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Believing Where We Cannot Prove

Opening moves

Simple distinctions come all too easily. Frequently we open the way for later puzzlement by restricting the options we take to be available. So, for example, in contrasting science and religion, we often operate with a simple pair of categories. On one side there is science, proof, and certainty; on the other, religion, conjecture, and faith.

The opening lines of Tennyson's *In Memoriam* offer an eloquent statement of the contrast:

Strong Son of God, immortal love,
Whom we, that have not seen Thy face,
By faith, and faith alone, embrace,
Believing where we cannot prove.

A principal theme of Tennyson's great poem is his struggle to maintain faith in the face of what seems to be powerful scientific evidence. Tennyson had read a popular work by Robert Chambers, *Vestiges of the Natural History of Creation,* and he was greatly troubled by the account of the course of life on earth that the book contains. *In Memoriam* reveals a man trying to believe where he cannot prove, a man haunted by the thought that the proofs may be against him.

Like Tennyson, contemporary Creationists accept the traditional contrast between science and religion. But where Tennyson agonized, they attack. While they are less eloquent, they are supremely confident of their own solution. They open their onslaught on evolutionary theory by denying that it is a science. In *The Troubled Waters of Evolution,* Henry Morris characterizes evolutionary theory as maintaining that large amounts of time are required for evolution to produce "new kinds." As a result, we should not expect to see such "new kinds"

emerging. Morris comments, "Creationists in turn insist that this belief is not scientific evidence but only a statement of faith. The evolutionist seems to be saying, Of course, we cannot really *prove* evolution, since this requires ages of time, and so, therefore, you should accept it as a proved fact of science! Creationists regard this as an odd type of logic, which would be entirely unacceptable in any other field of science" (Morris 1974b, 16). David Watson makes a similar point in comparing Darwin with Galileo: "So here is the difference between Darwin and Galileo: Galileo set a demonstrable *fact* against a few words of Bible poetry which the Church at that time had understood in an obviously naive way; Darwin set an unprovable *theory* against eleven chapters of straightforward history which cannot be reinterpreted in any satisfactory way" (Watson 1976, 46).

The idea that evolution is conjecture, faith, or "philosophy" pervades Creationist writings (Morris 1974a, 4–8; Morris 1974b, 22, 172; Wysong 1976, 43–45; Gish 1979, 11–13, 26, 186; Wilder-Smith 1981, 7–8). It is absolutely crucial to their case for equal time for "scientific" Creationism. This ploy has succeeded in winning important adherents to the Creationist cause. As he prepared to defend Arkansas law 590, Attorney General Steven Clark echoed the Creationist judgment. "Evolution," he said, "is just a theory." Similar words have been heard in Congress. William Dannemeyer, a congressman from California, introduced a bill to limit funding to the Smithsonian with the following words: "If the theory of evolution is just that—a theory—and if that theory can be regarded as a religion . . . then it occurs to this Member that other Members might prefer it not to be given exclusive or top billing in our Nation's most famous museum but equal billing or perhaps no billing at all."

In their attempt to show that evolution is not science, Creationists receive help from the least likely sources. Great scientists sometimes claim that certain facts about the past evolution of organisms are "demonstrated" or "indubitable" (Simpson 1953, 70, 371; also Mayr 1976, 9). But Creationists also can (and do) quote scientists who characterize evolution as "dogma" and contend that there is no conclusive proof of evolutionary theory (Matthews 1971, xi; Birch and Ehrlich 1967, 349; quoted in Gish 1979, 15–16; similar passages are quoted in Morris 1974a, 6–8, and in Wysong 1976, 44). Evolution is not part of science because, as evolutionary biologists themselves concede, science demands proof, and, as other biologists point out, proof of evolution is not forthcoming.

The rest of the Creationist argument flows easily. We educate our children in evolutionary theory as if it were a proven fact. We subscribe officially, in our school system, to one faith—an atheistic, materialistic faith—ignoring rival beliefs. Antireligious educators deform the minds of children, warping them to accept as gospel a doctrine that has no more scientific support than the true Gospel. The very least that should be done is to allow for both alternatives to be presented.

We should reject the Creationists' gambit. Eminent scientists notwithstanding, science is not a body of demonstrated truths. Virtually all of science is an exercise in believing where we cannot prove. Yet, scientific conclusions are not embraced by faith alone. Tennyson's dichotomy was too simple.

Inconclusive evidence

Sometimes we seem to have conclusive reasons for accepting a statement as true. It is hard to doubt that $2 + 2 = 4$. If, unlike Lord Kelvin's ideal mathematician, we do not find it obvious that

$$\int_{-\infty}^{+\infty} e^{-x^2}\, dx = \sqrt{\pi},$$

at least the elementary parts of mathematics appear to command our agreement. The direct evidence of our senses seems equally compelling. If I see the pen with which I am writing, holding it firmly in my unclouded view, how can I doubt that it exists? The talented mathematician who has proved a theorem and the keen-eyed witness of an episode furnish our ideals of certainty in knowledge. What they tell us can be engraved in stone, for there is no cause for worry that it will need to be modified.

Yet, in another mood, one that seems "deeper" or more "philosophical," skeptical doubts begin to creep in. Is there really anything of which we are so certain that later evidence could not give us reason to change our minds? Even when we think about mathematical proof, can we not imagine that new discoveries may cast doubt on the cogency of our reasoning? (The history of mathematics reveals that sometimes what seems for all the world like a proof may have a false conclusion.) Is it not possible that the most careful observer may have missed something? Or that the witness brought preconceptions to the observation that subtly biased what was reported? Are we not *always* fallible?

I am mildly sympathetic to the skeptic's worries. Complete certainty is best seen as an ideal toward which we strive and that is rarely, if ever, attained. Conclusive evidence always eludes us. Yet even if we ignore skeptical complaints and imagine that we are sometimes lucky enough to have conclusive reasons for accepting a claim as true, we

should not include scientific reasoning among our paradigms of proof. Fallibility is the hallmark of science.

This point should not be so surprising. The trouble is that we frequently forget it in discussing contemporary science. When we turn to the history of science, however, our fallibility stares us in the face. The history of the natural sciences is strewn with the corpses of intricately organized theories, each of which had, in its day, considerable evidence in its favor. When we look at the confident defenders of those theories we should see anticipations of ourselves. The eighteenth-century scientists who believed that heat is a "subtle fluid," the atomic theorists who maintained that water molecules are compounded out of one atom of hydrogen and one of oxygen, the biochemists who identified protein as the genetic material, and the geologists who thought that continents cannot move were neither unintelligent nor ill informed. Given the evidence available to them, they were eminently reasonable in drawing their conclusions. History proved them wrong. It did not show that they were unjustified.

Why is science fallible? Scientific investigation aims to disclose the general principles that govern the workings of the universe. These principles are not intended merely to summarize what some select groups of humans have witnessed. Natural science is not just natural history. It is vastly more ambitious. Science offers us laws that are supposed to hold universally, and it advances claims about things that are beyond our power to observe. The nuclear physicist who sets down the law governing a particular type of radioactive decay is attempting to state a truth that holds throughout the entire cosmos and also to describe the behavior of things that we cannot even see. Yet, of necessity, the physicist's ultimate evidence is highly restricted. Like the rest of us, scientists are confined to a relatively small region of space and time and equipped with limited and imperfect senses.

How is science possible at all? How are we able to have any confidence about the distant regions of the cosmos and the invisible realm that lies behind the surfaces of ordinary things? The answer is complicated. Natural science follows intricate and ingenious procedures for fathoming the secrets of the universe. Scientists devise ways of obtaining especially revealing evidence. They single out some of the things we are able to see as crucial clues to the way that nature works. These clues are used to answer questions that cannot be addressed by direct observation. Scientific theories, even those that are most respected and most successful, rest on indirect arguments from the observational evidence. New discoveries can always call those arguments into question, showing scientists that the observed data should be understood in a different way, that they have misread their evidence.

But scientists often forget the fallibility of their enterprise. This is not just absentmindedness or wishful thinking. During the heyday of a scientific theory, so much evidence may support the theory, so many observational clues may seem to attest to its truth, that the idea that it could be overthrown appears ludicrous. In addition, the theory may provide ways of identifying quickly what is inaccessible to our unaided senses. Electron microscopes and cloud chambers are obvious examples of those extensions of our perceptual system that theories can inspire. Trained biochemists will talk quite naturally of seeing large molecules, and it is easy to overlook the fact that they are presupposing a massive body of theory in describing what they "see." If that theory were to be amended, even in subtle ways, then the descriptions of the "observed characteristics" of large molecules might have to be given up. Nor should we pride ourselves that the enormous successes of contemporary science secure us against future amendments. No theory in the history of science enjoyed a more spectacular career than Newton's mechanics. Yet Newton's ideas had to give way to Einstein's.

When practicing scientists are reminded of these straightforward points, they frequently adopt what the philosopher George Berkeley called a "forlorn skepticism." From the idea of science as certain and infallible, they jump to a cynical description of their endeavors. Science is sometimes held to be a game played with arbitrary rules, an irrational acceptance of dogma, an enterprise based ultimately on faith. Once we have appreciated the fallibility of natural science and recognized its sources, we can move beyond the simple opposition of proof and faith. Between these extremes lies the vast field of cases in which we believe something on the basis of good—even excellent—but inconclusive evidence.

If we want to emphasize the fact that what scientists believe today may have to be revised in the light of observations made tomorrow, then we can describe all our science as "theory." But the description should not confuse us. To concede that evolutionary biology is a theory is not to suppose that there are alternatives to it that are equally worthy of a place in our curriculum. All theories are revisable, but not all theories are equal. Even though our present evidence does not *prove* that evolutionary biology—or quantum physics, or plate tectonics, or any other theory—is true, evolutionary biologists will maintain that the present evidence is overwhelmingly in favor of their theory and overwhelmingly against its supposed rivals. Their enthusiastic assertions that evolution is a proven fact can be charitably understood as claims

that the (admittedly inconclusive) evidence we have for evolutionary theory is as good as we ever obtain for any theory in any field of science.

Hence the Creationist try for a quick Fools' Mate can easily be avoided. Creationists attempt to draw a line between evolutionary biology and the rest of science by remarking that large-scale evolution cannot be observed. This tactic fails. Large-scale evolution is no more inaccessible to observation than nuclear reactions or the molecular composition of water. For the Creationists to succeed in divorcing evolutionary biology from the rest of science, they need to argue that evolutionary theory is less well supported by the evidence than are theories in, for example, physics and chemistry. It will come as no surprise to learn that they try to do this. To assess the merits of their arguments we need a deeper understanding of the logic of inconclusive justification. We shall begin with a simple and popular idea: Scientific theories earn our acceptance by making successful predictions.

Predictive success

Imagine that somebody puts forward a new theory about the origins of hay fever. The theory makes a number of startling predictions concerning connections that we would not have thought worth investigating. For example, it tells us that people who develop hay fever invariably secrete a particular substance in certain fatty tissues and that anyone who eats rhubarb as a child never develops hay fever. The theory predicts things that initially appear fantastic. Suppose that we check up on these predictions and find that they are borne out by clinical tests. Would we not begin to believe—and believe reasonably—that the theory was *at least* on the right track?

This example illustrates a pattern of reasoning that is familiar in the history of science. Theories win support by producing claims about what can be observed, claims that would not have seemed plausible prior to the advancement of the theory, but that are in fact found to be true when we make the appropriate observations. A classic (real) example is Pascal's confirmation of Torricelli's hypothesis that we live at the bottom of an ocean of air that presses down upon us. Pascal reasoned that if Torricelli's hypothesis were true, then air pressure should decrease at higher altitudes (because at higher altitudes we are closer to the "surface" of the atmosphere, so that the length of the column of air that presses down is shorter). Accordingly, he sent his brother-in-law to the top of a mountain to make some barometric measurements. Pascal's clever working out of the observational predictions of Torricelli's theory led to a dramatic predictive success for the theory.

The idea of predictive success has encouraged a popular picture of science. (We shall see later that this picture, while popular, is not terribly accurate.) Philosophers sometimes regard a theory as a collection of claims or statements. Some of these statements offer generalizations about the features of particular, recondite things (genes, atoms, gravitational force, quasars, and the like). These statements are used to infer statements whose truth or falsity can be decided by observation. (This appears to be just what Pascal did.) Statements belonging to this second group are called the *observational consequences* of the theory. Theories are supported when we find that their observational consequences (those that we have checked) are true. The credentials of a theory are damaged if we discover that some of its observational consequences are false.

We can make the idea more precise by being clearer about the inferences involved. Those who talk of inferring observational predictions from our theories think that we can *deduce* from the statements of the theory, and from those statements alone, some predictions whose accuracy we can check by direct observation. Deductive inference is well understood. The fundamental idea of deductive inference is this: We say that a statement *S* is a valid deductive consequence of a group of statements if and only if it is *impossible* that all the statements in the group should be true and that *S* should be false; alternatively, *S* is a valid deductive consequence (or, more simply, a valid consequence) of a group of statements if and only if it would be self-contradictory to assert all the statements in the group and to deny *S*.

It will be helpful to make the idea of valid consequence more familiar with some examples. Consider the statements "All lovers of baseball dislike George Steinbrenner" and "George Steinbrenner loves baseball." The statement "George Steinbrenner dislikes himself" is a deductively valid consequence of these two statements. For it is impossible that the first two should be true and the third false. However, in claiming that this is a case of deductively valid consequence, we do not commit ourselves to maintaining that *any* of the statements is true. (Perhaps there are some ardent baseball fans who admire Steinbrenner. Perhaps Steinbrenner himself has no time for the game.) What deductive validity means is that the truth of the first two statements would guarantee the truth of the third; that is, *if* the first two *were* true, then the third would have to be true.

Another example will help rule out other misunderstandings. Here are two statements: "Shortly after noon on January 1, 1982, in the Oval Office, a jelly bean was released from rest more than two feet above any surface"; "Shortly after noon on January 1, 1982, in the Oval Office, a jelly bean fell." Is the second statement a deductively valid consequence of the first? You might think that it is, on the grounds that it would have been impossible for the unfortunate object to have been released and not to have fallen. In one sense this is correct, but that is not the sense of impossibility that deductive logicians have in mind. Strictly speaking, it is not *impossible* for the jellybean to have been released without falling; we can imagine, for example, that the law of gravity might suddenly cease to operate. We do not *contradict* ourselves when we assert that the jellybean was released but deny that it fell; we simply refuse to accept the law of gravity (or some other relevant physical fact).

Thus, *S* is a deductively valid consequence of a group of statements if and only if there is *absolutely no possibility* that all the statements in the group should be true and *S* should be false. This conception allows us to state the popular view of theory and prediction more precisely. Theories are collections of statements. The observational consequences of a theory are statements that have to be true if the statements belonging to the theory are all true. These observational consequences also have to be statements whose truth or falsity can be ascertained by direct observation.

My initial discussion of predictive success presented the rough idea that, when we find the observational consequences of a theory to be true, our findings bring credit to the theory. Conversely, discovery that some observational consequences of a theory are false was viewed as damaging. We can now make the second point much more precise. Any theory that has a false observational consequence must contain some false statement (or statements). For if all the statements in the theory were true, then, according to the standard definitions of *deductive validity* and *observational consequence*, any observational consequence would also have to be true. Hence, if a theory is found to have a false observational consequence, we must conclude that one or more statements of the theory is false.

This means that theories can be conclusively falsified, through the discovery that they have false observational consequences. Some philosophers, most notably Sir Karl Popper (Popper 1959; 1963), have taken this point to have enormous significance for our understanding of science. According to Popper, the essence of a scientific theory is that it should be *falsifiable*. That is, if the theory is false, then it must be possible to show that it is false. Now, if a theory has utterly no observational consequences, it would be extraordinarily difficult to unmask that theory as false. So, to be a genuine scientific theory, a group of statements must have observational consequences. It is important to realize that Popper is not suggesting that every good theory must be false. The difference between being falsifiable and being false is like the difference between being vulnerable and actually being hurt. A good scientific theory should not be false. Rather, it must have observational consequences that could reveal the theory as mistaken if the experiments give the wrong results.

While these ideas about theory testing may seem strange in their formal attire, they emerge quite frequently in discussions of science. They also find their way into the creation-evolution debate.

Predictive failure

From the beginning, evolutionary theory has been charged with just about every possible type of predictive failure. Critics of the theory have argued that (a) the theory makes no predictions (it is unfalsifiable and so fails Popper's criterion for science), (b) the theory makes false predictions (it is falsified), (c) the theory does not make the kinds of predictions it ought to make (the observations and experiments that evolutionary theorists undertake have no bearing on the theory). Many critics, including several Creationists (Morris 1974a; Gish 1979; Wysong 1976), manage to advance all these objections in the same work. This is somewhat surprising, since points (a) and (b) are, of course, mutually contradictory.

The first objection is vitally important to the Creationist cause. Their opponents frequently insist that Creationism fails the crucial test for a scientific theory. The hypothesis that all kinds of organisms were separately fashioned by some "originator" is unfalsifiable (Gould 1981b). Creationists retort that they can play the same game equally well. *Any* hypothesis about the origins of life, including that advanced by evolutionary theory, is not subject to falsification. Hence we cannot justify a decision to teach evolutionary theory and not to teach Creationism by appealing to the Popperian criterion for genuine science.

The allegation that evolutionary theory fails to make any predictions is a completely predictable episode in any Creationist discussion of evolution. Often the point is made by appeal to the authority of Popper. Here are two sample passages:

> The outstanding philosopher of science, Karl Popper, though himself an evolutionist, pointed out cogently that evolution, no less than creation, is untestable and thus unprovable. (Morris 1974b, 80)

> Thus, for a theory to qualify as a scientific theory, it must be supported by events, processes or properties which can be observed, and the theory must be useful in predicting the outcome of future natural phenomena or laboratory experiments. An additional limitation usually imposed is that the theory must be capable of falsification. That is, it must be possible to conceive some experiment, the failure of which would disprove the theory.
>
> It is on the basis of such criteria that most evolutionists insist that creation be refused consideration as a possible explanation for origins. Creation has not been witnessed by human observers, it cannot be tested experimentally, and as a theory it is nonfalsifiable.
>
> The general theory of evolution also fails to meet all three of these criteria, however. (Gish 1979, 13)

These passages, and many others (for example, Morris 1974a, 150; Morris 1975, 9; Moore 1974, 9; Wilder-Smith 1981, 133), draw on the picture of science sketched above. It is not clear that the Creationists really understand the philosophical views that they attempt to apply. Gish presents the most articulate discussion of the falsifiability criterion. Yet he muddles the issue by describing falsifiability as an "additional limitation" beyond predictive power. (The previous section shows that theories that make predictions are automatically falsifiable.) Nevertheless, the Creationist challenge is a serious one, and, if it could not be met, evolutionary theory would be in trouble.

Creationists buttress their charge of unfalsifiability with further objections. They are aware that biologists frequently look as though they are engaged in observations and experiments. Creationists would allow that researchers in biology sometimes make discoveries. What they deny is that the discoveries support evolutionary theory. They claim that laboratory manipulations fail to teach us about evolution in nature: "Even if modern scientists should ever actually achieve the artificial creation of life from non-life, or of higher kinds from lower kinds, in the laboratory, this would not *prove* in any way that such changes did, or even could, take place in the past by random natural processes" (Morris 1974a, 6). The standards of evidence to be applied to evolutionary biology have suddenly been raised. In this area of inquiry, it is not sufficient that a theory yield observational consequences whose truth or falsity can be decided in the laboratory. Creationists demand special kinds of predictions, and will dismiss as irrelevant any laboratory evidence that evolutionary theorists produce. [In this way, they try to defend point (c).]

Oddly enough, however, the most popular supplement to the charge that evolutionary theory is unfalsifiable is a determined effort to falsify it [point (b)]. Creationists cannot resist arguing that the theory is actually falsified. Some of them, Morris and Gish, for example, recognize the tension between the two objections. They try to paper over the problem by claiming that evolutionary theory and the Creationist account are both "models." Each "model" would "naturally" incline us to expect certain observational results. A favorite Creationist ploy is to draw up tables in which these "predictions" are compared. When we look at the tables we find that the evolutionary expectations are confounded. By contrast, the Creationist "model" leads us to anticipate features of the world that are actually there. Faced with such adverse results, the benighted evolutionary biologist is portrayed as struggling to "explain away" the findings by whatever means he can invent.

Morris's own practice of this form of evolution baiting can serve as an example. Morris constructs a table (1974a, 12; see facing page) whose function is to indicate "the predictions that would probably be made in several important categories" (1974a, 12). Morris admits magnanimously that "these primary models may be modified by secondary [additional] assumptions to fit certain conditions. For example, the basic evolution model may be extended to include harmful, as well as beneficial, mutations, but this is not a natural prediction of the basic concept of evolution" (1974a, 13). The idea that the "natural predictions" of the evolution "model" are at odds with the phenomena is used to suggest that evolutionary biologists are forced to desperate measures to protect their "faith." As Morris triumphantly concludes, "The data must be *explained* by the evolutionist, but they are *predicted* by the creationist" (1974a, 13).

The careful reader ought to be puzzled. If Morris really thinks that evolutionary theory has been falsified, why does he not say so? Of course, he would have to admit that the theory is falsifiable. Seemingly, however, a staunch Creationist should be delighted to abandon a relatively abstruse point about unfalsifiability in favor of a clear-cut refutation. The truth of the matter is that the alleged refutations fail. No evolutionary theorist will grant that (for example) the theory predicts that the fossil record should show "innumerable transitions." Instead, paleontologists will point out that we can deduce conclusions about what we should find in the rocks only if we make assumptions about the fossilization process. Morris makes highly dubious assumptions, hails them as "natural," and then announces that the "natural predictions" of the theory have been defeated.

(This example suggests a method for coping with Morris's "table of natural predictions." Each of these predictions can be deduced from evolutionary theory only if the theory is extended by adding extra assumptions. Morris saddles evolutionary theory with faulty additional

Category	*Evolution Model*	*Creation Model*
Structure of Natural Law	Constantly changing	Invariable
Galactic Universe	Galaxies changing	Galaxies constant
Structure of Stars	Stars changing into other types	Stars unchanged
Other Heavenly Bodies	Building up	Breaking down
Types of Rock Formations	Different in different "Ages"	Similar in all "Ages"
Appearance of Life	Life evolving from non-life	Life only from life
Array of Organisms	Continuum of Organisms	Distinct Kinds of Organisms
Appearance of Kinds of Life	New Kinds Appearing	No New Kinds Appearing
Mutations in Organisms	Beneficial	Harmful
Natural Selection	Creative Process	Conservative Process
Age of Earth	Extremely Old	Probably Young
Fossil Record	Innumerable Transitions	Systematic Gaps
Appearance of Man	Ape-Human Intermediates	No Ape-Human Intermediates
Nature of Man	Quantitatively Superior to Animals	Qualitatively Distinct from Animals
Origin of Civilization	Slow and Gradual	Contemporaneous with Man

claims. These are the source of the false predictions. Later, I shall show this in detail for some of Morris's "natural predictions" and the similar difficulties raised by other Creationists [Gish 1979, 53–54; Wysong 1976, 421–426].)

To make a serious assessment of these broad Creationist charges, we must begin by asking some basic methodological questions. We cannot decide whether evolutionary biologists are guilty of trying to save their theory by using ad hoc assumptions (new and implausible claims dreamed up for the sole purpose of protecting some cherished ideas) unless we have some way of deciding when a proposal is ad hoc. Similarly, we cannot make a reasoned response to the charge that laboratory experiments are irrelevant, or to the fundamental objection that evolutionary theory is unfalsifiable, unless we have a firmer grasp of the relation between theory and evidence.

Naive falsificationism

The time has come to tell a dreadful secret. While the picture of scientific testing sketched above continues to be influential among scientists, it has been shown to be seriously incorrect. (To give my profession its due, historians and philosophers of science have been trying to let this particular cat out of the bag for at least thirty years. See, for example, Hempel 1951; Quine 1952.) Important work in the history of science has made it increasingly clear that no major scientific theory has ever exemplified the relation between theory and evidence that the traditional model presents.

What is wrong with the old picture? Answer: Either it debars most of what we take to be science from counting as science or it allows virtually anything to count. On the traditional view of "theory," textbook cases of scientific theories turn out to be unfalsifiable. Suppose we identify Newtonian mechanics with Newton's three laws of motion plus the law of gravitation. What observational consequences can we deduce from these four statements? You might think that we could deduce that if, as the (undoubtedly apocryphal) story alleges, an apple became detached from a branch above where Newton was sitting, the apple would have fallen on his head. But this does not follow at all. To see why not, it is only necessary to recognize that the failure of this alleged prediction would not force us to deny any of the four statements of the theory. All we need do is assume that some other forces were at work that overcame the force of gravity and caused the apple to depart from its usual trajectory. So, given this simple way of applying Popper's criterion, Newtonian mechanics would be

unfalsifiable. The same would go for any other scientific theory. Hence none of what we normally take to be science would count as science. (I might note that Popper is aware of this problem and has suggestions of his own as to how it should be overcome. However, what concerns me here are the *applications* of Popper's ideas, that are made by Creationists, as well as by scientists in their professional debates.)

The example of the last paragraph suggests an obvious remedy. Instead of thinking about theories in the simple way just illustrated, we might take them to be far more elaborate. Newton's laws (the three laws of motion and the law of gravitation) are *embedded* in Newtonian mechanics. They form the core of the theory, but do not constitute the whole of it. Newtonian mechanics also contains supplementary assumptions, telling us, for example, that for certain special systems the effects of forces other than gravity are negligible. This more elaborate collection of statements *does* have observational consequences and *is* falsifiable.

But the remedy fails. Imagine that we attempt to expose some self-styled spiritual teacher as an overpaid fraud. We try to point out that the teacher's central message—"Quietness is wholeness in the center of stillness"—is unfalsifiable. The teacher cheerfully admits that, taken by itself, this profound doctrine yields no observational consequences. He then points out that, by themselves, the central statements of scientific theories are also incapable of generating observational consequences. Alas, if all that is demanded is that a doctrine be embedded in a group of statements with observational consequences, our imagined guru will easily slither off the hook. He replies, "You have forgotten that my doctrine has many other claims. For example, I believe that if quietness is wholeness in the center of stillness, then flowers bloom in the spring, bees gather pollen, and blinkered defenders of so-called science raise futile objections to the world's spiritual benefactors. You will see that these three predictions are borne out by experience. Of course, there are countless others. Perhaps when you see how my central message yields so much evident truth, you will recognize the wealth of evidence behind my claim. Quietness is wholeness in the center of stillness."

More formally, the trouble is that *any* statement can be coupled with other statements to produce observational consequences. Given any doctrine D, and any statement O that records the result of an observation, we can enable D to "predict" O by adding the extra assumption, "If D, then O." (In the example, D is "Quietness is wholeness in the center of stillness"; examples of O would be statements describing the blooming of particular flowers in the spring, the pollen gathering of specific bees, and so forth.)

The falsifiability criterion adopted from Popper—which I shall call the *naive falsificationist* criterion—is hopelessly flawed. It runs aground on a fundamental fact about the relation between theory and prediction: On their own, individual scientific laws, or the small groups of laws that are often identified as theories, do not have observational consequences. This crucial point about theories was first understood by the great historian and philosopher of science Pierre Duhem. Duhem saw clearly that individual scientific claims do not, and cannot, confront the evidence one by one. Rather, in his picturesque phrase, "Hypotheses are tested in bundles." Besides ruling out the possibility of testing an individual scientific theory (read, small group of laws), Duhem's insight has another startling consequence. We can only test relatively large bundles of claims. What this means is that when our experiments go awry we are not logically compelled to select any particular claim as the culprit. We can always save a cherished hypothesis from refutation by rejecting (however implausibly) one of the other members of the bundle. Of course, this is exactly what I did in the illustration of Newton and the apple above. Faced with disappointing results, I suggested that we could abandon the (tacit) additional claim that no large forces besides gravity were operating on the apple.

Creationists wheel out the ancient warhorse of naive falsificationism so that they can bolster their charge that evolutionary theory is not a science. The (very) brief course in deductive logic plus the whirlwind tour through naive falsificationism and its pitfalls enable us to see what is at the bottom of this seemingly important criticism. Creationists can appeal to naive falsificationism to show that evolution is not a science. But, given the traditional picture of theory and evidence I have sketched, one can appeal to naive falsificationism to show that *any* science is not a science. So, as with the charge that evolutionary change is unobservable, Creationists have again failed to find some "fault" of evolution not shared with every other science. (And, as we shall see, Creationists like some sciences, especially thermodynamics.) Consistent application of naive falsificationism can show that anybody's favorite science (whether it be quantum physics, molecular biology, or whatever) is not science. Of course, what this shows is that the naive falsificationist criterion is a very poor test of genuine science. To be fair, this point can cut both ways. Scientists who charge that "scientific" Creationism is unfalsifiable are not insulting the theory as much as they think.

Successful science

Despite the inadequacies of naive falsificationism, there is surely something right in the idea that a science can succeed only if it can fail. An invulnerable "science" would not be science at all. To achieve a more adequate understanding of how a science can succeed and how it runs the risk of failure, let us look at one of the most successful sciences and at a famous episode in its development.

Newtonian celestial mechanics is one of the star turns in the history of science. Among its numerous achievements were convincing explanations of the orbits of most of the known planets. Newton and his successors viewed the solar system as a collection of bodies subject only to gravitational interactions; they used the law of gravitation and the laws of motion to compute the orbits. (Bodies whose effects were negligible in any particular case would be disregarded. For example, the gravitational attraction due to Mercury would not be considered in working out the orbit of Saturn.) The results usually tallied beautifully with astronomical observations. But one case proved difficult. The outermost known planet, Uranus, stubbornly followed an orbit that diverged from the best computations. By the early nineteenth century it was clear that something was wrong. Either astronomers erred in treating the solar system as a Newtonian gravitational system or there was some particular difficulty in applying the general method to Uranus.

Perhaps the most naive of falsificationists would have recommended that the central claim of Newtonian mechanics—the claim that the solar system is a Newtonian gravitational system—be abandoned. But there was obviously a more sensible strategy. Astronomers faced one problematical planet, and they asked themselves what made Uranus so difficult. Two of them, John Adams and Urbain Leverrier, came up with an answer. They proposed (independently) that there was a hitherto unobserved planet beyond Uranus. They computed the orbit of the postulated planet and demonstrated that the anomalies of the motion of Uranus could be explained if a planet followed this path. There was a straightforward way to test their proposal. Astronomers began to look for the new planet. Within a few years, the planet—Neptune—was found.

I will extract several morals from this success story. The first concerns an issue we originally encountered in Morris's "table of natural predictions:" What is the proper use of auxiliary hypotheses? Adams and Leverrier saved the central claim of Newtonian celestial mechanics by offering an auxiliary hypothesis. They maintained that there were more things in the heavens than had been dreamed of in previous natural philosophy. The anomalies in the orbit of Uranus could be explained on the assumption of an extra planet. Adams and Leverrier worked out the exact orbit of that planet so that they could provide a detailed account of the perturbations—and so that they could tell their fellow astronomers where to look for Neptune. Thus, their auxiliary hypothesis was *independently testable*. The evidence for Neptune's existence was not just the anomalous motion of Uranus. The hypothesis could be checked independently of any assumptions about Uranus or about the correctness of Newtonian celestial mechanics—by making telescopic observations.

Since hypotheses are always tested in bundles, this method of checking presupposed other assumptions, in particular, the optical principles that justify the use of telescopes. The crucial point is that, while hypotheses are always tested in bundles, they can be tested in *different* bundles. An auxiliary hypothesis ought to be testable independently of the particular problem it is introduced to solve, independently of the theory it is designed to save.

While it is obvious in retrospect—indeed it was obvious at the time—that the problem with Uranus should not be construed as "falsifying" celestial mechanics, it is worth asking explicitly why scientists should have clung to Newton's theory in the face of this difficulty. The answer is not just that nothing succeeds like success, and that Newton's theory had been strikingly successful in calculating the orbits of the other planets. The crucial point concerns the way in which Newton's successes had been achieved. Newton was no opportunist, using one batch of assumptions to cope with Mercury, and then moving on to new devices to handle Venus. Celestial mechanics was a remarkably *unified* theory. It solved problems by invoking the same pattern of reasoning, or *problem-solving strategy*, again and again: From a specification of the positions of the bodies under study, use the law of gravitation to calculate the forces acting; from a statement of the forces acting, use the laws of dynamics to compute the equations of motion; solve the equations of motion to obtain the motions of the bodies. This single pattern of reasoning was applied in case after case to yield conclusions that were independently found to be correct.

At a higher level, celestial mechanics was itself contained in a broader theory. Newtonian physics, as a whole, was remarkably unified. It offered a strategy for solving a diverse collection of problems. Faced with *any* question about motion, the Newtonian suggestion was the same: Find the forces acting, from the forces and the laws of dynamics work out the equations of motion, and solve the equations of motion. The method was employed in a broad range of cases. The revolutions of planets, the motions of projectiles, tidal cycles and pendulum oscillations—all fell to the same problem-solving strategy.

We can draw a second moral. A science should be *unified*. A thriving science is not a gerrymandered patchwork but a coherent whole. Good theories consist of just one problem-solving strategy, or a small family of problem-solving strategies, that can be applied to a wide range of problems. The theory succeeds as it is able to encompass more and more problem areas. Failure looms when the basic problem-solving strategy (or strategies) can resolve almost none of the problems in its intended domain without the "aid" of untestable auxiliary hypotheses.

Despite the vast successes of his theory, Newton hoped for more. He envisaged a time when scientists would recognize other force laws, akin to the law of gravitation, so that other branches of physics could model themselves after celestial mechanics. In addition, he suggested that many physical questions that are not ostensibly about motion—questions about heat and about chemical combination, for example—could be reduced to problems of motion. *Principia*, Newton's masterpiece, not only offered a theory; it also advertised a program:

> I wish we could derive the rest of the phenomena of Nature by the same kind of reasoning from mechanical principles, for I am induced by many reasons to suspect that they may all depend upon certain forces by which the particles of bodies, by some causes hitherto unknown, are either mutually impelled towards one another, and cohere in regular figures, or are repelled and recede from one another. These forces being unknown, philosophers have hitherto attempted the search of Nature in vain; but I hope the principles here laid down will afford some light either to this or some truer method of philosophy. (Newton 1687/Motte-Cajori 1960, xviii)

Newton's message was clear. His own work only began the task of applying an immensely fruitful, unifying idea.

Newton's successors were moved, quite justifiably, to extend the theory he had offered. They attempted to show how Newton's main problem-solving strategy could be applied to a broader range of physical phenomena. During the eighteenth and nineteenth centuries, the search for understanding of the forces of nature was carried into hydrodynamics, optics, chemistry, and the studies of heat, elasticity, electricity, and magnetism. Not all of these endeavors were equally successful. Nevertheless, Newton's directive fostered the rise of some important new sciences.

The final moral I want to draw from this brief look at Newtonian physics concerns *fecundity*. A great scientific theory, like Newton's, opens up new areas of research. Celestial mechanics led to the discovery of a previously unknown planet. Newtonian physics as a whole led to the development of previously unknown sciences. Because a theory presents a new way of looking at the world, it can lead us to ask new questions, and so to embark on new and fruitful lines of inquiry. Of the many flaws with the earlier picture of theories as sets of statements, none is more important than the misleading presentation of sciences as static and insular. Typically, a flourishing science is incomplete. At any time, it raises more questions than it can currently answer. But incompleteness is no vice. On the contrary, incompleteness is the mother of fecundity. Unresolved problems present challenges that enable a theory to flower in unanticipated ways. They also make the theory hostage to future developments. A good theory should be productive; it should raise new questions and presume that those questions can be answered without giving up its problem-solving strategies.

I have highlighted three characteristics of successful science. *Independent testability* is achieved when it is possible to test auxiliary hypotheses independently of the particular cases for which they are introduced. *Unification* is the result of applying a small family of problem-solving strategies to a broad class of cases. *Fecundity* grows out of incompleteness when a theory opens up new and profitable lines of investigation. Given these marks of successful science, it is easy to see how sciences can fall short, and how some doctrines can do so badly that they fail to count as science at all. A scientific theory begins to wither if some of its auxiliary assumptions can be saved from refutation only by rendering them untestable; or if its problem-solving strategies become a hodgepodge, a collection of unrelated methods, each designed for a separate recalcitrant case; or if the promise of the theory just fizzles, the few questions it raises leading only to dead ends.

When does a doctrine fail to be a science? If a doctrine fails sufficiently abjectly as a science, then it fails to be a science. Where bad science becomes egregious enough, pseudoscience begins. The example of Newtonian physics shows us how to replace the simple (and incorrect) naive falsificationist criterion with a battery of tests. Do the doctrine's problem-solving strategies encounter recurrent difficulties in a significant range of cases? Are the problem-solving strategies an opportunistic collection of unmotivated and unrelated methods? Does the doctrine have too cozy a relationship with auxiliary hypotheses, applying its strategies with claims that can be "tested" only in their applications? Does the doctrine refuse to follow up on unresolved problems, airily dismissing them as "exceptional cases"? Does the doctrine restrict the domain of its methods, forswearing excursions into new areas of investigation where embarrassing questions might arise? If all, or many, of these tests are positive, then the doctrine is not a poor scientific theory. It is not a scientific theory at all.

The account of successful science that I have given not only enables us to replace the naive falsificationist criterion with something better. It also provides a deeper understanding of how theories are justified. Predictive success is one important way in which a theory can win our acceptance. But it is not the only way. In general, theories earn their laurels by solving problems—providing answers that can be independently recognized as correct—and by their fruitfulness. Making a prediction is answering a special kind of question. The astronomers who used celestial mechanics to predict the motion of Mars were answering the question of where Mars would be found. Yet, very frequently, our questions do not concern *what* occurs, but *why* it occurs. We already know that something happens and we want an explanation. Science offers us explanations by setting the phenomena within a unified framework. Using a widely applicable problem-solving strategy, together with independently confirmed auxiliary hypotheses, scientists show that what happened was to be expected. It was known before Newton that the orbits of the planets are approximately elliptical. One of the great achievements of Newton's celestial mechanics was to apply its problem-solving strategy to deduce that the orbit of any planet will be approximately elliptical, thereby explaining the shape of the orbits. In general, science is at least as concerned with reducing the number of unexplained phenomena as it is with generating correct predictions.

The most global Creationist attack on evolutionary theory is the claim that evolution is not a science. If this claim were correct, then the dispute about what to teach in high school science classes would be over. In earlier parts of this chapter, we saw how Creationists were able to launch their broad criticisms. If one accepts the idea that science requires proof, or if one adopts the naive falsificationist criterion, then the theory of evolution—and every other scientific theory—will turn out not to be a part of science. So Creationist standards for science imply that there is no science to be taught.

However, we have seen that Creationist standards rest on a very poor understanding of science. In light of a clearer picture of the scientific enterprise, I have provided a more realistic group of tests for good science, bad science, and pseudoscience. Using this more sophisticated approach, I now want to address seriously the global

Creationist questions about the theory of evolution. Is it a pseudoscience? Is it a poor science? Or is it a great science? These are very important questions, for the appropriateness of granting equal time to Creation "science" depends, in part, on whether it can be regarded as the equal of the theory of evolution.

Darwin's daring

The heart of Darwinian evolutionary theory is a family of problem-solving strategies, related by their common employment of a particular style of historical narrative. A *Darwinian history* is a piece of reasoning of the following general form. The first step consists in a description of an ancestral population of organisms. The reasoning proceeds by tracing the modification of the population through subsequent generations, showing how characteristics were selected, inherited, and became prevalent. (As I noted in chapter 1, natural selection is taken to be the primary—but not the only—force of evolutionary change.)

Reasoning like this can be used to answer a host of biological questions. Suppose that we want to know why a contemporary species manifests a particular trait. We can answer that question by supplying a Darwinian history that describes the emergence of that trait. Equally, we can use Darwinian histories to answer questions about relationships among groups of organisms. One way to explain why two species share a common feature is to trace their descent from a common ancestor. Questions of biogeography can be addressed in a similar way. We can explain why we find contemporary organisms where we do by following the course of their historical modifications and migrations. Finally, we can tackle problems about extinction by showing how characteristics that had enabled organisms to thrive were no longer advantageous when the environment (or the competition) changed. In all these cases, we find related strategies for solving problems. The history of the development of populations, understood in terms of variation, competition, selection, and inheritance, is used to shed light on broad classes of biological phenomena.

The questions that evolutionary theory has addressed are so numerous that any sample is bound to omit important types. The following short selection undoubtedly reflects the idiosyncrasy of my interests: Why do orchids have such intricate internal structures? Why are male birds of paradise so brightly colored? Why do some reptilian precursors of mammals have enormous "sails" on their backs? Why do bats typically roost upside down? Why are the hemoglobins of humans and apes so similar? Why are there no marsupial analogues of seals and whales? Why is the mammalian fauna of Madagascar so distinctive? Why did the large, carnivorous ground birds of South America become extinct? Why is the sex ratio in most species one to one (although it is markedly different in some species of insects)? Answers to these questions, employing Darwinian histories, can be found in works written by contemporary Darwinian biologists. Those works contain answers to a myriad of other questions of the same general types. Darwinian histories are constructed again and again to illuminate the characteristics of contemporary organisms, to account for the similarities and differences among species, to explain why the forms preserved in the fossil record emerged and became extinct, to cast light on the geographical distribution of animals and plants.

We can see the theory in action by taking a brief look at one of these examples. The island of Madagascar, off the east coast of Africa, supports a peculiar group of mammals. Many of these mammals are endemic. Among them is a group of relatively small insectivorous mammals, the *tenrecs*. All tenrecs share certain features that mark them out as relatively primitive mammals. They have very poor vision, their excretory system is rudimentary, the testes in the male are carried within the body, their capacity for regulating their body temperature is poor compared with that of most mammals. Yet, on their simple and rudimentary body plan, specialized characteristics have often been imposed. Some tenrecs have the hedgehog's method of defense. Others have the forelimbs characteristic of moles. There are climbing tenrecs that resemble the shrews, and there are tenrecs that defend themselves by attempting to stick their quills into a would-be predator. Hedgehogs, moles, tree shrews, and porcupines do not inhabit Madagascar. But they seem to have their imitators. (These are examples of convergent evolution, cases in which unrelated organisms take on some of the same characteristics.) Why are these peculiar animals found on Madagascar, and nowhere else?

A straightforward evolutionary story makes sense of what we observe. In the late Mesozoic or early Cenozoic, small, primitive, insectivorous mammals rafted across the Mozambique channel and colonized Madagascar. Later the channel widened and Madagascar became inaccessible to the more advanced mammals that evolved on the mainland. Hence the early colonists developed without competition from advanced mainland forms and without pressure from many of the normal predators who make life difficult for small mammals. The tenrecs have been relatively protected. In the absence of rigorous competition, they have preserved their simple body plan, and they have exploited unoccupied niches, which are filled elsewhere by more advanced creatures. Tenrecs have gone up the trees and burrowed in the ground because those are good ways to make a living and because they have had nobody but one another to contend with.

The same kind of story can be told again and again to answer all sorts of questions about all sorts of living things. Evolutionary theory is unified because so many diverse questions—questions as various as those I listed—can be addressed by advancing Darwinian histories. Moreover, these narratives constantly make claims that are subject to independent check. Here are four examples from the case of the triumphant tenrecs. (1) The explanation presupposes that Madagascar has drifted away from the east coast of Africa. That is something that can be checked by using geological criteria for the movement of landmasses, criteria that are independent of biology. (2) The account claims that the tenrecs would have been able to raft across the Mozambique channel, but that the present channel constitutes a barrier to more advanced mammals (small rodents). These claims could be tested by looking to see whether the animals in question can disperse across channels of the appropriate sizes. (3) The narrative assumes that the specialized methods of defense offered advantages against the predators that were present in Madagascar. Studies of animal interactions can test whether the particular defenses are effective against local predators. (4) Central to the explanatory account is the thesis that the tenrecs are related. If this is so, then studies of the minute details of tenrec anatomy should reveal many common features, and the structures of proteins ought to be similar. In particular, the tenrecs ought to be much more like one another than they are like hedgehogs, shrews, or moles.

Looking at one example, or even at a small number of examples, does not really convey the strength of evolutionary theory. The same patterns of reasoning can be applied again and again, in book after book, monograph after monograph, article after article. Yet the particular successes in dealing with details of natural history, numerous though they are, do not exhaust the accomplishments of the theory. Darwin's original theory—the problem-solving strategies advanced in the *Origin*, which are, in essence, those just described—gave rise to important new areas of scientific investigation. Evolutionary theory has been remarkably fruitful.

Darwin not only provided a scheme for unifying the diversity of life. He also gave a structure to our ignorance. After Darwin, it was important to resolve general issues about the presuppositions of Darwinian histories. The way in which biology should proceed had been made admirably plain, and it was clear that biologists had to tackle

questions for which they had, as yet, no answers. How do new characteristics arise in populations? What are the mechanisms of inheritance? How do characteristics become fixed in populations? What criteria decide when a characteristic confers some advantage on its possessor? What interactions among populations of organisms affect the adaptive value of characteristics? With respect to all of these questions, Darwin was forced to confess ignorance. By raising them, his theory pointed the way to its further articulation.

Since Darwin's day, biologists have contributed parts of evolutionary theory that help to answer these important questions. Geneticists have advanced our understanding of the transmission of characteristics between generations and have enabled us to see how new characteristics can arise. Population geneticists have analyzed the variation present in populations of organisms; they have suggested how that variation is maintained and have specified ways in which characteristics can be fixed or eliminated. Workers in morphology and physiology have helped us to see how variations of particular kinds might yield advantages in particular environments. Ecologists have studied the ways in which interactions among populations can affect survival and fecundity.

The moral is obvious. Darwin gambled. He trusted that the questions he left open would be answered by independent biological sciences and that the deliverances of these sciences would be consistent with the presuppositions of Darwinian histories. Because of the breadth of his vision, Darwin made his theory vulnerable from a number of different directions. To take just one example, it could have turned out the mechanisms of heredity would have made it impossible for advantageous variations to be preserved and to spread. Indeed, earlier in this century, many biologists felt that the emerging views about inheritance did not fit into Darwin's picture, and the fortunes of Darwinian evolutionary theory were on the wane.

When we look at the last 120 years of the history of biology, it is impossible to ignore the fecundity of Darwin's ideas. Not only have inquiries into the presuppositions of Darwinian histories yielded new theoretical disciplines (like population genetics), but the problem-solving strategies have been extended to cover phenomena that initially appeared troublesome. One recent triumph has been the development of explanations for social interactions among animals. Behavior involving one animal's promotion of the good of others seems initially to pose a problem for evolutionary theory. How can we construct Darwinian histories for the emergence of such behavior? W. D. Hamilton's concept of inclusive fitness, and the deployment of gametheoretic ideas by R. L. Trivers and John Maynard Smith, revealed how the difficulty could be resolved by a clever extension of traditional Darwinian concepts.

Yet puzzles remain. One problem is the existence of sex. When an organism forms gametes (sperm cells or egg cells) there is a meiotic division, so that in sexual reproduction only half of an organism's genes are transmitted to each of its progeny. Because of this "cost of meiosis," it is hard to see how genotypes for sexual reproduction might have become prevalent. (Apparently, they will spread only half as fast as their asexual rivals.) So why is there sex? We do not have a compelling answer to the question. Despite some ingenious suggestions by orthodox Darwinians (notably G. C. Williams 1975; John Maynard Smith 1978), there is no convincing Darwinian history for the emergence of sexual reproduction. However, evolutionary theorists believe that the problem will be solved without abandoning the main Darwinian insights—just as early nineteenth-century astronomers believed that the problem of the motion of Uranus could be overcome without major modification of Newton's celestial mechanics.

The comparison is apt. Like Newton's physics in 1800, evolutionary theory today rests on a huge record of successes. In both cases, we find a unified theory whose problem-solving strategies are applied to illuminate a host of diverse phenomena. Both theories offer problem solutions that can be subjected to rigorous independent checks. Both open up new lines of inquiry and have a history of surmounting apparent obstacles. The virtues of successful science are clearly displayed in both.

There is a simple way to put the point. Darwin is the Newton of biology. Evolutionary theory is not simply an area of science that has had some success at solving problems. It has unified biology and it has inspired important biological disciplines. Darwin himself appreciated the unification achieved by his theory and its promise of further development (Darwin 1859/Mayr 1964, 188, 253–254, 484–486). Over a century later, at the beginning of his authoritative account of current views of species and their origins, Ernst Mayr explained how that promise had been fulfilled: "The theory of evolution is quite rightly called the greatest unifying theory in biology. The diversity of organisms, similarities and differences between kinds of organisms, patterns of distribution and behavior, adaptation and interaction, all this was merely a bewildering chaos of facts until given meaning by the evolutionary theory" (Mayr 1970, 1). Dobzhansky put the point even more concisely: "Nothing in biology makes sense except in the light of evolution" (Dobzhansky 1973).

3

Darwin Redux

The tautology objection

Consistency is not the hobgoblin of the Creationist mind. However, Creationist motives are not hard to discern. Creationists would like to show that the theory of evolution is simply false. To this end, they hunt diligently for observational findings that would cast doubt on parts of the theory, and they revel in unresolved disputes among evolutionary biologists. On the other hand, the claim that evolution is untestable is essential to their case for equal time. Since "[n]either evolution nor creation is accessible to the scientific method," both can aptly be described as "religion." So both "models of origins" should be taught in the classroom (Morris 1974b, 172–173). Given the strong pull of their objectives, Creationists throw consistency to the winds and try to press both types of criticism.

Because the point about untestability is so vital to their cause, they try to support it in a number of different ways. In the last chapter, we saw how the criterion of naive falsificationism (adapted from Popper) failed to pin any "fault" on evolution that is not shared by every other science. But the appeal to this criterion does not exhaust Creationist efforts.

One popular way to try to argue that evolutionary theory is not testable, that it differs from real science, can be called the *tautology objection*. The central idea is that evolutionary theory reduces to an empty truism. This is another ancient warhorse, which Creationists ride with zeal. Here are some typical Creationist versions:

> [Macbeth] points out that although evolutionists have abandoned classical Darwinism, the modern synthetic theory they have proposed as a substitute is equally inadequate to explain progressive change as