that shapes the course of research, see Peter Galison's *Image and Logic* (Chicago: University of Chicago Press, 1998), which pursues this theme in the context of particle physics.

12. Originally published in the *New York Review of Books* and reprinted in *Biology as Ideology*.

13. Lewontin's attacks on this specific form of genetic determinism are quite devastating. I've tried to argue similar points in Philip Kitcher, *The Lives to Come* (New York: Simon and Schuster, 1996), especially chapter 11. In general, however, my critique of genetic determinism differs from that which has featured most prominently in Lewontin's recent writings. See my essay "Battling the Undead" (chapter 13 of this book).

14. I argued for the in principle possibility in *The Lives to Come*. Since I finished writing that book, there has been virtually no progress in addressing the problems of the proliferation of genetic tests, not only in the United States but also in other affluent nations. Of course, the United States is especially backward because of its notable lack of commitment to universal health care coverage. My current position is thus much closer to Lewontin's pessimistic view of the likely social effects of the HGP.

15. See, for example, Alexander Rosenberg, "Subversive Reflections on the Human Genome Project," *PSA 1994* (East Lansing, Mich.: Philosophy of Science Association, 1995), Volume 2, 329–335, and A. Tauber and S. Sarkar, "The Human Genome Project: Has Blind Reductionism Gone Too Far?" *Perspectives in Biology and Medicine* 35 (1992): 222–235.

16. In a forthcoming essay, Kenneth Schaffner argues for similar themes [Genes, Behavior, and Emergentism," *Philosophy of Science* 65 (1998): 209–252]. Schaffner's lucid analysis of investigations of behavioral genetics in the nematode *C. elegans* reveals exactly the need for multileveled studies that I've been emphasizing. It seems to me also to show the fruitful possibilities of combining molecular work with mathematical studies of the properties of networks. Interestingly, the same cross-fertilization of intellectual disciplines is already envisaged in work on the development of the soil ameba *Dictyostelium discoideum* (in the work of William Loomis and his colleagues).

IN MENDEL'S MIRROR: Philosophical Reflections on Biology Philip Kitcher, Oxford University Press, 2003, Pages 45-93

3

Darwin's Achievement (1985)

I

By 1844 Charles Darwin had begun to feel that his growing friendship with Joseph Hooker was strong enough to be tested by the revelation of his unorthodox ideas on "the species question." Darwin's disclosure cost him some misgivings: "it is like confessing a murder," he wrote.[1] Yet, little more than a quarter of a century later, Darwin's heresy had won endorsements from many prominent scientists in Britain, Europe, and the United States. By 1871, Thomas Henry Huxley was prepared to declare that "in a dozen years the 'Origin of Species' has worked as complete a revolution in Biological Science as the 'Principia' did in Astronomy."[2]

How was so swift a victory accomplished? Part of the answer must give credit to Darwin's political skills. We should not be beguiled by the picture of the unworldly invalid of Down, whose quiet walks in his beloved garden were the occasion only for lofty musings on points of natural philosophy. Darwin's study was the headquarters of a brilliant campaign (which he sometimes saw in explicitly military terms),[3] directed with enormous energy and insight. His letters are beautifully designed to make each of his eminent correspondents—Hooker and Huxley, Lyell, Wallace, and Asa Gray—feel that he is the crucial lieutenant, the man on whose talents and dedication the cause depends.[4] Morale is kept up, and the troops are deployed with skill.

Yet Darwin's brilliant use of the social structure of British (and American) science is not the entire secret of his success. Those who fought on his behalf were initially recruited through Darwin's careful presentation of the arguments for his theory,[5] and in their public defenses of that theory, they explained and amplified the reasoning distilled in *The Origin of Species*.[6] As Darwin himself clearly saw[7] the recruitment of eminent allies was necessary to secure a hearing for his ideas. Despite the suggestions of his opponents to the contrary, Darwin's adroit politicking did not dictate the verdict.[8]

In what follows I shall defend an old-fashioned view. The *Origin* is what Darwin advertised it as being—"one long argument" for the theory of evolution.[9] Ultimately, the Darwinian revolution was resolved by reason and evidence, and the reasons and the evidence are crystallized in the *Origin*. We would do well to remember that, for several of Darwin's closest friends and staunchest supporters, it was the reading of the *Origin* that stiffened their convictions and fired their enthusiasm.[10]

Nevertheless, if my claim that Darwin's heresy triumphed because of the reasons he provided is traditional, my defense of that claim will break with the usual views about how those reasons work. I believe that historians and philosophers of science have brought to the study of Darwin a conception of theory and evidence that distorts his achievement.[11] I shall offer a different approach to the theory advanced in the *Origin*, an approach which will, I hope, enable us to see clearly why Darwin's "long argument" was so successful.

II

Virtually everyone would agree that the *Origin* offers a new theory, the theory of evolution by natural selection.[12] When one attempts to specify exactly what this novel theory is, the result is inevitably influenced by general ideas about scientific theories. Once, there was a well-articulated philosophical view on this topic. Scientific theories were held to be axiomatized sets of statements, among whose axioms occurred statements formulated in a special vocabulary, the "theoretical vocabulary" of the theory in question. Expressions in this theoretical vocabulary were supposed to apply to unobservable entities, and because philosophers harbored worries about how they could do so, the account required that there be special statements ("correspondence rules") whose function was to fix the meaning of the theoretical terms. In general, it was supposed that the axioms of the theory would include laws, that these laws would be used in conjunction with particular statements ("boundary conditions" or "initial conditions") to derive previously unaccepted statements whose truth or falsity could be determined by observation, and that theories were confirmed by yielding a large number of such observational consequences which investigation revealed to be true.

There are a number of excellent reasons why this account of scientific theories is no longer *aptly* called the "received view."[13] But a battered and truncated version of it lingers on. Even those who are skeptical about the need for distinctive theoretical vocabulary, or correspondence rules, or axiomatizability, are likely to suppose that any scientific theory worthy of the name must consist of a set of statements, among which are some general laws (laws that set forth the most fundamental regularities in the domain of natural phenomena under investigation), and that such laws should be used to derive previously unaccepted statements whose truth values are subject to empirical determination. When this residual thesis about theories is applied to the case of Darwin, we are led to expect that the *Origin* advances some collection of new general principles about organisms. After all, what else could Darwin's theory be?

The expectation is fostered when we turn to the opening chapters of the *Origin*, where we seem to discover exactly the kind of principles that were anticipated. Darwin's theory apparently rests on four fundamental claims.

1. At any stage in the history of a species, there will be variation among the members of the species; different organisms belonging to the species will have different properties (*Principle of Variation*; *Origin*, chapters 1–2, passim).

2. At any stage in the history of a species, more organisms are born than can survive to reproduce (*Principle of the Struggle for Existence*; *Origin*, chapter 3).
3. At any stage in the history of a species, some of the variation among members of the species is variation with respect to properties that affect the ability to survive and reproduce; some organisms have characteristics that better dispose them to survive and reproduce (*Principle of Variation in Fitness*; *Origin*, 80).
4. Heritability is the norm; most properties of an organism are inherited by its descendants (*Strong Principle of Inheritance*; *Origin*, 5, 13).

From these principles—more exactly, from (2), (3), and (4)—one can obtain by a plausible argument

5. Typically, the history of a species will show the modification of that species in the direction of those characteristics which better dispose their bearers to survive and reproduce; properties which dispose their bearers to survive and reproduce are likely to become more prevalent in successive generations of the species (*Principle of Natural Selection*; *Origin*, chapter 4).

The justification for reconstructing Darwin's theory in this way is relatively straightforward. The first four principles are assembled and defended at the beginning of the *Origin*, and the main theoretical work then appears to be the derivation of the principle of natural selection from them.

Expositors of Darwin from T. H. Huxley to Richard Lewontin have reconstructed the "heart" of his theory in the way that I have done.[14] Nor will my own account of the theory entirely forsake this great tradition. But it should trouble us that the suggested reconstruction is at odds with an assumption that historians and philosophers of science often tacitly and legitimately make. We expect that the fundamental principles of a novel scientific theory should be those statements introduced by the theory that most stand in need of defense and confirmation, and that the arguments assembled by the innovative theorist should be directed at the fundamental principles of the new theory. My reconstruction of Darwin's theory is crucially inadequate not so much for refined scientific reasons (for example, concerns about the need for additional assumptions in the passage to [5]), nor because of esoteric philosophical scruples (my failure to make plain the role of Darwin's key theoretical concept, the concept of fitness). The trouble is that the theory I have ascribed to Darwin is uncontroversial—so uncontroversial as to border on triviality.

Virtually all of Darwin's opponents would have accepted (1)–(4). None of the great scientists of the mid-nineteenth century would have denied—and none should have denied—that species vary, that the increase of a species is checked, that some variation affects characters relevant to the ability to survive and reproduce, that many properties are heritable. Moreover, they would have seen the force of the argument for (5), assenting to the idea that natural selection has the power to adjust the properties of a species, eliminating variants whose properties render them less able to compete in a struggle for limited resources. What was in dispute in the

Darwinian revolution was not so much the *truth* of (1)–(5), as their *significance*.[15] For the committed Darwinian, these principles were the key to understanding a vast range of biological phenomena, and the principal theoretical and argumentative work of the *Origin* consists in showing how the seemingly banal observations about variation, competition, and inheritance might answer questions that had previously seemed to be beyond the scope of scientific treatment.

Acceptance of (1)–(5) is compatible with the doctrine of the fixity of species (which states that species are closed under reproduction). But Darwin did not simply accept (1)–(5) and add the historical claim that lineages (ancestor-descendant sequences of organisms) have been modified. Something like that view was defended by one of his most bitter critics, the disappointed dean of British biology, Richard Owen.[16] Nor did Darwin simply conjoin (1)–(4) with the historical thesis of evolution and the vague declaration that natural selection has been the primary force of evolution. The *Origin* contains a novel and well-articulated theory precisely because it fuses (1)–(5) with the suggestion that species are mutable to fashion powerful techniques of biological explanation. Darwin, Darwinians, and critics of Darwin agreed on what was at stake.

> Nothing is easier than to admit in words the truth of the universal struggle for life, or more difficult—at least I have found it so—than constantly to bear this conclusion in mind. Yet unless it be thoroughly engrained in the mind, I am convinced that the whole economy of nature, with every fact on distribution, rarity, abundance, extinction, and variation, will be dimly seen or quite misunderstood.[17]

> Few can deny the reality of this struggle for existence, and few can dispute the method of its action and the tendency of its results. The main ground of controversy is this, Will this constant accumulation of inherited variations ever constitute a specific difference?[18]

The major theoretical work of the *Origin* lies in displaying the unanticipated significance of (1)–(5).

Before I make this suggestion more precise, let me respond to an obvious objection. The triviality of the reconstructions that I have envisaged might be thought to stem from the fact that I have remained at a very informal level. Perhaps a more significant version of Darwin's theory could be obtained by disambiguating (5), presenting rigorous derivations of the disambiguated principles, and thus exposing the precise conditions that are needed for the Darwinian argument to go through. Quite evidently, if one imports the ideas of contemporary mathematical population genetics, then it is possible to replace (5) with precise claims about the sequential frequencies of phenotypes (or genotypes) found in successive generations of abstractly characterized populations, and to derive them from precise versions of (1)–(4). Equally evidently, this approach ascribes to Darwin a theory of heredity that he did not have, and it is appropriate to point out that one can hardly claim to have found a precise and nontrivial theory in Darwin by inserting such a theory in a field about which he confessed his own ignorance.[19]

A much more promising approach, pursued by Mary Williams,[20] is to attempt to formalize (1)–(5) without making use of any specific theory of heredity. Williams succeeds in showing that a formal version of (5) can be stated using only primitive notions that are arguably Darwinian, and that, given certain extra assumptions, this version can be obtained from formalizations of (1)–(4). Moreover, her approach could easily be extended to provide explicit commitment to Darwin's gradualism and to incorporate stochastic elements in a more satisfactory way than her original version.[21] However, none of this is of any avail in meeting the complaint that I have been developing. Given a little training in logic, Owen, Sedgwick, and Agassiz would all have endorsed Williams's axioms and her derivations. Their objections would not have concerned the truth of the statements put forward, but the fuss that was being made about matters of so little biological importance.

My brief survey of attempts to find a small set of general principles about organisms which can be hailed as Darwin's theory hardly shows that the enterprise is inevitably doomed. However, I hope that it provides some motivation for a different approach to the nature of scientific theories in general, and of Darwin's theory in particular. In the next few sections I shall offer a picture of Darwin's theory that is explicitly designed to focus the main claims and arguments of the *Origin*. Once this has been done, I shall return to the broader issues concerning the nature and confirmation of scientific theories.

III

Darwin's theory of evolution by natural selection is an explanatory device, aimed at answering some general families of questions, questions which Darwin made central to biology, by presenting and applying what I shall call *Darwinian histories*. To fix ideas, I shall characterize a Darwinian history in a preliminary way as a narrative that traces the successive modifications of a group of organisms from generation to generation in terms of various factors, most notably that of natural selection. The main claim of the *Origin* is that we can understand numerous biological phenomena in terms of the Darwinian histories of the organisms involved.

Consider first issues of biogeographical distribution. For any group G of organisms, characterized, perhaps, on the grounds of similar morphology, similar behavior, or a propensity to interbreed in nature, we can identify the range of that group. With respect to any such group we can envisage a complete description of its history. From the Darwinian perspective, this historical description will trace the modification of the current group from its ancestors, revealing how properties change along the ancestral-descendant line, and how, as these changes occur, the area occupied by members of the group alters. Darwinian histories provide the basis for answers to biogeographical questions.

One general form of biogeographical question concerns the distribution of particular groups. Thus, for any group G with range R, we may inquire why the range of G is exactly R. Let us call questions of this form *pure questions about particular features of organismic distribution*.[22] Obvious examples of pure biogeographical questions about particular groups are the question of why pangolins are found in southern Africa and Southeast Asia, and in these regions alone, and why koala bears are confined to Australia. Darwin's suggestion is that we can answer such questions by relating Darwinian histories for the pangolins and the koalas respectively.

We can envisage an ideal Darwinian answer to pure biogeographical questions about the distribution of particular groups. That answer would trace the modification of the ancestral-descendant sequence from generation to generation, showing how, at each stage, the range of the existing group of organisms resulted from their properties and the local environment. Quite evidently, the gory details would not only be irrelevant, but also confusing. As Darwin conceded, he could "in no one instance explain the course of modification in any particular instance."[23] However, he suggests that, despite the difficulty of the "descent to details," his theory owes its "chief" support to its ability to connect "under an intelligible point of view a host of facts."[24] I understand his point as follows: complete Darwinian histories would provide ideal answers to pure biogeographical questions, and it is the fact that the same *form* of answer is always to be given that constitutes the unifying power of the theory; but, in our practical study of biogeography we do not need (nor could we use) such detailed narratives; our explanation-seeking questions are answered by noting certain major features of the Darwinian history of a group of organisms.[25]

Darwinian histories provide the *basis* for acts of explanation, and, confronted with a practical question of biogeographical distribution, *incomplete* knowledge of a Darwinian history will suffice to enable us to offer an answer. When we ask why a group G occupies a range R, we typically have a more particular puzzle in mind. For example, someone who wonders why many marsupial species are found in Australia is likely to be puzzled by the fact that so few are found elsewhere. That puzzlement is relieved by outlining the Darwinian history of the marsupials—describing how they were able to reach Australia before the evolution of successful placental competitors, how the placentals were able to invade many marsupial strongholds, and how the placentals were prevented from reaching Australia. Similarly, people who inquire why the birds known as "Darwin's finches" are confined to the Galapagos are typically concerned to know what accounts for the presence on these islands of forms similar to, but specifically different from, mainland American birds. That concern is adequately addressed by pointing out that the Galapagos finches are the evolved descendants of South American birds who managed to reach the islands and successfully colonized them. In both cases, a general, unfocused, explanation-seeking question is determined in context as a more precise request for information. The request can be honored by abstracting from the Darwinian history, so that the needed information can be given despite considerable ignorance about the details.

Darwin's new proposal thus consists, in part, of two general claims: first, complete Darwinian histories provide ideal answers to pure biogeographical questions; second, incomplete knowledge about Darwinian histories can be used to answer the biogeographical questions that arise in practice. Darwin's theory displays the way in which such questions will be answered. The theory is not simply an *assertion that* certain questions are important and that they can be answered by a description of the history of a lineage, but a *demonstration of the form* that the answers are to take.

It is helpful to contrast the biogeographical part of Darwin's theory with the corresponding parts of the rival theories available in Darwin's day. One (Creationist) approach would take the history of a group of organisms to be irrelevant to their current distribution: because each group of organisms was created to inhabit a particular region, and because it has always inhabited that region, our understanding of biogeographical distribution is advanced by recognizing those features of the organisms in the group that fit them to live where they do. By 1859, this approach had fallen into well-deserved disrepute. It was well known that organisms transported by humans could thrive in areas that they had previously been unable to reach, and naturalists knew of other cases in which organisms seem ill-suited to their natural habitat (a popular example is that of those woodpeckers who inhabit treeless terrain). More promising was a Creationist view that provides some scope for history. On this view, it is suggested that the current range of an organismic group is the result of a process in which an unmodified (or relatively unmodified) sequence of organisms has dispersed from an original "center of creation." Unless this approach is supplemented with a scheme for explaining the distribution of original centers of creation, then it is evident that it will terminate our biogeographical inquiries more rapidly than Darwin's proposal does. Although we may sometimes be able to understand the current distribution of a species in terms of its dispersal from an original center of creation, there will be all too many cases in which this only postpones our puzzlement. Darwin makes the point forcefully:

> But if the same species can be produced at two separate points, why do we not find a single mammal common to Europe and Australia or South America? The conditions of life are nearly the same, so that a multitude of European plants and animals have become naturalised in America and Australia; and some of the aboriginal plants are identically the same at these distant points of the northern and southern hemispheres?[26]

Darwin's challenge is to provide a comprehensible distribution of centers of creation that will allow for the disconnected distribution of the plants common to the hemispheres, while explaining the failure of the mammals to radiate into regions for which they are well suited. The thrust is that Creationists will ultimately be forced into conceding that the distribution of original centers of creation is inexplicable. By contrast, as Darwin will emphasize, the theory of evolution claims for scientific investigation questions which rival theories dismiss as unanswerable.

I have begun with the example of biogeography because it is the case on which Darwin often lays the greatest stress, suggesting that it was reflection on biogeography that originally led him to the theory of evolution.[27] However, biogeography is only part of the story. The rest is more of the same kind of thing. With respect to comparative anatomy, embryology, and adaptation, Darwin also provided strategies for answering major families of questions.

Consider comparative anatomy. Here, the task is to provide answers to question of the general form

> Why do organisms belonging to the groups G, G′ share the property P?

where G and G′ will typically be acknowledged taxa (e.g. species, genera, families, etc.) and P will be some structural property (such as bone structure in a forelimb, for example). A Darwinian answer to these questions will take one of two common forms. In cases where P is recognized as a homology (perhaps on the grounds that it is one element in a rich collection of common properties), the presence of P in both G and G' will be ideally explained by relating the history of descent of G and G' from a common ancestor which also possessed P. In cases where P is a "mere analogy" (perhaps recognized as such because it is only an isolated example of a shared property), its common presence will be understood by tracing the history of the emergence of P in the groups G, G', showing how ancestors of the present members of those groups were modified so that they came to possess P, perhaps as the result of similar environmental pressures. Classic cases of both types were already described by Darwin: Similarities in the bone structure of the forelimbs in various mammalian groups—moles, seals, bats, ruminants—are to be understood in terms of descent from a common ancestor. By contrast, the existence of wings in birds, bats, flying reptiles, and insects, is understood by recognizing the paths which these groups have followed in evolving the ability to fly.[28]

As in the case of biogeography, while relating the complete Darwinian histories of the groups involved would provide an ideal answer to a question about the relationships among them, our practical questions about the similarities among organisms do not require such detail. Quite frequently, the question of why two groups of organisms agree in a morphological property stems from puzzlement that organisms so different in other respects should share the morphological property in question. In the case where the property is a homology, the puzzle is resolved by outlining enough of the Darwinian histories of the organisms to reveal the main lines of their modifications from a common ancestor. Similarly, in the case of analogy, we need to tell enough of the Darwinian history to recognize how a similar feature has been produced in unrelated lineages.

Moreover, like Darwin's treatment of biogeography, the approach to comparative anatomy is easily contrasted with potential Creationist accounts. Appeals to common design for common environments are difficult to defend when the Creationist comes to details:

> It is difficult to imagine conditions of life more nearly the same than deep limestone caverns under a nearly similar climate; so that on the common view of the blind animals having been separately created for the American and European caverns, close similarity in their organization and affinities might have been expected; but, as Schiödte and others have remarked, this is not the case, and the cave-insects of the two continents are not more closely allied than might have been anticipated from the general resemblance of the other inhabitants of North America and Europe.[29]

Darwin's point is that, when we come to investigate the details of the similarities and differences among groups of organisms, his own proposal will offer answers to questions that rival approaches have to dismiss as unanswerable.

The third example that I shall consider is historically crucial, in that it represents the most promising field for the tradition of natural theology. Darwin confesses that his theory could not be admitted as satisfactory "until it could be shown how the innumerable species inhabiting this world have been modified, so as to acquire that perfection of structure and coadaptation which most justly excites our admiration."[30] However, he proposes that questions of adaptation, like questions of

biogeography and organismic relationships, can be answered by rehearsing the historical process through which the adaptation emerged. The general form of question to be addressed is:

> Why do organisms belonging to group G living in environment E have property P?

where the property P is a characteristic which appears to assist its bearers in environment E. A complete answer to this question would trace the Darwinian history of G from the time just prior to the first occurrence of P, showing how the variation producing P first arose, how it was advantageous to its bearer in the original environment, and how that advantage enabled P to become progressively more prevalent in subsequent generations of the lineage. (Here I am deliberately overdrawing the adaptationist commitments of Darwin's theory. I shall consider later whether Darwin allows a more pluralistic approach to the evolutionary explanation of apparently beneficial characteristics.)

As before, our understanding of the presence of properties in current groups of organisms is not dependent on our ability to recognize all the details of the historical processes through which those properties were selected. It is enough to understand the general character of the ancestral form, the way in which a variant might have arisen from that form, and the kinds of advantages that the variant could have been expected to serve. In different contexts, different features will require emphasis. So, for example, with "organs of extreme perfection" the trouble is to recognize the advantages that such structures might serve before they are fully developed.

Since the problem of adaptation is the stronghold of approaches that emphasize the design of nature, the *Origin* contains numerous passages in which Darwin contrasts the explanatory power of his own theory with the deficiencies of its main rival. In some places, he stresses the difficulty of finding any coherent account of the creative design that will do justice to the aspects of nature which are "abhorrent to our ideas of fitness."

> We need not marvel at the sting of the bee causing the bee's own death; at drones being produced in such large numbers for one single act and then being slaughtered by their sterile sisters; at the astonishing waste of pollen by our fir-trees; at the instinctive hatred of the queen bee for her own fertile daughters; at ichneumonidae feeding within the live bodies of caterpillars.[31]

Other passages descant on the "Panda's thumb theme,"[32] the existence of many cases in which it is evident that natural contrivances fall far short of the standards of good design we would expect from a competent engineer, and in which it is more plausible to suppose that the available materials dictated a clumsy solution to a design problem.

> He who believes in the struggle for existence and in the principle of natural selection, will acknowledge that every organic being is constantly endeavouring to increase in numbers; and that if any one being vary ever so little, either in habits or structure, and thus gain an advantage over some inhabitant of the country, it will seize on the place of that inhabitant, however different it may be from its own place. Hence it will cause him no surprise that there should be geese and

> frigate-birds with webbed feet, either living on the dry land or most rarely alighting on the water; that there should be long-toed corncrakes living in meadows instead of in swamps; that there should be woodpeckers where not a tree grows; that there should be diving thrushes, and petrels with the habits of auks.[33]

This theme receives its most detailed treatment in Darwin's book on orchids—characterized as "a 'flank movement' on the enemy."[34] Again, it points toward the same moral: questions that rival approaches must dismiss as unanswerable can be tackled by adopting the Darwinian perspective.

IV

Darwin's theory is a collection of problem-solving patterns, aimed at answering families of questions about organisms, by describing the histories of those organisms. The complete histories will always take a particular form in that they will trace the modification of lineages of organisms in response to various factors—"Natural Selection has been the main but not exclusive means of modification."[35] The time has come to take a closer look at the notion of a Darwinian history and to distinguish "grades of Darwinian involvement."

There is a notion of Darwinian history that is minimal in the sense of embodying the fewest assumptions about the tempo and mode of evolutionary change. This conception can be characterized as follows:

> A Darwinian history for a group G of organisms between t_1 and t_2 with respect to a family of properties *F* consists of a specification of the frequencies of the properties belonging to *F* in each generation between t_1 and t_2.[36]

This minimal conception allows for evolutionary change, for the property frequencies may vary from generation to generation—indeed, properties initially absent may ultimately be found in every member of the group—but it does not offer any account of why this change occurs. At times, it appears that Darwin saw his primary achievement in the *Origin* in terms of the introduction of the minimal conception of a Darwinian history. Perhaps believing that half a loaf might be better than none, Darwin responded to a criticism in the *Athenaeum* by claiming that the commitment to a particular view about how evolution has occurred "signifies extremely little in comparison with the admission that species have descended from other species and not been created immutable; for he who admits this as a great truth has a wide field open to him for further inquiry."[37] Darwin was right to suggest that *some* of the questions he proposed to answer could be undertaken by constructing minimal Darwinian histories. Faced with a question of biogeography—for example, the question of why the Galapagos contains endemic species of finches which are similar to mainland South American forms—one might respond by describing a history of descent with modification that offered no account of the modifying factors.[38] Similarly, some questions about the relationships among groups of organisms can be addressed by rehearsing histories of descent with modification that do not explore the causes of the alterations which have occurred in the relevant lineages. However, Darwin's own voyage to the advocacy of evolution makes it clear

that a minimal evolutionary theory which proposed to answer biological questions by offering minimal Darwinian histories would be vulnerable to serious challenges if it failed to specify any possible mechanisms for the modification of organisms.

More satisfactory is a suggestion that Darwin sometimes seems to favor in his most cautious moments—those moments at which he contends that the important point is to accept the existence of evolution, whatever one's views about the actual mechanisms of evolutionary change. Minimal Darwinian histories are to be used to answer biological questions, but, while we remain agnostic about the causes of modification in any particular case, we do regard ourselves as understanding the general ways in which evolutionary change is to be explained. Thus natural selection is identified as a *possible* agent of evolutionary change, in conjunction with such other agents as use and disuse, correlation and balance, direct action of the environment, stochastic factors, and so forth.[39] On this approach, we would not pretend to explain the modifications that have taken place along a particular lineage, and we would answer only those biological questions that can be addressed through the construction of minimal Darwinian histories. Quite evidently, we would have to forego attempts to tackle questions about organic adaptations.[40]

Numerous passages in Darwin's writings indicate that he preferred to be more ambitious.[41] A stronger conception of Darwinian history involves not only a specification of the changes that take place from generation to generation in a group of organisms, but also a sequence of derivations that will infer the distribution of properties in descendant generations from those in ancestral generations. These derivations will exemplify certain patterns, patterns that reflect views about the agents of evolutionary change. The *selectionist* pattern proposes to derive increased frequencies of properties in descendant generations by identifying the advantages which those properties conferred on their bearers in ancestral generations. Ideally, one would show precisely how the possession of a property *P* gave to ancestral organisms an identifiable increase in the propensity for survival and reproduction, and how this exact enhancement of fitness led to the subsequent increase in the frequency of *P*. Other patterns involve use and disuse, and correlation and balance of characters. The former traces decreasing frequencies of structures in descendant generations to the fact that the structures were unused by the ancestors who possessed them. The latter explains the increased frequency of a characteristic by contending that it is correlated with a property whose increased frequency can be explained in other ways, perhaps by invoking the selectionist pattern.[42] In each of these patterns one can give more or less scope to stochastic factors by allowing for greater or less disagreement between the expected outcome of a derivation of frequencies of properties and the actual distribution.[43]

Quite evidently, a commitment to a stronger conception of Darwinian history makes it possible to answer questions, such as those that involve "perfections of structure," which lie beyond the scope of minimal Darwinian histories. This commitment may be undertaken more or less pluralistically. That is, one may allow as equally appropriate a number of different patterns for deriving changes in property frequencies, or one may insist that a particular style of explanation should predominate. So, for example, Darwin's suggestion that natural selection is the major agent of evolutionary change can be interpreted as a commitment to preferring to understand the distribution of characteristics in a group of organisms by invoking the selectionist pattern. However, there are several different possibilities of interpretation even here. One (implausibly strong) construal is to suppose that, for virtually any characteristic of virtually any organismic group, the prevalence of that characteristic is to be understood in terms of the advantages which the characteristic conferred on those ancestors who bore it. A more moderate interpretation is to suggest that prevalent characteristics are to be explained either directly, by citing their advantages, or indirectly, by pointing to correlations with characteristics which brought advantages. Or one may decrease the scope of selectionist explanations, proposing that they are appropriate for most instances of *major* evolutionary change, that is, for those cases in which we endeavor to understand the prevalence of some property which distinguishes an organismic group.

The point I have been making is that the *Origin* not only allows for the use of more or less ambitious notions of Darwinian history, but also covers a range of positions on the priority of selectionist explanations. My conclusion underscores a claim made by Stephen Gould and Richard Lewontin, who note that "the master's voice" is often more tolerant of alternatives than is usually thought.[44] In further support of this shared judgment, it is noteworthy that the correspondence between Darwin and Wallace on the origin of sterility shows that Darwin needed evidence in *favor* of a selectionist explanation, rather than holding that selectionist explanations were preferable until proved impossible.[45] Moreover, the following passage from the first edition[46] of the *Origin* shows Darwin's anticipation of the possibility that biologists might want to impose selectionist explanations as widely as possible.

> If green woodpeckers alone had existed, and we did not know that there were many black and pied kinds, I dare say that we should have thought that the green colour was a beautiful adaptation to hide this tree-frequenting bird from its enemies; and consequently that it was a character of importance and might have been acquired through natural selection.[47]

Darwin is sensitive to an important point. The presence of properties in contemporary organisms, even properties that suggest to us some benefit which they confer, is not necessarily to be explained by applying the selectionist pattern.

So far I have indicated a number of different theories which might be reconstructed from the *Origin*. These theories differ first in whether they attempt to explain changes in property frequencies along a lineage and, second, about the forms of explanation which they admit or to which they give emphasis. Unfortunately, this does not exhaust the variety of versions of Darwinism. Nothing I have said recognizes Darwin's commitment to evolutionary gradualism, nor have I allowed for a possible Darwinian flirtation with selection of groups rather than individuals. Both of these further variants can be accommodated within the framework I have proposed.

Huxley complained that "Mr. Darwin has unnecessarily hampered himself by adhering so strictly to his favourite '*Natura non facit saltum*,'"[48] Darwin's gradualism is not easy to characterize in general,[49] but I view it as the imposition of a constraint on Darwinian histories. An admissible specification of the successive frequencies of properties in a family *F* along a lineage must reveal distributions that

are (in some sense) continuous and which are modified in "small" steps. At the very least, Darwin would ban histories in which a property absent in one generation is fixed in the next or in which a magnitude which admits of degrees shows an increase in degree without taking on intermediate values. Darwin's writings are full of passages that suggest a more stringent constraint.[50] But not all Darwinians agreed. Huxley preferred to allow for Darwinian histories which are liberated from any such requirement.

Finally, there are some passages in the *Origin* that may be read as indicating a different strategy of biological explanation than any so far considered. In his discussion of social insects, Darwin suggests that certain properties of communities of organisms, to wit, the existence within those communities of organisms with particular characteristics (for example, sterile workers), are present because those properties have proved advantageous in the past to the communities which possessed them.[51] These remarks point toward an alternative conception of a Darwinian history. All the notions discussed so far are *individual-oriented*. An individual-oriented history assigns frequency distributions of properties to successive generations of a lineage, and, if it is not minimal, supplies derivations of those frequencies, derivations that accord with particular preferred patterns. By contrast, a *group-oriented* history specifies the distributions of groups of organisms with particular properties at particular times, and attempts to derive these distributions using preferred patterns of reasoning. So, for example, it may be argued that the current dominance of groups of organisms in which reproduction is, at least occasionally, sexual, is the result of a historical process in which sexually reproducing groups of organisms have been able to produce more varied descendant groups and thus "to seize on many and widely diversified places in the polity of nature."[52]

Darwin's own preferred examples of "selection applied to the family"[53] are not developed in any great detail, so that it is hard for a contemporary champion of group selection to derive much support from them. Nevertheless, the ambiguous remarks about advantages that enable a species to give rise to descendants capable of occupying more niches do suggest another variant of Darwinism. Hence, among our versions of Darwinian evolutionary theory, minimal, pluralistic, selectionist, gradualistic, and so forth, I have included one which allows for group-oriented Darwinian histories.

V

We have begun to understand how the *Origin* might make a novel, controversial, and nontrivial contribution to biological theory—indeed how it might contain suggestions of a number of different theories with greater or lesser degrees of daring. Yet the identification of Darwin's theory with a collection of schemata for answering questions (or his theor*ies* with collections of schemata for answering questions) is only a beginning. To recognize the extent of Darwin's achievement we must give substance to the idea that Darwin reconstructed the field of biology. I shall now try to embed my account in a more general discussion of scientific change.

Any adequate conception of scientific change must contain a view of how the state of science at any given time is to be represented. For many philosophers, it is tacitly assumed that the representation will identify the language which is in use and the statements of the language which the scientists of that time accept. Thus changes within a field of science will be charted by looking for the ways in which the language of the field develops, how new statements come to be accepted, old statements rejected.[54] I believe that so simple an account of the state of science at a time will not do. If we are to understand the transition from the state of science at one time to its state at a subsequent time (or at subsequent times), we need a more complex and refined characterization of these states. To this end, I shall introduce the concept of a *scientific practice*.[55]

A *scientific practice* consists in a language, a set of statements in that language accepted by the scientists whose practice it is, a set of questions which are accepted as the important unanswered questions by those scientists, a set of schemata which specify the forms which answers to those questions are to take, a set of experimental techniques, and a set of methodological directives designed to aid scientists in assessing the credentials of rival proposals for answering open questions. For present purposes, I intend to concentrate solely on the first four components of the practice (language, accepted statements, important unanswered questions, and schemata for specifying the forms of answers to those questions). Darwin's achievement can best be understood, I think, by recognizing the ways in which he modified these four components of scientific practice.[56]

Let me preface my reconstruction of Darwin's transformation of scientific practice by noting explicitly that I doubt very much that all the episodes that are typically identified as major cases of "theoretical change" form a homogeneous class. I suspect that there are examples of theoretical change in physics—perhaps the case of the transition from classical Newtonian dynamics to the special theory of relativity is one—in which we can take the primary focus of the change to be the language of the practice and the set of accepted statements of the practice.[57] In instances like these, the traditional approach of concentrating on the introduction of new concepts and new general principles will prove adequate. Indeed, we may be able to identify the newly introduced theory by writing down some small set of theoretical postulates, we may be able to understand the efforts of the innovators as directed at confirming these postulates, and we may regard the newly accepted statements as the deductive consequences of the new postulates. But not all cases of major theoretical change in science are like this. Specifically, the case of Darwin is not.

Darwin's modification of the language of biology was relatively minor.[58] Certainly, after the acceptance of the *Origin*, it was necessary to abandon some criteria that had traditionally been used to identify the referent of "species," and Darwin introduced a new method of fixing the referent of "homology," but there is nothing comparable to the massive conceptual shifts we find in other cases during the course of the history of science.[59] Darwin did effect large changes in the set of accepted statements. His work introduced a large variety of new claims about particular organisms, their histories, relationships, distribution, and so forth. It would be counterintuitive to identify Darwin's theory with this motley of information, and as we

have already seen, it is not easy to find a small set of general claims from which the descriptions of specific organisms are to flow. The problem with approaching the Darwinian revolution by asking what new statements Darwin advanced is not that he puts forth no novel assertions but that the *Origin* is a hodge-podge of specific original claims about barnacles, pigeons, South American mammals, social insects, arctic flowers, Scotch fir, and so forth. As Huxley noted, the *Origin* is a hard book, and the reader may all too easily find it "a sort of intellectual pemmican—a mass of facts crushed and pounded into shape, rather than held together by the ordinary medium of an obvious logical bond."[60] William Hopkins offered a more negative perspective on the same difficulty, commenting that "many details are apt to perplex the mind and to draw it off from general principles and real arguments."[61]

The trouble is that Darwin's primary achievement is the introduction of schemata for answering certain families of biological questions, and the identification of the questions that biologists should set for themselves. The mass of details is a cornucopia of illustrations. Darwin's initial claim is that certain questions—the questions of why organisms have the properties, distributions, and interrelationships that they do—should be taken as the central questions of biology. Because the principal reasons for *not* viewing these questions as the major unsolved problems of biology depended on the apparent impossibility of answering them, Darwin's principal task in introducing them lay in showing that it is indeed possible to provide informative answers to them.[62] Questions which had inevitably seemed to belong to the province of theological speculation were claimed for scientific discussion.[63] Quite evidently, Darwin's specification of the general forms that answers to questions about adaptations, homologies, distribution, and so forth, would take required him to defend the claim that his preferred schemata are applicable on a broad scale. As we shall see, much of the argumentative work of the *Origin* consists in attempting to demonstrate that the schemata advanced by Darwin can be broadly instantiated.

Finally, the introduction of the new schemata sets new questions for biology, in that, after Darwin, naturalists are given the tasks of (i) finding instantiations of the Darwinian schemata (i.e., developing Darwinian explanations of particular biological phenomena), (ii) finding ways of testing the hypotheses that are put forward in instantiating Darwinian schemata, (iii) developing theoretical accounts of the processes which are presupposed in Darwinian histories (specifically such processes as hereditary transmission, and the origination and maintenance of variation). These tasks arise in different ways. The first, (i), is simply the result of Darwin's claiming of the questions about distribution, adaptation, and relationships as legitimate questions for scientific investigation. (ii) is generated by the fact that, in attempting to instantiate Darwinian schemata, biologists are compelled to advance hypotheses about the historical development of life, and it is incumbent on them to specify ways of testing these hypotheses, so as to avoid the charge that evolutionary theory is simply an exercise in fantasizing.[64] Finally, (iii) stems from the fact that Darwin's theory is not only open-ended in provoking many specific inquiries into the properties, relationships, and distribution of particular organisms, but also in raising very general questions about the historical processes through which organisms have become modified.[65]

Thus, a summary of Darwin's modification of the state of science should take the following form. By including major families of questions that had traditionally been assigned to speculative theology, Darwin changed the set of biological questions accepted as important. He amended the orthodox views about how questions of these kinds should be answered (insofar as such questions were taken to be susceptible of treatment at all) by proposing schemata which answers should exemplify, schemata which invoke the general idea of understanding the current features of organisms by relating a history of descent with modification. Darwin's own efforts at instantiating these schemata led him to put forward hypotheses about the histories of particular organisms. As a result, the *Origin* contains a motley of new theses about individual types of organisms—thus taking on the character of an "intellectual pemmican" (in Huxley's phrase). Moreover, Darwin's schemata and his own instantiations of those schemata introduced new questions concerning the testing of hypotheses about the history of organisms and the general character of the processes presupposed in Darwinian histories. Finally, because of the general presuppositions of the notion of a Darwinian history—in particular the view that it is possible for descendants of one species to belong to a different species—it was necessary to modify the language of biology in certain respects.

What Darwin constantly emphasized, and what his contemporaries recognized, was that the *Origin* was not only a confession of ignorance but also a structuring of our ignorance.[66] As my summary indicates, its primary accomplishment lay in identifying the questions that biologists ought to ask. It is because of this primary accomplishment that Darwin may truly be said to have revolutionized the field. The nature of that revolution is captured in one of Hooker's letters to Darwin:

> But, oh Lord!, how little do we know and have known to be so advanced in knowledge by one theory. If we thought ourselves knowing dogs before you revealed Natural Selection, what d—d ignorant ones we must surely be now we do know that law.[67]

VI

I claim that if Darwin's achievement is construed in the way I have just suggested then we can give an illuminating reconstruction of his "long argument."[68] I divide the reasoning of the *Origin* into three main parts:

1. An attempt to show that it is possible to modify organisms extensively through a natural process (natural selection).
2. An attempt to show that, given the possibility of hypothesizing that organisms now classed in separate species (or higher taxa) are related by descent from a common ancestor, the introduction of such hypotheses would enable us to answer many questions about these organisms.
3. An attempt to respond to difficulties that threaten the introduction of hypotheses about common descent.

The early chapters are directed at (1), and it is in these chapters that the celebrated argument by analogy with artificial selection plays its crucial role.[69] Darwin adduces a number of examples, most prominently examples of different kinds of pigeons, to show that the conscious selection employed by plant and animal breeders has been able to produce striking modifications of organisms. Claiming that the struggle for existence imposes a selective process which is analogous to the deliberate selection of the breeder, he concludes that it is possible to suppose that large modifications can also be produced in nature. Hence it is unwarranted to maintain that hypotheses asserting the modification of an ancestral species to produce a quite different descendant are, in principle, inevitably false.

The role of the analogy with artificial selection is thus to clear the way for subsequent claims about the genealogical relationships of organisms. Darwin believes that he can support such claims by showing how they enable us to answer large numbers of questions about the characteristics, relationships, and distribution of organisms. This indirect support would be of little help if opponents could always charge that it is impossible that the attributions of descent with modification could be true.[70] The study of "variation under domestication" together with the recognition of variation and competition in nature blocks the charge by explaining how some natural modification of organisms is possible. Darwin's adversaries are thus compelled to meet his explanatory attributions of genealogical relationships with the claim that there are limits to the power of selection to modify a group of organisms.

Darwin's critics rose to the challenge. Several reviews of the *Origin* protested that Darwin had no direct evidence of large-scale modifications by natural or artificial selection. Typical were the comments of Thomas Vernon Wollaston:

> There is no reason why *varieties*, strictly so called, . . . and also geographical "sub-species," may not be brought about, even *as a general rule*, by this process of "natural selection": but this, unfortunately, expresses the limits between which we can imagine the law to operate, and which any evidence, fairly deduced from facts, would seem to justify: it is Mr. Darwin's fault that he presses his theory too far.[71]

Because Darwin could only suggest the *possibility* of unlimited variation, he was roundly chided by his critics for deserting the true path of science. Drawing an invidious contrast, William Hopkins descanted on the accomplishments of the physicists:

> They are not content to say that it *may* be so, and thus to build up theories based on bare possibilities. They *prove*, on the contrary, by modes of investigation that cannot be wrong, that phenomena exactly such as are observed would *necessarily*, not by some vague possibility, result from the causes hypothetically assigned, thus demonstrating those causes to be the true causes.[72]

In a letter to Asa Gray, Darwin explained clearly how Hopkins had failed to appreciate the force of his argument:

> I believe that Hopkins is so much opposed because his course of study has never led him to reflect much on such subjects as geographical distribution, homologies, &c., so that he does not feel it a relief to have some kind of explanation.[73]

Although Darwin took some trouble in the opening chapters of the *Origin* (and in the *Variation of Animals and Plants under Domestication*) to show that artificial selection is capable of producing quite dramatic modifications of organisms, his principal response to the charge that variation is only limited is that it is beside the point. The analogy with artificial selection is not intended to demonstrate—nor does it need to demonstrate—that variation is unlimited. Unless some reason can be given for supposing that there are limits to variation, then the explanatory power of the hypotheses that attribute descent with modification justifies us in accepting them, even though modifications as extensive as those which are hypothesized have not been directly observed. Only someone insensitive to the explanatory power of the novel theory—a nonbiologist like Hopkins, for example—will fail to realize that there is evidence for supposing that selection has quite extensive powers whose action cannot be directly demonstrated. The opening chapters of the *Origin* thus clear some space within which Darwin can defend his schemata for tackling biological questions by appealing to their power to unify the phenomena.[74]

If I am right, then the principal burden of argumentation should fall on the concluding chapters of the *Origin* in which the explanatory power of the theory is most extensively elaborated. Darwin himself seems to have seen his book in this way: he begs Lyell to keep his mind open until reading the "latter chapters, which are the most important of all on the favourable side."[75] Darwin's approach is to marshall an impressive array of puzzling cases of geographical distribution, affinity of organisms, adaptation, and so forth, aiming to convince his reader that there are numerous questions to which answers fitting his schemata would bring welcome relief. Consider, for example, Darwin's partial agenda for biogeography. After describing the "American type of structure" found in the birds and rodents of South America, Darwin suggests that biologists ought to ask what has produced this common structure. The similarities are too numerous just to be dismissed as beyond the province of scientific explanation.

> We see in these facts some deep organic bond, prevailing throughout space and time, over the same areas of land and water, and independent of their physical conditions. The naturalist must feel very little curiosity, who is not led to inquire what this bond is.
>
> This bond, on my theory, is simply inheritance, that cause which alone, as far as we know, produces organisms quite like, or, as we see in the case of varieties, nearly like each other. The dissimilarity of the inhabitants of different regions may be attributed to modification through natural selection, and in a quite subordinate degree to the direct influence of physical conditions.[76]

The message of this passage—and of numerous similar passages that occur in the last four chapters of the *Origin*[77]—is clear. There are many details about particular organisms that cry out for explanation. Darwin's proposal to answer questions about distribution (and so forth) by instantiating a particular schema (particular schemata)[78] explains—or at least promises to explain—these otherwise inexplicable details.

But is the theory to be praised for its explanatory promise, or does it actually deliver explanations? Darwin was sometimes inclined to make the stronger claim:

> Thus, on the theory of descent with modification, the main facts with respect to the mutual affinities of the extinct forms of life to each other and to living forms, seem to me explained in a satisfactory manner. And they are wholly inexplicable on any other view.[79]

Some reviewers were unconvinced. Hopkins protested:

> A phenomenon is properly said to be *explained*, more or less perfectly, when it can be proved to be the necessary consequent of preceding phenomena, or more especially, when it can be clearly referred to some recognised cause; and any theory which enables us to do this may be said in a precise and logical sense, to explain the phenomenon in question. But Mr. Darwin's theory can explain nothing in this sense, because it cannot possibly assign any necessary relation between the phenomena and the causes to which it refers them.[80]

Hopkins's remarks make it clear that he regards Darwin's "explanations" as falling short in two main respects: the hypotheses about descent with modification which are invoked in answering biological questions are not independently confirmed, nor are those hypotheses linked by a gapless sequence of inferences to a description of the phenomena to be explained. The first demand is easily resisted. Darwin was fond of remarking that his proposal was no different from that of the physicists who introduced "the undulatory theory of light," without any direct demonstration of the passage of waves through the luminiferous ether, on the basis of its ability to explain the phenomena of diffraction, interference, polarization, and so forth.[81] The second point is more tricky. I suggest that Darwin appreciated the fact that claims that a theory explains the phenomena are ambiguous. Explanations are responses to questions, actual or anticipated, and what is enough to answer one question may not suffice to answer another, even a question posed in the same form of words. To ask why a group of organisms shares a common feature may simply be to wonder about the nature of the bond that unites them, or it may be already to presuppose the character of that bond and to inquire how the feature in question has been preserved through a course of modifications. The latter question will require a different—and more detailed—answer than the former.

Consider the case of the South American fauna. Darwin envisages a naturalist struck with the similar morphology of the South American rodents and the differences between these rodents and the European forms. The *first* question that arises is why the South American organisms are so similar to one another and distinct from the European rodents, and this question can be answered by pointing out that there is a history of descent with modification which traces all of the American organisms to a common ancestor more recent than any ancestor that they share with any of the European forms. The naive naturalist's puzzlement is completely answered when we know that *there is* a Darwinian history of this general form; we do not need to know the exact details of that history. Hence Darwin is entitled to claim that his theory does deliver some explanations, for it adequately answers some explanation-seeking questions. Equally, it is correct to note that Darwin only *promises* other explanations, for there are questions which can only be completely answered by recognizing much more of the detail of the Darwinian history of the organisms concerned. A more sophisticated naturalist, one who already presupposes that the common bond among the South American rodents is "simply inheritance," may inquire why the coypu and capybara are so similar, and in this context, what is needed is to relate enough of the Darwinian histories of these organisms to show how the common features have been preserved, while other characteristics have been modified.

Not only is there a distinction here, but it is (as I have already hinted) a distinction of which Darwin was aware. Although his published and unpublished writings are full of passages which claim that the theory of evolution provides explanations, Darwin explicitly notes that many questions require more detail than he is able to give:

> Very many difficulties remain to be solved. I do not pretend to indicate the exact lines and means of migration, or the reason why certain species and not others have migrated; why certain species have been modified and have given rise to new groups of forms, and others have remained unaltered.[82]

The right response to Hopkins is to maintain that his conditions on explanation are too restrictive, that the *Origin* already offers some explanations and that it indicates the lines along which further explanations are to be sought.

The final chapters of the *Origin* contain the most extensive discussions in which Darwin parades the power and promise of his theory. But there are earlier passages in which he tries to show how the rehearsing of Darwinian histories could enable us to understand some general features of the organic world. Thus Darwin is at pains to make clear how his theory accounts for the existence of discrete taxa and for the "great fact" that these taxa form nested sets.[83] Similarly, he attempts to explain why specific characteristics should be more variable than generic characteristics, why "a part developed in any species in extraordinary degree or manner, in comparison with the same part in allied species, tends to be highly variable," and why we find cases of "reversion," in which organisms of one species show characteristics found in allied or ancestral species.[84] Darwin's discussion of this last topic is especially interesting, in that it serves as the occasion for one of his most aggressive comparisons of the explanatory merits of his own theory with the deficiencies of previous views:

> He who believes that each equine species was independently created, will, I presume, assert that each species has been created with a tendency to vary, both under nature and under domestication, in this particular manner, so as often to become striped like other species of the genus; . . . To admit this view is, as it seems to me, to reject a real for an unreal, or at least for an unknown, cause. It makes the works of God a mere mockery and deception; I would almost as soon believe with the old and ignorant cosmogonists, that fossil shells had never lived, but had been created in stone so as to mock the shells now living on the sea-shore.[85]

Let us now turn to the third part of the argument of the *Origin*, which focuses on apparent difficulties that might be held to stand in the way of constructing Darwinian histories. To construct a Darwinian history will typically involve the scientist in advancing a hypothesis about the existence of certain ancestral organisms with particular properties. In many cases, the fossil record will contain no remnants of such organisms. How is this embarrassing lack of evidence to be understood?[86] Moreover, there are some properties of organisms which, in their final form, obviously assist their bearers, but which would appear to be at best useless if

they were present in an incomplete state. If a Darwinian history is to show us how natural selection favored the emergence of a property of this kind—the presence of an eye, to cite the most hackneyed case—then it seems that we must show how, contrary to appearances, the incipient characteristics were themselves useful.[87] Similarly, Darwin devotes attention to the problem of understanding how the emergence of a sterility barrier might be explained. In all these cases, by turning back a challenge which would initially appear to limit the scope of the strategy of answering biological questions through constructing Darwinian histories, Darwin defends the broad claim that the entire families of questions that he has made central to biology can be answered in the way that he suggests.

Darwin's most acute critic, Fleeming Jenkin, saw clearly that the argument of the *Origin* ultimately rested on Darwin's contention that he could explain a very broad range of biological phenomena, a contention that could be undercut by showing that the class of explanations was far more limited than had been claimed.[88] Jenkin put the point as follows:

> The general form of his argument is as follows:—All these things have been, therefore my theory is possible, and since it is a possible one, all those hypotheses that it requires are rendered probable. There is little direct evidence that any of these maybe's actually *have been*.
>
> In this essay [Jenkin's review] an attempt has been made to show that many of these assumed possibilities are actually impossibilities, or at the best have not occurred in this world.[89]

Behind Jenkin's caricature of Darwin's argument is a sound point. If it could indeed be shown that some of the questions that Darwin hoped to claim for science were unanswerable in the ways that he suggested, then he would be vulnerable to the charge that his proposals for answering other questions of the same types were nothing more than idle speculations. Hence, the *Origin* contains numerous passages in which Darwin labors to show that apparent impossibilities are only apparent.[90] The three enterprises I have described dovetail to provide good reasons for modifying the practice of biology. Consider the situation from which biologists began. Certain features of the organic world had to be dismissed as brute facts, because it was felt that there was no means of answering the question of why they are present which would not appeal (at best, quite quickly, at worst, immediately) to the unfathomable fiat of a creator. Darwin's primary task is to show that such questions are indeed answerable. To do this he must emphasize the puzzling character of the phenomena to be explained and show how his schemata for answering the questions provide immediate relief from some forms of ignorance and promise relief from other such forms. He must also rebut two types of skepticism. One doubts that it is possible to achieve any kind of modification of organisms in nature. This worry is addressed by using the results of plant and animal breeders, and by showing how the struggle for limited resources provides a way for nature to select. The second type of skepticism objects to the possibility of applying the Darwinian strategies to all of the examples that they are intended to address. Darwin tackles this issue head on, by arguing that the difficulties with absent transitional forms, instincts, and complex adaptations dissolve under closer scrutiny.

Given that the argument for changing scientific practice so as to include questions about biogeography, adaptation, relationships, and so forth is cogent, then further modifications of that practice follow easily. The Darwinian schemata are introduced to specify the forms of admissible answers to the newly introduced questions. New statements are accepted because they form part of Darwinian answers to biological questions.[91] Linguistic usage is altered because it is no longer possible to maintain the theoretical presuppositions of certain terms. Any method of fixing the reference of the names of species taxa that presupposes that species are closed under reproduction will fail.[92] Finally, and most importantly, by introducing the Darwinian schemata, one recognizes further phenomena about which questions must subsequently be raised. Any Darwinian history presupposes variation, competition, and inheritance, and the restructuring of biology around the provision of Darwinian histories focuses attention on new theoretical issues surrounding these phenomena. How much variation is there in a naturally occurring population? How does this variation arise? How is it maintained? In what ways do different organisms (and different taxa) compete with one another? How are characteristics transmitted between generations? How are properties of organisms correlated with one another? These are large questions which assume great importance in the post-Darwinian context, and as both Darwin and Huxley foresaw,[93] the subsequent history of evolutionary theory is, in large measure, an attempt to find answers to them.

VII

Darwin's "long argument" does not explicitly confront an objection that was put forward by his most astute critics and that has played an important part in subsequent discussion of the merits of evolutionary theory. The criticism centers on the idea that it will be all too easy to produce stories about the histories of groups of organisms that meet the conditions imposed by Darwin's schemata. Because these conditions are so loose, the critics charge, one can always make up an appropriate account for whatever relationships, distributions, or characteristics one finds in the organic world. Hence the idea that evolutionary theory provides unified answers to questions that are unanswerable on rival approaches is simply false advertising.

Early reviewers of the *Origin* sought instances in which the application of a Darwinian schema would yield some definite statement whose truth value could be determined by observation. Each of them had his own favorite case in which it appeared that this *ought* to occur, and in which it seemed that the *Origin* frustrated legitimate expectations. Because definite predictions from Darwin's theory were so elusive, the critics concluded that the theory was equipped with devices that would permit it to dodge any uncomfortable observational finding. Pictet adduced a popular example, the lack of significant change in animals whose properties have been documented over centuries:

> If the 4000 years which separate us from the mummies of Egypt have been insufficient to modify the crocodile and the ibis, then Mr. Darwin can always reply that this period of time is really trifling. I dare not argue with such weapons whose range I cannot appreciate.[94]

Hopkins perceived troubles for Darwin in the richness of the fauna revealed in the oldest fossil deposits:

> Our author is perplexed with the existence of trilobites, comparatively highly-organized animals, in almost the earliest fossiliferous strata, and to make the fact square with his theory, he at once creates a hypothetical world of indefinite duration for the due elaboration of the ancestral dignity of these intrusive crustaceans.[95]

The most vigorous objection was made by Fleeming Jenkin. After challenging Darwin's suggestions that female choice might suffice to account for "the wonderful minutiae of a peacock's tail," Jenkin offered an inventory of Darwinian strategies for dodging refutations:

> A true believer can always reply, "You do not know how closely Mrs. Peahen inspects her husband's toilet, or you cannot be absolutely certain that under some unknown circumstances that insignificant feather was really unimportant"; or finally, he may take refuge in the word correlation, and say, other parts were useful, which by the law of correlation could not exist without these parts; and although he may not have one single reason to allege in favour of any of these statements, he may safely defy us to prove the negative, that they are not true. The very same difficulty arises when a disbeliever tries to point out the difficulty of believing that some odd habit or complicated organ can have been useful before fully developed. The believer who is at liberty to invent any imaginary circumstances, will very generally be able to conceive some series of transmutations answering his wants.
>
> He can invent trains of ancestors of whose existence there is no evidence; he can marshal hosts of equally imaginary foes; he can call up continents, floods, and peculiar atmospheres, he can dry up oceans, split islands, and parcel out eternity at will; surely with these advantages he must be a dull fellow if he cannot scheme some series of animals and circumstances explaining our assumed difficulty quite naturally.[96]

The objections leveled by Pictet, Hopkins, and Jenkin are seemingly very powerful, and much of the continued suspicion about the scientific status of evolutionary theory reflects the fact that, a century and a quarter after the *Origin*, it is still hard to say exactly what is wrong with them. The criticism can be presented in two different forms. One version begins from the premise that genuine scientific theories ought to be testable, uses the examples discussed by Jenkin et al. to deny that evolutionary theory lends itself to any possible test, and draws the obvious unflattering conclusion about Darwin's theory. The second version explicitly addresses Darwin's "long argument." As we have seen, that argument rests on the claim that the same schemata can be applied again and again to answer a host of otherwise unanswerable biological questions. The critic who has been frustrated by reading Darwin's responses to the apparent difficulties of applying these schemata (for example, in the cases of "organs of extreme perfection" and of lineages where the fossil record does not furnish traces of the alleged ancestors), may protest that the schemata are only so broadly applicable because there are no real constraints on instantiating them. The alleged unification achieved by evolutionary theory is therefore spurious.[97]

Defenders of Darwin sometimes insist that these criticisms are already forestalled in the *Origin*, and that Darwin is at pains to specify conditions under which his theory would be demonstrably inadequate. The defense is based on a much-quoted passage:

> If it would be proved that any part of the structure of any one species had been formed for the exclusive good of another species, it would annihilate my theory, for such could not have been produced through natural selection.[98]

But the critics will rightly point out that this passage only appears to give hostages to fortune. For how is one supposed to show that some characteristic of a species was "formed for the exclusive good of another species" (or, perhaps more exactly, was formed through some process which is not covered by Darwin's inventory of agents of evolutionary change)? The worry that pervades the remarks I have quoted from Jenkin, Hopkins, and Pictet is that there are ready-made stratagems which defenders of evolutionary theory can use to brush off any suggestions that such characteristics exist: one can conjure trains of ancestors to whom the property in question might be supposed to have been beneficial. Nor do contemporary defenders of Darwin succeed in doing better when they insist that the theory of evolution precludes the possibility of finding certain fossils, for example, hominid remains in pre-Cambrian deposits.[99] Such defenses are inadequate because they do not address the real worry, namely, that defenders of evolutionary theory have resources which enable them to reinterpret uncomfortable fossil findings, either by questioning the alleged connection between fossil and organism or by assigning a different age to the pertinent strata.

To turn back the criticism leveled by Jenkin et al., we must start with a relatively obvious distinction. In the history of science there have been some theories which, at an early stage of their development, have been difficult, even practically impossible, to test. Such theories are not to be confused with those objectionable doctrines which are impossible *in principle* to test. Someone who proposes that all natural phenomena are to be understood as effects of God's will *and* who also refuses to admit any independent way of fathoming the divine will may legitimately be reprimanded in the way that Jenkin reproved Darwin. In this case, there really are no checks on the ability of the proposal to match itself to whatever phenomena are found. But Darwin's approach is importantly different.

Consider the kind of application of a Darwinian schema which evidently worried Jenkin. Imagine that we are attempting to answer a question about the distribution of a group of organisms, and that, in doing so, we advance claims about the existence of ancestral forms that inhabited particular regions, about the previous connections of land masses, and the abilities of the organisms in question to disperse. Suppose that there is no fossil record of the alleged ancestral organisms. A naive opponent might think that this suffices to demonstrate that the supposed ancestors never existed, and that the Darwinian history is therefore incorrect. The chapters of the *Origin* which discuss the incompleteness of the fossil record are designed to show how naive this evaluation is. Yet that discussion lends plausibility to Jenkin's charge that, in emphasizing the fragmentary character of the record, Darwin is paving the way for accommodating *any* case in which there are no signs of the existence of hypothetical ancestors. But the charge is an overreaction. Jenkin and his fellow critics overlook the possibility of fashioning independent ways of specifying just how fragmentary the fossil record is. Unlike the person who appeals to the divine will without honoring any independent criterion for fathoming that will, Darwin allows for the possibility of a theory of fossilization, a theory which will generate well-founded expectations about the likelihood that records of ancestral organisms will be preserved. Indeed, we should go further. Evolutionary theory was committed, from the beginning, to the development of ancillary theories, of which our envisaged theory of fossilization is one, which could be used to supply constraints on Darwinian histories. This commitment results not simply from the need to remedy the initial difficulty of testing Darwinian histories, and thus to rebut the accusation that Darwin created a game without rules, but also from the need to decide among alternative Darwinian histories that might be proposed for the same phenomena. In the case at hand, development of a theory of fossilization would be expected not to test Darwinian histories individually but to provide an evaluation of the class of Darwinian histories actually proposed. We are to compare the totality of hypothetical ancestors for which no fossil forms are found with our theoretical knowledge of the fossilization process, asking whether it is probable that so many fossils should be missing.

Analogous points can be made about the Darwinian claims of previous continental arrangements and of possibilities for dispersal. Those who assert that the situations of land masses were formerly different are committed to finding some geological account of the hypothetical process through which the alteration has been effected. Darwin's own practice reveals how the deliverances of geology led him to test and reject an otherwise attractive hypothesis about animal distribution on oceanic islands. Hooker had urged the merits of a doctrine (due to Edward Forbes) which allowed the former extension of existing continents to make them continuous with what are now islands. Darwin replied:

> There never was such a predicament as mine: here you continental extensionists would remove enormous difficulties opposed to me, and yet I cannot honestly admit the doctrine, and must therefore say so. I cannot get over the fact that not a fragment of secondary or palaeozoic rock has been found on any island above 500 or 600 miles from a mainland.[100]

Similarly, Darwin's own work demonstrates the possibilities for testing claims about the dispersal of organisms. In his attempt to understand the distribution of plants, Darwin was concerned to discover the extent to which seeds could survive very harsh conditions. The *Origin* describes his careful experiments on seed germination after soaking in sea water, his discovery that seeds extracted from earth which had been enclosed in wood for fifty years could nonetheless germinate, and his investigations on the possibility of seed transport by birds.

In all these cases, Darwin is testing what initially appeared to him and his contemporaries as ambitious hypotheses about the dispersal powers of organisms, and he uncovers, in the process, some facts about germination that would initially have seemed highly unlikely.[101] Plant dispersal was one topic on which Darwin seemed to be driven to hypotheses with surprising consequences. Perhaps even more vexing was the problem of accounting for the distribution of fresh water molluscs. A postscript from a letter to Hooker indicates Darwin's sense that his ideas about the dispersal of these organisms seemed quite at odds with what was known about them.

> The distribution of fresh-water molluscs has been a horrid incubus to me, but I think I know my way now; when first hatched they are very active, and I have had thirty or forty crawl on a dead duck's foot; and they cannot be jerked off, and will live fifteen and even twenty-four hours out of water.[102]

Existing sciences, such as geology and physiology, thus provide ways of testing some of the claims advanced in Darwinian histories. In addition, as we have already seen, Darwin's reform of the practice of biology points the way to the construction of new sciences around the unanswered questions concerning variation and heredity. The pursuit of these new sciences makes Darwin's theory vulnerable from new directions. If it should be discovered that the principles that govern heredity cannot be integrated with the idea of modification through natural selection, or if it can be shown that organisms are not variable in the ways that Darwin's accounts require, then evolutionary theory will be tested and found wanting. Indeed, as the subsequent history of Darwinism clearly shows, it is precisely in the unfolding of the facts of heredity and variation that Darwinian evolutionary theory has faced some of its most serious challenges.[103]

I hope that this discussion makes it clear how the initial difficulty of testing Darwinian claims should not be confused with the view that Darwin's theory is in principle untestable and therefore worthy of the kinds of objections that Pictet, Hopkins, and Jenkin leveled against it. Let us now return to those objections, and confront them directly. The worries expressed by Hopkins and Pictet can be soothed by noting that there are sciences independent of evolutionary theory (sciences such as geology) which can, either in practice, or at least in principle, be employed to check the claims made in the Darwinian histories which the critics find objectionable. Jenkin's concern is more subtle. For Jenkin does not simply raise the problem of testing the hypotheses advanced in particular Darwinian histories; he charges that Darwinism is so flexible that the failure of particular Darwinian accounts need not prove troublesome; others can always be found. Thus, in the spirit of Jenkin's original critique, one might respond to the observations about the probative power of geology and physiology as follows: even if we grant that these other sciences can force the true believer to abandon a particular Darwinian history, that does not directly affect the main Darwinian claim, to wit, that there is some such history to be found; the Darwinian may simply set out to construct some other imaginative story to dodge the known difficulties, or if problems multiply and imagination runs out, may resort, *in extremis*, to the claim that there is some (unknown) Darwinian history which will overcome all the difficulties, and that the task of finding this history is an interesting research project for the theory.

This line of criticism seems to me to be implicit in Jenkin's original review, and I take it to constitute the most powerful methodological objection to Darwin's theory. Three points need to be made in response to it. First, one should not overlook the possibility of global challenges to the presuppositions of any Darwinian history, for example, results from the investigations of heredity and variation which

would call into question the possibility of the processes which Darwinian histories constantly invoke. Thus the new sciences whose domains are described in Darwin's reform of the practice of biology may furnish tests of the theory and not simply of particular instantiations of it. Second, there are grounds for believing that Jenkin has overrated the flexibility of *at least part* of Darwinian evolutionary theory. In the light of the constraints imposed by geology, physiology, and morphology, minimal Darwinian histories which will address questions about organismic distribution and relationships may turn out to be rather hard to find. Finally, the idea that admission of ignorance and heralding a new research problem is a universal strategy for promoting the survival of doctrines in distress is seriously flawed.

Consider the second point in the light of our imagined question about organismic distribution. Suppose that we have originally proposed to explain the distribution of a group of organisms, some of which occur on an island, others on the nearest continent, by hypothesizing an earlier continental connection, subsequently submerged, followed by a modest amount of evolutionary divergence between the mainland and insular forms. On testing the geological claim we discover that the alleged continental connection is highly suspect. We now suggest that recent common ancestors of the continental and island organisms were able to traverse the sea that separates continent and island. But, when we investigate the dispersal powers of the contemporary organisms, we find that they are unable to swim the distance, that they are too large to be carried by birds, that they are unable to cling to pieces of driftwood sufficiently tightly to survive the rough seas, and so forth. Finally, we explore the possibility that the organisms in question are not related in the ways that we had originally conjectured. Here we are foiled by our knowledge of morphology, and by our practice of explaining such morphological relationships in other cases by appeal to the existence of a recent common ancestor. Consistency with other Darwinian explanations requires that we understand the morphological similarities by appealing to common ancestry, and our commitment to rapid evolutionary change among related organisms requires us to hold that the common ancestor is recent. Thus we are constrained, forced to construct a particular type of Darwinian history, and all the instances of this type that we are able to produce encounter difficulties with the geological and physiological findings.[104]

Hence there is reason to believe that, sometimes, when Darwinian attempts at explanation go awry, no substitute Darwinian histories will be readily available, so that the evolutionary theorist will be driven to a confession of ignorance. Jenkin perceives this ultimate evasion as a free move. However, familiar points about the character of scientific testing make it apparent that the usual ways of showing scientific theories to be inadequate involve demonstrating that unsolved "research problems" are multiplying at a much faster rate than successful solutions.[105] Immediate trouble can be avoided by pleading that the correct Darwinian history has not yet been conceived, but, if Darwinians are forced to make this plea again and again, then their claims will sound ever more hollow. This sad, imaginary, destiny for Darwin's theory would depict it as collapsing in just the ways that Ptolemaic astronomy collapsed in the sixteenth and seventeenth centuries and the pancorpuscularianism of some Newtonians collapsed in the eighteenth century.[106] Thus the comparison with other sciences, which Darwin liked to use in addressing methodological challenges, will serve to meet Jenkin's objection. Ptolemaic astronomers were compelled to insist, again and again, that there was some combination of allowed motions that would account exactly for the planetary orbits, and that the discovery of this combination was an important research problem for their theory. Darwinian evolutionary theory was potentially vulnerable to a similar predicament—and a similar fate.

A simple comparison between a new theory whose credentials are questioned and previous scientific theories can quell objections, but it does not provide a satisfactory explanation of why the new theory is methodologically sound. The previous analysis of Darwin's theory prepares the way for us to go a little further. On the account I have proposed, Darwin's theory is a collection of problem-solving patterns aimed at answering major families of questions. So construed, the theory plainly makes no definite predictions which can be evaluated by relatively direct observation. Indeed, the relation between theory and observation is doubly loose. In the first place, the theory does not dictate the particular Darwinian histories which are to be constructed. In the second place, individual Darwinian histories will not always imply definite claims about expected observational findings. (In fact, it will be relatively rare for a Darwinian history to imply any statement whose truth value can be ascertained by observation.) As we have already seen, the assessment of individual Darwinian histories must be undertaken with the aid of ancillary theories. Thus, in understanding the relationship between Darwinian theory and observation, one must consider a number of possible cases.

A. In attempting to answer a question about some group of organisms, one finds that there is only one Darwinian history that one can think of which is compatible with the constraints that have already been discovered. (These constraints are imposed by previous observational findings, together with ancillary theories and the prior practice of constructing Darwinian histories.)[107] This unique history implies claims about the existence of certain ancestral organisms (or of contemporary organisms with certain definite properties), claims which would not antecedently have been accepted. Evidently, these claims can be more or less improbable in the light of prior information. On investigation, we discover that there is observational evidence for the truth of some of the claims.

B. As for *A*, except that there are several available Darwinian histories, one of which implies existence claims for which there is observational evidence.

C. As for *B*, except that there are several alternative Darwinian histories, all of which imply common existence claims that are supported by observation. There is no evidence for any distinctive claim of any of these histories.

D. As for *A*, except that investigation does not reveal any evidence of the presence of the hypothetical organisms.

E. As for *D*, except that there are several alternative Darwinian histories none of which receives positive observational evidence for its existence claims.

F. As in *A*, there is, initially, a unique available Darwinian history. Observational evidence together with ancillary theories implies a statement that is inconsistent with some consequence of the unique Darwinian history.

G. As in *F*, except that there are, initially, several alternative Darwinian histories available. With respect to each of these, there is some body of observational evidence which, in conjunction with some body of ancillary theory, yields a consequence incompatible with some claim of the Darwinian history.

I do not want to suggest that these cases exhaust all the relevant possibilities. Rather I propose that any thorough reply to Jenkin's methodological criticism will need to take into account at least this much variety of relationships between Darwin's theory and observational findings.[108]

Consider, first, the cases that are clearly positive. Examples of type *A* redound to the credit of Darwin's theory, because they involve tests of the theory where it is apparently weakest.[109] In such instances, the resources of the theory are relatively impoverished, and there is only one available way of providing a theoretical explanation of the relevant phenomenon. Confirmation will be more or less dramatic according to the prior degree of improbability of the existence claims that receive observational support.[110] Examples of types *B* and *C* are somewhat less forceful in providing support for Darwinian theory, precisely because they show the theory succeeding in an area where it had more room for maneuver. (Instances of type *B*, unlike instances of type *C*, have the ability to confirm *particular* Darwinian accounts. This confirmation should be seen as dependent on support for the *general* theory, that is, the collection of problem-solving patterns.) In each of these three types of case we may view Darwin's theory as "leading to the discovery of new facts." I suggest that the frequent claims that the theory "uncovered new facts" are based on appreciation of this kind of relationship between theory and observation, and should not be confused with assertions that the theory makes predictions (where "prediction" is interpreted in the usual way, namely in terms of the deduction of some independently checkable statement from a body of theoretical statements and items of background knowledge).

Cases *D* and *E* are neutral with respect to the general theory. In examples of type *D*, as in examples of type *B*, support for Darwin's theory would provide reason to accept the individual Darwinian history, despite the fact that that history has no positive evidence in its favor. Such cases, like the more thoroughly neutral cases of type *E*, will usually invoke the fragmentary character of the fossil record to account for the disappointing absence of organisms which are hypothesized. As I have already noted, the strategy of making such invocations is subject to evaluation in the light of independent knowledge about the likelihood of fossilization. The consequences of this point will be apparent as the reply to Jenkin's objection is developed in more detail.

The last two types of case reveal the theory as encountering troubles. We have already seen the possibility of using ancillary theories together with observational findings to test a particular Darwinian history. Examples of types *F* and *G* involve tests with negative outcomes for all available Darwinian histories (with respect to some group of organisms and some question concerning those organisms). Faced with such cases, the defender of Darwinian evolutionary theory must plead temporary ignorance and recognize another "unsolved research problem." The admission is more serious in examples of type G, just as the support is strongest in cases of type A. One does the greatest damage to the theory by showing that it is inadequate to handle a problem for which it initially seemed to have abundant resources.[111]

At this stage, we can meet Jenkin's critique head-on, recognizing what is salutary in it and simultaneously clearing Darwin's theory of the main charge. The theory would indeed be methodologically suspect if it precluded the possibility of cases of types F and G. However, we have seen that such cases might occur, and it is relatively simple to describe conditions under which the occurrence of these cases would lead to the rational rejection of Darwin's theory. The most obvious dismal scenario is for the class of cases of types F and G to increase at a much more rapid rate than the class of cases of types A–C, and for the latter to contain few, if any, instances in which observation supports some antecedently highly improbable existence claim. To specify exactly the conditions under which it would be reasonable to abandon Darwin's theory in favor of a search for some alternative, one would need to describe a complicated function of several arguments: the rationality of rejection seems to depend upon the distribution of cases among the types A–G, the prior improbabilities of the existence claims that are supported in the positive cases, the extent to which the lack of evidence in the neutral cases diverges from our expectations about fossilization, the extent to which the "research problems" generated by the negative cases have resisted sustained attempts at solution, and, perhaps, the degree to which alternative approaches have already been explored. Luckily, for our present purposes, it is simply necessary to note that there are indeed *some* conditions under which it would be reasonable to reject Darwin's theory. We have enough understanding of the shape of the complicated function to recognize that rejection would be dictated if the arguments were to assume certain extreme combinations of values.

It is worth noting explicitly that mere multiplication of neutral cases, even without examples of types F and G, might lead to the downfall of Darwin's theory. If one were constantly to hypothesize organisms whose existence was never to be confirmed through observational investigation, then Darwin's theory would face an obvious difficulty: our understanding of the process of fossilization might teach us that the record is fragmentary, but it would be necessary to plead extreme bad luck, if remains of hypothetical, extinct organisms were never found. The situation would be less clear cut, though still damaging, if the record only occasionally revealed signs of the hypothetical organisms. More precisely, given a class of hypothetical ancestral organisms and a theory of fossilization, one will be able to make estimates of the probability that fossils of only n percent of the hypothetical organisms are found (where n is the frequency of the hypothetical organisms for which fossils are actually found), and the smaller this probability the more reasonable it will be to reject Darwin's theory.

Darwin did not defend his theory by pointing to definite predictions which were observationally verified. Noting the absence of predictions, some of his critics charged that the new theory was not a genuine piece of science. What is correct about their objections is the demand that the theory should be testable in principle and that its proponents should develop it so as to make it testable in practice.

As we have seen, despite the initial difficulties of testing Darwin's theory, that theory was, in principle, susceptible to test. I have described some scenarios which would have led to its rational rejection. However, these scenarios do not represent the actual course of history. Shortly after the publication of the *Origin*, Darwin's theory began to receive support in the ways that my account has suggested. Naturalists started to discover remains of organisms whose existence had been hypothesized in Darwinian histories. One of the most striking findings, the discovery of *Archaeopteryx*, was viewed by some of Darwin's supporters as an important boost for the new theory.[112] Darwin himself placed greater stock in a discovery about living organisms, the discovery of organs in nonelectric fish which are homologous to the electrical organs.

This example is a beautiful illustration of type A. In the *Origin*, Darwin confessed to two problems with the electric organs of fish. The first concerns the steps which led to the production of these organs. Darwin continues

> The electric organs offer another and even more serious difficulty; for they occur in only about a dozen fishes, of which several are widely remote in their affinities. Generally when the same organ appears in several members of the same class, especially if in members having very different habits of life, we may attribute its presence to inheritance from a common ancestor; and its absence in some of the members to loss through disuse or natural selection. But if the electric organs had been inherited from one ancient progenitor thus provided, we might have expected that all electric fishes would have been specially related to each other. Nor does geology at all lead to the belief that formerly most fishes had electric organs, which most of their modified descendants have lost.[113]

If we attempt to construct a Darwinian history for the electric fishes, then the constraints seem to rule out the two types of history that might antecedently have been viewed as most likely. We cannot treat the electric fish as a taxonomic group which share a more recent (electric) ancestor with one another than any common ancestor that they have with nonelectric fish; as Darwin notes, the electric fish are a diverse group, and the practice of constructing *other* Darwinian histories for the fish will rank some electric fish as evolutionarily quite distinct from other electric fish. Moreover, the fossil record militates against the alternative claim that the possession of an electric organ was a primitive condition that has been lost in most recent fish. The only possible evolutionary solution, as Darwin goes on to confess, seems to be that "natural selection, working for the good of each being and taking advantage of analogous variations, has sometimes modified in very nearly the same manner two parts in two organic beings which owe but little of their structure in common to inheritance from the same ancestor."[114]

Here, then, we seem to have a unique type of Darwinian history, to which Darwin's theory is forced, which will adduce multiple instances of unrelated productions of electric organs in fish. Combining this apparently necessary consequence with his knowledge of the morphology of fish, one of Darwin's contemporaries developed what he took to be a crucial objection to evolutionary theory. Darwin describes the episode, and its unexpected outcome, in the postscript of a letter to Lyell:

> I must tell you one little fact which has pleased me. You may remember that I adduce electrical organs of fish as one of the greatest difficulties which have occurred to me, . . . Well, McDonnell, of Dublin (a first-rate man), writes to me that he felt the difficulty of the whole case as overwhelming against me. Not only are the fishes which have electric organs very remote in scale, but the organ is near the head in some, and near the tail in others, and supplied by wholly different nerves. It seems impossible that there could be any transition. Some friend, who is much opposed to me, seems to have crowed over McDonnell, who reports that he said to himself, that if Darwin is right, there must be homologous organs both near the head and the tail in other nonelectric fish. He set to work, and, by Jove, he has found them! so that some of the difficulty is removed; and is it not satisfactory that my hypothetical notions should have led to pretty discoveries?[115]

So it appears that Darwin's theory was quickly tested in some of the ways that I have indicated, and that the happy outcome of those tests served to buttress Darwin's "long argument."

VIII

I began by promising an account of the structure of evolutionary theory and of the early arguments which were given in its favor that would enable us to see that the Darwinian revolution was settled by appeal to reason and evidence. In giving my account, I have departed from the main philosophical traditions about the structure of scientific theories and about the confirmation of theories. I have claimed that the theory (or, more precisely, the theories) contained in the *Origin* should be seen as a collection (or, collections) of problem-solving patterns, and that the decisive change effected by Darwin was the incorporation within biology of questions which had previously seemed inaccessible to science, together with strategies for answering those questions. Moreover, in analyzing the evidence which led Darwin's contemporaries to accept his theory—however tentative their acceptance may have been—I have emphasized the "long argument" of the *Origin*, rather than any "predictive successes" that might be attributed to Darwin's theory. Indeed, the burden of the previous section is that the relationship between Darwin's theory and observation is very loose, so that the absence of reports of confirmed predictions from the first edition of the *Origin* is no accident.

Are these departures from orthodox views really neccessary? Is it possible to provide an account of Darwin's achievement that will be more consonant with orthodox ideas about theory structure and about the confirmation of scientific theories? These are natural questions about the analysis that I have offered. In conclusion, I shall try briefly to answer them.

If we begin from the amorphous thesis that theories are sets of statements, the residue of what was once a complex and ambitious doctrine about the nature of scientific theories, then it is natural to attempt to find in the *Origin* some principles about organisms and the general features of their histories. As we discovered in section 2, the early chapters of the *Origin* do indeed contain some candidates. The trouble with the resultant collection of principles is that it turns out to be trivial and uncontroversial, and we do not achieve distinctively Darwinian doctrine even if we conjoin the thesis that species can be extensively modified so as to give rise to new taxonomic groups (the thesis of evolutionary change). A natural suggestion, at this stage, is to propose that (in Darwin's phrase) "Natural Selection has been the main but not exclusive means of modification."[116] Here, by adding a rather vague thesis, we obtain at last a collection of principles whose conjunction was first held by Darwin.

Is the result Darwin's theory? If it is, then Darwin's theory is plainly inferior to the scientific achievements which philosophers have admired and which have furnished the traditional views of the structure of theories. Crucial elements of the collection of principles we have assembled are deplorably vague, and there is no warrant for ascribing more precise versions of them to Darwin. Moreover, it is legitimate to wonder how there could be any interesting theoretical articulation of the "theory" so identified. In such theories as classical mechanics, electromagnetic theory, and quantum mechanics, we are used to seeing significant theoretical work: interesting and often surprising theorems are derived from the deductive depth. There is little point to heralding the principles we have collected as the "axioms of Darwin's theory" because there is so little of interest that we can derive from them. Yet the *Origin* is a long book, full of subtle discussions that one might naively think of as articulating evolutionary theory. All these discussions are bypassed when Darwin's theory is condensed in the suggested way, and we are left, in effect, with a single argument from chapter 4 of the *Origin* together with two imprecise claims that are made at various places in the book. The result is an impoverishment of Darwin's achievement.

The differences between Darwin's theory and a theory like classical mechanics can be made clear by considering one of the main achievements in the history of mathematics. As it was originally developed by Newton and Leibniz, the calculus had two parts. One part was a collection of claims about differentiation and integration. In this part, proceeding from (not very satisfactory) definitions of the derivative and the integral, Newton, Leibniz, and their successors deduced results about the derivatives and integrals of particular functions (for example, that the derivative of x^n is nx^{n-1}, for positive integral values of n). The second part consisted of proposed methods for using the concepts of derivative and integral in answering questions in geometry, kinematics, and dynamics. Here, Newton and Leibniz showed how one could find subtangents, subnormals, areas, and volumes, by employing the central concepts of the calculus. In its original form, the calculus appears as a deductively organized set of theorems about functions coupled with a set of techniques for solving traditional problems in geometry and mechanics.[117]

Some scientific theories are like the calculus from the very first moment of their careers. They are introduced with axioms containing their distinctive terms (or, perhaps, with explicit definitions of those terms) from which theorems are deduced. Often, these theorems obtain their interest because they can be applied to answer an entire class of questions: the theory offers general strategies for answering families of questions, and the theorem supplies information that enables the method to be used in a large subclass of cases. Classical mechanics, for example, gives a problem-solving pattern for addressing the questions of the trajectories of systems of bodies; the two-body theorem enables the method to resolve, at a stroke, an entire class of cases. But some areas of science need not proceed in this way. It is possible for a new theory—a *good* new theory—to be weak in terms of deriving theorems from axioms (or from definitions) and yet strong in its provision of problem-solving patterns for addressing important questions. This could have occurred in the case of the calculus: we can imagine that Newton and Leibniz had seen how to use derivatives and integrals in answering geometric and kinematic questions, but had failed to find any systematic way of computing derivatives and integrals. Moreover, we can expect this to occur in any domain of inquiry in which the problem-solving patterns involve a concept which, by the theory's own lights, can be realized in vast numbers of ways in nature. The primary achievement of plate tectonics lies in showing how such phenomena as mountain building, earthquake zones, geomagnetic variations, and so forth, can be understood in terms of the central concept of an interaction among plates. But the possibilities (and actualities) of plate interaction are numerous enough to defy any easy systematization of them. Thus, when one asks for the axioms of the theory of plate tectonics, the results are disappointing. By assuming that all good scientific theories must be cast in the mold exhibited in *one part of* the calculus, one prepares the way for dismissing such theories as plate tectonics and Darwinian evolutionary theory as poor theories.[118]

Because I believe that traditional views of scientific theories generate impoverished reconstructions of Darwin's theory, reconstructions that make the idea that the *Origin* provides a detailed articulation of that theory quite baffling, I believe that the apparatus I have introduced is necessary to make sense of Darwin's reform of biology.[119] I now want to defend my departure from traditional ideas about theory confirmation. On the account I have offered, Darwin is committed to certain higher-level claims, statements that do not directly describe organisms but which hail certain questions about organisms as important and which identify certain patterns for answering them. The "long argument" of the *Origin* defends these claims by appealing to a conception of the goals of science and arguing that the proposed change of biological practice will lead to the attainment of those goals. For Darwin, a principal goal of science is to achieve understanding, and this goal is attained by providing unified answers to questions about nature. Again and again, in the *Origin* and in his letters, Darwin sounds the theme of unification and advertises the unifying power of his theory.[120] His task in the *Origin* is to defend the unifying power of his problem-solving patterns, showing that it is in principle possible to instantiate them (the analogy with artificial selection), that they are broadly applicable (the lengthy rehearsal of the phenomena to which they can be applied), and that objections to the applicability of the patterns can be turned back (the responses to difficulties with "organs of extreme perfection," the fossil record, and so forth). It is worth asking what kinds of arguments are involved here. As I understand them, Darwin is drawing on premises about the main goals of science and on statements furnished by prior biological practice to argue *deductively* for the conclusion that his proposed modification of biological practice is designed to promote the aims of science.

On the usual approaches to the confirmation of scientific theories, it is assumed that the problem is to show how statements whose truth values can be ascertained on the basis of observation provide support for more general theoretical claims. Most attempts to explain the early acceptance of Darwin's theory presuppose the simplest form of hypothetico-deductivism.[121] Darwin is credited with having used his theory to derive statements which were not antecedently accepted and whose truth values are determinable by observation. Yet, even if hypothetico-deductivism is not hopeless in general, it is certainly inadequate in this particular instance. As I have noted in section 7, the connection between Darwin's theory and observation is doubly loose, and when we understand the character of this connection it is clear why claims about observational predictions are so remarkably absent from the *Origin*. (It is also evident why, a year after the publication of the *Origin*, Darwin should have expressed satisfaction that his ideas should have led to the discovery of new facts; this was not so familiar an occurrence that it could be taken for granted.) The hypothetico-deductivist reading of Darwin can only be sustained if we do not ask too pointedly what the supposed observational predictions are.

The analysis which I have given presents a quite different view of the justification of scientific claims. Darwin did not begin *ab initio*. He inherited from his predecessors and contemporaries a scientific practice which was justified because of its rational emergence from prior scientific practices. Darwin's task was to discover the best way to modify this practice in the light of his own experience. Using a wealth of statements bequeathed to him by earlier scientists, together with his own empirical findings and his understanding of the goals of science, he constructed an argument for abandoning the creationist assumptions favored by most of his contemporaries in favor of a new approach to biological phenomena. Once that approach had been adopted, it supplied a framework within which biologists could begin confirming hypotheses about the details of the history of life, according to the usual canons of inductive support. The heart of the "long argument" is the claim that Darwin's proposals for reforming biology satisfy canons of scientific inquiry that are unsatisfiable on the available rival approaches.

Those who championed Darwin's cause most fervently sometimes praised him by declaring that he had, at last, given biologists a hypothesis by which they could work. With that hypothesis—or, more exactly, that practice—in place, the detailed testing of Darwinian claims (along the lines indicated in section 7) could begin. Many of the tests were carried out by scientists who had already been persuaded by Darwin's "long argument." Their accounts of their reasons reveal the power of Darwin's contention that his theory promoted the goals of science by bringing new questions within its domain. Here, for example, is Asa Gray's comparison of the views of Darwin and Louis Agassiz:

> The one naturalist [Agassiz], perhaps too largely assuming the scientifically unexplained to be inexplicable, views the phenomena only in their supposed relation to the Divine Mind. The other, naturally expecting many of these phenomena to be resolvable under investigation, views them in their relations to one another, and endeavors to explain them as far as he can (can perhaps farther) through natural causes.[122]

The general view of the aims of science on which Darwin's argument turns is even more apparent in Huxley's evaluation of the qualities of Darwin's theory:

> In ultimate analysis everything is incomprehensible, and the whole object of science is simply to reduce the fundamental incomprehensibilities to the smallest possible number.[123]

My departures from the traditional ideas about scientific theories and confirmation are motivated by the desire to construct a perspective from which remarks like these can be taken seriously.

Notes

Earlier versions of parts of this essay were read at the University of Pittsburgh, the University of Michigan, and MIT. I am grateful to those who offered me comments on these occasions. Many of the suggestions I received have helped me to shape the final version of the essay. I am especially grateful to Gerald Massey, Larry Sklar, Peter Railton, and Paul Horwich, all of whom issued independent challenges to my thesis that the Darwinian revolution cannot be understood without departing from orthodox philosophical views of theories and confirmation. Although the present version of the essay does not contain a complete reply to their questions, I hope that it does indicate the lines along which my response would go. My final version of the essay has also benefited from constructive comments from Malcolm Kottler and Elliott Sober. Finally, I would like to thank the American Council of Learned Societies and Harvard University's Museum of Comparative Zoology for their support and hospitality during the time that I was at work on the early stages of this project.

1. Francis Darwin, ed., *The Life and Letters of Charles Darwin* (3 vols., London: John Murray, 1888); reprinted New York: Johnson, 1969, 2:23. (I shall henceforth cite this three-volume work as *Letters*, with volume and pagination.) For Darwin's hesitancy, see also F. Darwin, ed., *More Letters of Charles Darwin*, vol. 1 (London: John Murray, 1903), 47. (I shall henceforth cite this as *More Letters*.)

2. T. H. Huxley, *Darwiniana* (New York: Appleton, 1896), 120. The passage is quoted in *Letters* 3:132. Three years earlier, Darwin was prepared to talk of "the almost universal belief in the evolution (somehow) of species" (*More Letters* 1:304).

3. See *More Letters* 1:202. The passage is quoted on p. 54. Michael Ghiselin has given a concise analysis of Darwin's tactics. See Ghiselin, *The Triumph of the Darwinian Method* (Berkeley: University of California Press, 1969), esp. chapter 6.

4. See *Letters* 2:165, 216 ff., 232, 273, 302–303, 308, 330; 3:11. Two of these passages provide an interesting comparison. In November 1859, Darwin wrote to Huxley that, in advance, he had "fixed in [his] mind three judges, on whose decision [he] determined mentally to abide" (*Letters* 2:232). By early 1860, the trio had expanded to a quartet: Huxley, Hooker, and Lyell had been joined by Asa Gray. Darwin concluded a letter to Gray by writing, "It is the highest possible gratification to me to think that you have found my book worth reading and reflection; for you and three others I put down in my own mind as the judges whose opinions I should value most of all" (*Letters* 2:272). There is no inconsistency here, but the juxtaposition of the passages does reveal Darwin's tact.

5. See, for example, Huxley's account of how he came to espouse Darwin's theory in *Darwiniana*, p. 246. Huxley's letters provide an interesting perspective on his attitude toward Darwin during the 1850s. Before he became a defender of Darwin's theory (and a personal friend of Darwin's) he offered a ranking of the leading British biologists of the day. Darwin figures in the second tier: he is described as "one who might be anything if he had good health." (See L. Huxley, ed., *The Life and Letters of Thomas Henry Huxley*, vol. 1 [London: Macmillan, 1902], 94.)

6. Huxley attributes the rapidity of the Darwinian revolution to the impression made by the *Origin*. See Huxley, *Darwiniana*, 286.

7. *Letters* 2:308; *More Letters* 1:157.

8. Mivart accused Darwin of citing and overpraising the work of his friends and supporters: "We allude to the terms of panegyric with which he introduces the names or opinions of every disciple of evolutionism, while writers of equal eminence, who have not adopted Mr. Darwin's views, are quoted, for the most part, without any commendation." (David Hull, ed., *Darwin and His Critics* [Cambridge, Mass.: Harvard University Press, 1973], 380; I shall henceforth refer to Hull's fine anthology as *Critics*.) Louis Agassiz attributed some of Darwin's success to his "attractive style" (see *Critics*, 440). However, such suggestions to the effect that Darwin substitutes political or aesthetic skill for argument are very rare, even in the writings of his most dedicated opponents.

9. *Origin*, 459. My references to this work will be to Ernst Mayr's edition of it (Cambridge, Mass.: Harvard University Press, 1964). This edition is a facsimile of the first edition (London: John Murray, 1859).

10. To recognize this, one need only follow the correspondence between Darwin and each of Hooker, Huxley, and Lyell during the period around the publication of the *Origin*. (See *Letters* 2:171, 172–173, 175, 197, 206, 221, 225, 228, 230–231.) Darwin is initially concerned about what his "three judges" will think. In a postscript to a letter to A. R. Wallace in November 1859, Darwin announces that "Hooker is a complete convert." He adds: "If I can convert Huxley I shall be content" (Letters 2:221). A letter from Huxley, written ten days later, fulfilled his wish. Huxley wrote: "Since I read von Baer's essays, nine years ago, no work on Natural History Science I have met with has made so great an impression upon me, and I do most heartily thank you for the great store of new views you have given me." (*Letters* 2:230). The correspondence reveals a similar progression in the support of Hooker and of Lyell.

11. Specifically, the excellent and informative accounts offered by Ghiselin (*The Triumph of the Darwinian Method*) and Michael Ruse (*The Darwinian Revolution* [Chicago: University of Chicago Press, 1979]) seem to me to be flawed by suggestions about the structure of Darwin's theory and about the evidence for it that cannot be sustained under careful scrutiny. Ghiselin, following Mayr, celebrates Darwin's use of the "hypotheticodeductive method." As will become clear below, Darwin did not use any such method, and Ghiselin's discussions of his alleged employment of it (*Triumph*, 63, 145–146) rest on very generous assessments of observational consequences obtainable from Darwin's theory. (See section 7, especially notes 98 and 99). In similar fashion, Ruse hails Darwin's arguments as "much closer to the hypothetico-deductive ideal than to anything, say, in Lyell" (*Darwinian Revolution*, 190). Despite the fact that Ruse and Ghiselin have provided the most thorough available analyses of how the Darwinian revolution was won, their narratives ought to foster suspicion in anyone who is prepared to look soberly at Darwin's alleged instantiation of the philosophical views that they attribute to him. To put the point bluntly, if Darwin was a scientist practicing by the canons favored by Ghiselin and Ruse, then he was a poor practitioner. On my account, Darwin will emerge as a successful scientist who answers to rather different methodological ideals.

12. Of course, there are periodic attempts to claim that Darwin was anticipated by earlier thinkers. Some popular candidates are discussed in Loren Eiseley, *Darwin's Century* (New York: Doubleday, 1958). For a lucid assessment of the forerunners and of Darwin's

originality, see Ernst Mayr, *The Growth of Biological Thought* (Cambridge, Mass.: Harvard University Press, 1982), chapters 8, 9, and 11, especially pp. 498–500.

13. For a sustained account of the received view and its problems, there is no better source than Fred Suppe's introduction to *The Structure of Scientific Theories*, 2d ed. (Urbana: University of Illinois Press, 1977).

14. Huxley, *Darwiniana*, 287; Lewontin, "The Units of Selection," *Annual Review of Ecology and Systematics* 1 (1970): 1.

15. Malcolm Kottler has pointed out to me that there is a parallel with the debates about Lyell's geological views, in that what was primarily in dispute was the significance of Lyell's claims about the time-scale. See M. J. S. Rudwick, "The Stategy of Lyell's *Principles of Geology*," *Isis* 61 (1970): 5–33.

16. Owen's complicated views about evolution and natural selection can be reconstructed from his (anonymous) review of the *Origin*. (See *Critics*, 175–213.) The review frequently compares Darwin unfavorably with "Professor Owen."

17. *Origin*, 62; the point is repeated at *Origin*, 319.

18. These are the words of a fellow-traveler, Henry Fawcett, in a review of the *Origin*. See *Critics*, 282. Darwin's American lieutenant, Asa Gray, saw the issue in a similar fashion (Gray, *Darwiniana* [Cambridge, Mass.: Harvard University Press, 1876], 30–31) as did two leading opponents, Mivart and Fleeming Jenkin. Mivart explicitly claims that "the one distinguishing feature of [Darwin's] theory was the all-sufficiency of 'natural selection'" (*Critics*, 356), and Jenkin, with characteristic precision, focuses the dispute as follows:

> All must agree that the process termed natural selection is in universal operation. The followers of Darwin believe that by that process differences might be added even as they are added by man's selection, though more slowly, and that this addition might in time be carried to so great an extent as to produce every known species of animal from one or two pairs, perhaps from organisms of the lowest type. (*Critics*, 303–304)

19. Typical of Darwin's many confessions of ignorance is *Origin*, 13. In a forthcoming book (*The Nature of Selection* [Cambridge, Mass.: Bradford, 1984]), Elliott Sober argues cogently that contemporary evolutionary theory—or, more exactly, the part of evolutionary theory that deals with the genetics of the evolutionary process—can be conceived as a theory of forces. Sober begins from the insight that the Hardy-Weinberg equilibrium principle plays the same role as the law of inertia in Newtonian mechanics, and he goes on to show how evolutionary theory is concerned with the forces (mutation, migration, selection) that perturb equilibrium. In my view, Sober's lucid discussion shows that a part of evolutionary theory that was unavailable to Darwin, can be reconstructed along the lines of traditional philosophical thinking about theories. However, I think that there are large parts of contemporary evolutionary theory—the parts associated with the names of Mayr and Simpson—that extend the work of Darwin, which are not susceptible to the usual philosophical reconstructions, and which can best be understood from the perspective I shall develop below.

20. Mary B. Williams "Deducing the Consequences of Evolution: A Mathematical Model," *Journal of Theoretical Biology* 29 (1970): 343–385. Williams's approach is refined, simplified, and defended by Alex Rosenberg in *The Structure of Biological Science* (Cambridge: Cambridge University Press, 1985).

21. In discussing "descent with modification" in section 16 of her essay, Williams attempts to allow for accidents that might interfere with the workings of selection by weakening the Darwinian conclusion that she is attempting to derive. Because drift is a factor in evolution, one obviously cannot commit Darwinian theory to the inevitable triumph of the fitter. Williams's solution to this problem—if I understand her correctly—is to formulate her theorems about differential perpetuation not as claims about the increasing frequency of the fitter in *every* generation of a lineage, but as increasing frequency in a subsequence of future generations. However, this move will prove of no avail if there are some lineages in which the workings of selection are obliterated by some freak of nature that wipes out all the organisms bearing some advantageous mutation. I see no way to resolve this problem within Williams's framework without explicitly introducing probabilistic considerations, and because I believe that these considerations turn on probabilites which are very difficult to estimate, I suspect that any successful resolution would forfeit much of the precision of Williams's analysis.

22. I should note that there is far more to biogeography than these local concerns. Biologists are also interested in such general issues as why island faunas contain a large number of endemic species. For a classic work of recent theory, see R. H. MacArthur and E. O. Wilson, *The Theory of Island Biogeography* (Princeton, N.J.: Princeton University Press, 1967).

23. *More Letters*, 1:173. Darwin means that there is no group for which he can provide a complete account of the modifications of any characteristic.

24. *Letters* 3:25. I shall have more to say about Darwin's claims of the explanatory unification provided by his theory in later sections of this paper.

25. I have emphasized the idea that the sciences provide bases for acts of explanation, and that those who give explanations adapt material provided by the sciences to the needs of their actual or intended audiences, in the early sections of Philip Kitcher, "Explanatory Unification," *Philosophy of Science* 48 (1981): 507–531. Peter Railton articulates a similar approach by using the notion of an "ideal text," and by suggesting that we often perform explanatory acts by giving incomplete information about ideal texts. See Railton, "Probability, Explanation, and Information," *Synthese* 48 (1981): 233–256. Either approach can be used to elaborate the claims about Darwinian explanation that I make here.

26. *Origin* 352–353. See also *Origin*, 394.

27. *Letters* 1:336; 2:34. *More Letters* 1:118–119. Darwin's elder brother, Erasmus, confessed, "To me the geographical distribution, I mean the relation of islands to continents is the most convincing of the proofs, and the relation of the oldest forms to the existing species" (*Letters* 2:233). Huxley describes the importance of biogeography in his own reception of the theory of evolution at *Darwiniana*, 276. Darwin's official statement of the role of biogeography in his own route to evolution occurs in the opening sentences of the *Origin*.

28. Darwin provides a very clear account of the homology/analogy distinction and is well aware that his theory enables him to refine the concept of homology; see *Origin*, 427, and *More Letters* 1:306.

29. *Origin*, 138.

30. *Origin*, 3. Richard Lewontin perceptively discusses the way in which prior emphasis on the problem of design made the discussion of adaptation central to Darwin's evolutionary thinking, and how this fact, in turn, led Darwin to emphasize the selectionist commitments of his theory. See Lewontin's article "Adaptation," *Scientific American* 239 (1978): 212–231.

31. *Origin*, 472.

32. See the title essay in Stephen Jay Gould's collection *The Panda's Thumb* (New York: Norton, 1980). I think that Gould is correct to view Darwin's sounding of this theme as central to his case for evolution.

33. *Origin*, 186.

34. Darwin's book is entitled *On the Various Contrivances by Which British and Foreign Orchids Are Fertilized by Insects*. It was originally published in 1862. Ghiselin provides a penetrating analysis of the argumentative strategy and of its significance in Darwin's defense of evolution, and I cannot improve on his presentation of the crucial point:

[Darwin] attempted to show, in other words, that structures were not designed with the end in mind of engaging in their present biological role, but rather that they originated as parts adapted to quite different functions. The flower makes use of whatever parts happen to be available, and their availability and utility are purely accidental. (*Triumph*, 137)

Darwin's military characterization of the role of the orchid book is from *More Letters* 1:202.

35. *Origin*, 6. The difficulty of interpreting Darwin's sentence is obvious. Natural selection might be heralded as the force which produces most evolutionary changes, or, perhaps, as the force which produces the most important evolutionary changes. An alternative conception would be to suppose that the modifications produced by other forces are somehow impermanent, so that the large-scale course of evolution follows the trajectory laid down by selection. Finally, one might concentrate on those evolutionary changes which produce new taxa (for example, speciation events), understanding these as being effected by natural selection.

36. Here I mimic the approach of some of the most prominent models in population genetics, assuming discrete generations. An analogous conception is readily definable for the continuous case: the Darwinian history specifies a function that assigns to the properties in F their frequency values at each point of the interval between t_1 and t_2.

37. *Letters* 3:22.

38. I do not claim that this will be possible with respect to all questions of geographical distribution of organisms. There are obviously many instances in which understanding the range of a species will involve recognition of the competitive relations with other species, and in which resolution of the biogeographical questions will turn on issues of coadaptation. Nonetheless, it is sometimes possible to answer biogeographical questions without investigating issues of adaptation, and in such cases the history of descent with modification will suffice.

39. There is something peculiar about referring to all of these as *agents* of evolutionary change. Consider, for example, stochastic factors. These seem to be not so much a force of evolution as *filters* that modify the effects of other evolutionary forces. Similarly, correlation and balance—or, as contemporary evolutionary theory would put it, pleiotropy, linkage, and allometry—are constraints on the working of the force of selection.

40. A position akin to this seems to have been advanced by Niles Eldredge and Joel Cracraft, *Phylogenetic Patterns and the Evolutionary Process* (New York: Columbia University Press, 1980).

41. See, for example, *Origin*, 3, 84, 170.

42. I should note that, although Darwin sometimes invokes use and disuse as a separate agent of evolutionary change, it is quite common for him to reduce it to natural selection: useless structures disappear because organisms which squander resources on developing them are at a disadvantage in the struggle for existence (they will prove inferior to rivals that make better use of the resources in question). The most important nonselectionist strand in Darwin's thinking is the appeal to correlation and balance, an appeal that would now be understood in terms of linkage, allometry, pleiotrophy, and perhaps, some other forms of developmental constraint.

43. Darwin recognizes the importance of chance and rarity in questions about extinction. See *Origin*, 109.

44. Gould and Lewontin, "The Spandrels of San Marco and the Panglossian Paradigm: A Critique of the Adaptationist Programme," *Proceedings of the Royal Society of London* B. 205 (1979): 581–598. While I think that Gould and Lewontin are correct both in their indictment of adaptationism and in their claim that Darwin is less selectionist than he is often read as being, the latter historical thesis seems overstated. As Ernst Mayr has noted ("How to Carry Out the Adaptationist Program," *American Naturalist*, 1983), many of the "alternative forces of evolution" discussed by Darwin are now widely discredited. What does remain—and this is sufficient to enable Gould and Lewontin to make their case—is the variety of factors that Darwin would have lumped together as "correlation and balance." The heart of the Gould and Lewontin critique is the idea that many characteristics of organisms may not be the direct target of selection, but may belong to ensembles that are selected as wholes. The answer to Mayr's charge that the assumption that a character has been shaped by selection is a necessary working hypothesis only to be discarded as a matter of last resort is that there are alternative ways to investigate the evolutionary history of such characters, namely by learning more about ontogeny and genetic connections. Hence, my judgment that the demise of some of Darwin's alternative agents of evolutionary change still allows for a cogent argument against adaptationism.

45. See *More Letters* 1:288ff. However, there are contrary suggestions at *More Letters* 1:306.

46. It is common for commentators to remark parenthetically that the first edition of the *Origin* is the least pluralistic, and I followed this practice in an earlier draft of the present essay. However, as Malcolm Kottler pointed out to me, this quick assessment is misleading. Line-by-line comparison of the editions shows Darwin inserting disjunctions of possible causes of evolutionary changes where he had previously appealed to natural selection alone, but there is no addition of a new mechanism in later editions. Hence, a more global comparison of the editions undermines the idea that Darwin became more pluralistic in response to critics who were skeptical about the efficacy of selection.

47. *Origin*, 197. The continuation explains the presence of the color by appealing to sexual selection, which Darwin often counts as an alternative mechanism to natural selection. To herald Darwin as a card-carrying antiadaptationist it would be more convincing to find him turning to "correlation and balance" at this stage, but the passage I have quoted does show that he is aware of some pitfalls of vulgar adaptationism.

48. Huxley, *Darwiniana*, 97, also quoted in *Letters* 2:231. See also Huxley, *Darwiniana*, 77.

49. This point is made by Peter Vorzimmer, in *Charles Darwin: The Years of Controversy* (Philadelphia: Temple University Press, 1970).

50. *Letters* 2:274–276; 3:33. *More Letters* 1:147–148. *Origin*, 32, 84, 95. For the relation to Darwin's selectionism, see *Origin*, 194, and, for a particularly strong statement of gradualism, *Origin*, 189.

51. *Origin*, 236ff. Darwin's remarks can be interpreted either as advocating group selection or as preliminary gropings towards the notion of inclusive fitness. For a concise and sensitive discussion of them, see Elliott Sober, *The Nature of Selection*.

52. *Origin*, 112. Those with a fine eye for anticipations may see glimmers here of the approach to the phenomenon of sex elaborated in Steven Stanley's *Macroevolution* (San Francisco: Freeman, 1979).

53. *Origin*, 237.

54. I take this to be the approach to scientific change which is tacit in the writings of the logical empiricists (for example, in the works of C. G. Hempel, Rudolf Carnap, and Ernest Nagel), and it is explicit in the discussions of scientific change within the Popper-Lakatos tradition. The approach to scientific theories and scientific change developed by Thomas Kuhn and the different ideas of Sylvain Bromberger mark a radical break with the idea of charting change by looking at changes in sets of statements. As I have argued elsewhere (Kitcher, *The Nature of Mathematical Knowledge* [New York: Oxford University Press,

1983], chapter 7), the concept of a paradigm, introduced by Kuhn in *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1970) serves two different functions: the emphasis on paradigm change is supposed to do justice both to the complexity of what it is that changes when a science evolves and to recognize important units of segmentation in the history of science. One can appreciate the insight behind the first function while remaining agnostic about preferred ways to divide up the history of the sciences. The account that follows owes considerable intellectual debts to Kuhn, although my reading of his seminal book is both idiosyncratic and selective. The influence of Bromberger's ideas about theories should also be apparent. I am especially indebted to his essays "A Theory about the Theory of Theories and about the Theory of Theory," in B. Baumrin, ed., *Philosophy of Science: The Delaware Seminar* (New York: Interscience, 1963); "Questions," *Journal of Philosophy* 63 (1966): 597–606; and "Science and the Forms of Ignorance," in M. Mandelbaum, ed., *Observation and Theory in Science* (Baltimore: Johns Hopkins University Press, 1971).

55. An analogous notion of a mathematical practice was characterized in chapters 7–9 of *The Nature of Mathematical Knowledge*. I have elaborated the idea of a scientific practice and attempted to use it to give an account of intertheoretic relations in genetics, in Kitcher, "1953 and All That: A Tale of Two Sciences," *Philosophical Review* (July 1984); see also chapter 1 of this book.

56. Darwin also deserves credit for introducing new techniques into experimental biology—see, for example, his careful analyses of seed dispersal. But this accomplishment pales in comparison with his more theoretical contributions. I shall not consider here whether he affected the methodological component of the practice of his day. Ruse claims that Darwin was greatly influenced by the influential Victorian philosophers of science, Herschel and Whewell. This evaluation only seems plausible to me if the philosophical standards are left in soft focus. Darwin's critics were vigorous in suggesting that he had deserted the true path of science, and they sometimes based their charges on the remarks of the contemporary philosophers. Many Victorian scientists paid lip service to the methodological dicta of the alleged experts. I think it would be interesting to explore in detail whether the work of men like Lyell and Owen really embodies the standards of the prevailing philosophy of science. In arguing below for an analysis of the testability of Darwin's theory, I think that I provide good reasons for believing that the methodology ascribed by Ruse (in *The Darwinian Revolution*) is not Darwin's.

By contrast, Michael Ghiselin emphasizes the originality of Darwin's methodological ideas, and in this he seems to me to be correct. (See *Triumph*, 4.) Unfortunately, Ghiselin develops his insight by interpreting Darwin as a proponent of the hypothetico-deductive method. The discussion of section 7 will show the difference between Darwin's work and the hypothetico-deductive ideal, and ironically, Ruse's account shows clearly that hypothetico-deductivism was hardly news to the Darwinian community.

57. In the discussion at Pittsburgh, Adolf Grunbaum pointed out to me that reconstructions in the history and philosophy of physics are often sensitive to the need to take into account changes in other components of the practice. This is correct, but it remains true that the most detailed philosophical models simplify the scientific changes by focusing on modification of the class of accepted statements. This is evident both in Bayesian accounts and in the quite different approach defended by Lakatos and his students.

58. In conversation, Kuhn has questioned this claim. His reason for skepticism is that it seems that Darwin affected a radical change in the meaning of the term *species*, and, because of the centrality of this term to biological discourse (at least after Darwin, if not before), this linguistic change can hardly be termed "minor." I reply that the refixing of the referent of "species" given by Darwin barely modified the division of organisms into species taxa. As a result, there was virtually no breakdown of communication between Darwin and his opponents. As the references given in note 18 reveal, everybody could agree on a formulation of the issues. Thus the adjustment of the concept of species seems of far less import than Darwin's radical changes in the biological questions that were addressed and in the explanatory framework accepted as appropriate for biology.

59. Kuhn views such shifts as marking scientific revolutions. See Kuhn, "What Are Scientific Revolutions?" *The Structure of Scientific Revolutions* (MIT Center for Cognitive Science Working Paper), and Kuhn, "Commensurability, Comparability, Communicability," in Peter Asquith and Thomas Nickles, eds., *PSA 1982* [East Lansing, Mich.: Philosophy of Science Association, 1983], 669–688). I have tried to give a different interpretation of the phenomena to which Kuhn has drawn attention. See Kitcher, "Theories, Theorists, and Theoretical Change," *Philosophical Review* 87 (1978): 519–547; Kitcher, "Genes," *British Journal for the Philosophy of Science* 33 (1982): 337–359; Kitcher, "Implications of Incommensurability," *PSA 1982*, pp. 689–703. The construal of the case of phlogiston theory offered in the first and third of these papers reveals what I see as a much larger conceptual shift than anything that is found in the Darwinian revolution.

60. Gray, *Darwiniana*, 25. See also ibid., 286, where the *Origin* is described as "one of the hardest books to master."

61. *Critics*, 249.

62. The urgency of questions about the origins of organic forms is beautifully captured in a passage by Asa Gray. After emphasizing the importance of the search for unity in science, Gray remarks that we allow that "the inquiry transcends our powers, only when all endeavors have failed." See Gray, *Darwiniana*, 78–79.

63. In seeing this as Darwin's main contention, I hope to make it clear why Darwin argues hard against creationism. That the *Origin* is an onslaught on creationism has been clearly shown by Neal Gillespie in *Charles Darwin and the Problem of Creation* (Chicago: University of Chicago Press, 1979). But, as Elliott Sober has pointed out to me, the idea that Darwin's principal target is not a prevailing scientific tradition is something that requires explanation. In a nutshell, the explanation I offer is that there is no previous biological tradition, that Darwin defines biology as an area of inquiry, and that he does so by showing that it is possible to give scientific answers to questions that had previously been thought to lie outside science.

64. See section 7. As I shall argue, one also needs to show which areas of evolutionary theory are most open to test and confirmation. Those who are skeptical about unbridled selectionism (for example, Eldredge, Gould, and Lewontin) can be understood as giving *limited* endorsement to the traditional worry that evolutionary hypotheses are not readily tested and confirmed. This skepticism is quite compatible with acceptance of one of the more cautious theories outlined in section 4.

65. Darwin was very clear about the open-ended character of his theory, and about its potential to give rise to "new sciences" (see Bromberger, "A Theory about the Theory" for some suggestive ideas about the generation of new sciences). Not only is the last chapter of the *Origin* prophetic, but Darwin's letters also indicate his hopes for the future development of biology. See, for example, a letter of 1858 to Hooker:

> Whenever naturalists can look at species changing as certain, what a magnificent field will be open,—on all the laws of variation,—on the genealogy of all living beings,—on their lines of migration, &c &c. (*Letters* 2:128)

66. See *Origin*, 6, 13, 43, 73, 75, 78, 132, 167, 462, 486; Gray, *Darwiniana*, 207, 224–225; Huxley in *Letters* 2:197–198.

67. *More Letters* 1:135.

68. The reconstruction that I shall give is a philosophical elaboration of a scheme for interpreting the reasoning of the *Origin* presented by Huxley (*Darwiniana*, 72) and articulated in an illuminating review article by M. J. S. Hodge ("The Structure and Strategy of Darwin's 'Long Argument,'" *British Journal for the History of Science* 10, [1977]: 237–246).

69. For a capsule version of the argument, see *Origin*, 25.

70. Compare the fate of Wegener's theory of continental drift. Despite its apparent ability to answer certain outstanding questions in meteorology, biogeography, and geology, this theory was widely rejected by the geological community during the 1920s and 1930s precisely because it seemed impossible that there should be a mechanism for moving the continents. It is easy to imagine that, lacking a similar mechanism, Darwin's theory would have been equally vulnerable. Hence, even for the cautious Darwinian who remains agnostic about the causes of evolutionary changes in particular lineages (see above), it is still important to argue for the modifying power of natural selection. (For a concise account of Wegener's theory and its reception, see A. Hallam, *A Revolution in the Earth Sciences* [Oxford: Oxford University Press, 1972].)

71. *Critics*, 131. For similar remarks by Wollaston, Pictet, Haughton, Hopkins, and Jenkin, see *Critics*, 135, 145, 224, 253, 304ff.

72. *Critics*, 239.

73. *Letters* 2:237. Quoted in *Critics*, 229.

74. Darwin's main argument stresses the unifying power of his schemata (see the references in note 97). But he cannot resist giving subsidiary arguments. So, for example, the early chapters of the *Origin* campaign against the idea that there is a natural boundary around species. This subsidiary argument becomes very important to certain versions of Darwin's theory—for example, those which take a nominalistic approach to species and emphasize evolutionary gradualism. However, it is incidental to the more cautious versions of Darwinism.

75. *Letters* 2:166–167.

76. *Origin*, 349–350.

77. *Origin*, 318–319, 339–341, 394, 440–444, 452–453, 471–480.

78. There is obviously a tricky issue here. One might hold that the important unification is accomplished by a minimal version of Darwinian theory—for example, one which did not deploy the notion of natural selection in constructing particular Darwinian histories—and that the more ambitious claims about the power of selection and evolutionary gradualism are either otiose, or at best, only weakly supported. This was a position adopted by some scientists in Darwin's day, and it is accepted by some contemporary theorists. I take it to be a merit of my analysis of Darwin's theory that it focusses the disagreement on this traditionally vexed question.

79. *Origin*, 333. Note that a minimal version of Darwin's theory will achieve the explanatory dividends cited here.

80. *Critics*, 267. See also ibid., 268–269.

81. *Letters* 2:286 provides one example of this practice.

82. *Origin*, 380–381.

83. *Origin*, 111ff., especially 128.

84. *Origin*, 154–156; *Origin*, 150–154; *Origin*, 159–167.

85. *Origin*, 167.

86. Various forms of the problem of the poverty of the fossil record are posed forcefully at *Origin*, 280–281, 287–288, 292, 301–303. Wollaston saw the state of the fossil record as "the gravest of all objections" to Darwin's theory, but he noted Darwin's frankness in admitting the facts (*Critics*, 136).

87. Darwin's critics seized on the point. There are clear formulations of it by Wollaston, Pictet, and Jenkin (*Critics*, 133, 150, 314), and it became a major theme of Mivart's attack on the power of selection (see *On the Genesis of Species* [London: Macmillan, 1871]). Darwin anticipated the objection, and attempted to meet it, at *Origin*, 188–189, and he gives his strongest endorsement of selectionist gradualism in this context. For a lucid contemporary discussion of the objection and its resolution, see Ernst Mayr, "The Emergence of Evolutionary Novelties," in his *Evolution and the Diversity of Life* (Cambridge, Mass.: Harvard University Press, 1976).

88. This would then open the way for the use of different methods of explanation that might subsequently replace the evolutionary schemata. Many of Darwin's critics argued that Darwin was himself committed to two different "explanatory principles"

> [Darwin] says that life has been breathed into the first primordial form. It is our creative force that has done it. Consequently, both theories acknowledge the existence of the two *forces* and differ only to the degree that each is employed. (*Critics*, 147–148)

Several of Darwin's critics can be viewed as limiting the power of selection to make room for creation, so that Darwin's softening of his naturalism about the origins of life may have been something of a tactical mistake.

89. *Critics*, 339.

90. Most prominent are chapters 6 and 7, which address the issues of "organs of extreme perfection" and the problems that arise in connection with instincts and social behavior, and chapter 9 "on the imperfection of the geological record."

91. Of course, this presupposes that there are ways of choosing among rival Darwinian histories. I shall consider this question in section 7.

92. The modification of linguistic usage can readily be understood from the perspective advanced in my papers cited in footnote 59. Huxley describes the conceptual shift in language which is very close to the terms of my analysis: he speaks of the criteria for species "falling apart" (Huxley, *Darwiniana*, 44).

93. Darwin, *Origin*, 484 ff.; Huxley in *Letters* 2:197–199.

94. *Critics*, 144.

95. *Critics*, 264.

96. *Critics*, 319; see also *Critics*, 342.

97. For a discussion of spurious unification, see the final section of my essay "Explanatory Unification." The emphasis on the power of Darwin's theory to explain biological phenomena by unifying them is notable both in his own writings and in those of some of his supporters. Again and again, Darwin admits his inability to "prove" his large claims about the history of life and describes himself as accepting those claims because they "explain large classes of facts." Moreover, he characterizes the explanations he has given of these classes (the affinities of organisms, the details of biogeography, and so forth) by suggesting that, on his theory, the "facts fall into groups." I take my analysis to make explicit the ideas that are tacit in numerous passages: *Letters* 2:13, 29, 78–79, 110, 121–122, 210–211, 240, 285, 327, 355, 362; 3:25, 44 (which advances similar arguments in favor of the abortive theory of pangenesis), 74; *More Letters* 1:139–140, 150, 156, 184; *Origin*, 188, 243–244, 482. Similar ideas are advanced by Huxley (in *Letters* 2:254, and in *The Life and Letters of Thomas Henry Huxley* 1:479), and by Asa Gray (*Darwiniana*, 19, 78–81, 88, 90, 195–196).

98. *Origin*, 201. Compare also *Origin*, 189, on the formation of complex organs. Ghiselin alludes to the former passage (*Triumph*, 63), using it to buttress his claim that Darwin obeyed the "falsification principle."

> A theory is refutable, hence scientific, if it is possible to give *even one* conceivable state of affairs incompatible with its truth. Such conditions were specified by Darwin himself, who observed that the existence of an organ in one species, solely "for" the benefit of another species, would be totally destructive of his theory.

In paraphrasing Darwin, Ghiselin has dropped the crucial reference to *proving* the existence of the organ in question. Of course, it is trivial to state conditions that are incompatible with the truth of a theory T—the condition that T is false will do the trick. What is nontrivial is to find conditions that can be independently checked. As noted in the text, this is the trouble with Darwin's example, for it is far from obvious that there is any way to show that an organ was formed solely for the good of another species.

99. This suggestion about the possible falsification of evolutionary theory is made by Douglas Futuyma, *Science on Trial* (New York: Pantheon, 1983), 170. Besides the difficulty noted in the text, Futuyma's proposal faces the problem that what would have to be given up would be a particular claim about the history of life. It would remain logically possible to embrace Darwinian evolutionary theory and contend that humans are evolutionarily very old. Although this is hardly a plausible position, it does show that Futuyma's case does not directly falsify the theory whose falsifiability is at issue. For a discussion of the misleading use of a falsifiability criterion in debates about evolutionary theory, see chapter 2 of Kitcher, *Abusing Science* (Cambridge, Mass.: MIT Press, 1982).

100. *Letters* 2:80. See also *Letters* 2:72, 81. Ghiselin provides a very perceptive discussion of Darwin's use of independent evidence in advancing geological claims. See *Triumph*, 20, 40. The credentials of Forbes's theory are discussed in the *Origin*, 357–358.

101. *Origin*, 358ff.

102. *Letter* 2:93.

103. For example, in the early days of Mendelian genetics (that is, in the first decade of the century), many biologists believed that the new findings about heredity were incompatible with Darwin's theory of evolution by natural selection. The conflict was resolved by the development of theoretical population genetics. An excellent account of the difficulty and its resolution is given in William B. Provine, *The Origins of Theoretical Population Genetics* (Chicago: University of Chicago Press, 1971). Similarly, the investigation of the maintenance of variation in natural populations has led some biologists to advance claims about the importance of random factors in evolution. (From the perspective of the present article, neutralist proposals are minimal versions of Darwinism rather than accounts of "nonDarwinian evolution.") The controversies about variation are brilliantly analyzed in R. C. Lewontin, *The Genetic Basis of Evolutionary Change* (New York: Columbia University Press, 1974). One major challenge to classical Darwinian ideas that does not emerge from the development of new sciences of heredity and variation is the current proposal (due to Gould, Eldredge, Stanley, and others) that Darwinian gradualism should give way to punctuated equilibrium. The distinctions made in section iv of this paper offer a framework for seeing what is at stake in this dispute.

104. It is not so evident that we are similarly constrained when we attempt to construct selectionist histories for revealing certain characteristics as adaptations. There are some cases in which adaptationist hypotheses prove testable—the classic examples are industrial melanism in moths and cowbird parasitism of oropendulas. However, those who are skeptical of the adaptationist program can best be understood as arguing that, in many cases where selectionist stories are told, there are no ways of finding independent checks on the hypotheses that ascribe past advantages.

105. This approach to the question of analyzing the ways in which scientific theories (or programs of research) come to be rationally rejected absorbs a familiar Duhemian insight.

(See Pierre Duhem, *The Aim and Structure of Physical Theory* [New York: Atheneum, 1954]; I have discussed the implications of Duhem's point for naive falsificationism in chapter 2 of Kitcher, *Abusing Science*.) The approach is common to the work of thinkers as distinct as Kuhn and Lakatos.

106. See Thomas Kuhn, *The Copernican Revolution* (Cambridge, Mass.: Harvard University Press, 1957), and R. W. Schofield, *Mechanism and Materialism* (Princeton, N.J.: Princeton University Press, 1969). Although it is common to suppose that old theories are only rationally rejected when a new rival is available, these examples seem to me to show that the conventional wisdom is mistaken. It is perfectly reasonable to give up a decaying theory and to *look for* something better. I suspect that this is just what Copernicus did in the early decades of the sixteenth century, and what the first field theorists did in the mid-eighteenth century.

107. It is important to recognize that evolutionary theory itself supplies some constraints. Previously accepted problem solutions are not sacrosanct, but one cannot legitimately abandon a sizeable collection of past successes in the interests of fashioning one new solution.

108. The above cases are generated according to the following principle. There are two degrees of freedom: theory plus context (including work in ancillary sciences and past work on the theory itself) may allow a greater or lesser number of available solutions; the observational evidence may be positive, neutral, or negative. Quite evidently, the treatment is simplified by the fact that only one form of implication is considered (claims about the existence of particular organisms). It would surely be necessary to consider a broader class of implications from Darwinian histories if one were to assess the testability of selectionist histories.

109. Here I apply a methodological principle discussed in some detail by Richard Boyd in "Realism, Underdetermination, and a Casual Theory of Evidence," *Nous* 7 (1973): 1–12.

110. Of course, this is a classic result of Bayesian confirmation theory (which is not to say that it is unobtainable on rival approaches). The most famous example is the confirmation of the wave theory of light through observation of the Poisson bright spot. A similar example occurred in the early days of evolutionary theory (see below).

111. The brief analysis given in the text enables me to explain the excitement of some recent theoretical work in evolutionary theory. Great breakthroughs can be made if a scientist shows that problems for which no solution is available can be resolved by applying a new schema, one that was readily available within the theoretical framework but never antecedently recognized. The introduction of the notions of inclusive fitness and of evolutionary stable strategy seem to me to be breakthroughs of this type. W. D. Hamilton and John Maynard Smith demonstrated how fitness could be gained in subtle ways, so that characteristics which had previously seemed to be insusceptible of selectionist explanation could now be viewed as the products of natural selection. (See Hamilton, "The Genetical Theory of Social Behavior," *Journal of Theoretical Biology* 7 [1964]: 1–16, 17–51; Maynard Smith, *Evolution and the Theory of Games* [Cambridge, Mass.: Cambridge University Press, 1982].) Other subtle analyses of fitness that permit the broader application of selectionist schemata have been given by R. L. Trivers, E. O. Wilson, and George Oster. In all these cases, the initial situation reveals a characteristic of some organismic group for which there is no available selectionist Darwinian history. After certain unobvious ramifications of the concept of fitness have been exposed, one sees that it is possible to instantiate a selectionist schema. *It does not follow that the correct explanation of the presence of the characteristic is by appealing to natural selection.* For there may be a number of rival selectionist and nonselectionist explanations which cannot be discriminated by the evidence so far collected (or even by the evidence that one is in a position to collect). One may welcome the extension of the class of Darwinian problem-solving techniques while remaining agnostic about the application of

the new techniques to particular cases. In the terms of the analysis of the text, the breakthrough takes us from a position in which there was no available selectionist solution to a problem (although nonselectionist solutions may have been available) to a position of type *C* or type *E*.

112. See Huxley, *Darwiniana*, 234. Darwin is much more restrained (compare *Letters* 3:6). Perhaps he needed no further convincing.

113. *Origin*, 193.

114. *Origin*, 194. Note that Darwin's selectionism here is purely gratuitous. The issue can be treated without making any reference to natural selection at all. All that is needed is convergence *by some force or other*.

115. *Letters* 3:352–353.

116. *Origin*, 6.

117. See chapter 10 of *The Nature of Mathematical Knowledge* and the introduction to Judith V. Grabiner, *The Origins of Cauchy's Rigorous Calculus* (Cambridge, Mass.: MIT Press, 1981).

118. Of course, it would be possible to abandon the word *theory* in application to problematic areas of science like biology and geology. We could retain the classic idea of a theory as a deductively organized set of statements whose axioms include general laws. There would be no harm in this so long as we recognized the existence of scientific disciplines with important, articulated accomplishments in which there are no theories, and so long as we freed ourselves from any prejudice to the effect that sciences which have theories are somehow superior.

Although I have developed my account primarily by opposing the residue of the "received view" of scientific theories, I think it right to note that the so-called semantic conception of theories seems no more adequate in characterizing Darwin's evolutionary theory. On the "semantic conception," a theory is given by specifying a type of system, and the theorist then derives from the specifications conclusions about all systems of the type or about interesting subtypes. (This is a simplification of views presented with considerable sophistication by Joseph Sneed, Bas van Fraassen, Fred Suppe, and others. See Sneed, *The Logical Structure of Mathematical Physics* [Dordrecht: Reidel, 1979]; van Fraassen, *The Scientific Image* [Oxford: Oxford University Press, 1980]; and pp. 221–230 of Suppe's introduction to *The Structure of Scientific Theories*.) If we now try to apply this approach to the case of Darwin, we encounter problems that are exactly parallel to those that beset the "received view." Darwin's specification of a type of system is as elusive as the set of axioms of Darwinian evolutionary theory. Moreover, seeing him as deriving results about "types of evolutionary systems" seems to me to have no more connection with the project of the *Origin* than an interpretation which supposes that the *Origin* contains derivations from axioms. Perhaps a more refined version of the "semantic conception" has the resources to overcome these problems, but it has appeared to me to be more promising to begin anew, and to develop an account of Darwin's theory which has some direct relevance to his text.

119. An additional advantage of my approach is that it makes sense of the varying commitments that we find in Darwin, and the varying commitments that are available for his successors. Many previous approaches to the *Origin* seem to err by failing to recognize the differences among the theories I have distinguished in section iv and the variations in argumentative strength that can be assembled for each of them. See, for example, Elisabeth A. Lloyd, "The Nature of Darwin's Support for the Theory of Natural Selection," *Philosophy of Science* 50 (1983): 112–129.

120. See note 97 and the reference cited therein.

121. This, of course, has been my complaint about the excellent and illuminating accounts offered by Ghiselin and Ruse. I have chosen their works for criticism not because they are alone in applying a simple hypothetico-deductive methodology but because their application is made in the context of insightful historical analyses.

122. Gray, *Darwiniana*, 16.

123. Huxley, *Darwiniana*, 165. It is interesting to compare Michael Friedman's account of why unification produces understanding: "Our total picture of nature is simplified via a reduction in the number of independent phenomena that we have to accept as ultimate" ("Explanation and Scientific Understanding," *Journal of Philosophy* 71 [1974], pp. 5–19, 18).