Introduction

"Origin of man now proved.—Metaphysics must flourish.—he who understands baboon would do more toward Metaphysics than Locke."

—Darwin, Notebook M, August 16, 1838

It happened after a high-table dinner at Saint Catharine's College in Cambridge, over port and madeira in the Senior Combination Room. Sydney Smith, the world's greatest Darwin scholar, was telling about having spent some time in the field with David Lack, and watching that famous ornithologist rappel down a cliff with a telescope strapped to his back. He remarked that "if you are a real ornithologist you don't carry binoculars, you carry a telescope." I turned to him and said, "Sydney, if you are a *real* ornithologist, you carry a shot gun!"

In the same spirit might I suggest that there are not many *real* evolutionists in this world. Merely admitting that evolution has in fact occurred does not make one an evolutionist, any more than believing in birds makes one an ornithologist. To be a real ornithologist, or a real evolutionist, requires a much deeper commitment, a sense of priority, and an enthusiasm that far transcends the ordinary professional loyalties.

Even a real ornithologist, of course, does not always go armed with a shot-gun, and not everybody who carries a shot-gun is an ornithologist, so we need not expect an evolutionist to bear some infallible emblem of his identity. But consider how one might answer some diagnostic questions. When, for example, did the dinosaurs become extinct? A real evolutionary biologist will tell you that they never did become extinct. They are still flying around in the trees, to the delight of mere bird-watchers and real ornithologists alike. Which came first, the chicken or the egg? The egg, of course. Ova and sperm evolved a billion years or so before chickens did. When does human life begin? Never, for it is part of an unbroken series of generations that goes back to Darwin's warm little pond. What does evolution teach us about human nature? It tells us that human nature is a superstition.

To dismiss such answers as hyperbole or persiflage would completely miss the point. Our evolutionist does not abandon his commitments or compromise his fundamental principles simply because his interlocutors might have expected a different answer! The questions are loaded. They presuppose, if not the falsehood, at least the irrelevancy, of a thoroughly evolutionary point of view. A real evolutionist rather starts out with the evolutionary question, and insists upon providing the evolutionary answer.

One may hesitate to use the term 'anti-evolutionist' for someone who simply is not particularly interested in evolution, or whose enthusiasm for it is merely lukewarm. And yet what passes for indifference may actually be antipathy, and sometimes the neglect that seems benign may be downright malevolent. If it is reasonable to call somebody who believes that the world was created in six days an anti-evolutionist, it is hardly unreasonable to apply the same term to somebody who wants others to believe that it was so created—whatever he himself may believe.

All sorts of persons maintain that teaching about evolution, especially evolution by natural selection, is apt to corrupt the morals of the youth. Often enough their motives are political rather than religious. The subject can be ill-taught, and hence removed from public attention, merely by not explaining how evolution makes life intelligible. And getting it out of the curriculum in the universities these days is largely a matter of hiring yet another molecular biologist. Anti-evolutionism is at least partly responsible for the failure of our educational system to train a new generation of systematists at a time when we face a "crisis" in biotic diversity.

Because modern systematics emphasizes the historical aspect of biology, anti-evolutionists have been particularly assiduous in their efforts to suppress it. They may seek to trivialize it by depicting systematics as a sort of mindless cataloging. Or they claim that although phylogenetics has a certain limited interest as an intellectual curiosity, and perhaps was worth attending to in the nineteenth century, what happened in the Paleozoic is purely a matter of speculation. We have no "direct" knowledge of the past, and since we cannot experiment upon it, paleontology is no real exception.

When the history of systematics is depicted from an anti-evolutionary point of view, every effort is made to give the impression that nothing really happened to it when Darwin came along. If Darwin still used the Linnaean hierarchy, and if a few early nineteenth-century authors drew branching diagrams, it is deemed justified to write off the differences as trivial. The notion that no such changes have occurred, even to the present day, has not been a thesis to be defended upon empirical grounds. Rather it is an assumption that is pre-supposed dogmatically, and one that derives from the anti-evolutionary attitudes that ought to have been challenged and rejected. It is about time for serious students of the past to repudiate such Tory history.

The Swedish literary historian Ellegård (reprinted 1990) surveyed the British periodical press of Darwin's day, and documented a whole range of enthusiasm or the lack of it for his views, ranging all the way from biblical literalism to accepting not just evolution, but natural selection as well. Selectionists, especially enthusiastic selectionists like Fritz Müller and Alfred Russel Wallace, represented but a small minority, and real understanding was far less widespread than it might have been. Things at present remain far from satisfactory.

We need hardly dwell upon details that have become so widely known and are so readily accessible in standard histories of evolutionary biology (e.g., Mayr, 1982a, 1991b; Bowler, 1984; Becquemont, 1992). It is notorious that although the general concept of evolution became widely accepted within the decade that followed publication of *The Origin of Species,* natural selection did not. Natural selection was crucial in providing a plausible mechanism, but its very success in that respect helped pave the way for various alternatives. Shortly after Darwin's death in 1882, a reaction set in, not so much against evolution as such, but against natural selection. This situation was exacerbated in the early part of the twentieth century by developments in genetics, which seemed out of line with natural selection. Or so the story goes, though one always wonders whether a reason is more than just an excuse. As what at least appears to have been a consequence of such difficulties, there was a long period during which "Lamarckian" theories, orthogenesis, saltationism, and other alternatives to natural selection were widely advocated.

The emergence of theoretical population genetics, thanks especially to the brilliant work of Chetverikov, Haldane, Fisher, and Wright in the twenties and thirties, set the stage for the emergence of the so-called Evolutionary Synthesis. At any rate it helped. The apparent conflict between genetics and natural selection was seen to be illusory, and there followed a unification of various separate disciplines into what came to be called the Synthetic Theory. The work of Dobzhansky was particularly important because he was a geneticist with a sound grasp of systematics and geographical variation. Mayr (1980) emphasizes the importance of systematists, and his work on animal speciation was one of the most important parts of the Synthesis. Stebbins did much the same for plants, and Simpson gets credit for important paleontological contributions. Other major figures were Schmalhausen, Rensch, and Julian Huxley. In addition to these "architects" as they are called, many others were involved who were far more than just hod-carriers.

I have often entertained, and even publicly expressed, serious doubts as to whether the Synthetic Theory was even a theory, let alone a synthesis. What actually seems to have happened was that the alternatives to natural selection became less and less tenable, and finally lost all credibility, so that the only alternative was natural selection, and that meant a somewhat modernized version of Darwin's original theory. But the sort of unification that is suggested by the term 'synthesis' implies that there were stronger links between the parts than the evidence really seems to justify. This lack of unification has lately been noted by Futuyma (1988) and Mayr (1992a).

The body of knowledge in question did have enough conceptual unity and diversity of content that calling it the Synthetic Theory is, if somewhat hyperbolic, hardly a misnomer. Even so, the pedagogical tradition that has arisen with respect to its historiography seriously distorts matters. The student is told that Darwin's theory was defective because he did not understand genetics, and that genetics came to the rescue. This is hardly what happened. Although genetics was important, it has not played the particular role that has generally been attributed to it. Insofar as the species is concerned, the modern definition that emerged from the Synthesis was often discussed in a genetical context, but the basic idea was that of a reproductive community. Conceptualizing them as genetical populations between which gene flow does not take place may have helped keep the discussion within the mainstream of the discipline, but that does not mean that the Synthesis produced a genetical theory of speciation. Indeed, as Coyne (1992) has emphasized, we still lack anything that really deserves to be called a genetical theory of speciation. No doubt we would be better off if we had one, though how much better off is hard to say.

Darwin was quite successful in creating what remains to this very day our basic evolutionary theory without any knowledge of Mendel's laws or the principles of modern population genetics. Even today, if one wants to understand all sorts of phenomena, one need only ask who out-reproduces whom and when, and that is precisely what Darwin did. The supposed problems with Darwin's theory, such as "blending" inheritance and the inheritance of acquired characteristics, were the consequence of misinformation, not lack of information, about genetics. The problems at hand were soluble without any genetical theory, even though a good one might have helped.

The objections to Darwinism that arose soon after the rediscovery of Mendel's laws were likewise due to misunderstandings on the part of geneticists themselves about genetics, and not due to something having been wrong with natural selection. We must not err in the opposite direction and say that genetics did not help, or that it did nothing more than repudiate its own errors. Genetics made lots of problems easier to solve, and the solutions themselves more compelling. And it provided some very good materials that had not been available before. For instance, if you want a good example of how to do phylogenetics, just consider what has been accomplished using the chromosomes of *Drosophila* and processing the data in exactly the same way that comparative anatomists have (Dobzhansky and Sturtevant, 1938). For a real conceptual novelty, one needs something without clear precedents. There are very few important examples. Genetic drift is one of these. How many more might be listed is hard to say. So without denigrating genetics or denying its legitimate role, we had better seek elsewhere to understand what has been going on.

Instead, let us attack the problem from a very different direction and turn to systematics and the philosophy of classification. The Darwinian revolution may then be seen to have been resisted, not just because it was hard to conceive of how species might get transformed by natural selection, but because it was so difficult to conceive of change in general. And rather than focus upon Darwin's lack of a proper theory of heredity, let us emphasize his lack of a proper species concept. For him, species had not yet taken on the sort of theoretical importance that they have in modern speciation theory. The fundamental units of classification had not yet become the fundamental units of evolutionary theory. Darwin was indeed the great reformer of taxonomy, but this was because he advocated strictly genealogical classification, and not because of anything he added to our concept of species beyond the fact that they give rise to genealogical lineages.

The Synthesis made a positive advance, insofar as it developed a new conception of the species, but it retrogressed insofar as it waffled and compromised with respect to the higher categories. Continued resistance, both to genealogical classification and to the new conception of species, is symptomatic of failure to attain the sort of unity that ought to be provided by good philosophy. Although Darwin's original theory was neither perfect nor complete, it was comprehensive and unitary: it envisioned the entire cosmos from a single point of view. And it was more than just consistent. It was coherent, in the sense that its various elements were interconnected so that they lent one another mutual support.

Darwin's theory was one, not many, and the very lack of such unity is what makes the term 'Synthetic Theory' somewhat misleading. One might even go so far as call it the "Syncretic" rather than the "Synthetic" theory. The term 'syncretism' means a flagrant and illogical disregard for basic principles, and was rightly applied by Hennig (1966) to what was then common practice in systematics above the species level. Organisms were classified according to an arbitrary and capricious mixture of genealogical relationships on the one hand, and something called "similarity" on the other. Consequently systems of classification were uninformative and downright misleading to anybody who wanted to use them for scientific research. At the species level, the term 'pluralism' is used instead of 'syncretism,' and it is used in an honorific sense, rather than a pejorative one, even by some professed followers of Hennig, for a similar laxity of standards with respect to principles. The effect is the same. So many different kinds of things, both real and imaginary, can be called "species" that figuring out what, if anything, is meant when this term is used can be pure guesswork.

The differences between the old ways of thinking and the new are not always apparent. When one reads old books, or for that matter new books on an unfamiliar topic, it is easy to deceive oneself into thinking that one has understood what the author meant when one really hasn't. We are more than just tempted to use words in the sense to which we have been accustomed. We do not always ask whether the author presupposes exactly what we presuppose. Therefore when we go back to writings of Darwin's predecessors and contemporaries, we may fail to appreciate the difference between his view of things and those of his contemporaries. If we see, for example, a branching diagram, we might wrongly interpret it as a phylogenetic tree, and get the false impression that the author believed in evolution. The word 'evolution' itself is tricky, for all too often it has meant "ontogeny" rather than "phylogeny." Upon the basis of such considerations we can cross a lot of names off our list of early "evolutionists" and so-called "precursors" of Darwin—and some later figures as well.

But there is a profound, albeit not always obtrusive, difference between what Darwin and such quasi-evolutionists had in mind. For Darwin, change was real. It was a fundamental aspect of reality as he envisioned it. It was not something that could be glossed over or dismissed as superficial or trivial. His predecessors had indeed moved in the direction of imposing an historical interpretation upon their traditional perspectives, but it was hardly a matter of thinking in evolutionary terms as Darwin understood them, let alone as they are understood by a few of us today. They were still trying to make sense out of Darwin's accomplishment by forcing it to conform to the very assumptions that Darwin had deprived of their legitimacy. And those assumptions have continued to be presupposed in efforts at coming to grips with Darwin's accomplishments, especially from a philosophical point of view. We have, for example, a vast literature on the "form-function" controversy. The usual approach is to contrast the Platonist Geoffroy with the Aristotelian Cuvier, and ask who was "right." How often are we given Heraclitus and Darwin as the answer?

For Darwin, as for Heraclitus, change was the fundamental reality. It was not something superficial, or illusory, or something to be explained away. Ancient Greek philosophers treated change as a very serious problem indeed, and many features of their systems were designed to cope with it (Popper, 1945; revised edition 1966). Although we have learned a great deal in the last two thousand years, the problem is still with us. And so are its mostly unsatisfactory solutions.

Plato dealt with the problem of change by positing a timeless and unchanging ideal world that was populated by eternal objects. The particular things that we encounter through daily experience are but imperfect copies of "ideas" or "forms" that supposedly exist in the ideal, or as it came to be called, "archetypal" world. The object of knowledge was not particular things, which were deemed inferior, but the ones in the archetypal world. Because the archetypal world was to some extent conceptualized as a world of ideas, and because it had a sacred character, the attainment of knowledge was pursued as a kind of divine mind-reading. Plato was a follower of Pythagoras, and therefore it was perfectly reasonable for him to take mathematics as the ideal for knowledge in general. If we look upon the inhabitants of the archetypal world as things like "the triangle" and "the circle," then the timeless and immutable character of reality thus conceived makes a lot of sense. After all, the triangle will always be the triangle, not the square.

In a famous passage in his dialog *Meno,* Plato has Socrates interrogate a slave boy and lead him to prove a theorem in geometry. This Plato takes as compelling evidence that learning is really a kind of memory. Here we have a fine example of somebody seeming to have changed, but the change is, if not downright illusory, at least superficial. There is a profound difference between remembering something that one already knows and the active process of making what might be called a discovery, even if it be not an original one. In his dialog *Timaeus,* Plato presents a sort of creation myth in which the earth is compared to a divine animal, which was created. Plato's own views on such matters are of little concern in the present context, so if the foregoing account strikes the erudite as a bit oversimplified it doesn't really matter. The point is merely that systematic biology has been profoundly influenced by Platonism. If one believes that organisms are somehow "modeled" upon the inhabitants of some timeless and ideal world, then one hardly qualifies as an evolutionist, even if one admits that different forms have been manifest at different times.

Plato's forms and things like them are often called "essences," and the term 'essentialism' (often called "typology") is a general rubric for the doctrines, thought-habits, and other things that go along with the acceptance of essences. Rather than attempt to give a precise definition to this term, a task that seems downright hopeless given the vagueness and diversity of usage that we might have to deal with, it seems better to develop the topic of essentialism as we proceed, and come back to it from time to time as the arguments unfold.

As is well known, Aristotle did not endorse the notion of an archetypal world. Although he did invoke "essences" that functioned much like Plato's Ideas, he did not believe that they exist apart from particular things. In Scholastic jargon, he treated them as immanent rather than transcendent. They remained, however, the basic object of theoretical, or we might even say scientific, knowledge. To understand a thing was to know its essence. The essences, however, retained their timeless and unchanging character. Individual men might change, but for the class "man" to change would not even have made any sense. Species could not originate. Furthermore, and in some ways this is even more important, Aristotle did not believe that the universe itself originated: it is eternal.

Aristotle's universe did allow for a certain amount of change, but only within distinct limits. Much of the work of providing for a world that was basically static was accomplished by means of cycles. For example, what we might call a "steady-state" universe was accomplished by circular motion. The stars might move around a bit, but they always come back to the same place. In the *Meteorologia* Aristotle shows how the water moves from land to sea, to air and back to the land again; the erosion and deposition of sediments is treated from the same point of view. So too are the life cycles of animals. The same thing keeps recurring with perhaps minor variations upon the same basic theme.

The kind of geology that Darwin practiced was profoundly affected by revisionist Aristotelianism. It was more or less explicit in the writings of early uniformitarians. Lyell, whom Darwin esteemed as a major role model, advocated a "steady state" model for geology (Lyell, 1830, 1832). So too did Darwin at times, with his elevation and subsidence of the continents (Darwin, 1842, 1846). Lyell long denied that there is any evidence for progressive change in the fossil record, and his stubborn resistance to evolution in general and natural selection in particular fits the same pattern. So although one might think that research upon historical geology would be strongly conducive to evolutionary thinking, that impetus was far from sufficient.

When pre-Darwinian biologists were entertaining the possibility of something like evolution, they were often misled by the sort of change that is undergone by a developing embryo. They had various ways of conceiving how and why embryos develop, ones that were largely incorrect, and the changes they had in mind were bound to be superficial and more apparent than real. To understand their mind-set, just think about how we now understand the development of an embryo. It would not happen at all were it not for the developmental "program" that exists in the egg and the zygote and provides a set of instructions that guide embryogenesis along stringently restricted lines, and which, albeit modified somewhat by mutation and recombination, is not much altered from generation to generation.

Now, if we attempt to explain the last few billion years of phylogenetics upon the assumption that the ancestral prokaryote contained the "blueprints" of all the subsequent radiations of organic beings that have ever taken place, we run into some theoretical difficulties. We could simplify matters just a trifle by denying cladogenesis (branching), and running every species as a single, autonomous lineage all the way back to the *Urschleim*. Nonetheless, the "blueprints" themselves would not change, and that is what makes such thinking just quasi-evolutionary. Furthermore one has to account for the initial existence of each lineage, and to endow the successive forms that are produced with the capacity to deal adaptively with future circumstances, down to the last minute adjustment between flowers and their pollinators. Given an omnipotent Creator, that was perfectly acceptable to such luminaries as Richard Owen (1849:86), in whose words "the Divine Mind which planned the archetype foreknew all its modifications." The necessity of invoking supernatural agency here is all too obvious, but we do need to emphasize once more that the reality of change has been denied. Such "preformationist orthogenesis" is fundamentally different from what we moderns call "evolution."

One alternative is "epigeneticist orthogenesis," which has its roots in another misconception about embryology. Rather than think of God as a Divine Architect stuffing blueprints into primordial ova, think of Him as a Divine Legislator ordaining Laws of Nature that determine the properties of organic matter. One would rather compare the developing embryo to a growing crystal, with its form determined by physical forces. From here it was a minor bit of extrapolation to make the laws of nature do a little bit more work and have them determine what goes on upon a geological scale as well. No matter if nobody had the foggiest notions what such laws were supposed to be, or in what particular manner they were supposed to operate! Thomas Henry Huxley, whose career was virtually one long series of battles with Owen, initially presupposed that some kind of physico-chemical laws would explain organic diversity. That evidently explains a lot about Huxley, not the least the surprise he experienced when Darwin came up with the solution, and very likely his preference for evolution by leaps or saltations. He never did come out and advocate an epigeneticist orthogenesis, but we have plenty of examples of biologists who did, including Owen later in his career.

We will have more to say about epigeneticist orthogenesis later on, for to this very day it remains one of the most popular forms of anti-evolutionary thinking. For the moment, however, we need to emphasize, once again, that it treats change as if it were not real. The various "species" of crystals differ from the species of evolutionary biology in a most fundamental manner. There is nothing historical about them. To be sure, any given mineral crystal that you can pick up and hold in your hand has a history, a location, a beginning, and an end. But there is nothing fundamentally different about the crystals of calcite that formed in the Cambrian from those that are being formed today. The laws of nature that determine their structure have not changed in the least. Calcite is calcite, it always has been, and it always will be, for ever and ever, everywhere. A Cambrian mollusk may have incorporated calcite in its shell, and so too may its descendants at the present day, though some of them might have shells of calcite, others of aragonite, yet others no shell at all. Although it makes perfectly good sense to say that the mollusks in question have speciated, and otherwise evolved, it would be utter nonsense to say that one mineral "species" has proliferated or evolved into another.

For some of us moderns it may be difficult to imagine how an advocate of some epigeneticist version of orthogenesis would be able to make sense out of the fossil record and out of the data of comparative biology in general. But that is mainly because we reason from different assumptions and because certain alternatives do not occur to us. If one does not assume that the various groups of organisms are related to one another by descent, but instead that they come into being by special creation, by spontaneous generation, by the fortuitous concourse of atoms, or by natural or supernatural means that are unknown and perhaps unknowable, then the problem has a ready solution. Just as different kinds of crystals precipitate under different conditions, so too might different kinds of organisms come into being in analogous fashion. Given local differences in temperature, or a gradual cooling of the earth through time, it stands to reason that different kinds of organisms would predominate at different places and at different times. But this would not be evolution. Rather it would be the sort of change that happens when winter sets in and what falls is no longer rain, but snow.

Now, if one wishes to propound an epigeneticist version of orthogenesis, in which the present inhabitants of the globe are literally the descendants of the earlier ones, things obviously become a bit more difficult. One has to think of the changes that are undergone within lineages as analogous to the stages through which a crystal passes as it grows. The analogy becomes all too forced, especially if one tries to allow for the splitting and proliferation of lineages. But the result is only quasi-evolutionary again, for inevitably the changes that take place are superficial, at least in the sense that nothing really new has come into existence. The laws of nature that are responsible for the organisms having the properties that they do remain unchanged.

So the mere fact that someone believes that things seem to be different now from what they were in the past hardly justifies calling him an evolutionist. And it seems to me that the application of that epithet to all sorts of people has been anachronistic and grossly misleading. It would be interesting to reconsider such "forerunners" of Darwin as De Maillet and Buffon from this point of view, but doing that here would divert attention from the tasks at hand. Nonetheless it deserves emphasis that virtually everybody seems to take it for granted that persons like Lamarck and Herbert Spencer were evolutionists, and of course, in a certain sense, they were. But the very failure to realize how different their views were from those of Darwin can only serve to illustrate how fundamental were the differences. Darwin was contemptuous of both Lamarck and Spencer, and for the best of reasons, as can be seen from the correspondence and the unexpurgated autobiography (Barlow, 1959).

Darwin was much vexed by Lyell's persistent incapacity to distinguish Lamarck's theory from his own, as is clear from their correspondence with respect to *The Origin of Species*. Lyell found it far easier grasp Lamarck's theory than Darwin's largely because of Darwin's fundamentally different outlook upon change. For Lyell and for Lamarck the mutability of species meant that they were not "real"—not the sort of fundamental units that numbers are (see Coleman, 1962). Lamarck conceived of life originating spontaneously, as a consequence of physico-chemical laws, then heading in a particular direction, namely, toward Man. His position is a bit complicated by the fact that he allowed for a certain amount of deviation from the straight path by virtue of the fact that animals diversify upon an ecological basis. Nonetheless, his was definitely a version of epigeneticist orthogenesis. Lamarck seems to have allowed for a certain amount of branching, but his branching diagrams were not real genealogies and did not function as such in his research. In at least one important respect, however, Lamarck's views agreed with Darwin's. The organism was conceived of as playing an active role in the evolutionary process. Behavioral evolutionary ecology owes a great deal to Darwin, and, in one sense, through Darwin to Lamarck. But that is not what is generally thought to have made Lamarck, or, for that matter, Erasmus Darwin, an "evolutionist."

Herbert Spencer is also widely perceived as an important evolutionist, especially by historians who write from an anti-evolutionary point of view (Greene, 1977). Of course he was very influential, but the very fact that such interpretations are forced upon Spencer's views merely serves to underscore how badly understood have been Darwin's. Having read all of Spencer's collected essays and his entire *Synthetic Philosophy* from beginning to end, I get more than a mere impression that he was just another advocate of orthogenesis upon a cosmic scale (Ghiselin, 1974a). He treated human society and the world biota alike as the analogue of a developing embryo. And if the various groups of plants and animals diversified, they did so in a manner that is analogous to the specialization of tissues within the body of an organism. In his sociopolitical writings the message was clear: we should not interfere with the course of nature during the genesis of either embryos or societies. In his earlier writings, Spencer (e.g., 1850) had God take a more active role in arranging things, whereas later more of the burden was laid upon the laws of nature. The theology fades into the background, and some contingency was admitted, but the "evolution" remained orthogenetic.

Darwin, as should be much better known, did indeed believe that there is an important relationship between embryology and evolution in the modern sense of that term. He thought that the properties of the developing embryo impose all sorts of constraints upon the course of evolutionary history. Variation, the "raw material" of evolution, resulted from modifications in developmental processes. Because the developmental processes could be modified in some ways but not in others, there was an order to the evolutionary process over and above that which results from selection alone. Darwin by no means believed that selection is "random" in the sense that all mutations are equally probable. All this is clearly spelled out, and in great detail, in his book *The Variation of*

Animals and Plants under Domestication (Darwin, 1868, second edition 1875). The details need not concern us here, for what really matters is what differentiates Darwin from his predecessors. Namely, he believed that what causes the organisms to develop as they do is something that itself evolves. In modern parlance, again, some persons would say that the "genetic program" evolves. Neither preformationist nor epigeneticist orthogenesis would allow for this. If the developmental processes change, then we have to attribute the properties of the organisms to historical contingencies, and not to laws of nature as envisioned by the advocates of orthogenesis.

Having explained some basic ideas about evolution, let us now consider how they relate to philosophy. The distinction between "real" and "apparent" change, or between "real" and "superficial" change may be said to be a "metaphysical" one. Discussing it at some length has made it possible to avoid having to define the term 'metaphysics' in abstract terms. It seemed easier to present a little essay on a philosophical topic, and let that stand as an example, and a contextual definition, of the term 'metaphysics'. Metaphysics deals with such topics as the "what," "how," and "why" of reality. The example of the reality of change suggests that metaphysics is something of great importance to evolutionary biologists, probably much more important than most of them realize. Even for those readers who have a strong background in philosophy, this exercise may not have been altogether superfluous. It provides an introduction to the kind of issue that will be treated in later chapters, and may help to avoid "metaphysics" being taken in a sense that was not intended. (Emphatically not in the sense of "occult metaphysics" that fills the shelves of what are called "metaphysical bookstores.")

Metaphysics is usually considered a branch of philosophy, equal in autonomy—perhaps even in dignity—to ethics, aesthetics, epistemology, and logic. Here, however, it will be treated as one of the natural sciences. Indeed, it will be treated as the most fundamental among them, and in some ways the most important. To take this step, and thereby in a sense to redefine metaphysics, is neither more nor less radical than my earlier efforts to redefine that supremely fundamental category of systematic biology: the species. Indeed, it would seem to be little more than carrying an investigation to its logical conclusion. If a systematic biologist is justified in revising a genus of mollusks, why not the entire Kingdom Animalia? Why stop with the animals? Why not revise everything? At the very least, the exercise might prove instructive, especially in telling us about classification in general.

Some readers may perhaps respond by saying that classification is not particularly interesting. It is common knowledge and intuitively obvious that classification schemes are merely matters of convenience and do not tell us anything really important. So, for example, my suggestion that metaphysics is a natural science is like deciding to shelve my books by size, rather than by author or subject. It should be obvious that such an argument begs the question: it presupposes that which is at issue. If we consider the larger issues of classification in a scientific spirit, we come to quite different conclusions.

I stress this point if for no other reason that in the past some of my philosopher critics have taken it for granted that I am even less enterprising and innovative than they themselves. As they see it, the way to do what is called philosophy of science is to learn what professors of philosophy say, and use that supposed wisdom to cast light upon what goes on in the laboratory. But one does not have to read much philosophical literature to learn that philosophers have been wrong in the past, and that they disagree among themselves at present. Why not treat philosophical ideas as hypotheses? Why not develop their implications and test them by seeing how well they allow us to deal with the data of scientific experience? If one does that, one is behaving like a natural scientist. Indeed it is not always easy to draw a clear line between philosophy and science. The two are inseparable, and if they are different, the distinction is not a matter of academic convention.

My investigations on species illustrate what I have in mind. As a young comparative anatomist trying to develop a better phylogenetic tree of a subclass of gastropods, I became concerned about the methodological controversies that had lately begun to rage about me. The knowability of the topic of my doctoral dissertation itself was subject to skeptical attack. Partly in order to defend myself, I ransacked the philosophical literature, and did not give up simply because so much of it was of very little use. Among other things I learned how to deal with equivocations, and one of these that struck my attention was the two senses of the word 'individual'. In biology, 'individual' is usually synonymous with 'organism', as it is in everyday life. In metaphysics and logic, it has a more general sense, namely a particular thing, including not only an organism like Fido or me, but a chair, the Milky Way, and all sorts of other things. The chair upon which I sit as I write this book is obviously an individual, and it is a member of the class of chairs. I cannot even imagine what it would mean to sit on "chair" in the abstract, nor have I ever heard of anybody having done so. The distinction between classes and the individuals which are said to be their "members" is not, therefore, particularly difficult. Indeed, the particular chair that I sit on is so familiar a part of daily life, that we all take it for granted. But since one cannot sit upon the chair in the abstract, such general terms as "chair" have seemed quite a puzzle. Therefore a lot of attention has been paid to the so-called "problem of universals" whereas the particular things have been neglected.

The literature devoted to the "species problem" includes a lot of discussion about the so-called "reality" of species. According to one very popular philosophical notion, nominalism, individuals are "real" but classes are not. This makes a certain amount of sense: a nominalist would say that this chair is real, whereas 'chair' in general or in the abstract is not. Nominalists generally go somewhat further than that, and might even say that chairs share nothing at all except the name 'chair'—hence the etymology. One does not have to be a nominalist to admit that "chair" and the one on which I sit do not have the same ontological status. In other words, one does not have to go so far as they do and deny that "chair" in general has some kind of "reality"—whatever that is supposed to mean.

In the controversies that surround the species problem, one position that has been taken is the so-called nominalistic species concept. According to this view, species are classes, classes are not real, therefore species are not real. Consequently, perhaps, they are mere conventions and have no role to play in biological thinking. The traditional response to this kind of nominalism was to deny nominalism itself, and take a "realistic" view of species. Species are classes, and they are real, so there was no problem. But the nominalistic argument made a certain amount of sense: if species are classes, how could they evolve or become extinct? It would be like sitting on chair in the abstract. So I turned the problem on its head. Species are not classes; they are individuals. The truth of the solution seemed to me self-evident, one that followed from the definitions of 'species' and of 'individual'.

At this point the impatient reader might call for a precise and simple definition of the term 'individual' and perhaps of 'class' and 'species' as well. I can only answer that one cannot have it both ways. To be sure, I might take a hint from Aristotle and say that a class is a "such" and an individual a "that" or from Dobzhansky and say that species are the largest Mendelian populations. But such definitions do no more than serve as guideposts. If you really want to know what a word means in scientific discourse, such terse little formulae as one encounters in glossaries not only do not suffice, they can be downright misleading. This follows from the very nature of definitions themselves, a point which will be developed in later chapters.

I pointed out that species are individuals in a paper that I submitted to *Systematic Zoology* in 1965 and was published the next year (Ghiselin, 1966b). It was sent for review to David Hull, the leading philosopher of taxonomy, and he hit the roof. A long series of letters followed. He remained unconvinced, but I left the manuscript as it was. I did, however, try to explain matters more fully in my book *The Triumph of the Darwinian Method* (Ghiselin, 1969d:53–54):

> It should thus be clear that metaphysical preconceptions profoundly influence the course of scientific investigation. The effects of divergent philosophical points of view are readily seen in attitudes toward species. Aristotelian definition leaves no room for changes in properties. In a sense the species is the set of properties which distinguish between the individuals of different groups. If species change, they do not exist.

for things that change cannot be defined and hence cannot exist. A Platonist, by way of contrast, is interested only in the ideal organism, and ignores the individual differences which are so crucial to an understanding of changes in species by natural selection. If one is a radical nominalist, species cannot exist in principle, and any study of change must treat only individuals. It is possible to accept species as real and still embrace a kind of nominalism, if one looks upon species as individuals. Buffon (1707–1788), for example, would seem to have entertained the notion that a species is a group of interbreeding organisms. This point of view has certain analogies with the biological species definition of the modern biologist: "Species are groups of actually or potentially interbreeding natural populations, which are reproductively isolated from other such groups." A species is thus a particular, or an 'individual'—not a biological individual, but a social one. It is not a strictly nominal class—that is, it is not an abstraction or mere group of similar things—because the biological individuals stand in relation to the species as parts to a whole. To attain this divergence in attitude required more than simple affirmation. It was necessary to conceive of biological groupings in terms of social interaction and not merely in terms of taxonomic characters.

The new manner of thinking about groups of organisms entailed the concept of a population as an integrated system, existing at a level above that of the biological individual. A population may be defined as a group of things which interact with one another. A group of gaseous molecules in a single vessel or the populace of a country form units of such nature. Families, the House of Lords, a hive of bees, or a shoal of mussels—in short, social entities—all constitute populations. The class of red books, thanatocoenoses, or all hermaphrodites do not. The best test of whether or not a group forms a population is to ask whether or not it is possible to affect one member of the group by acting on another. Removing a worker from a hive of bees, for example, should influence the number of eggs the queen may lay, and therefore both worker and queen are parts of the same population. The concept in use is often exceedingly abstract. If one is to conceive of a population at all, one must use a logic in which the relationships are not treated in terms of black and white. The emphasis must be placed on dynamic equilibria and on processes. The interaction between the parts may be intermittent, dispositional, or even only potential. Entities are components of the same population to the degree that their interaction is probable. That the definition of 'population' allows for no distinct boundary between populations and nonpopulations creates difficulties for traditional logic which can only be surmounted with some effort.

Hull still remained unconvinced, even going so far as to take a public stand against the individuality of species (Hull, 1969). However, his own investigations soon led him to realize that species function as individuals in evolutionary theory. He changed his mind as a result of reading a book by J. J. C. Smart (1963), who claimed that biology is unscientific, because there are no laws for such entities as *Homo sapiens*. But Hull realized that the reason why there are no laws for *Homo sapiens* is the same reason why there are no laws for the Solar System or the Milky Way: these entities are all individuals, and there are no laws for individuals in any science. Hull spent years getting the point across to philosophers and biologists alike, and made the individuality of species the nucleus of a very productive research program. Perhaps the most important outcome of this program was a book in which he treated evolving concepts as individuals in the history of systematic biology (Hull, 1988b).

Except for Hull, nobody seems to have paid any real attention to my efforts to explain the individuality of species before the publication of my paper entitled *A radical solution to the species problem* (Ghiselin, 1974c). This is generally considered the starting point for subsequent discussion on such matters, even though, as Mayr (1987b; response in Ghiselin 1987e) has pointed out, there were many precursors, both real and imagined. The reaction was a mixture of entrenched and determined resistance in some quarters, a bandwagon effect in others. In general, heresy has evolved into consensus. However, there has been much discussion, especially of the broader issues, and people continue to explore alternatives. That is what has led me to write this book.

It began as a little essay responding to the commentaries that had been appearing in the literature. The manuscript grew all out of proportion. I found that there was no way that I could explain anything without explaining just about everything. For one thing, nobody seems to have given a really satisfactory explanation of what it means to be an individual. For another, many implications of the individuality thesis remain unexplored, or at least inadequately appreciated. Darwin's book *On the Origin of Species* said very little about the origin of species, but it said a lot about all sorts of other things. This one says a lot about species as well as about metaphysics. The title alludes to those of earlier works by Dobzhansky and Mayr, which seems fitting insofar as it aims at a metaphysical synthesis that continues the tradition of its justly illustrious predecessors.

Some persons may object to my using the term 'metaphysics' in the title, and for much the same reason that they may react negatively when they encounter 'individual' in an ontological sense. Perhaps they are unfamiliar with anything but "occult" metaphysics, such as astrology and kindred mysticism. Or someone told them that scientists are not supposed to be involved in such things. But it seems better to raise the world's consciousness of truth, rather than to perpetuate misunderstanding. After all, in 1859 the very idea that species might originate, especially through some natural process, seemed downright paradoxical.

A glance at the Contents may suggest the structure of this work and see how it constitutes an integrated whole. We have already begun to define 'metaphysics' and otherwise to explain what the book is all about. The chapter that follows explains a lot of basic metaphysical concepts, and Chapters Three and Four explain what an individual is and is not. Chapter Five deals with the various kinds of definitions. At last, in Chapter Six we are in a position to define such terms as 'hierarchy' and 'species' and to explicate the biological species concept, then proceed, in Chapter Seven, to examine its alternatives. In Chapter Eight we rebut all sorts of objections to the thesis that species are individuals.

Having explained the individuality thesis and shown how it applies to species, puts us in a position to further justify it by showing that it has all sorts of important, and largely unanticipated consequences. The rest of the book explores the broader implications, beginning, in Chapter Nine, with language and other cultural entities. Then we see, in Chapter Ten, how some controversial issues in evolutionary theory might be clarified. Chapters Eleven through Thirteen address some long-standing issues in philosophy of systematics, such as what is meant by such terms as 'character' and 'homology'.

The individuality thesis has led to important new insights about the role of laws of nature and that of history in biology and in other sciences. These implications are addressed in Chapters Fourteen and Fifteen, then illustrated by means of a discussion on embryology in Chapter Sixteen. Further implications, having to do with macroevolution and the fossil record, are sketched out in Chapter Seventeen. In the final chapter, it is argued that the historical narrative aspect of evolutionary biology is far more important than has generally been recognized.

An Appendix encapsulates many of the more important points. It can be used to get an overview of the work. With the help of the index, or by just scanning for words in **boldface,** it can also serve as a kind of glossary.

Such a look at the work as a whole should make it abundantly clear that its subject matter extends far beyond the topic of what a species is. Classification, contrary to what its etymology suggests, is not just the making of classes, or the product of that process. Rather it is the organization of knowledge. Therefore the revolution that is going on in systematic biology has profound implications for our understanding of everything that is known and is knowable, especially of knowledge itself.