MACROEVOLUTION QUESTIONED

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This article is intended as a critique of the whole doctrine of macroevolution, particularly as the doctrine is commonly presented at schools and colleges. The well known textbook, **Physical Anthropology**, by Lasker,¹ is cited to show how the doctrine is, in fact, presented. Citations from many authors show that practically every assumption of the macroevolutionary doctrine is, at best, questionable.

It will be understood that this article is not intended as an attack on Lasker, nor on his book. Rather, it is a criticism of the doctrine which the author assumed in his book.

Introduction

The thesis to be maintained in this article is that the doctrine of evolution at the "macro" level is without real scientific support. I have no quarrel with "microevolution" (except, possibly, that the name is a bad one), the kind of change, for example, by which breeders develop toy poodles or juicier grapefruit. My objection is to the attempt to *extrapolate* these small changes into assumed *macro*evolutionary changes, by which fish supposedly became philosophers and random irrational particles were transformed into progressively more and more complex organisms.

Professor Lasker's, *Physical Anthropology*,¹ is a popular and widely used textbook. It accurately represents current evolutionary doctrine. For these reasons, I have chosen it as a vehicle for presenting my own contrary point of view.

The criticisms which follow, therefore, are directed, not at Professor Lasker, but at the doctrine upon which his book is based. As will become apparent, such criticisms are possible to a considerable extent because of the willingness of evolutionary thinkers to admit frankly that there are problem areas.

Obviously, all of the evolutionists quoted herein remain evolutionists, even while admitting the problems in their own fields. They sometimes assume that evidence from other fields nevertheless continues to support evolution, or that future discoveries will somehow vindicate the doctrine, despite the contrary facts which are known now.

I should suggest that when one considers the breadth and depth of the problems, he must agree that the entire doctrine is called into question. To the extent that this article raises questions, which is the main purpose, the careful reader will want to refer to the original sources to judge for himself whether the doctrine of macroevolution is adequate, or even reasonable.

One may begin, as Professor Lasker does, with the origin of life.

The Origin of Life

In the latter half of the seventeenth century Francisco Redi demonstrated that, contrary to what many believed, flies are not generated spontaneously from meat. Louis Pasteur proved the same for bacteria and sour milk in 1861. Nevertheless, spontaneous generation remains an essential ingredient in evolutionary doctrine. What is the evidence? Professor Lasker says: "So far as we know, the naturally occurring organic compounds of today are the result of life processes: life seems always to depend on prior life." (p. 26) (Remember that in this context "organic" does not mean "living"; it simply refers to any compound containing carbon.)

Having thus disposed of the facts, Lasker goes on to speculate on pages 26-28: "Imagine, then, a time before there was life. Even if organic compounds were not being synthesized in cells, at least there would be no microorganisms to degrade such organic compounds as existed. Since there was nothing to cause compounds to decay, evolution would have caused compounds to become increasingly complex, not simpler . . ."

A few paragraphs later he adds: "In any case, constituents of the proteins and nucleic acids of living cells have been synthesized in the laboratory under primitive earth conditions. No one has yet come close to creating a living cell from primitive 'air', however, but the chemical steps have been shown to be plausible." (p. 32)

The "plausibility" of these steps should be considered in some detail. First of all, it is misleading to say that in the absence of microorganisms "there was nothing to cause compounds to decay". In the experiments which Lasker cites (Miller, Sagan and Khare, Wollin and Ericson, Stephen-Sherwood and Oparin) the amino acids and other simple compounds had to be removed quickly from the source of energy which produced them (electrical sparks or ultraviolet light) because the rate of destruction in the presence of these, or of any of the other proposed energy sources, far exceeds the rate of production.²

Even if it be assumed that these compounds could have been formed, and survived in the atmosphere long enough to reach the ocean, most of them would have been destroyed by water at ordinary temperatures. For example, Miller and Orgel have stated that: "The rates of depurination of DNA, of hydrolysis of peptide and polynucleotide polymers, and of decomposition of sugars, are so large that it seems impossible that such compounds could have accumulated in aqueous solution and have been used in the first organism, unless the temperature was low."³

Such low temperatures, however (Miller and Orgel prefer freezing or below) appear inconsistent with the evolutionary assumption that life formed soon after the earth cooled from a molten mass, while the oceans would still have been quite warm.

Moreover, these compounds are also destroyed by oxygen. Thus, proponents of spontaneous generation of life are forced to claim that the primitive atmosphere contained no oxygen. (See Lasker's Figure III-3, p. 26.) At present, the atmosphere is about 21% oxygen.

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It appears, though, that even if there were originally no oxygen, photolysis of atmospheric water vapor by ultraviolet light would have caused a significant amount of oxygen very early in the history of the Earth.⁴

Moreover, there is no evidence that the atmosphere ever contained methane,⁵ the gas most commonly used in experiments alleged to have to do with the origin of life. Ammonia, which is needed along with methane to produce the compounds, is also subject to rapid photolysis by ultraviolet light. It would have been reduced to concentrations much lower than those used in the experiments.⁶

Suppose, however, for the sake of argument, that amino acids could have formed. Could they have arranged themselves into "chains" to form proteins? Lasker says:

In attempting the abiogenetic synthesis of proteins, S. W. Fox (1960) was able to produce substances from amino acids that are in some ways like proteins. They are capable of assuming the shape of little uniform spheres that can be seen under the microscope. In some cases the electron microscope reveals differentiation and such cell-like characteristics as double-walled membranes in the droplets. (p. 33)

Sidney Fox produced these chains of amino acids by heating a pure, dry mixture of certain amino acids, usually from 6 to 10 hours at 170-200 °C, or for a week at 120°. After that he stirred in hot water, and then removed insoluble material by filtration. When the solution cooled, microscopic globules or "microspheres" containing the amino acid chains precipitated out. Fox claims that these "proteinoid microspheres" are stable and exhibit a kind of prebiological evolution. But Oparin says:

Fox's microspheres, since they are obtained thermally, do not present very promising results from this point of view (i.e., evolving to include metabolic processes). *Their structure is static*. This ... creates many difficulties when it comes to converting them into dynamic systems which could be used for modeling the evolution of metabolism.⁷ (Emphasis added.)

Of course, Fox's experiments depend upon a supply of pure, dry amino acids, a requirement which, as has been noticed, seems impossible to fulfill. If such a supply were available, however, Fox has shown that it is possible under certain conditions to link amino acids together. He suggests that such conditions might have prevailed on the primitive earth on the edges of volcanoes. But Miller and Orgel are critical of this suggestion, pointing out that when lava solidifies, its surface is hardly warmer than the air. They add:

Another way of examining this problem is by asking whether there are places on the earth today with appropriate temperatures where we could drop say 10 grams of a mixture of amino acids and obtain a significant yield of polypeptides...We cannot think of a single such place.⁸

Of even more significance is Fox's contention that his polypeptides (chains of amino acids, shorter than proteins) are similar to proteins in the *order* in which the links are formed. The fact that even the simplest proteins require a precise order in their amino acid links, and that the probability that such an order would occur spontaneously is virtually zero, has been a source of considerable dismay to supporters of spontaneous generation.

So far as is known, there is nothing about the physical and chemical properties of amino acids, nucleotides (the building blocks of DNA), and sugars, to indicate that polymerizations involving mixtures of any of them produce anything other than *random* sequences or structures. This randomness has been experimentally demonstrated by two of Fox's colleagues, Peter Mora and Gerhard Schramm, for polymerization of sugars,⁹ and of nucleotides.¹⁰

Fox's supposed contrary evidence for his amino acid chains has been criticized by Miller and Orgel, who accuse him of deception: "The degree of nonrandomness in thermal polypeptides so far demonstrated is minute compared with the nonrandomness in proteins. It is deceptive, then, to suggest that thermal polypeptides are similar to proteins in their nonrandomness".¹¹

One of the most puzzling facts of biology relates to this problem of order versus randomness. All but one of the amino acids found in proteins (glycine is the exception) may exist in two forms, designated D- and L- forms (sometimes called right- and left- handed). Chemically and physically they exhibit identical properties. (Except in certain optical properties, which would not seem to have anything to do with their biological use.)

Biologically, however, the difference is enormous; for all proteins produced in living cells contain exclusively the L- or left-handed form. All those produced artificially, however, contain random mixtures of D and L forms. The replacement of a single amino acid in a protein with its corresponding D form completely destroys all biological activity.¹² When all of this is considered, Fox's claim of non-randomness seems indeed questionable.

In view of the problems brought to light by the experiments mentioned, spontaneous generation appears less and less plausible. Consider the following figures, offered by Gerhard Schramm regarding the probability of random formation of one of the simplest forms of life (indeed, some question whether it should even be called a form of life) the tobacco mosaic virus:

The ribonucleic acid (RNA) of the tobacco mosaic virus (TMV) contains 6000 nucleotides. The probability that this special molecule results by random combination of the four nucleotides is $\frac{1}{4}^{6000} = 10^{-2000}$. Since the whole cosmos contains an estimated 10^{80} protons, it is practically impossible to obtain such a ribonucleic acid in 10^9 years, the age of the world, even if the whole world were to consist of a reacting mixture of nucleotides.¹³

Schramm and other spontaneous generation proponents attempt to get out of this dilemma by having some kind of "natural selection" for prebiological molecules. But since such molecules are not selfreplicating, this is a contradiction in terms.¹⁴

Peter Mora is more forthright. Commenting on the common practice of evolutionists in relying upon "infinite escape clauses", such as that mentioned above, he says:

I believe we developed this practice to avoid facing the conclusion that the probability of a selfreplicating state is zero... When for practical purposes the concept of infinite time and matter has to be invoked, that concept of probability is annulled. By such logic we can prove anything, such as that no matter how complex, everything will repeat itself, exactly and immeasurably.¹⁵

Mora properly suggests that the "illusion that the problem can be explained with existing knowledge" is a "dangerous mental attitude", and that we should "even dare to ask whether there is something special in the living, which cannot be treated by physics as we know it, but is still amenable to proof or disproof".¹⁶

Mutations

Even if the problem of spontaneous generation could be overcome, how would one kind of organism become a different, more complex, kind? Professor Lasker admits that: "The only way in which totally new possibilities arise is through change in the genetic material itself.", i.e., by *mutations*. (p. 87.) It is surprising, therefore, to see him concede in the very next sentence: "it has been argued that mutations could have little to do with evolution because those actually observed in the laboratory or in man are nearly always detrimental (or at least neutral)". (p. 87.) (Emphasis added.)

Mutations are central to evolution, yet the observed facts again seem to contradict the doctrine. Lasker dismisses the problem, stating that "no one would claim that mutation alone produces evolution. It would do so chiefly in the presence of other factors, such as natural selection \dots " (p. 87.) But consider the following calculations by Sir Julian Huxley with respect to the presumed evolution of the horse. (And then multiply the result by the number of supposed evolutionary steps between the amoeba and man.):

A proportion of favorable mutations of one in a thousand does not sound much, but is probably generous . . . And a total of a million mutational steps sounds a great deal but is probably an understatement . . . However, let us take these figures as being reasonable estimates. With this proportion, but without any selection, we should clearly have to breed a million strains (a thousand squared) to get one containing two favorable mutations; and so on, up to a thousand to the millionth power to get one containing a million. Of course this could not really happen, but it is a useful way of visualizing the fantastic odds against getting a number of favorable mutations in one strain through pure chance alone. A thousand to the millionth power, when written out, becomes the figure 1 with three million noughts after it; and that would take three large volumes of about 500 pages each, just to print! ... No one would bet on anything so improbable happening. And yet it has happened! It has happened, thanks to the working of natural selection and the properties of living substance which make natural selection inevitable!17

Natural Selection

Logically, then, the next subject is natural selection. Can natural selection really convert Huxley's (and Schramm's) impossibility into an inevitability?

As Lasker states on p. 19, a contrary kind of selection, normalizing selection, was recognized as a fact before Darwin's time: "This process, now called normalizing selection, in fact slows and stabilizes evolution by eliminating offspring which differ greatly from their parents". But he goes on to say:

It was the establishment of progressive evolution, however, and its firm foundation in the numerous facts established by Darwin and his followers, that produced a revolution in biological thought. Progressive evolution is the selection of changed offspring that are better adapted than their parents. (p. 20.)

Curiously, despite those "numerous facts assembled by Darwin and his followers", I find George Gaylord Simpson, who is perhaps the most influential evolutionary paleontologist, admitting of natural selection, in one of this books, "it might be argued that the theory is quite unsubstantiated and has status only as a speculation".¹⁸

Before I am accused of quoting Simpson out of context, let me hasten to add that Simpson's problem is not in proving that selection occurs, but in demonstrating that it has any effect on evolution. Professor Lasker offers the favorite example of evolutionists: the peppered moth (p. 94.). But as L. Harrison Matthews, Fellow of the Royal Society, states in the introduction to a recent edition of Darwin's Origin of Species:

The (peppered moth) experiments beautifully illustrate natural selection—or survival of the fittest —in action, but they do not show evolution in progress, for however populations may alter their content of light, intermediate or dark forms, all the moths remain from beginning to end, *Biston betularia*.¹⁹

Moreover, the peppered moths may not even demonstrate natural selection, which is no longer defined in terms of *survival*, but rather *differential reproduction*. (Glossary, p. 382.) The evidence is equivocal as to whether there has actually been an increase in the proportion of dark-colored moths being reproduced. Paul Ehrilich and Richard Holm report that in some experiments the expected deficiency of light-colored individuals was not found. In another experiment the results were inconsistent; while the offspring of some matings showed a significantly higher proportion of dark moths, the offspring from others did not.²⁰

Furthermore, in a recent article in Scientific American the author notes that "melanic (dark) moths remain common in areas where theoretically the birds should have exterminated them. Some factor may be counterbalancing the melanics' disadvantage, perhaps acting during the larval or pupal stages that make up most of the moths' life history. Complexities of this kind still remain to be investigated".²¹ It thus appears that barring total extinction of the whole species, the peppered moths will remain *polymorphic*.

But as Lasker points out on page 103, the existence of many polymorphisms is difficult to understand in terms

of natural selection. In fact some scientists, including King and Jukes (1969) whom Lasker cites, use polymorphisms and other evidence to argue that evolution must occur through "neutral" variations, which are neither advantageous nor disadvantageous, and thus "invisible" to natural selection. Lasker puts it mildly: "This is at variance with the theory of natural selection". (p. 103.)

Harvard's Stephen Jay Gould, summarizing these viewpoints, is more worried: "But something even more fundamental is threatened, namely Darwinism itself".²² It would seem that the evidence from the peppered moths supports this last viewpoint as much as it supports the view that natural selection is important.

Darwin was well acquainted with the changes in domestic plants and animals achieved by artificial breeding; and he supposed that it was legitimate to extrapolate them:

Slow though the process of selection may be, if feeble man can do so much by his powers of artificial selection, I can see no limit to the amount of change, to the beauty and infinite complexity of co-adaptions between all organic beings, one with another and with their physical conditions of life, which may be effected in the long course of time by nature's power of selection.²³

But over 100 years after Darwin, all experimental evidence indicates that variation is confined within limits.

For instance, the much-publicized experiments on fruit flies, by mutations induced artificially, and artificial selection, resulted in considerable variability. But attempts to push these variations beyond certain limits lead to mass sterility and death. Attempts to hold the variations near their limits led to reversion to the norm. A leading evolutionist, Ernst Mayr of Harvard, regards these results as entirely normal:

Obviously any drastic improvement under selection must seriously deplete the store of genetic variability... The most frequent correlated response of one-sided selection is a drop in general fitness. This plagues virtually every breeding experiment.²⁴

Mayr continues to believe that major changes are possible, but he cites no observed cases, and concedes that "much of this is obviously speculative".²⁵

Not only is there a disconcerting paucity of evidence; it appears also that evolutionists have been unable to come up with a workable definition of natural selection. "Natural" means "not artificial"; that much is clear. "Selection", according to Lasker, means "that there are biological variants favorable in terms of the circumstances under which their selection occurs". (p. 92.) Aside from the fact that this definition assumes its own conclusion, i.e. that selection *does* occur, the problem is in objectively determining which variants are *favorable* in terms of the circumstances of their occurrence.

Another leading evolutionist, G. Ledyard Stebbins, now at the University of California, Davis, has admitted:

Obviously... a final estimate of the importance of selection in evolution must depend largely on determining what... differences are... adaptive ... Unfortunately, however, the determination of the adaptive character of many types of differences between organisms is one of the most difficult problems in biology.²⁶

We can, therefore, do little more than speculate.²⁷ Mayr agrees: "... one can never assert with confidence that a given structure does not have selective significance."²⁸

Simpson regards this as a matter of common knowledge: "The fallibility of personal judgements as to the adaptive value of particular characters, most especially when these occur in animals quite unlike any now living, is notorious."²⁹

In fact, most evolutionists now agree that "fitness" cannot be objectively analyzed. Thus Lasker cautions, "Fitness is defined by the test of survival, not by 'fitness' in some evaluative human sense." (p. 92.) That is to say, those who survive are the fittest because the fittest are the one who survive. This is a tautology: a logical fallacy that explains nothing.³⁰ With this kind of definition it is easy to see how natural selection can simultaneously "explain", in Lasker's example, the evolution of both early and delayed parturition and weaning in mammals. (p. 93.)

The shortcomings of mutations and natural selection as mechanisms of evolution have led to a recent emphasis on random genetic drift coupled with fluctuations in the sizes of populations. (See Lasker, pp. 107-118.) But since genetic drift alone cannot produce anything other than the same kind and level of organism, this emphasis seems unjustified. Those interested may wish to read Mayr's short history of the concept of genetic drift and a review of many untenable applications.³¹ Mayr apparently still believes that there may be a role for genetic drift in evolution. But he appears to have doubts about the role of other random mechanisms:

Random phenomena like recombination . . . and the founder principle introduce a considerable degree of indeterminacy into evolution. Temporarily they may be even stronger than selection in completely isolated and at least initially small populations. How important such populations are for speciation and ultimately for evolution is still rather obscure.³²

Evolutionists are faced with a dilemma here. For the greater the role assigned to random processes, the smaller are the chances that Huxley's impossibility will be converted into an inevitability by natural selection.

I believe that I have raised substantial questions about the "evidence" that any known biological mechanism can produce macroevolutionary changes. It is commonly assumed, however, that despite the shortcomings of the explanatory mechanisms, the fossil record provides clear evidence for macroevolution. So the fossils may now be considered.

Gaps in the Fossil Record

Lasker states: "There is now much fossil evidence for all sorts of intermediate forms that show the nature of man's relationship to other animals, and new finds are being added continuously... Our problem is not in finding missing links as such." (p. 18.) Actually, the problem initially is one of semantics. As Lasker later states: "One cannot properly speak of missing links and known links in evolution because evolution runs in lines rather than chains and it is always possible to refine one's knowledge as more points on the line become known and the unknown segments between them become smaller." (p. 242.) He is saying, in other words, that since evolution occurs on a continuum, with innumerable points, one cannot expect to find all of the intermediate forms in a given line.

Of course, once one presupposes an evolutionary continuum, the distance between presumed intermediate ancestral forms presents, as Lasker states, no "problem".

No one has ever asked that all of the fossils of all of the individuals in an alleged line of descent be produced. But it does seem reasonable to ask that a series be produced, in which each one does not differ very much from its neighbor. Whatever may be presupposed, the fact is that the gaps in the fossil record, between allegedly related forms, are large and systematic.

Simpson has stated: "It is a feature of the known fossil record that most taxa appear abruptly . . . Gaps among species are sporadic and often small. Gaps among known orders, classes and phyla are systematic and almost always large".³³

Another paleontologist, T. Neville George states: "There is no need to apologize any longer for the poverty of the fossil record. In some ways it has become almost unmanageably rich, and discovery is outpacing integration. . . . The fossil record continues to be composed mainly of gaps."³⁴

In a recent article in *Evolution*, a paleontologist at the University of Oklahoma says:

Despite the bright promise that paleontology provides a means of "seeing" evolution, it has presented some nasty difficulties for evolutionists the most notorious of which is the presence of "gaps" in the fossil record. Evolution requires intermediate forms between species and paleontology does not provide them. ³⁵ (Emphasis added.)

So great are the gaps that the presumed ancestral relationships are highly arbitrary. Mayr, who is one of the leading American taxonomists, says:

Most taxa above the family level are sharply delimited. Mollusks, penquins, beetles, and indeed most higher taxa are separated from their nearest relatives by a decided gap, far more so than most genera and families. Nevertheless it remains true that the higher categories in which we place these taxa are ill-defined . . . (No) yardstick has yet been found for the non-arbitrary ranking of taxa . . . It is in the arbitrariness of definition that all higher categories differ from the species category.³⁶

A reviewer of Mayr's book says that "nearly all biologists must share the view that the species is the only taxonomic category that has, at least in more favorable examples, a completely objective existence. Higher categories are all more or less a matter of opinion."³⁷

In this regard, Lasker's discussion of *parallelism* and *convergence* (p. 200.), and *homology* and *analogy* (p. 205.) is instructive. He give numerous examples of quite similar forms of life which are nevertheless shown

by other evidence to be unrelated in ancestry. If this is true (and all evolutionists agree that it is), it casts doubt upon any attempt to draw conclusions about ancestry from structural or other similarities in the fossil record. But macroevolution is built upon these similarities; so anything which casts doubt upon group similarities must cast doubt upon the entire doctrine.

There is one group of fossils, however, that is repeatedly referred to whenever the question of gaps is raised: the horse series. It appears in virtually every book on the subject of evolution. Yet, according to Simpson, "this usual example has been badly misrepresented."³⁰ Garrett Hardin points out that an early exhibit at the American Museum of Natural History purporting to show straight-line evolution in horses was widely photographed before its erroneous nature was pointed out; and these misleading photographs still appear in some textbooks.³⁹

Lasker shows the fossils correctly in a diagram on page 207. Note that despite the "abundant fossil material" only *three* fossils are thought to be ancestral to *Equus*, the modern horse: *Eohippus*, *Miohippus*, and *Merychippus*.

Simpson, upon whom Lasker relies for his diagram, is probably the greatest authority on the horse series. Consider Simpson's own statements:

Every feature of horse evolution tells a comparably complex story if this is examined in detail and in all the divergent lines of the horse family. The feet . . . hardly evolved at all in the Eocene, then evolved rapidly to a basic three toed Oligocene type which remained nearly static in some later lines . . . and in one line only finally evolved rapidly in one phase to a one-toed type. This again, did not really continue the usual trend among the threetoed types but was a new evolutionary direction for them. In the particular lineage from Eohippus to Equus, general foot mechanics became first more complex, then simpler. The number of toes did not change at even pace from four (in the forefoot) to one, but changed in two spurts, first from four to three, then much later from three to one, each rapid transition followed by slower mechanical adjustment to the new sort of foot and to changes in the weight of the animals.

The horses even provide us with exceptions to the rule that animals tend to increase in size in their evolution. In fact, the known late Eocene horses average rather smaller that *Eohippus* in the Eocene. Then still later, in the Miocene and Pliocene, there were at least three different branches of the horse family characterized by decrease in size . . . while at the same time others were, according to the "rule" increasing in size. At that time, too, others were fluctuating around a mean size without notable change and still others developed different species of decidedly different sizes—as, indeed, is the case in Equus today.⁴⁰ (Emphasis added.)

Given this kind of evidence, one need only add Simpson's own statement, "You can establish any 'rule' you like if you start with the rule and then interpret the evidence accordingly."⁴¹

As Lasker's (Simpson's) diagram shows, to follow even the three presumed stages requires a geographical leap from the Old World to South America. Moreover, the placement of *Eohippus* (more broadly called *Hyracotherium*), as the first link in the series, is highly arbitrary. In an earlier article, Simpson said:

Matthew has shown and insisted that Hyracotherium (including Eohippus) is so primitive⁴² that it is not much more definitely equid than taprid, rhinocerotid, et cetera, but it is customary to place it at the root of the equid group.⁴³ (Emphasis added.)

The rest of the fossil record offers little comfort to macroevolutionists. Simpson says that there is a considerable gap between *Eohippus* (*Hyracotherium*) and its supposed ancestral order. He then goes on to say:

This is true of all of the thirty-two orders of mammals... The earliest and most primitive members of every order already have the basic ordinal characters, and in no case is an approximately continuous sequence from one order to another known. In most cases the break is so sharp and the gap so large that the origin of the order is speculative and much disputed.⁴⁴ (Emphasis added.)

This regular absence of transitional forms is not confined to mammals, but is an *almost universal phenomenon*, as has long been noted by paleontologists. It is true of almost all orders of all classes of animals, both vertebrate and invertebrate. A *fortiori*, it is also true of the classes, and of the major animal phyla, and it is apparently true also of analogous categories of plants.⁴⁵ (Emphasis added.)

There is... a tendency toward systematic deficiency in the record of the history of life. It is thus possible to claim that such transitions are not recorded because they did not exist, that the changes were not by transition but by sudden leaps in evolution.⁴⁶ (Emphasis added.)

A few evolutionists have accepted the gaps in the fossil record at face value, and assert that the major categories must have arisen by "macrogenesis", or systemic mutation, rather than the millions of small steps ("phyletic gradualism") proposed by virtually all others.⁴⁷ The most prominent of these was Richard Goldschmidt, of the University of California at Berkeley, who used the term "hopeful monster". Goldschmidt's book contains a valuable critique of other evolutionary ideas.⁴⁸ Speaking of Goldschmidt's concept, however, Mayr says:

The occurrence of genetic monstrosities by mutation, for instance the homeotic mutants in Drosophila (fruit flies) is well substantiated, but they are such evident freaks that these monsters can be designated only as "hopeless". They are so utterly unbalanced that they would not have the slightest chance of escaping elimination through stabilizing selection. . . . to believe that such a drastic mutation would produce a viable new type, capable of occupying a new adaptive zone, is equivalent to believing in miracles.⁴⁹

Simpson also attacks this view, which depends upon multiple, simultaneous mutations:

The chances of multiple, simultaneous mutations seem to be even smaller, indeed negligible. Postulation of a mutation rate of .00001 and of each mutation's doubling the chances of another in the same nucleus would correspond with the most favorable circumstances warranted by laboratory evidence. Under these postulates, the probability of five mutations in the same nucleus would be approximately 10⁻²². With an average effective breeding population of 100 million individuals and an average length of generation of one day, again extremely favorable postulates, such an event would be expected only once in about 274 billion years, or about a hundred times the probable age of the earth. Obviously, unless there is an unknown factor tremendously increasing the chance of simultaneous mutations, such a process has played no part whatever in evolution.⁵⁰ (Emphasis added.)

Stratigraphic Position

One argument used by macroevolutionists to avoid confronting the gaps in the fossil record is the assertion that the layers of the geologic column at least demonstrate a broad progression from simple to complex forms of life. But the fossil record is full of inconsistencies on this issue. Simpson may be quoted once again:

Most (phyla) . . . begin in the Cambrian . . . (until recently thought to be the strata in which life first evolved). There is little logical order in time of appearance. The Arthropoda appear in the record as early as do undoubted Protozoa, although by general consensus the Protozoa are the most primitive phylum and the Arthropoda the most "advanced"-that is, structurally the most complicatedamong the nonchordates (or invertebrates, as all the phyla other than the Chordata are often called). Corals and bryozoans do not appear until the Ordovician, although they are less highly organized than many groups that do occur in the early Cambrian ... (The phylum Chordata) is to be considered in some sense the most progressive of all the phyla, and it is indeed the last to appear in the record, although not much antedated by the lowly bryozoans.51

The fossil record is so equivocal, in fact, that three prominent paleontologists recently concluded that stratigraphic position should not even be considered in making the initial determination of phylogeny (line of supposed evolutionary descent):

It is our opinion that the spectrum of primitivederived character states, or polarity, must be worked out (and, in fact usually is worked out), at least initially, on the basis of morphologic criteria. (Rather than time-sequence criteria.) The reason for this conclusion is that the sequence in the rocks may not offer a true picture of polarity. *Primitiveness and apparent ancientness are not necessarily correlated.*⁵² (Emphasis added.)

The chronocline (time sequence) concept implies, wrongly, we believe, that the temporal sequence is, in itself, meaningful in evaluating relationship.⁵³

These same writers point out that one of the problems with reliance upon stratigraphic data is that the question of time sequence is often decided on the basis of the fossils; and thus the whole argument is circular: "The prime difficulty with the use of presumed ancestral-descendant sequences to express phylogeny is that biostratigraphic data are often used in conjunction with morphology in the initial evaluation of relationships, which leads to obvious circularity".⁵⁴

But at least as damaging to the idea that fossil sequences such as the horse demonstrate progressive evolution is the assertion that:

All organisms are bound to be relatively primitive in some respects, and relatively derived in others (the concept of mosaic evolution: DeBeer, 1954). The probability of finding a fossil taxon that is primitive in all respects to another, younger taxon, is small.⁵⁵

In fact, Schaeffer and his colleagues say that if one were to consider only the relative "primitiveness" of the horse fossils, the sequence could just as easily be Equus-Hyracotherium as Hyracotherium-Equus.⁵⁶

Supposed Ancestors of Mankind

Finally, consider the fossil ancestors of mankind. Lasker follows current thinking, and places the supposed evolutionary ancestors of man into three categories: (1) Australopithecus, (2) Homo habilis, and (3) Homo erectus. The latter category includes the famous "Peking man" and "Java man" fossils, as well as a number of others. (p. 263.) Not included at all, of course, is Neanderthal Man, whom virtually all evolutionists now include in Homo sapiens. (p. 292.)

At just the time that Lasker's book was being written in 1973, however, Richard Leakey and others discovered new fossils in Africa that have virtually eliminated the first category, *Australopithecus*, as an ancestor of man. In Leakey's own words, his Skull 1470 "leaves in ruins the notion that all early fossils can be arranged in an orderly sequence of evolutionary change.⁵⁷ Leakey now views *Australopithecus* as "apelike", and "probably a relative rather than a forebear of mankind".⁵⁸

The reason for Leakey's dramatic statements is that Skull 1470, except for its small size, about 800 cc., is apparently more "modern" in form than that of any of the heretofore supposed ancestors of man. For instance, the sloping forehead and prominent brow ridges of *Homo erectus* fossils are not present in Skull 1470.⁵⁹ Yet Skull 1470 is said to be about 1.8 million years old; a contemporary of *Australopithecus*, and older than any *Homo erectus* fossil.⁶⁰

Lasker discusses Skull 1470 on pages 264-267, basing his remarks on Leakey's early reports, and states that it is possible to conclude that 1470 is "true man". "In any case, those who believe there has been a long history of true men have a new fossil candidate to honor . . ." (p. 265.) Ralph L. Holloway, Jr., an anthropologist at Columbia University, has concluded that 1470 "was capable of some kind of human language" because the skull has "a bulge known as Broca's area that is lacking in apes but present in man and is considered a speech center".⁶¹

Despite its modern configuration, Leakey views Skull 1470 as an ancestral link to man because of its small brain size. But Stephen Molnar, an associate editor of the American Journal of Physical Anthropology, after commenting on the "usefulness" of comparing cranial capacities in fossils, makes the following rather startling comments:

In modern populations . . . there is such a wide range in variation that the lower end of the range is well below the capacity for certain fossil hominids, yet there is no evidence that these individuals are any less intelligent than persons with larger cranial vaults. It is unlikely that the differences in modern population brain size have any relevance to variation in mental ability-a factor that renders comparisons of cranial capacity between modern groups a futile and meaningless exercise. As Von Bonin (1963), a foremost neuroanatomist, once stated, the correlation between brain size and mental capacity is insignificant in modern man. A good example can be seen in the case of females who have, on the average, 10 percent smaller cranial capacities than males; no one has dared suggest that this indicates lower mental capacity. Also, many famous men in history have, upon their deaths, had their brain weighed and measured; and the ranges encompassed the total range of H. Sapiens, from Anatole France (cranial capacity of approximately 1100 cc.) to Oliver Cromwell and Lord Bryon (cranial capacity of approximately 2200 cc.)... Variation of plus or minus 400 cc. about the mean is seen in most European populations. These individuals with larger or smaller cranial capacities are normally functioning and intellectually competent individuals; in fact, there are many persons with 700 to 800 cubic centimeters.⁶² (Emphasis added.)

If brain size means nothing for modern populations, is it not fair to suggest that it means nothing for fossil populations also? For all practical purposes, Leakey's Skull 1470 eliminates any imagined evolutionary ancestry of mankind.

The broader question aside, other anthropologists are coming to agree with Leakey's elimination of Australopithecus. David Pilbeam and Stephen Jay Gould, two prominent experts, using allometric techniques.⁶³ and Charles F. Oxnard, anatomist and anthropologist at the University of Chicago, using a computerized multivariate analysis technique.⁶⁴ have concluded that Australopithecus was not ancestral to man. So Australopithecus is rapidly on its way to being disinherited as man's ancestor.

The Homo habilis category was first proposed by the late Louis S. B. Leakey (Richard Leakey's father) in 1961. As Lasker indicates, the separate status of that category has been in doubt from the beginning:

Some students of the problem believe that the degree of variation in size from one group of teeth to another (or in a particular tooth from one dimension to another) seen between Australopithecus and Homo habilis occurs only between species. Others (see for instance Brace et al., 1971) call attention to the fact that the size of teeth can vary greatly within a species. (p. 264.)

Recently Brace and Wolpoff, both anthropologists at the University of Michigan, made the following comments in response to the article by David Pilbeam and Stephen Jay Gould, cited in reference 63 above:

The very existence of the taxon *Homo habilis* owes more to an extraordinarily wide press... than to actual data ... Pilbeam now agrees with us in believing that the dental variation in the *Australopithecines* is so great that jaws and teeth alone are of no use in distinguishing taxa, a conclusion recently emphasized by the discovery of a Rudolph cranium with a 500 cc. brain and very small teeth (ER 1813).

The sample which Pilbeam is now willing to attribute to *Homo habilis* consists of only four specimens...There is no specimen with both known brain size and known tooth size...There is not a single Lower Pleistocene cranium with a known cranial capacity that is associated with any postcranial material useful in determining body size...This is true also of *Homo erectus*...The varied use of the taxon has created nothing but confusion. Hence we reiterate the previous suggestion that "*Homo habilis* be formally sunk".⁶⁵

This leaves only *Homo erectus* to fill the gap between mankind and non-human beings. But there is no evidence for macroevolution here, for consider Lasker's statement: "Only one or two of these fossils fall outside the range of a small series of American White subjects." (p. 284.)

Given the range of variation among world populations, can it be said that *Homo erectus* has any meaningful evolutionary significance? S. M. Garn (upon whom Lasker relies) "considers the increased size of the *Homo sapiens* cranium to be significant" (p. 284), but as has been seen, such differences appear meaningless, particularly in light of the fact pointed out by Brace and Wolpoff, above, that we are ignorant of the *body* size of all *Homo erectus* skulls.

If brain size is significant, how does one account for Neanderthal Man? In explaining the large cranial capacity of some Neanderthal specimens (1.75 liter vs. 1.30 to 1.45 liter for the average modern European) Lasker finds it necessary to caution that "it is unwarranted to assume that Neanderthal man was in general significantly more brainy than subsequent man." (p. 291.)

Conclusion

The arguments for macroevolution fail at every significant level when confronted by the facts. The origin of life, mutations, natural selection, and the fossil record all fail to support the doctrine.

Why then do evolutionists continue to assert that evolution is a *fact*? The answer is that evolution has been *defined* by some authors in such a way that it is a fact. Lasker gives a common definition of evolution: "A continuous change over generations in frequencies of genetically determined characteristics." (p. 376.) Thus he can say that any change in gene frequency, for example "between father and son" is evolution.

Lasker says that evolution "is a *lawful* change in the genetic composition of the members of a population."

(p. 16. Emphasis added.) In so doing, he and other evolutionists include the laws of heredity within the definiton of evolution. But this can only lead to confusion, for as Theodosius Dobzhansky says:

All living things grow and reproduce their like ... This process of self production, of like begetting like, is the essence of heredity. *Heredity is ... the antithesis of evolution ...* Evolution is a process which makes the descendants unlike their ancestors.⁸⁶ (Emphasis added.)

This kind of non-definition should be discarded. The "neo-Darwinian synthesis" notwithstanding, the science of genetics has made it unnecessary to account for variability in evolutionary terms. Consider the following statement by Gould:

... there is far more genetic variability within natural populations than the classical school could possibly allow—and even more than many field geneticists ever dared to imagine. In several organisms, more than half the tested genes exist in alternate alleles within a population, while individuals may be variable in up to 25 percent of their genes (although 5 to 15 percent per individual is the usual range).⁶⁷

Thus what has been taken to be evidence of macroevolutionary changes may turn out to be simply normal genetic variation. And, as I have tried to show, there is no evidence, from either the fossil record, observations of nature, or biological experiment, that these variations can extend across natural genetic limitations and produce macroevolutionary changes.

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General Bibliography

I am indebted to the following sources for many of the ideas expressed herein. They are recommended to those who wish to pursue the subject further:

Gish, Duane T. 1972. Speculations and experiments related to theories on the origin of life: a critique. ICR Technical Monograph No. 1. Creation-Life Publishers, San Diego, California.

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Macbeth, Norman 1971. Darwin retried. Gambit, Inc., Boston. (1973. Delta Book 440-01732-245. Dell Publishing Co., Inc., New York)

Morris, Henry M. Editor. 1974. Scientific Creationism. Creation-Life Publishers, San Diego, California.

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A second edition of Lasker's book has just been published (in 1976). There are some differences in the numbering of the pages. Following is a convenient table, relating the numbers in the first edition, as given in the article, to those in the second edition:

First	Second	First	Second
16	12	118	119
19	17	200	196
20	17	205	199
24	20	207	201
26	21 and 24	242	235
28	22	263	262-267
32	23	264	255-258
33	24	265	259
87	89	267	259
92	94	284	276
93	94	291	281-282
94	95	292	283
103	105	376	368
107	108	382	374

It may be noticed also that in the second edition, the statement referred to on p. 103 of the first edition is omitted. Instead, on p. 105 of the second edition, is this statement: "Some students of molecular genetics believe that non-Darwinian evolution of this kind is an important aspect of evolutionary change".

In regard to reference 61, in the second edition it is mentioned, apparently without a citation, that Holloway estimated the cranial capacity of Skull 1470 at 0.770 to 0.775 liter.

²Hulett, J. R. 1969. Limitations on prebiologic synthesis, Journal of Theoretical Biology 24(1):56-72. (See also Hull, D. E. 1960. Thermodynamics and kinetics of spontaneous generation, Nature 186 (4726):693-695.)

³Miller, S. L., and L. E. Orgel 1973. The origins of life on the Earth. Prentice-Hall, Englewood Cliffs, New Jersy, p. 126.

Brinkman, R. T. 1969. Dissociation of water vapor and evolution of oxygen in the terrestrial atmosphere, Journal of Geophysical Research 74(23):5335-5368.

⁵Abelson, P. H. 1966. Chemical events on the primitive Earth, Proceedings of the National Academy of Sciences 55(6):1365-1372.

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Miller, S. L., and L. E. Orgel 1973. The origins of life on the Earth. Prentice-Hall, Englewood Cliffs, New Jersey, p. 145.

^eMora, Peter T. 1965. Random polycondensation of sugars (in) The origins of prebiological systems and their molecular matrices. (Fox, Sidney W., Editor) Academic Press, New York, p. 287.

¹⁰Schramm, Gerhard. Synthesis of nucleosides and polynucleotides, Ibid., p. 307.

- ¹¹Miller, S. L., and L. E. Orgel 1973. The origins of life on the Earth.
- Prentice-Hall, Englewood Cliffs, New Jersey. Footnote on p. 144. ¹²Bernal, J. D. 1967. The origin of life. World Publishing Co., Cleveland, p. 144. (See also Oparin, A. I. 1961. Life, its nature, origin, and development. Academic Press, New York, pp. 59 and 60.)

¹³Schramm, reference 10, p. 300.

^{1*}See Dobzhansky, Theodosius 1967. The biology of ultimate con-cern. New American Library, New York, p. 48. Dobzhansky makes the same point, although somewhat more tolerantly, in the discussion following Schramm's article, reference 10.

¹⁵Mora, Peter T. 1965. The folly of probability (in) The origins of prebiological systems (Fox, Sidney Editor) Academic Press, New York, p. 45.

"Huxley, Julian 1953. Evolution in action. Harper and Brothers Co., New York, p. 41.

¹⁸Simpson, George Gaylord 1953. The major features of evolution. Columbia University Press, New York, pp. 118 and 119.

¹⁹Matthews, L. Harrison 1971. Introduction to Darwin's origin of species. J. M. Dent and Sons, Ltd., London, p. xi.

²⁰Ehrlich, Paul W., and Richard W. Holm 1963. The process of evolution. McGraw-Hill, New York, p. 130. ²¹Bishop, J. A., and Laurence M. Cook 1975. Moths, melanism, and

clean air, Scientific American 232(1):98.

²²Gould, Stephen Jay 1975. A threat to Darwinism, Natural History, December, p. 9.

²³Darwin, Charles 1859. The origin of species. Facsimile edition printed by Harvard University Press, 1966, p. 9.

²⁴Mayr, Ernst 1963. Animal species and evolution. Harvard University Press, p. 290.

²⁵*Ibid.*, pp. 586, 613 and 615.

²⁸Stebbins, G. Ledyard 1950. Variation and evolution in plants. Columbia University Press, p. 118.

27 Ibid., p. 506.

²⁸Mayr, Ernst, reference 24, p. 190.

- ²⁹Simpson, G. G. 1953. The major features of evolution. Columbia University Press (Paperback by Simon and Schuster, 1967.), p. 278.
- ³⁰"(Natural selection) has been criticized often as being a tautology, (citing Waddington, C. H. 1957. The strategy of the genes. Allen and Unwin, London, p. 64) . . . so long as fitness is defined in terms of survival and selection is measured in terms of gene frequencies.' Harris, C. Leon 1975. An axiomatic interpretation of the neo-Darwinian theory of evolution, Perspectives in Biology and Medicine, Winter, p. 182.
- "Mayr, Ernst, reference 24, pp. 204-214.
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- ³⁴George, T. Neville 1960. Fossils in evolutionary perspective, Science Progress 48(189):1.3.
- ³⁵Kitts, David B. 1974. Paleontology and evolutionary theory, Evolution 28(3):467
- ³⁶Mayr, Ernst 1969. Principles of systematic zoology. McGraw-Hill, New York, pp. 91 and 92.
- ³⁷Richards, G. W. 1970. A guide to the practice of modern taxonomy, Science 167(3924):1477-1478
- ³⁸Simpson, G. G. 1949. The meaning of evolution. Yale University Press, p. 130.
- ³⁹Hardin, Garrett 1961. Nature and man's fate. Mentor Books, p. 225 and 226
- ⁴⁰Simpson, G. G., reference 38, pp. 133-136.
- ⁴¹Ibid., p. 137.
- ⁴²For reasons given in the text accompanying reference 56, I quibble with Simpson's use of the word "primitive" here. In the context, I think that he means simply "distant".
- ⁴³Simpson, G. G. 1945. The principles of classification and a classification of mammals, Bulletin of the American Museum of Natural History 83, p. 254.
- "Simpson, G. G. 1944. Tempo and mode in evolution. Columbia University Press, New York, p. 105.
- 45*Ibid.*, p. 107.
- ⁴⁶Simpson, G. G., reference 38, p. 231.
- ⁴⁷See, e.g., Stanley, Steven M. 1975. A theory of evolution above the species level, Proceedings of the National Academy of Sciences 72 (2):646-650
- "Goldschmidt, Richard B 1940. The material basis of evolution. Yale University Press.
- ⁴⁹Mayr, Ernst, reference 32, p. 253.
- ⁵⁰Simpson, G. G., reference 29, p. 96.
- ⁵¹Simpson, G. G., reference 38, p. 31.
- ^{s2}Schaeffer, B., M. K. Hecht, and N. Eldredge 1972. Phylogeny and paleontology. Chapter 2 in Evolutionary Biology, vol. 6, edited by T. Dobzhansky, M. K. Hecht, and W. C. Steere. Appleton-Century Crofts, New York, p. 33.
- ⁵³*Ibid*., p. 35.
- 5*Ibid., p. 39. The same point is made by Kitts, David B. 1974. Paleontology and evolutionary theory, Evolution 28(3):466.
- ^{ss}Ibid., pp. 39 and 40.
- ⁵⁰Ibid., p. 37.
- ⁵⁷Leakey, Richard 1973. Skull 1470-new clue to earliest man? National Geographic 143(6):819.
- ^{5*}Ibid., comments accompanying the illustration on page 829.
- ⁵⁹For a more complete description of Leakey's finds see Leakey, Richard 1974. Further evidence of Lower Pleistocene hominids from East Rudolf, North Kenya, 1973, Nature 248(5450):653-656; also Leakey, Richard 1973. Evidence for an advanced Plio-Pleistocene hominid from East Rudolf, Kenya, Nature 242(5398):447-450. (See

¹⁶Ibid., pp. 50 and 51.

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also items by Day, M. H., and Richard Leakey 1973. American Journal of Physical Anthropology 39, p. 341; and 1974. American Journal of Physical Anthropology 41, p. 367. ⁶⁹The age of this skull is somewhat controversial. Leakey's figure is

- ⁹The age of this skull is somewhat controversial. Leakey's figure is 2.6 million years, based upon potassium-argon dating of a layer of volcanic tuff. The figures $1.60 \pm .05$ MY and $1.82 \pm .04$ MY were obtained by the same method at the University of California, Berkely. (See also Curtis, G. H., T. Cerling Drake, and Hampel. 1975. Age of KBS tuff in Koobi Fora formation, E. Rudolf, Kenya, *Nature* 258 (5534):395-398.)
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- **Molnar, Stephen 1975. Races, types, and ethnic groups—the problem of human variation. Prentice-Hall, Inc., Englewood Cliffs, New Jersey, pp. 56 and 57. (See also Robinson, J. T. 1967. The origins and adaptive radiation of Australopithecines (in) Human Evolution (Korn, N., and F. Thompson, Editors) Holt, Rinehart, and Winston, New York, p. 296. They indicate that 800 cc. brains have been re-

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Apparently the exact figure for Anatole France is 1017 gm. (See Cobb, Stanley 1960. Brain and personality, American Journal of Psychiatry 116(10):938.)

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- ⁶⁹Dobzhansky, Theodosius 1958 (in) Julian S. Huxley, et al. A book that shook the world: anniversary essays on Charles Darwin's origin of species. University of Pittsburg Press, p. 16.
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PANORAMA OF SCIENCE

Ancient Astronomy in Western Hemisphere

For a long time scholars have known that some natives of the Western Hemisphere, such as the Mayas, had a considerable knowledge of astronomy before the time of Columbus. Now there is evidence to show that such knowledge was quite widespread. Also, such knowledge seems to have led to the building of structures for astronomical purposes. There is evidence of such structures all the way from near what are now St. Louis and Wichita to Peru.¹

Some of these monuments are in the form of towers, in Yucatan for instance, and at the Casa Grande National Monument, near Coolidge, Arizona. Others seem to have been something like Stonehenge, and similar circular structures, which Thom and others have recently maintained had astronomical uses.² Some of this latter kind may have been made of wood; naturally a wooden one would leave few remains after all these years.

So there seem to be ancient astronomical structures, and astronomical knowledge, scattered all the way from the Middle East to the Americas. To what is such a wide distribution to be ascribed? The following explanation is suggested.

Some time between about 2400 B. C. and 2000 B. C., according to a straightforward reading of Genesis, there was a concentrated, highly advanced civilization in what was later called Babylonia. These people began to build some kind of huge center at Babel. Maybe it was intended as a sort of rallying-point for all people, something like what Delphi and Olympia were later to the Greeks.

In some way, these plans were contrary to God's will. It may be that the fault lay, not in any use intended for the structures, but in the attitude of the builders: that attitude of overweening pride which the Greeks called *hubris*. The question, whether *hubris* may be found in the world today, may be left to the reader's consideration.

The tower of Babel may not necessarily have been a tower like a lighthouse. Also, the Hebrew may not say exactly that the top was to reach to the heavens; but it had something to do with the heavens.

It is suggested, then, that the tower was some kind of astronomical structure. There are several possibilities as to its intended use. Some have suggested that the present system of seasons began only after the Flood. If so, maybe it was intended to make observations to determine the seasons for sowing, etc., and more generally for purposes of a calendar. It may be noteworthy, in this connection, that the calendar of the Hebrews, although it had lunar months, was not tied in to the behavior of any star, e.g., to determine the beginning of a year, but rather to climate and growth.³

Again, it may be that people then feared another Flood, and that they thought that astonomical observations of some kind might provide a warning against a Flood. This would be a bit analogous to the view, common in the last century but apparently abandoned nowadays, that the (alleged) ice age was to be explained by astronomical causes.⁴

There is another possibility. It seems to be an old idea that the motions in the heavens are tied in with somehow, and even control, happenings on Earth. Men as intelligent as Aristotle⁵ and St. Thomas Aquinas⁶ seem to have believed that there might be something in this notion. Is it possible, then, that the builders of Babel proposed, in their observatory, to follow and imitate the motions of the stars and planets, maybe by moving markers around, or by holding processions? In that way, it could be, they proposed to participate in the activity of the heavens, with beneficial results to themselves.

The Tower of Babel, then, it is suggested, may, in part, have been something like Stonehenge. It may well have incorporated one or more towers, in the narrower sense of the word.

There may have been a reason for a huge size. Thom and others have suggested that observatories such as Stonehenge often depended on such points as the tops of distant hills as sights for the observations. In the plains of Babylonia, there may have been few suitable hills