm.

CONJUNCTIVA. The membrane surrounding the eyeball.

CRINOIDS (or crinoidia). A class of echinoderms (q.v.) which includes the ocean lilies.

CRUSTACEA. A class of arthropods (q.v.) which, generally speaking, is distinguished from others by a hard horny covering which is periodically shed. The lobster, shrimp and woodlouse are good examples of this class.

DEMIURGE. This term refers to a number of early Christian heresies, one of the tenets of which was that the world was not created directly by the Divine Being, but by an intermediate agent known as a Demiurge. This is set forth at length in the writings of Irenaeus.

DIASTEMA. The gap between the front and back teeth found in a number of mammals, e.g. horses and cattle. More generally—any gap between the teeth.

DIGITIGRADE. A term implying that the animal walks on the toes only, not on the body of the foot, as for example, cats.

ECHINODERMS (or echinodermata). A phylum of invertebrates of which the star-fishes and the sea-urchins are probably the most widespread.

EPIDERMIS. The outer layer of the skin. Adjective—epidermal.

FIBULA. See Carpals.

GANGLION. A swollen part of the nerve system, acting as a nerve centre.

GLYCOGEN. A starch-like substance found in animal tissues. It decomposes to form the sugars which are consumed in vital processes. Sugar, it should be noted, is a generic term, and ordinary sugar (sucrose) is merely one of a number of sugars.

HETERODONT. A term applied to teeth implying that they are of different kinds, as distinguished from homodont, which means that they are all of the same kind. A good example of the latter is the dolphin.

HOMO NEANDERTHALENSIS. See Palaeolithic.

HUMERUS. See Carpals.

LARVA. The immature form of an animal which passes through more than one phase of active existence. The caterpillar is the larva of the butterfly or moth. Adjective—larval.

LINEA ALBA. A band of fibres running along the middle of the front abdominal wall interlaced with the sheaths of the abdominal muscles.

MAMMAL. A class of vertebrates (q.v.) which includes all the ordinary quadrupeds as well as bats and such sea animals as whales and seals. The name implies the existence of mammae or milk glands in the female which enable them to suckle the young. Hair is another structure peculiar to mammals, though some mammals such as whales are practically hairless.

MAXILLAE AND MANDIBLES. Appendages or limbs commonly found in arthropoda. The first act as jaws and the second as a grasping organ.

METACARPALS. See Carpals.

METATARSALS. See Carpals.

MOLARS. See Canines.

MOLLUSCS. A phylum of animals generally distinguished by a hard and rigid external shell which is loosely fitting, as distinguished from the closely fitting one of the crustacea (q.v.). In a number of molluscs the shell is rudimentary. The phylum includes oysters and other shell-fish, snails and slugs, and cuttle-fish.

MOUSTERIAN. See Palaeolothic.

NICTATING LID OR MEMBRANE. The third eyelid which, when functional, is an elastic membrane covering the eye from the inner corner. In human beings and in some other mammals it is rudimentary.

PALAEOLITHIC. The term means in general the period of time in which men, or creatures similar to men, existed and made implements previous to the Neolithic, that is the age of polished stone and of agriculture. The Palaeolithic is divided into a number of sections based on the implements which have been found. The implements are usually of flint, but occasionally implements of bone horn and other substances have survived. The order of the cultures working backwards in time is as follows :

(a) *Upper Palaeolithic.* Azilean, Magdalenean, Solutrian, Aurignacian. A few skulls and skeletons have been found which are invariably similar to those of modern man. The names are usually derived from the district in which the implements are first found, or from those in which they have

been found in unusual abundance. For example the Solutrian culture is named after the village of Solutré, in the Saone drainage area.

(b) *Lower Palaeolithic.* Mousterian, Acheulean, Chellean, Strepyan. The Mousterian culture is mainly found in the last stage (Wurm) of the glacial epoch. The implements were made by Homo Neanderthalensis, which was a different species from modern man, in particular different in the form of the brain, the teeth and generally in the structure of the bones.

Before the Mousterian, again, there is an absolute break in the character of the culture, but the previous cultures : Acheulean, Chellean and Strepyan grade into each other. The exact physical form of the men who made these implements is not known, but it is generally believed that they were much nearer to modern man than is Homo Neanderthalensis, and may well be his ancestors. The last two cultures almost certainly date back to the main interglacial (Mindel-Riss). Before the Strepyan the order of the cultures is not clearly made out. A very considerable time elapsed between the Strepyan culture and the implements found by the late Mr. Reid Moir in the Pliocene.

(*Note.*—The ice age consists of at least four periods of glaciation known as the Gunz, Mindel, Riss and Wurm. These periods were separated by warm interglacial periods known as the Gunz-Mindel, Mindel-Riss and Riss-Wurm. Note the alphabetical order of the names. The Gunz, of course, was the earliest.)

PARALLAX. In general this means the apparent difference of position of nearby objects when viewed from different places. Annual parallax is an astronomical term meaning the (very slight) difference of position of the nearest stars when viewed from different parts of the orbit of the earth.

PARAMECIUM. A complicated microscopic unicellular organism sometimes known as the slipper animalcule from its shape.

PILTDOWN MAN. Otherwise known as Eoanthropus Dawsoni, is named from an incomplete skull found near the village of Piltdown, near Lewes (Sussex). A full account of the details will be found in Keith's *Antiquity of Man.* It is sufficient to say here that the brain is of human size but there are sufficient differences in the character of the skull and jaw to justify the classification of the specimen in a different genus.

PROPLIOPITHECUS. A fossil ape found in the lower Oligocene.

RADIUS. See Carpals.

TRYPSIN. The chief digestive ferment of the pancreatic juice, which converts proteins to peptones.

ULNA. See Carpals.

UNGULATES. Animals having hoofs. This is a very large order of mammals, conventionally divided into a number of sub-orders.

VERTEBRATES. Animals known as vertebrates are distinguished from others by the possession of a jointed backbone. The jointed parts of the backbone are called vertebrae. The five classes of vertebrae are fishes, amphibia, reptiles, birds and mammals. The vertebrae, particularly in

mammals, are classified as : cervical (those in the neck), dorsal or trunk (those in the back), sacral (those which are fused to form a support for the hind limbs), and caudal (in the tail). The sacral vertebrae are sometimes called the sacrum.

Animals not vertebrates are sometimes called invertebrates, but they are divided into several phyla, e.g. mollusca, arthropoda, etc. (q.v.).

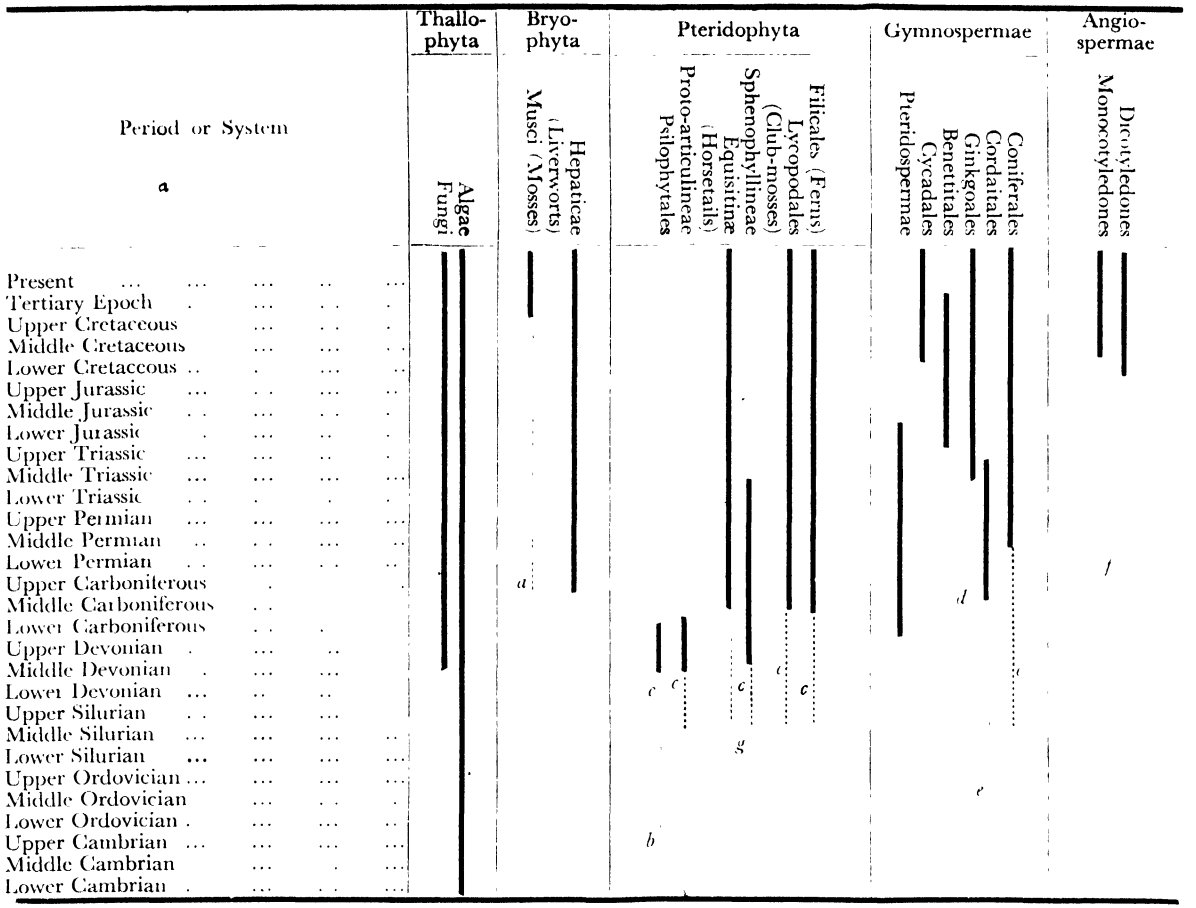
VIVERRIDAE. The family of carnivora which includes the mongoose and the civet cat.

VORTICELLA. A microscopic unicellular organism, sometimes called the bell animalcule from its shape. It has a long stalk at the end of which is the principal part of the living organism, which is shaped something like a bell.

(Reproduced from the *Journal of the Transactions of the Victoria Institute*, vol. LXXVI, 1944)

CHART II OF THE GEOLOGICAL RECORD

Periods during which the classes of plants are known to have existed.



NOTES ON CHART II

(*a*) Some authorities deem *Muscites polytrichaceus* and *M. bertrandi* from the Upper Carboniferous deposit in France to be Mosses.

(*b*) W. C. Darrah found a fossil in a Cambrian deposit in Sweden which he considers to be the shoot of a land plant.

(*c*) Fossils of Psilophytales, Equisitinae, Sphenophyllineae, Cordaitales and Filicales occur in the Fern Ledges of New Brunswick, (See note (*b*), Chart I.)

(*d*) *Psygomophyllum* from the Upper Devonian of Bear Island may be the impression of the leaf of a Ginkgo.

(*e*) Fossils *Dadoxylon hendricksi* occur in deposits in Cornwall deemed to be Upper Ordovician and Lower Devonian by the Geological Survey.

(*f*) The fossil *Angiospermum americanum* from a Carboniferous deposit in the U.S.A. is held by its discoverer, Dr. Noe, to be part of the stem of a monocotyledonous flowering plant. Seward and others consider it to be that of a Pteridosperm very like the Maize plant.

(*g*) The fossil *Buthotrepis harknessi* found in an Ordovician deposit in England is deemed by its discoverer, Nicholson, to be a sea-weed, but Sir J. Dawson considers it to be an Equisetum and he changed its name to *Protoannula harknessi*.

That six Classes of plants have become extinct as opposed to only two of animals may be ascribed to the fact that the sea is less affected than the surface of the earth by climatic changes resulting from geological disturbances.

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