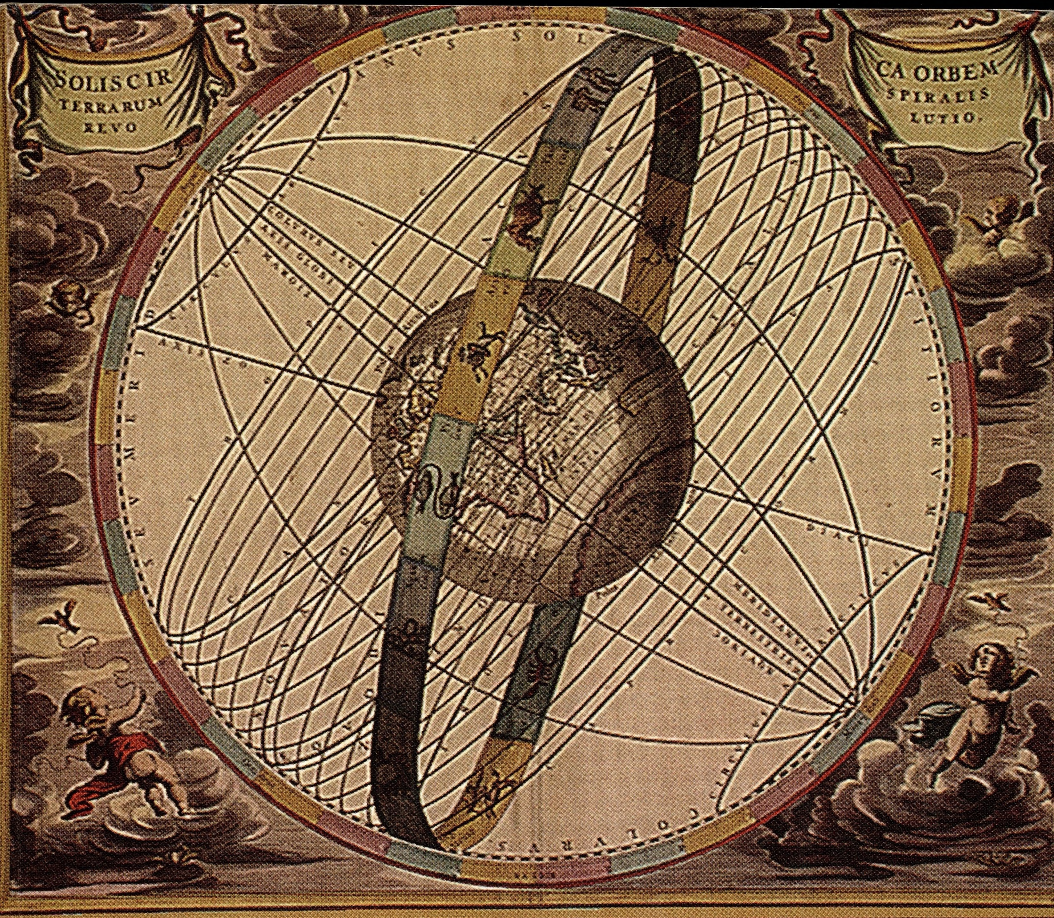


PHILOSOPHY OF SCIENCE

THE CENTRAL ISSUES

MARTIN CURD & J. A. COVER

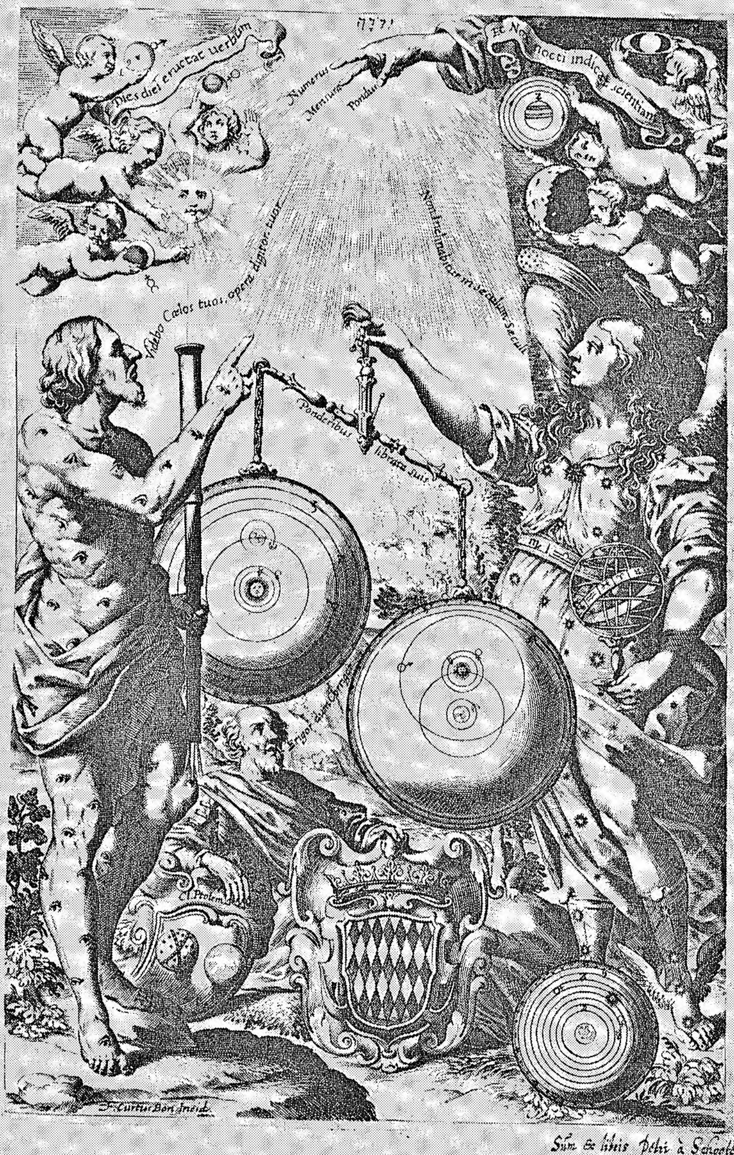
Philosophy of Science

About the Frontispiece

The frontispiece is from *Almagestum Novum* (Bologna, 1651), by the Jesuit astronomer Giambattista Riccioli (1598–1671). In the decades following the condemnation of Galileo, Riccioli was an ardent critic of the Copernican theory. He conceded that Galileo's discovery of the phases of Venus had refuted the Ptolemaic system but insisted that Tycho Brahe's system, in which the earth does not move, captured all the observational and mathematical advantages of the Copernican theory with none of its physical and theological disadvantages. Riccioli's book (whose title is a deliberate reference to the "old" *Almagest* of Ptolemy, now discredited) gives an exhaustive survey of arguments for and against the Copernican theory, and concludes that Tycho Brahe's system (modified slightly by Riccioli) is more plausible.

Thus, Riccioli's frontispiece shows his own version of the Tychonic system weighing more heavily in the scales of evidence than its Copernican rival. In Riccioli's variant, Mercury, Venus, and Mars are satellites of the sun, but, unlike Brahe's original scheme, Jupiter and Saturn are centered on the earth. The figure holding the scales and the armillary sphere combines features of Urania (the muse of astronomy) and Astraea (the goddess of justice). On the left is hundred-eyed Argus, observing the sun through a telescope held to an eye on his knee. His words allude to Psalm 8, verse 3: "When I consider thy heavens, the work of thy fingers. . . ." At the bottom lies Ptolemy with his discarded system. Ptolemy rests his hand on the coat of arms of the prince of Monaco (to whom the *Almagestum Novum* was dedicated) magnanimously acknowledging the correction of his errors. At the top are depicted recent astronomical discoveries of the seventeenth century: Mercury and Venus displaying crescent phases; Saturn with two "handles"—this was prior to Huyghens's ring hypothesis; Jupiter with four moons and two bands parallel to its equator (a feature first noted by Riccioli); a heavily cratered moon; and a comet soaring through the heavens like a spotted cannonball. In the center at the top is the Hebrew word *Yah-Veh* and a reference to the Wisdom of Solomon 11, verse 20: "But thou hast ordered all things by measure and number and weight." On the left and right are quotations from Psalm 19, verse 2: "Day unto day uttereth speech, and night unto night sheweth knowledge."

Although Riccioli's book had no effect on the debate over the Copernican theory—by the middle of the seventeenth century, almost all scientists and astronomers were Copernicans—it illustrates one of the most important contests between rival theories in the history of science.


יהוה
Dies diei eructat verbum
Et Nox nocti indicat scientiam
Numerus
Mensura
Pondus
Videbo Caelos tuos, opera digitor. tuor.
Non inclinabitur in saeculum saeculi
Ponderibus librata suis
Cl. Ptolem.
F. Curtius Bon. Incid.

Philosophy of Science

The Central Issues

Martin Curd | J. A. Cover

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To the memory of
Carl G. Hempel (1905–1997)
Thomas S. Kuhn (1922–1996)

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PREFACE

Many people have helped us during the writing and preparation of this book. We are especially indebted to the philosophers who commented at length on earlier versions of the manuscript. As these reviewers can now verify, their criticisms and suggestions have played a major role in shaping the book's final form. They are Marthe Chandler, Len Clark, Keith J. Cooper, Richard Creath, Evan Fales, Stuart S. Glennan, Paul Humphreys, David Magnus, Deborah G. Mayo, and Bonnie Paller. We are also grateful to Rod Bertolet, Clark Glymour, Bill Gustason, Dick Jeffrey, Dana Mason, Alan Musgrave, Tom Nickles, Kathleen Okruhlik, and Ted Ulrich for helping us on topics ranging from Ramsey's dot notation and three-valued logic to T. S. Eliot's poetry, pig farming, and the aetiology of AIDS. A special thanks is due to our secretary at Purdue, Pamela Connelly, and to our editor at Norton, Allen Clawson, for seeing this project through with skill, enthusiasm, and good humor. Without Allen, the book would not have been begun and, almost certainly, would not have been completed. The staff at Norton have been exemplary. We would particularly like to acknowledge Jane Carter (for her meticulous manuscript editing), Claire Acher (for obtaining the permissions and preparing the manuscript for production), and Roy Tedoff (who guided the entire production process). Finally, there are our wives, Patricia and Karen, to whom any formal expression of thanks seems inadequate.

General Introduction

The philosophy of science is at least as old as Aristotle, but it has risen to special prominence in the twentieth century. As scientists have made tremendous advances in fields as diverse as genetics, geology, and quantum mechanics, increasing numbers of philosophers have made science their focus of study. In its broadest terms, the philosophy of science is the investigation of philosophical questions that arise from reflecting on science. What makes these questions philosophical is their generality, their fundamental character, and their resistance to solution by empirical disciplines such as history, sociology, and psychology.

The difference between the philosophy of science and other disciplines that study science can be brought out by contrasting different sorts of question. For example, "When was the planet Neptune discovered?" is primarily a question for historians, not for philosophers.[1] Similarly, "Why did Soviet biologists under Stalin reject Mendelian genetics?" or "Why did James Watson underrate the contributions of Rosalind Franklin to the work that led to the discovery of the double helix structure of DNA?" fall within the domains of sociology, political science, and psychology. Contrast these questions with the following: "When is a theory confirmed by its predictions?" "Should we be realists about all aspects of well-established theories?" "What is a law of nature?" These question are philosophical. They cannot be answered simply by finding out what has happened in the past or what people now believe.

For similar reasons, philosophical questions about science cannot be answered by the sciences themselves (although being able to answer these questions often depends on having a good understanding of scientific theories). A geneticist at the National Cancer Institute, for example, might ask whether certain people are born with a natural immunity to AIDS and set out to answer this question through empirical research. But if our geneticist asked "What is a law of nature?" or "What is science?" or "When is a theory confirmed?" she would not discover the answer by doing more science.

The central questions in the philosophy of science do not belong to science as such; they are *about* science, but not *part of it*. Of course, scientists can be (and sometimes have been) philosophers of science. The point is that when people are doing philosophy of science, they are not (usually) doing science per se, and most philosophers of science (at least in the twentieth century) have not been practicing scientists. Thus, the philosophy of science is not a branch of science but belongs to philosophy, and it intersects with other areas of philosophy, such as epistemology, metaphysics, and the philosophy of language.

The aim of *Philosophy of Science: The Central Issues* is to introduce the reader to the main currents in twentieth-century philosophy of science. It is primarily intended for use in introductory courses at both the undergraduate and graduate levels. In order to keep the book within manageable bounds, some difficult decisions had to be made about what to include and what to exclude. In making these decisions we were guided by our own experience in teaching at Purdue and the recommendations of our reviewers who contributed significantly to the book's development.

The first and in some ways the easiest decision was to exclude the social sciences and concentrate exclusively on the natural sciences. In this we followed the lead of other texts. The philosophical questions raised by disciplines such as history, psychology, sociology, and anthropology are fascinating and important. But they are so different from the questions one encounters in physics, biology, and chemistry that they would require another volume, comparable in length to this one, in order to address them adequately.

A second decision, which we made at the outset, was to avoid foundational questions about the concepts, structure, and content of particular theories and to focus instead on general issues that arise across scientific disciplines. Thus, this volume is organized around wide-ranging philosophical topics and problems, not individual theories or sciences. Details of particular sciences are introduced rarely and only when necessary for evaluating a philosophical position or argument (as, for example, in the chapter on reduction). In this way we hope to avoid the trap of turning a philosophy of science course into a minicourse in science and to keep the focus on the *philosophy* in the philosophy of science. It also has the advantage of making courses based on this book accessible to students (even those at the graduate level) whose background in the sciences may be slight or nonexistent. For the same reason of accessibility, we have confined our selections to readings that use no more than a bare minimum of logical or mathematical notation. The one place where a certain amount of formal notation is unavoidable is in chapter 5, on Bayesian approaches to confirmation theory. But even there, we have edited the readings (sometimes by adding an editorial footnote, sometimes by changing the notation) in order to make them easier to understand, and we have provided an introduction to Bayes's theorem and the probability calculus in the accompanying commentary.

Our approach, then, is focused on philosophical topics and problems, not on particular sciences and theories. A consequence of this topics-and-problems approach is that the chapters of *Philosophy of Science: The Central Issues* pay little attention to tracing the historical development of the philosophy of science in twentieth century. Although we devote some time to filling in some of the essential historical background in the commentaries, this volume is not historical in the way that it treats ideas, arguments, or philosophers. What connects the readings (and the discussions of them in the commentaries) is their focus on common themes, arguments, and criticisms, regardless of whether the authors share the same nationality, are writing in the same decade, or belong to the same school. Our approach is not antihistorical, but it is largely ahistorical.

Because of the many sharp disagreements within the philosophy of science and the unresolved character of nearly all the fundamental questions that philosophers ask about science, an anthology seemed to us to be the only sane choice for a book intended for use in the classroom. But the anthology format brings with it a problem that just about every teacher of philosophy of science has had to confront. Hardly any of the readings, whether old classics or brand-new articles, were written with students in mind. Rather, they were published in books and professional journals, addressed primarily to fellow professionals. Thus, they often presuppose an awareness of issues, positions, and arguments, both in the philosophy of science and in philosophy more generally, that most students lack. Consequently, even the brightest students can find it hard to understand the material they are being asked to read, discuss, and evaluate. The most common complaint voiced by the teachers we spoke with in the several years that went into planning and writing this book, is that many of the readings in the existing anthologies are too sophisticated—they make too many references to the history of science and allude too frequently to philosophical ideas and arguments for the beginning student to get much out of them. What was needed, and what we have tried to provide here, is a serious, comprehensive guide that will really help students in their first encounter with the readings. Thus, in addition to short introductions to each chapter, we have written extended and often detailed commentaries on the readings. Getting the tone and level of detail right in these commentaries has been the hardest and most rewarding part of the book's development. Much of the fine tuning and, in some cases, the inclusion and deletion of entire sections, was guided by our reviewers. We have strived to make each commentary and the sections within them self-contained so that each can be used independently of the rest. And in order to maximize the pedagogical usefulness of *Philosophy of Science: The Central Issues*, each reading is linked explicitly with one or more of the sections into which the commentaries are divided. In this way, where one should look in the commentaries for discussion, explanation, background, and analysis of any of the forty-nine separate readings in the book should be clear.

At the end of this volume there is a glossary, a bibliography, and indexes of names and subjects. The glossary is comprehensive: it covers most of the terms that may be new to the reader or that are being used in an unfamiliar way. The bibliography is divided into nine sections, one for each chapter. Inevitably, this involves some repetition of titles of books and articles, but our aim was to provide the reader with suggestions for further reading, at an appropriate level, about the issues discussed in each chapter's commentary. Consequently, not everything cited in the commentaries appears in the bibliography, some items appear in the bibliography more than once, and there are some things in the bibliography that are not mentioned in the commentaries

The difference between an anthology and a heap of articles lies in their organization. But any system of division will be, to some extent, artificial and misleading: artificial because of the interrelated character of the issues in the philosophy of science and misleading because it might suggest that the readings in one chapter are not connected with those in another. Thus, as with any collection of this kind, the reader or teacher needs to bear in mind that not everything pertinent to, say, the topic of laws will be found in the chapter devoted to laws and that relevant readings and commentaries might also appear in the chapters on explanation and confirmation (as indeed, in this case, they do). Moreover, this is a collection of readings on related topics, not an extended narrative with a beginning, a middle, and an end. Users of the book should not feel constrained by the order of the chapters or even, in most cases, by the order of the readings within those chapters, when deciding what to read first, what to read second, and so on. Obviously, we have arranged the material in an order that makes sense to us, trying wherever possible to juxtapose readings that speak to the same or closely related issues, but many different arrangements are possible and may be preferable, depending on one's interests and teaching goals.

Notes

1. This is not to deny that the question might raise philosophical issues concerning the concept of discovery For example, suppose that an astronomer takes a photograph of the night sky through a telescope that is powerful enough to render Neptune visible. Up to that time, no one has seen Neptune When the plate is developed, it contains an image of Neptune. Although the astronomer sees the image and records the position of the body which made it, he believes that it is "just another star," not a new planet. Has the astronomer discovered Neptune?

I Philosophy of Science

1 | Science and Pseudoscience

INTRODUCTION

Parapsychology is defined by its practitioners as the study of extrasensory perception (ESP) and paranormal powers such as telekinesis. ESP includes such alleged psychic phenomena as telepathy, clairvoyance, and precognition. Shunned for decades by the scientific establishment, parapsychologists received official recognition in 1969 when the American Association for the Advancement of Science (the AAAS) admitted the Parapsychological Association as an affiliate member. Many scientists are unhappy with this decision, since they regard parapsychology as a pseudoscience. In 1979, the renowned physicist John A. Wheeler wrote a blistering letter to the president of the AAAS urging that the parapsychologists be expelled from the association. Wheeler wrote, "We have enough charlatanism in this country today without needing a scientific organization to prostitute itself to it. The AAAS has to make up its mind whether it is seeking popularity or whether it is strictly a scientific organization."[1]

The debate about the nature of science—about its scope, methods, and aims—is as old as science itself. But this debate becomes especially heated when one group of practitioners accuses another group of practicing pseudoscience. In the twentieth century many individuals, groups, and theories have been accused of being pseudoscientific, including Freud and psychoanalysis, astrology, believers in the paranormal, Immanuel Velikovsky and Erich von Daniken (whose best-selling books *Worlds in Collision* and *Chariots of the Gods* excited the wrath of Carl Sagan and the scientific establishment), and, most recently, the self–styled advocates of creation–science. The proponents of astrology, the paranormal, psychoanalysis, and creation–science engage in research, write books, and publish articles, but their work is typically found in popular magazines and bookstores rather than refereed journals and science libraries. They are seldom funded by the National Science Foundation or elected to the National Academy of Sciences. They are outside of the scientific establishment and are kept out by those who regard themselves as real scientists.

If our only concern were to label certain people "pseudoscientists," we might simply check where their work is published and how their theories have been received by the scientific community. But we are concerned with the reasons certain doctrines are considered pseudoscientific; it is those reasons that interest philosophers of science.

Some philosophers have proposed necessary conditions for genuine science. That is, they have offered characteristics that any discipline or field of study must possess in order to qualify as genuine science. These characteristics are often called *demarcation criteria* because they can be used to differentiate science from its counterfeit: if a discipline fails to meet one of these conditions, then it is judged to be nonscientific.

In the twentieth century, philosophers of science have often disagreed about demarcation criteria. In this chapter Karl Popper, Thomas Kuhn, Imre Lakatos, and Paul Thagard each defend a different set of necessary conditions for genuine science. Popper's view, that a scientific theory must be open to refutation by making testable predictions, has been very influential, especially among working scientists. Kuhn, Lakatos, and Thagard all reject Popper's claim that falsifiability is the hallmark of genuine science but disagree about what should replace it. All three address whether a theory or discipline's claim to scientific legitimacy depends on historical considerations, such as how theories have developed over time.

The chapter ends with an exchange of views between Michael Ruse and Larry Laudan about the credentials of creation–science. Ruse, a prominent philosopher of biology, served as an expert witness in a trial concerning the constitutionality of an Arkansas law requiring public school biology teachers to present creationism as a viable scientific alternative to evolutionary theory. Under Ruse's guidance, the judge in the case drew up a list of five criteria for genuine science and concluded that creation–science failed on all five counts. Laudan not only criticizes the items on this list (which includes Popper's falsifiability) but also doubts whether there are any demarcation criteria that all scientific theories must satisfy.

Notes

1. Quoted in Jack W. Grove, *In Defence of Science* (Toronto: University of Toronto Press, 1989), 137. See also Martin Gardner, *Science: Good, Bad and Bogus* (Buffalo, N.Y.: Prometheus Books, 1981), 185–206. The Parapsychological Association is still a member of the AAAS.

KARL POPPER

Science: Conjectures and Refutations

Mr. Turnbull had predicted evil consequences, . . . and was now doing the best in his power to bring about the verification of his own prophecies.
—Anthony Trollope

I

When I received the list of participants in this course and realized that I had been asked to speak to philosophical colleagues* I thought, after some hesitation and consultation, that you would probably prefer me to speak about those problems which interest me most, and about those developments with which I am most intimately acquainted. I therefore decided to do what I have never done before: to give you a report on my own work in the philosophy of science, since the autumn of 1919 when I first began to grapple with the problem, '*When should a theory be ranked as scientific?*' or '*Is there a criterion for the scientific character or status of a theory?*'

The problem which troubled me at the time was neither, 'When is a theory true?' nor, 'When is a theory acceptable?' My problem was different. I *wished to distinguish between science and pseudo-science*; knowing very well that science often errs, and that pseudo-science may happen to stumble on the truth.

I knew, of course, the most widely accepted answer to my problem:

FROM Karl Popper, *Conjectures and Refutations* (London: Routledge and Kegan Paul, 1963), 33–39.

* This essay was originally presented as a lecture at Peterhouse College at Cambridge University in the summer of 1953 as part of a course on developments and trends in contemporary British philosophy, organized by the British Council. It was originally published as "Philosophy of Science: A Personal Report," in *British Philosophy in Mid Century*, ed. C. A. Mace, (London: Allen and Unwin, 1957).

that science is distinguished from pseudo-science—or from 'metaphysics' —by its *empirical method*, which is essentially *inductive*, proceeding from observation or experiment. But this did not satisfy me. On the contrary, I often formulated my problem as one of distinguishing between a genuinely empirical method and a non-empirical or even a pseudo-empirical method—that is to say, a method which, although it appeals to observation and experiment, nevertheless does not come up to scientific standards. The latter method may be exemplified by astrology, with its stupendous mass of empirical evidence based on observation—on horoscopes and on biographies.

But as it was not the example of astrology which led me to my problem I should perhaps briefly describe the atmosphere in which my problem arose and the examples by which it was stimulated. After the collapse of the Austrian Empire there had been a revolution in Austria: the air was full of revolutionary slogans and ideas, and new and often wild theories. Among the theories which interested me Einstein's theory of relativity was no doubt by far the most important. Three others were Marx's theory of history, Freud's psycho-analysis, and Alfred Adler's so-called 'individual psychology'.*

There was a lot of popular nonsense talked about these theories, and especially about relativity (as still happens even today), but I was fortunate in those who introduced me to the study of this theory. We all—the small circle of students to which I belonged—were thrilled with the result of Eddington's eclipse observations which in 1919 brought the first important confirmation of Einstein's theory of gravitation. It was a great experience for us, and one which had a lasting influence on my intellectual development.†

The three other theories I have mentioned were also widely discussed among students at that time. I myself happened to come into personal contact with Alfred Adler, and even to co-operate with him in his social

* For a fascinating autobiographical account of Popper's youthful flirtation and painful disenchantment with Marxism, see "A Crucial Year: Marxism; Science and Pseudoscience," in *The Philosophy of Karl Popper*, ed. Paul A. Schilpp (La Salle, Ill.: Open Court, 1974), 1:23–29. There is also an extended criticism of Freud in Karl R. Popper, *Realism and the Aim of Science* (New York: Routledge, 1983), 163–74.

† Einstein's general theory of relativity entails that light rays must bend in a gravitational field. Organized by Sir Arthur Eddington, two Royal Astronomical Society expeditions were dispatched to observe the solar eclipse of 1919, and verified that starlight was indeed deflected by the sun by the amount that Einstein had predicted. The *Times* of London reported this success as the most remarkable scientific event since the discovery of the planet Neptune. The light-bending test of relativity theory is discussed in "Popper's Demarcation Criterion," in the commentary on chapter 1, and in "Two Arguments for Explanationism," in the commentary on chapter 4.

work among the children and young people in the working-class districts of Vienna where he had established social guidance clinics.

It was during the summer of 1919 that I began to feel more and more dissatisfied with these three theories—the Marxist theory of history, psycho-analysis, and individual psychology; and I began to feel dubious about their claims to scientific status. My problem perhaps first took the simple form, 'What is wrong with Marxism, psycho-analysis, and individual psychology? Why are they so different from physical theories, from Newton's theory, and especially from the theory of relativity?'

To make this contrast clear I should explain that few of us at the time would have said that we believed in the *truth* of Einstein's theory of gravitation. This shows that it was not my doubting the *truth* of those other three theories which bothered me, but something else. Yet neither was it that I merely felt mathematical physics to be more *exact* than the sociological or psychological type of theory. Thus what worried me was neither the problem of truth, at that stage at least, nor the problem of exactness or measurability. It was rather that I felt that these other three theories, though posing as sciences, had in fact more in common with primitive myths than with science; that they resembled astrology rather than astronomy.

I found that those of my friends who were admirers of Marx, Freud, and Adler, were impressed by a number of points common to these theories, and especially by their apparent *explanatory power*. These theories appeared to be able to explain practically everything that happened within the fields to which they referred. The study of any of them seemed to have the effect of an intellectual conversion or revelation, opening your eyes to a new truth hidden from those not yet initiated. Once your eyes were thus opened you saw confirming instances everywhere: the world was full of *verifications* of the theory. Whatever happened always confirmed it. Thus its truth appeared manifest; and unbelievers were clearly people who did not want to see the manifest truth; who refused to see it, either because it was against their class interest, or because of their repressions which were still 'un-analysed' and crying aloud for treatment.

The most characteristic element in this situation seemed to me the incessant stream of confirmations, of observations which 'verified' the theories in question; and this point was constantly emphasized by their adherents. A Marxist could not open a newspaper without finding on every page confirming evidence for his interpretation of history; not only in the news, but also in its presentation—which revealed the class bias of the paper—and especially of course in what the paper did *not* say. The Freudian analysts emphasized that their theories were constantly verified by their 'clinical observations'. As for Adler, I was much impressed by a personal experience. Once, in 1919, I reported to him a case which to me did not seem particularly Adlerian, but which he found no difficulty in analysing in terms of his theory of inferiority feelings, although he had not even seen the child. Slightly shocked, I asked him how he could be so sure. 'Because of my thousandfold experience,' he replied; whereupon I could not help saying: 'And with this new case, I suppose, your experience has become thousand-and-one-fold.'

What I had in mind was that his previous observations may not have been much sounder than this new one; that each in its turn had been interpreted in the light of 'previous experience', and at the same time counted as additional confirmation. What, I asked myself, did it confirm? No more than that a case could be interpreted in the light of the theory. But this meant very little, I reflected, since every conceivable case could be interpreted in the light of Adler's theory, or equally of Freud's. I may illustrate this by two very different examples of human behaviour: that of a man who pushes a child into the water with the intention of drowning it; and that of a man who sacrifices his life in an attempt to save the child. Each of these two cases can be explained with equal ease in Freudian and in Adlerian terms. According to Freud the first man suffered from repression (say, of some component of his Oedipus complex), while the second man had achieved sublimation. According to Adler the first man suffered from feelings of inferiority (producing perhaps the need to prove to himself that he dared to commit some crime), and so did the second man (whose need was to prove to himself that he dared to rescue the child). I could not think of any human behaviour which could not be interpreted in terms of either theory. It was precisely this fact—that they always fitted, that they were always confirmed—which in the eyes of their admirers constituted the strongest argument in favour of these theories. It began to dawn on me that this apparent strength was in fact their weakness.

With Einstein's theory the situation was strikingly different. Take one typical instance—Einstein's prediction, just then confirmed by the findings of Eddington's expedition. Einstein's gravitational theory had led to the result that light must be attracted by heavy bodies (such as the sun), precisely as material bodies were attracted. As a consequence it could be calculated that light from a distant fixed star whose apparent position was close to the sun would reach the earth from such a direction that the star would seem to be slightly shifted away from the sun; or, in other words, that stars close to the sun would look as if they had moved a little away from the sun, and from one another. This is a thing which cannot normally be observed since such stars are rendered invisible in daytime by the sun's overwhelming brightness; but during an eclipse it is possible to take photographs of them. If the same constellation is photographed at night one can measure the distances on the two photographs, and check the predicted effect.

Now the impressive thing about this case is the *risk* involved in a prediction of this kind. If observation shows that the predicted effect is definitely absent, then the theory is simply refuted. The theory is *incompatible with certain possible results of observation*—in fact with results which everybody before Einstein would have expected.[1] This is quite different from ~~the situation I have previously~~ described, when it turned out that the theories in question were compatible with the most divergent human behaviour, so that it was practically impossible to describe any human behaviour that might not be claimed to be a verification of these theories.

These considerations led me in the winter of 1919–20 to conclusions which I may now reformulate as follows.

1 It is easy to obtain confirmations, or verifications, for nearly every theory—if we look for confirmations.

2 Confirmations should count only if they are the result of *risky predictions*; that is to say, if, unenlightened by the theory in question, we should have expected an event which was incompatible with the theory—an event which would have refuted the theory.

3 Every 'good' scientific theory is a prohibition: it forbids certain things to happen. The more a theory forbids, the better it is.

4 A theory which is not refutable by any conceivable event is non-scientific. Irrefutability is not a virtue of a theory (as people often think) but a vice.

5 Every genuine *test* of a theory is an attempt to falsify it, or to refute it. Testability is falsifiability; but there are degrees of testability: some theories are more testable, more exposed to refutation, than others; they take, as it were, greater risks.

6 Confirming evidence should not count *except when it is the result of a genuine test of the theory*; and this means that it can be presented as a serious but unsuccessful attempt to falsify the theory. (I now speak in such cases of 'corroborating evidence'.)

7 Some genuinely testable theories, when found to be false, are still upheld by their admirers—for example by introducing *ad hoc* some auxiliary assumption, or by re-interpreting the theory *ad hoc* in such a way that it escapes refutation. Such a procedure is always possible, but it rescues the theory from refutation only at the price of destroying, or at least lowering, its scientific status. (I later described such a rescuing operation ~~as a~~ '*conventionalist twist*' or a '*conventionalist stratagem*'.)

One can sum up all this by saying that *the criterion of the scientific status of a theory is its falsifiability, or refutability, or testability.*

II

I may perhaps exemplify this with the help of the various theories so far mentioned. Einstein's theory of gravitation clearly satisfied the criterion of falsifiability. Even if our measuring instruments at the time did not allow us to pronounce on the results of the tests with complete assurance, there was clearly a possibility of refuting the theory.

Astrology did not pass the test. Astrologers were greatly impressed, and misled, by what they believed to be confirming evidence—so much so that they were quite unimpressed by any unfavourable evidence. Moreover, by making their interpretations and prophecies sufficiently vague they were able to explain away anything that might have been a refutation of the theory had the theory and the prophecies been more precise. In order to escape falsification they destroyed the testability of their theory. It is a typical soothsayer's trick to predict things so vaguely that the predictions can hardly fail: that they become irrefutable.

The Marxist theory of history, in spite of the serious efforts of some of its founders and followers, ultimately adopted this soothsaying practice. In some of its earlier formulations (for example in Marx's analysis of the character of the 'coming social revolution') their predictions were testable, and in fact falsified.[2] Yet instead of accepting the refutations the followers of Marx re-interpreted both the theory and the evidence in order to make them agree. In this way they rescued the theory from refutation; but they did so at the price of adopting a device which made it irrefutable. They thus gave a 'conventionalist twist' to the theory; and by this stratagem they destroyed its much advertised claim to scientific status.

The two psycho-analytic theories were in a different class. They were simply non-testable, irrefutable. There was no conceivable human behaviour which could contradict them. This does not mean that Freud and Adler were not seeing certain things correctly: I personally do not doubt that much of what they say is of considerable importance, and may well play its part one day in a psychological science which is testable. But it does mean that those 'clinical observations' which analysts naïvely believe confirm their theory cannot do this any more than the daily confirmations which astrologers find in their practice.[3] And as for Freud's epic of the Ego, the Super-ego, and the Id, no substantially stronger claim to scientific status can be made for it than for Homer's collected stories from Olympus. These theories describe some facts, but in the manner of myths. They contain most interesting psychological suggestions, but not in a testable form.

At the same time I realized that such myths may be developed, and become testable; that historically speaking all—or very nearly all—scientific theories originate from myths, and that a myth may contain important anticipations of scientific theories. Examples are Empedocles' theory of evolution by trial and error, or Parmenides' myth of the unchanging block universe in which nothing ever happens and which, if we add another dimension, becomes Einstein's block universe (in which, too, nothing ever happens, since everything is, four-dimensionally speaking, determined and laid down from the beginning). I thus felt that if a theory is found to be non-scientific, or 'metaphysical' (as we might say), it is not thereby found to be unimportant, or insignificant, or 'meaningless', or 'nonsensical'.[4] But it cannot claim to be backed by empirical evidence in the scientific sense—although it may easily be, in some genetic sense, the 'result of observation'.

(There were a great many other theories of this pre-scientific or pseudo-scientific character, some of them, unfortunately, as influential as the Marxist interpretation of history; for example, the racialist interpretation of history—another of those impressive and all-explanatory theories which act upon weak minds like revelations.)

Thus the problem which I tried to solve by proposing the criterion of falsifiability was neither a problem of meaningfulness or significance, nor a problem of truth or acceptability. It was the problem of drawing a line (as well as this can be done) between the statements, or systems of statements, of the empirical sciences, and all other statements—whether they are of a religious or of a metaphysical character, or simply pseudo-scientific. Years later—it must have been in 1928 or 1929—I called this first problem of mine the '*problem of demarcation*'. The criterion of falsifiability is a solution to this problem of demarcation, for it says that statements or systems of statements, in order to be ranked as scientific, must be capable of conflicting with possible, or conceivable, observations. . . .

Notes

1. This is a slight oversimplification, for about half of the Einstein effect may be derived from the classical theory, provided we assume a ballistic theory of light.

2. See, for example, my *Open Society and Its Enemies* [Routledge & Kegan Paul, 1945], ch. 15, section iii, and notes 13–14.

3. 'Clinical observations', like all other observations, are *interpretations in the light of theories* . . . ; and for this reason alone they are apt to seem to support those theories in the light of which they were interpreted. But real support can be obtained only from observations undertaken as tests (by 'attempted refutations'); and for this purpose *criteria of refutation* have to be laid down beforehand: it must be agreed which observable situations, if actually observed, mean that the theory is refuted. But what kind of clinical responses would refute to the satisfaction of the analyst not merely a particular analytic diagnosis but psycho-analysis itself? And have such criteria ever been discussed or agreed upon by analysts? Is there not, on the contrary, a whole family of analytic concepts, such as 'ambivalence' (I do not suggest that there is no such thing as ambivalence), which would make it difficult, if not impossible, to agree upon such criteria? Moreover, how much headway has been made in investigating the question of the extent to which the (conscious or unconscious) expectations and theories held by the analyst influence the 'clinical responses' of the patient? (To say nothing about the conscious attempts to influence the patient by proposing interpretations to him, etc.) Years ago I introduced the term '*Oedipus effect*' to describe the influence of a theory or expectation or prediction *upon the event which it predicts* or describes: it will be

remembered that the causal chain leading to Oedipus' parricide was started by the oracle's prediction of this event. This is a characteristic and recurrent theme of such myths, but one which seems to have failed to attract the interest of the analysts, perhaps not accidentally. (The problem of confirmatory dreams suggested by the analyst is discussed by Freud, for example in *Gesammelte Schriften* [Complete works], III, 1925, where he says on p. 314: 'If anybody asserts that most of the dreams which can be utilized in an analysis . . . owe their origin to [the analyst's] suggestion, then no objection can be made from the point of view of analytic theory. Yet there is nothing in this fact', he surprisingly adds, 'which would detract from the reliability of our results.')

4. The case of astrology, nowadays a typical pseudo-science, may illustrate this point. It was attacked, by Aristotelians and other rationalists, down to Newton's day, for the wrong reason—for its now accepted assertion that the planets had an 'influence' upon terrestrial ('sublunar') events. In fact Newton's theory of gravity, and especially the lunar theory of the tides, was historically speaking an offspring of astrological lore. Newton, it seems, was most reluctant to adopt a theory which came from the same stable as for example the theory that 'influenza' epidemics are due to an astral 'influence'. And Galileo, no doubt for the same reason, actually rejected the lunar theory of the tides; and his misgivings about Kepler may easily be explained by his misgivings about astrology.

THOMAS S. KUHN

Logic of Discovery or Psychology of Research?

Among the most fundamental issues on which Sir Karl [Popper] and I agree is our insistence that an analysis of the development of scientific knowledge must take account of the way science has actually been practiced. That being so, a few of his recurrent generalizations startle me. One of these provides the opening sentences of the first chapter of the *Logic of Scientific Discovery*: 'A scientist', writes Sir Karl, 'whether theorist or experimenter, puts forward statements, or systems of statements, and tests them step by step. In the field of the empirical sciences, more particularly, he constructs hypotheses, or systems of theories, and tests them against experience by observation and experiment.'[1] The statement is virtually a cliché, yet in application it presents three problems. It is ambiguous in its failure to specify which of two sorts of 'statements' or 'theories' are being tested. That ambiguity can, it is true, be eliminated by reference to other passages in Sir Karl's writings, but the generalization that results is historically mistaken. Furthermore, the mistake proves important, for the unambiguous form of the description misses just that characteristic of scientific practice which most nearly distinguishes the sciences from other creative pursuits.

There is one sort of 'statement' or 'hypothesis' that scientists do repeatedly subject to systematic test. I have in mind statements of an individual's best guesses about the proper way to connect his own research problem with the corpus of accepted scientific knowledge. He may, for example, conjecture that a given chemical unknown contains the salt of a rare earth, that the obesity of his experimental rats is due to a specified component in their diet, or that a newly discovered spectral pattern is to be understood as an effect of nuclear spin. In each case, the next steps in his research are intended to try out or test the conjecture or hypothesis.

FROM Imre Lakatos and Alan Musgrave, eds., *Criticism and the Growth of Knowledge* (Cambridge: Cambridge University Press, 1970) 4–10.

If it passes enough or stringent enough tests, the scientist has made a discovery or has at least resolved the puzzle he had been set. If not, he must either abandon the puzzle entirely or attempt to solve it with the aid of some other hypothesis. Many research problems, though by no means all, take this form. Tests of this sort are a standard component of what I have elsewhere labelled 'normal science' or 'normal research', an enterprise which accounts for the overwhelming majority of the work done in basic science. In no usual sense, however, are such tests directed to current theory. On the contrary, when engaged with a normal research problem, the scientist must *premise* current theory as the rules of his game. His object is to solve a puzzle, preferably one at which others have failed, and current theory is required to define that puzzle and to guarantee that, given sufficient brilliance, it can be solved.[2] Of course the practitioner of such an enterprise must often test the conjectural puzzle solution that his ingenuity suggests. But only his personal conjecture is tested. If it fails the test, only his own ability not the corpus of current science is impugned. In short, though tests occur frequently in normal science, these tests are of a peculiar sort, for in the final analysis it is the individual scientist rather than current theory which is tested.

This is not, however, the sort of test Sir Karl has in mind. He is above all concerned with the procedures through which science grows, and he is convinced that 'growth' occurs not primarily by accretion but by the revolutionary overthrow of an accepted theory and its replacement by a better one.[3] (The subsumption under 'growth' of 'repeated overthrow' is itself a linguistic oddity whose *raison d'être* may become more visible as we proceed.) Taking this view, the tests which Sir Karl emphasizes are those which were performed to explore the limitations of accepted theory or to subject a current theory to maximum strain. Among his favourite examples, all of them startling and destructive in their outcome, are Lavoisier's experiments on calcination,* the eclipse expedition

* Calcination occurs when a metal is burned in air, forming a calx or oxide. According to the phlogiston theory, metals (and all other combustible substances) are compounds of an earthy calx and the fiery element, phlogiston. When a metal burns, the phlogiston is released, leaving the calx as a residue. Because metals gain weight when they are calcined, some proponents of the phlogiston theory conjectured that phlogiston must have negative weight. Others inferred that some other substance must combine with the metal when the phlogiston is released. By careful experiments in the 1770s, Antoine Lavoisier (1743–94) showed that the weight gained during calcination is entirely due to the metal combining with a gas in the air, which he named oxygen. Lavoisier's oxygen theory of calcination (and, more generally, of combustion) overthrew the phlogiston theory and gave rise to a revolution in chemistry. See James B. Conant, ed., *The Overthrow of the Phlogiston Theory: The Chemical Revolution of 1775–1789* (Cambridge, Mass.: Harvard University Press, 1950); reprinted in *Harvard Case Histories in Experimental Science*, ed. J. B. Conant and L. K. Nash (Cambridge, Mass.: Harvard University Press, 1966). See also Alan Musgrave, "Why Did Oxygen Supplant

of 1919,* and the recent experiments on parity conservation.†[4] All, of course, are classic tests, but in using them to characterize scientific activity Sir Karl misses something terribly important about them. Episodes like these are very rare in the development of science. When they occur, they are generally called forth either by a prior crisis in the relevant field (Lavoisier's experiments or Lee and Yang's[5]) or by the existence of a theory which competes with the existing canons of research (Einstein's general relativity). These are, however, aspects of or occasions for what I have elsewhere called 'extraordinary research', an enterprise in which scientists do display very many of the characteristics Sir Karl emphasizes, but one which, at least in the past, has arisen only intermittently and under quite special circumstances in any scientific speciality.[6]

I suggest then that Sir Karl has characterized the entire scientific enterprise in terms that apply only to its occasional revolutionary parts. His emphasis is natural and common: the exploits of a Copernicus or Einstein make better reading than those of a Brahe or Lorentz;‡ Sir Karl

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Phlogiston? Research Programmes in the Chemical Revolution," in *Method and Appraisal in the Physical Sciences*, ed. C. Howson (Cambridge: Cambridge University Press, 1976), 181–209.

* For information about the eclipse expedition of 1919 and its role in confirming Einstein's general theory of relativity, see the preceding reading by Karl Popper, "Science: Conjectures and Refutations." Further discussion can be found in "Popper's Demarcation Criterion," in the commentary on chapter 1, and in "Two Arguments for Explanationism," in the commentary on chapter 4.

† Kuhn is referring to the experiments performed by Chien-Shiung Wu and her associates in 1956–57, which verified the conjecture of Tsung Dao Lee and Chen Ning Yang that parity is not conserved in weak interactions. Wu's results were soon confirmed by other groups and Lee and Yang received the Nobel prize in physics in 1957 for their discovery of parity violation. For a description of Wu's experiment and an explanation of its revolutionary significance, see Eugene Wigner, "Violations of Symmetry in Physics," *Scientific American* 213 (1965): 28–36 and Martin Gardner, *The New Ambidextrous Universe*, 3d rev. ed. (New York: W. H. Freeman, 1990).

‡ For Kuhn, Tycho Brahe (1546–1601) and H. A. Lorentz (1853–1928) exemplify the conservative scientist practicing normal science. Brahe objected to Copernicus's revolutionary theory of a heliocentric universe on physical, astronomical, and religious grounds, proposing in its place his own version of a geostatic system. Like Ptolemy, Brahe had the sun moving around the earth, but unlike Ptolemy, he made the other planets orbit round the sun. In this way, Brahe was able to capture many of the explanatory features of Copernicus's theory without having to attribute any motion to the earth. Lorentz, like most physicists of his day, believed that light and other electromagnetic radiation propagates in an aether that is at rest with respect to absolute space. In order to account for the null result of the Michelson-Morley experiment, Lorentz (and, independently, Fitzgerald) postulated the famous Lorentz-Fitzgerald contraction according to which all physical objects contract in their direction of motion. Lorentz later introduced time dilation, thus obtaining the Lorentz transformations that lie at the heart of Einstein's special

would not be the first if he mistook what I call normal science for an intrinsically uninteresting enterprise. Nevertheless, neither science nor the development of knowledge is likely to be understood if research is viewed exclusively through the revolutions it occasionally produces. For example, though testing of basic commitments occurs only in extraordinary science, it is normal science that discloses both the points to test and the manner of testing. Or again, it is for the normal, not the extraordinary practice of science that professionals are trained; if they are nevertheless eminently successful in displacing and replacing the theories on which normal practice depends, that is an oddity which must be explained. Finally, and this is for now my main point, a careful look at the scientific enterprise suggests that it is normal science, in which Sir Karl's sort of testing does not occur, rather than extraordinary science which most nearly distinguishes science from other enterprises. If a demarcation criterion exists (we must not, I think, seek a sharp or decisive one), it may lie just in that part of science which Sir Karl ignores.

In one of his most evocative essays, Sir Karl traces the origin of 'the tradition of critical discussion [which] represents the only practicable way of expanding our knowledge' to the Greek philosophers between Thales and Plato, the men who, as he sees it, encouraged critical discussion both between schools and within individual schools.[7] The accompanying description of Presocratic discourse is most apt, but what is described does not at all resemble science. Rather it is the tradition of claims, counterclaims, and debates over fundamentals which, except perhaps during the Middle Ages, have characterized philosophy and much of social science ever since. Already by the Hellenistic period mathematics, astronomy, statics and the geometric parts of optics had abandoned this mode of discourse in favour of puzzle solving. Other sciences, in increasing numbers, have undergone the same transition since. In a sense, to turn Sir Karl's view on its head, it is precisely the abandonment of critical discourse that marks the transition to a science. Once a field has made that transition, critical discourse recurs only at moments of crisis when the bases of the field are again in jeopardy.[8] Only when they must choose between competing theories do scientists behave like philosophers. That, I think, is why Sir Karl's brilliant description of the reasons for the choice between metaphysical systems so closely resembles my description of the reasons for choosing between scientific theories.[9] In neither choice, as I shall shortly try to show, can testing play a quite decisive role.

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theory of relativity; but unlike Einstein, Lorentz worked within a classical framework of absolute space and time. For introductory accounts of the contrast between the theories of Lorentz and Einstein and their differing interpretations of the Michelson-Morley experiment, see Albert Einstein, *Relativity: The Special and General Theory*, trans. R. W. Lawson (New York: Crown, 1961), and Jonathan Powers, *Philosophy and the New Physics* (New York: Methuen, 1982), ch. 3.

There is, however, good reason why testing has seemed to do so, and in exploring it Sir Karl's duck may at last become my rabbit.* No puzzle-solving enterprise can exist unless its practitioners share criteria which, for that group and for that time, determine when a particular puzzle has been solved. The same criteria necessarily determine failure to achieve a solution, and anyone who chooses may view that failure as the failure of a theory to pass a test. Normally, as I have already insisted, it is not viewed that way. Only the practitioner is blamed, not his tools. But under the special circumstances which induce a crisis in the profession (e.g. gross failure, or repeated failure by the most brilliant professionals) the group's opinion may change. A failure that had previously been personal may then come to seem the failure of a theory under test. Thereafter, because the test arose from a puzzle and thus carried settled criteria of solution, it proves both more severe and harder to evade than the tests available within a tradition whose normal mode is critical discourse rather than puzzle solving.

In a sense, therefore, severity of test-criteria is simply one side of the coin whose other face is a puzzle-solving tradition. That is why Sir Karl's line of demarcation and my own so frequently coincide. That coincidence is, however, only in their *outcome*; the *process* of applying them is very different, and it isolates distinct aspects of the activity about which the decision—science or non-science—is to be made. Examining the vexing cases, for example, psychoanalysis or Marxist historiography, for which Sir Karl tells us his criterion was initially designed,[10] I concur that they cannot now properly be labelled 'science'. But I reach that conclusion by a route far surer and more direct than his. One brief example may suggest that of the two criteria, testing and puzzle solving, the latter is at once the less equivocal and the more fundamental.

To avoid irrelevant contemporary controversies, I consider astrology rather than, say, psychoanalysis. Astrology is Sir Karl's most frequently cited example of a 'pseudo-science'.[11] He says: 'By making their interpretations and prophecies sufficiently vague they [astrologers] were able to explain away anything that might have been a refutation of the theory had the theory and the prophecies been more precise. In order to escape falsification they destroyed the testability of their theory.'[12] Those generalizations catch something of the spirit of the astrological enterprise. But taken at all literally, as they must be if they are to provide a demarcation criterion, they are impossible to support. The history of astrology during the cen-

* The duck-rabbit is a visually ambiguous drawing, made popular among philosophers by Ludwig Wittgenstein in his *Philosophical Investigations* (1953). It can be seen either as a duck's head with a long beak or as a rabbit's head with long ears, but it cannot be seen as both at the same time. It is a favorite with philosophers of science (such as Kuhn, Hanson, and Feyerabend) wishing to emphasize the theory-ladenness of observation.

turies when it was intellectually reputable records many predictions that categorically failed.[13] Not even astrology's most convinced and vehement exponents doubted the recurrence of such failures. Astrology cannot be barred from the sciences because of the form in which its predictions were cast.

Nor can it be barred because of the way its practitioners explained failure. Astrologers pointed out, for example, that, unlike general predictions about, say, an individual's propensities or a natural calamity, the forecast of an individual's future was an immensely complex task, demanding the utmost skill, and extremely sensitive to minor errors in relevant data. The configuration of the stars and eight planets was constantly changing; the astronomical tables used to compute the configuration at an individual's birth were notoriously imperfect; few men knew the instant of their birth with the requisite precision.[14] No wonder, then, that forecasts often failed. Only after astrology itself became implausible did these arguments come to seem question-begging.[15] Similar arguments are regularly used today when explaining, for example, failures in medicine or meteorology. In times of trouble they are also deployed in the exact sciences, fields like physics, chemistry, and astronomy.[16] There was nothing unscientific about the astrologer's explanation of failure.

Nevertheless, astrology was not a science. Instead it was a craft, one of the practical arts, with close resemblances to engineering, meteorology, and medicine as these fields were practised until little more than a century ago. The parallels to an older medicine and to contemporary psychoanalysis are, I think, particularly close. In each of these fields shared theory was adequate only to establish the plausibility of the discipline and to provide a rationale for the various craft-rules which governed practice. These rules had proved their use in the past, but no practitioner supposed they were sufficient to prevent recurrent failure. A more articulated theory and more powerful rules were desired, but it would have been absurd to abandon a plausible and badly needed discipline with a tradition of limited success simply because these desiderata were not yet at hand. In their absence, however, neither the astrologer nor the doctor could do research. Though they had rules to apply, they had no puzzles to solve and therefore no science to practise.[17]

Compare the situations of the astronomer and the astrologer. If an astronomer's prediction failed and his calculations checked, he could hope to set the situation right. Perhaps the data were at fault: old observations could be re-examined and new measurements made, tasks which posed a host of calculational and instrumental puzzles. Or perhaps theory needed adjustment, either by the manipulation of epicycles, eccentrics, equants, etc., or by more fundamental reforms of astronomical technique. For more than a millennium these were the theoretical and mathematical puzzles around which, together with their instrumental counterparts, the astronomical research tradition was constituted. The astrologer, by contrast, had no such puzzles. The occurrence of failures could be explained, but particular failures did not give rise to research puzzles, for no man, however skilled, could make use of them in a constructive attempt to revise the astrological tradition. There were too many possible sources of difficulty, most of them beyond the astrologer's knowledge, control, or responsibility. Individual failures were correspondingly uninformative, and they did not reflect on the competence of the prognosticator in the eyes of his professional compeers.[18] Though astronomy and astrology were regularly practised by the same people, including Ptolemy, Kepler, and Tycho Brahe, there was never an astrological equivalent of the puzzle-solving astronomical tradition. And without puzzles, able first to challenge and then to attest the ingenuity of the individual practitioner, astrology could not have become a science even if the stars had, in fact, controlled human destiny.

In short, though astrologers made testable predictions and recognized that these predictions sometimes failed, they did not and could not engage in the sorts of activities that normally characterize all recognized sciences. Sir Karl is right to exclude astrology from the sciences, but his over-concentration on science's occasional revolutions prevents his seeing the surest reason for doing so.

That fact, in turn, may explain another oddity of Sir Karl's historiography. Though he repeatedly underlines the role of tests in the replacement of scientific theories, he is also constrained to recognize that many theories, for example the Ptolemaic, were replaced before they had in fact been tested.[19] On some occasions, at least, tests are not requisite to the revolutions through which science advances. But that is not true of puzzles. Though the theories Sir Karl cites had not been put to the test before their displacement, none of these was replaced before it had ceased adequately to support a puzzle-solving tradition. The state of astronomy was a scandal in the early sixteenth century. Most astronomers nevertheless felt that normal adjustments of a basically Ptolemaic model would set the situation right. In this sense the theory had not failed a test. But a few astronomers, Copernicus among them, felt that the difficulties must lie in the Ptolemaic approach itself rather than in the particular versions of Ptolemaic theory so far developed, and the results of that conviction are already recorded. The situation is typical.[20] With or without tests, a puzzle-solving tradition can prepare the way for its own displacement. To rely on testing as the mark of a science is to miss what scientists mostly do and, with it, the most characteristic feature of their enterprise. . . .

■ | Notes

1. Popper [1959], p. 27.

2. For an extended discussion of normal science, the activity which practitioners are trained to carry on, see my [1962], pp. 23–42, and 135–42. It is important to

notice that when I describe the scientist as a puzzle solver and Sir Karl describes him as a problem solver (e.g. in his [1963], pp. 67, 222), the similarity of our terms disguises a fundamental divergence. Sir Karl writes (the italics are his), Admittedly, our expectations, and thus our theories, may precede, historically, even our problems. *Yet science starts only with problems.* Problems crop up especially when we are disappointed in our expectations, or when our theories involve us in difficulties, in contradictions'. I use the term 'puzzle' in order to emphasize that the difficulties which *ordinarily* confront even the very best scientists are, like crossword puzzles or chess puzzles, challenges only to his ingenuity. *He* is in difficulty, not current theory. My point is almost the converse of Sir Karl's.

3. Cf. Popper [1963], pp. 129, 215 and 221, for particularly forceful statements of this position.

4. For example, Popper [1963], p. 220.

5. For the work on calcination see, Guerlac [1961]. For the background of the parity experiments see, Hafner and Presswood [1965].

6. The point is argued at length in my [1962], pp. 52–97.

7. Popper [1963], chapter 5, especially pp. 148–52.

8. Though I was not then seeking a demarcation criterion, just these points are argued at length in my [1962], pp. 10–22 and 87–90.

9. Cf. Popper [1963], pp. 192–200, with my [1962], pp. 143–58.

10. Popper [1963], p. 34 [p. 4–5, above].

11. The index to Popper [1963] has eight entries under the heading 'astrology as a typical pseudo-science'.

12. Popper [1963], p. 37 [p. 8, above].

13. For examples see, Thorndike [1923–58], 5, pp. 225 ff.; 6, pp. 71, 101, 114.

14. For reiterated explanations of failure see, *ibid.* I, pp. 11 and 514 f.; 4, 368; 5, 279.

15. A perceptive account of some reasons for astrology's loss of plausibility is included in Stahlman [1956]. For an explanation of astrology's previous appeal see, Thorndike [1955].

16. Cf. my [1962], pp. 66–76.

17. This formulation suggests that Sir Karl's criterion of demarcation might be saved by a minor restatement entirely in keeping with his apparent intent. For a field to be a science its conclusions must be *logically derivable* from *shared premises.* On this view astrology is to be barred not because its forecasts were not testable but because only the most general and least testable ones could be derived from accepted theory. Since any field that did satisfy this condition *might* support a puzzle solving tradition, the suggestion is clearly helpful. It comes close to supplying a sufficient condition for a field's being a science. But in this form, at least, it is not even quite a sufficient condition, and it is surely not a necessary one. It would, for example, admit surveying and navigation as sciences, and it would bar taxonomy, historical geology, and the theory of evolution. The conclusions of a

science may be both precise and binding without being fully derivable by logic from accepted premises. Cf. my [1962], pp. 35–51. . . .

18. This is not to suggest that astrologers did not criticize each other. On the contrary, like practitioners of philosophy and some social sciences, they belonged to a variety of different schools, and the inter-school strife was sometimes bitter. But these debates ordinarily revolved about the *implausibility* of the particular theory employed by one or another school. Failures of individual predictions played very little role. Compare Thorndike [1923–58], 5, p. 233.

19. Cf. Popper [1963], p. 246.

20. Cf. my [1962], pp. 77–87.

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IMRE LAKATOS

Science and Pseudoscience

Man's respect for knowledge is one of his most peculiar characteristics. Knowledge in Latin is *scientia*, and science came to be the name of the most respectable kind of knowledge. But what distinguishes knowledge from superstition, ideology or pseudoscience? The Catholic Church excommunicated Copernicans, the Communist Party persecuted Mendelians on the ground that their doctrines were pseudoscientific. The demarcation between science and pseudoscience is not merely a problem of armchair philosophy: it is of vital social and political relevance.

Many philosophers have tried to solve the problem of demarcation in the following terms: a statement constitutes knowledge if sufficiently many people believe it sufficiently strongly. But the history of thought shows us that many people were totally committed to absurd beliefs. If the strength of beliefs were a hallmark of knowledge, we should have to rank some tales about demons, angels, devils, and of heaven and hell as knowledge. Scientists, on the other hand, are very sceptical even of their best theories. Newton's is the most powerful theory science has yet produced, but Newton himself never believed that bodies attract each other at a distance. So no degree of commitment to beliefs makes them knowledge. Indeed, the hallmark of scientific behaviour is a certain scepticism even towards one's most cherished theories. Blind commitment to a theory is not an intellectual virtue: it is an intellectual crime.

Thus a statement may be pseudoscientific even if it is eminently 'plausible' and everybody believes in it, and it may be scientifically valuable even if it is unbelievable and nobody believes in it. A theory may even be of supreme scientific value even if no one understands it, let alone believes it.

FROM Imre Lakatos, *Philosophical Papers*, vol. 1 (Cambridge: Cambridge University Press, 1977), 1–7. Written in early 1973, this was originally presented as a radio lecture broadcast by the Open University (30 June 1973).

The cognitive value of a theory has nothing to do with its psychological influence on people's minds. Belief, commitment, understanding are states of the human mind. But the objective, scientific value of a theory is independent of the human mind which creates it or understands it. Its scientific value depends only on what objective support these conjectures have in facts. As Hume said:

> If we take in our hand any volume; of divinity, or school metaphysics, for instance; let us ask, does it contain any abstract reasoning concerning quantity or number? No. Does it contain any experimental reasoning concerning matter of fact and existence? No. Commit it then to the flames. For it can contain nothing but sophistry and illusion.*

But what is 'experimental' reasoning? If we look at the vast seventeenth-century literature on witchcraft, it is full of reports of careful observations and sworn evidence—even of experiments. Glanvill, the house philosopher of the early Royal Society, regarded witchcraft as the paradigm of experimental reasoning. We have to define experimental reasoning before we start Humean book burning.

In scientific reasoning, theories are confronted with facts; and one of the central conditions of scientific reasoning is that theories must be supported by facts. Now how exactly can facts support theory?

Several different answers have been proposed. Newton himself thought that he proved his laws from facts. He was proud of not uttering mere hypotheses: he only published theories proven from facts. In particular, he claimed that he deduced his laws from the 'phenomena' provided by Kepler. But his boast was nonsense, since according to Kepler, planets move in ellipses, but according to Newton's theory, planets would move in ellipses only if the planets did not disturb each other in their motion. But they do. This is why Newton had to devise a perturbation theory from which it follows that no planet moves in an ellipse.

One can today easily demonstrate that there can be no valid derivation of a law of nature from any finite number of facts; but we still keep reading about scientific theories being proved from facts. Why this stubborn resistance to elementary logic?

There is a very plausible explanation. Scientists want to make their theories respectable, deserving of the title 'science', that is, genuine knowledge. Now the most relevant knowledge in the seventeenth century, when science was born, concerned God, the Devil, Heaven and Hell. If one got one's conjectures about matters of divinity wrong, the consequence of one's mistake was eternal damnation. Theological knowledge cannot be

* These famous lines are from the final paragraph of David Hume's *An Enquiry Concerning Human Understanding*, first published in 1748 (under the title *Philosophical Essays Concerning Human Understanding*).

fallible: it must be beyond doubt. Now the Enlightenment thought that we were fallible and ignorant about matters theological. There is no scientific theology and, therefore, no theological knowledge. Knowledge can only be about Nature, but this new type of knowledge had to be judged by the standards they took over straight from theology: it had to be proven beyond doubt. Science had to achieve the very certainty which had escaped theology. A scientist, worthy of the name, was not allowed to guess: he had to prove each sentence he uttered from facts. This was the criterion of scientific honesty. Theories unproven from facts were regarded as sinful pseudoscience, heresy in the scientific community.

It was only the downfall of Newtonian theory in this century which made scientists realize that their standards of honesty had been utopian. Before Einstein most scientists thought that Newton had deciphered God's ultimate laws by proving them from the facts. Ampère, in the early nineteenth century, felt he had to call his book on his speculations concerning electromagnetism: *Mathematical Theory of Electrodynamic Phenomena Unequivocally Deduced from Experiment*. But at the end of the volume he casually confesses that some of the experiments were never performed and even that the necessary instruments had not been constructed!

If all scientific theories are equally unprovable, what distinguishes scientific knowledge from ignorance, science from pseudoscience?

One answer to this question was provided in the twentieth century by 'inductive logicians'. Inductive logic set out to define the probabilities of different theories according to the available total evidence. If the mathematical probability of a theory is high, it qualifies as scientific; if it is low or even zero, it is not scientific. Thus the hallmark of scientific honesty would be never to say anything that is not at least highly probable. Probabilism has an attractive feature: instead of simply providing a black-and-white distinction between science and pseudoscience, it provides a continuous scale from poor theories with low probability to good theories with high probability. But, in 1934, Karl Popper, one of the most influential philosophers of our time, argued that the mathematical probability of all theories, scientific or pseudoscientific, given *any* amount of evidence is zero.* If Popper is right, scientific theories are not only equally unprovable but also equally improbable. A new demarcation criterion was needed and Popper proposed a rather stunning one. A theory may be scientific even if there is not a shred of evidence in its favour, and it may be pseudoscientific even if all the available evidence is in its favour. That is, the scientific or non-scientific character of a theory can be determined independently of the facts. A theory is 'scientific' if one is prepared to specify in advance a crucial experiment (or observation) which can falsify it, and it is pseudoscientific if one refuses to specify such a 'potential

* Popper's argument for this claim can be found in Appendix *vii of *The Logic of Scientific Discovery* (New York: Basic Books, 1959), 363–67.

falsifier'. But if so, we do not demarcate scientific theories from pseudoscientific ones, but rather scientific method from non-scientific method. Marxism, for a Popperian, is scientific if the Marxists are prepared to specify facts which, if observed, make them give up Marxism. If they refuse to do so, Marxism becomes a pseudoscience. It is always interesting to ask a Marxist, what conceivable event would make him abandon his Marxism. If he is committed to Marxism, he is bound to find it immoral to specify a state of affairs which can falsify it. Thus a proposition may petrify into pseudoscientific dogma or become genuine knowledge, depending on whether we are prepared to state observable conditions which would refute it.

Is, then, Popper's falsifiability criterion the solution to the problem of demarcating science from pseudoscience? No. For Popper's criterion ignores the remarkable tenacity of scientific theories. Scientists have thick skins. They do not abandon a theory merely because facts contradict it. They normally either invent some rescue hypothesis to explain what they then call a mere anomaly or, if they cannot explain the anomaly, they ignore it, and direct their attention to other problems. Note that scientists talk about anomalies, recalcitrant instances, not refutations. History of science, of course, is full of accounts of how crucial experiments allegedly killed theories. But such accounts are fabricated long after the theory had been abandoned. Had Popper ever asked a Newtonian scientist under what experimental conditions he would abandon Newtonian theory, some Newtonian scientists would have been exactly as nonplussed as are some Marxists.

What, then, is the hallmark of science? Do we have to capitulate and agree that a scientific revolution is just an irrational change in commitment, that it is a religious conversion? Tom Kuhn, a distinguished American philosopher of science, arrived at this conclusion after discovering the naïvety of Popper's falsificationism. But if Kuhn is right, then there is no explicit demarcation between science and pseudoscience, no distinction between scientific progress and intellectual decay, there is no objective standard of honesty. But what criteria can he then offer to demarcate scientific progress from intellectual degeneration?

In the last few years I have been advocating a methodology of scientific research programmes, which solves some of the problems which both Popper and Kuhn failed to solve.

First, I claim that the typical descriptive unit of great scientific achievements is not an isolated hypothesis but rather a research programme. Science is not simply trial and error, a series of conjectures and refutations. 'All swans are white' may be falsified by the discovery of one black swan. But such trivial trial and error does not rank as science. Newtonian science, for instance, is not simply a set of four conjectures—the three laws of mechanics and the law of gravitation. These four laws constitute only the 'hard core' of the Newtonian programme. But this hard core is tenaciously protected from refutation by a vast 'protective belt' of auxiliary hypotheses. And, even more importantly, the research programme also has a 'heuristic', that is, a powerful problem-solving machinery, which, with the help of sophisticated mathematical techniques, digests anomalies and even turns them into positive evidence. For instance, if a planet does not move exactly as it should, the Newtonian scientist checks his conjectures concerning atmospheric refraction, concerning propagation of light in magnetic storms, and hundreds of other conjectures which are all part of the programme. He may even invent a hitherto unknown planet and calculate its position, mass and velocity in order to explain the anomaly.

Now, Newton's theory of gravitation, Einstein's relativity theory, quantum mechanics, Marxism, Freudianism, are all research programmes, each with a characteristic hard core stubbornly defended, each with its more flexible protective belt and each with its elaborate problem-solving machinery. Each of them, at any stage of its development, has unsolved problems and undigested anomalies. All theories, in this sense, are born refuted and die refuted. But are they equally good? Until now I have been describing what research programmes are like. But how can one distinguish a scientific or progressive programme from a pseudoscientific or degenerating one?

Contrary to Popper, the difference cannot be that some are still unrefuted, while others are already refuted. When Newton published his *Principia*, it was common knowledge that it could not properly explain even the motion of the moon; in fact, lunar motion refuted Newton. Kaufmann, a distinguished physicist, refuted Einstein's relativity theory in the very year it was published.* But all the research programmes I admire have one characteristic in common. They all predict novel facts, facts which had been either undreamt of, or have indeed been contradicted by previous or rival programmes. In 1686, when Newton published his theory of gravitation, there were, for instance, two current theories concerning comets. The more popular one regarded comets as a signal from an angry God warning that He will strike and bring disaster. A little known theory of Kepler's held that comets were celestial bodies moving along straight lines. Now according to Newtonian theory, some of them moved in hy-

* Here, as elsewhere in this reading, Lakatos is using the word *refuted* rather loosely. For Lakatos, a refutation is any apparently well-founded result that seems to be inconsistent with a theory. In the two cases he mentions — Newton and the moon, Einstein and Kaufmann's experiments on beta rays — the "refutations" were later shown to be spurious: the moon's motion is not actually inconsistent with Newton's theory, and Kaufmann's results were due to experimental error. For an account of Kaufmann's experiments and Einstein's reaction to them, see Arthur I. Miller, *Albert Einstein's Special Theory of Relativity* (Reading, Mass.: Addison-Wesley, 1981).

perbolas or parabolas never to return; others moved in ordinary ellipses. Halley, working in Newton's programme, calculated on the basis of observing a brief stretch of a comet's path that it would return in seventy-two years' time; he calculated to the minute when it would be seen again at a well-defined point of the sky. This was incredible. But seventy-two years later, when both Newton and Halley were long dead, Halley's comet returned exactly as Halley predicted. Similarly, Newtonian scientists predicted the existence and exact motion of small planets which had never been observed before. Or let us take Einstein's programme. This programme made the stunning prediction that if one measures the distance between two stars in the night and if one measures the distance between them during the day (when they are visible during an eclipse of the sun), the two measurements will be different. Nobody had thought to make such an observation before Einstein's programme. Thus, in a progressive research programme, theory leads to the discovery of hitherto unknown novel facts. In degenerating programmes, however, theories are fabricated only in order to accommodate known facts. Has, for instance, Marxism ever predicted a stunning novel fact successfully? Never! It has some famous unsuccessful predictions. It predicted the absolute impoverishment of the working class. It predicted that the first socialist revolution would take place in the industrially most developed society. It predicted that socialist societies would be free of revolutions. It predicted that there will be no conflict of interests between socialist countries. Thus the early predictions of Marxism were bold and stunning but they failed. Marxists explained all their failures: they explained the rising living standards of the working class by devising a theory of imperialism; they even explained why the first socialist revolution occurred in industrially backward Russia. They 'explained' Berlin 1953, Budapest 1956, Prague 1968. They 'explained' the Russian–Chinese conflict. But their auxiliary hypotheses were all cooked up after the event to protect Marxian theory from the facts. The Newtonian programme led to novel facts; the Marxian lagged behind the facts and has been running fast to catch up with them.

To sum up. The hallmark of empirical progress is not trivial verifications: Popper is right that there are millions of them. It is no success for Newtonian theory that stones, when dropped, fall towards the earth, no matter how often this is repeated. But so-called 'refutations' are not the hallmark of empirical failure, as Popper has preached, since all programmes grow in a permanent ocean of anomalies. What really count are dramatic, unexpected, stunning predictions: a few of them are enough to tilt the balance; where theory lags behind the facts, we are dealing with miserable degenerating research programmes.

Now, how do scientific revolutions come about? If we have two rival research programmes, and one is progressing while the other is degenerating, scientists tend to join the progressive programme. This is the rationale of scientific revolutions. But while it is a matter of intellectual honesty to keep the record public, it is not dishonest to stick to a degenerating programme and try to turn it into a progressive one.

As opposed to Popper the methodology of scientific research programmes does not offer instant rationality. One must treat budding programmes leniently: programmes may take decades before they get off the ground and become empirically progressive. Criticism is not a Popperian quick kill, by refutation. Important criticism is always constructive: there is no refutation without a better theory. Kuhn is wrong in thinking that scientific revolutions are sudden, irrational changes in vision. The history of science refutes both Popper and Kuhn: on close inspection both Popperian crucial experiments and Kuhnian revolutions turn out to be myths: what normally happens is that progressive research programmes replace degenerating ones.

The problem of demarcation between science and pseudoscience has grave implications also for the institutionalization of criticism. Copernicus's theory was banned by the Catholic Church in 1616 because it was said to be pseudoscientific. It was taken off the index in 1820 because by that time the Church deemed that facts had proved it and therefore it became scientific. The Central Committee of the Soviet Communist Party in 1949 declared Mendelian genetics pseudoscientific and had its advocates, like Academician Vavilov, killed in concentration camps; after Vavilov's murder Mendelian genetics was rehabilitated; but the Party's right to decide what is science and publishable and what is pseudoscience and punishable was upheld. The new liberal Establishment of the West also exercises the right to deny freedom of speech to what it regards as pseudoscience, as we have seen in the case of the debate concerning race and intelligence. All these judgments were inevitably based on some sort of demarcation criterion. This is why the problem of demarcation between science and pseudoscience is not a pseudo-problem of armchair philosophers: it has grave ethical and political implications.

PAUL R. THAGARD

Why Astrology Is a Pseudoscience

Most philosophers and historians of science agree that astrology is a pseudoscience, but there is little agreement on *why* it is a pseudoscience. Answers range from matters of verifiability and falsifiability, to questions of progress and Kuhnian normal science, to the different sorts of objections raised by a large panel of scientists recently organized by *The Humanist* magazine. Of course there are also Feyerabendian anarchists* and others who say that no demarcation of science from pseudoscience is possible. However, I shall propose a complex criterion for distinguishing disciplines as pseudoscientific; this criterion is unlike verificationist and falsificationist attempts in that it introduces social and historical features as well as logical ones.

I begin with a brief description of astrology. It would be most unfair to evaluate astrology by reference to the daily horoscopes found in newspapers and popular magazines. These horoscopes deal only with sun signs, whereas a full horoscope makes reference to the "influences" also of the moon and the planets, while also discussing the ascendant sign and other matters.

Astrology divides the sky into twelve regions, represented by the familiar signs of the Zodiac: Aquarius, Libra and so on. The sun sign represents the part of the sky occupied by the sun at the time of birth. For example, anyone born between September 23 and October 22 is a Libran. The ascendant sign, often assumed to be at least as important as the sun

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* Paul Feyerabend (1924–94) used the term *epistemological anarchism* in his *Against Method* (London: New Left Books, 1975), arguing that there is no rational method in science and that the only principle consistent with scientific progress is "anything goes."

sign, represents the part of the sky rising on the eastern horizon at the time of birth, and therefore changes every two hours. To determine this sign, accurate knowledge of the time and place of birth is essential. The moon and the planets (of which there are five or eight depending on whether Uranus, Neptune and Pluto are taken into account) are also located by means of charts on one of the parts of the Zodiac. Each planet is said to exercise an influence in a special sphere of human activity; for example. Mars governs drive, courage and daring, while Venus governs love and artistic endeavor. The immense number of combinations of sun, ascendant, moon and planetary influences allegedly determines human personality, behavior and fate.

Astrology is an ancient practice, and appears to have its origins in Chaldea, thousands of years B.C. By 700 B.C., the Zodiac was established, and a few centuries later the signs of the Zodiac were very similar to current ones. The conquests of Alexander the Great brought astrology to Greece, and the Romans were exposed in turn. Astrology was very popular during the fall of the Republic, with many notables such as Julius Caesar having their horoscopes cast. However, there was opposition from such men as Lucretius and Cicero.

Astrology underwent a gradual codification culminating in Ptolemy's *Tetrabiblos* [20], written in the second century A.D. This work describes in great detail the powers of the sun, moon and planets, and their significance in people's lives. It is still recognized as a fundamental textbook of astrology. Ptolemy took astrology as seriously as he took his famous work in geography and astronomy; this is evident from the introduction to the *Tetrabiblos*, where he discusses two available means of making predictions based on the heavens. The first and admittedly more effective of these concerns the relative movements of the sun, moon and planets, which Ptolemy had already treated in his celebrated *Almagest* [19]. The secondary but still legitimate means of prediction is that in which we use the "natural character" of the aspects of movement of heavenly bodies to "investigate the changes which they bring about in that which they surround." ([20], p. 3). He argues that this method of prediction is possible because of the manifest effects of the sun, moon and planets on the earth, for example on weather and the tides.

The European Renaissance is heralded for the rise of modern science, but occult arts such as astrology and alchemy flourished as well. Arthur Koestler has described Kepler's interest in astrology: not only did astrology provide Kepler with a livelihood, he also pursued it as a serious interest, although he was skeptical of the particular analyses of previous astrologers ([13], pp. 244–248). Astrology was popular both among intellectuals and the general public through the seventeenth century. However, astrology lost most of this popularity in the eighteenth century, when it was attacked by such figures of the Enlightenment as Swift [24] and Voltaire [29]. Only since the 1930's has astrology again gained a huge audience: most people today know at least their sun signs, and a great many believe that the stars and planets exercise an important influence on their lives.

In an attempt to reverse this trend, Bart Bok, Lawrence Jerome and Paul Kurtz drafted in 1975 a statement attacking astrology; the statement was signed by 192 leading scientists, including 19 Nobel prize winners. The statement raises three main issues: astrology originated as part of a magical world view, the planets are too distant for there to be any physical foundation for astrology, and people believe it merely out of longing for comfort ([2], pp. 9f.). None of these objections is ground for condemning astrology as pseudoscience. To show this, I shall briefly discuss articles written by Bok [1] and Jerome [12] in support of the statement.

According to Bok, to work on statistical tests of astrological predictions is a waste of time unless it is demonstrated that astrology has some sort of physical foundation ([1], p. 31). He uses the smallness of gravitational and radiative effects of the stars and planets to suggest that there is no such foundation. He also discusses the psychology of belief in astrology, which is the result of individuals' desperation in seeking solutions to their serious personal problems. Jerome devotes most of his article to the origins of astrology in the magical principle of correspondences. He claims that astrology is a system of magic rather than science, and that it fails "not because of any inherent inaccuracies due to precession or lack of exact knowledge concerning time of birth or conception, but rather because its interpretations and predictions are grounded in the ancients' magical world view" ([12], p. 46). He does however discuss some statistical tests of astrology, which I shall return to below.

These objections do not show that astrology is a pseudoscience. First, origins are irrelevant to scientific status. The alchemical origins of chemistry ([11], pp. 10–18) and the occult beginnings of medicine [8] are as magical as those of astrology, and historians have detected mystical influences in the work of many great scientists, including Newton and Einstein. Hence astrology cannot be condemned simply for the magical origins of its principles. Similarly, the psychology of popular belief is also in itself irrelevant to the status of astrology: people often believe even good theories for illegitimate reasons, and even if most people believe astrology for personal, irrational reasons, good reasons may be available.[1] Finally the lack of a physical foundation hardly marks a theory as unscientific ([22], p. 2). Examples: when Wegener [31] proposed continental drift, no mechanism was known, and a link between smoking and cancer has been established statistically [28] though the details of carcinogenesis remain to be discovered. Hence the objections of Bok, Jerome and Kurtz fail to mark astrology as pseudoscience.

Now we must consider the application of the criteria of verifiability and falsifiability to astrology. Roughly, a theory is said to be verifiable if it is possible to deduce observation statements from it. Then in principle, observations can be used to confirm or disconfirm the theory. A theory is scientific only if it is verifiable. The vicissitudes of the verification principle are too well known to recount here ([9], ch. 4). Attempts by A. J. Ayer to articulate the principle failed either by ruling out most of science as unscientific, or by ruling out nothing. Moreover, the theory/observation distinction has increasingly come into question. All that remains is a vague sense that testability somehow is a mark of scientific theories ([9], ch. 4; [10], pp. 30–32).

Well, astrology *is* vaguely testable. Because of the multitude of influences resting on tendencies rather than laws, astrology is incapable of making precise predictions. Nevertheless, attempts have been made to test the reality of these alleged tendencies, using large scale surveys and statistical evaluation. The pioneer in this area was Michel Gauquelin, who examined the careers and times of birth of 25,000 Frenchmen. Astrology suggests that people born under certain signs or planets are likely to adopt certain occupations: for example, the influence of the warlike planet Mars tends to produce soldiers or athletes, while Venus has an artistic influence. Notably, Gauquelin found *no significant correlation* between careers and either sun sign, moon sign, or ascendant sign. However, he did find some statistically interesting correlations between certain occupations of people and the position of certain planets at the time of their birth ([5], ch. 11, [6]). For example, just as astrology would suggest, there is a greater than chance association of athletes and Mars, and a greater than chance association of scientists and Saturn, where the planet is rising or at its zenith at the moment of the individual's birth.

These findings and their interpretation are highly controversial, as are subsequent studies in a similar vein [7]. Even if correct, they hardly verify astrology, especially considering the negative results found for the most important astrological categories. I have mentioned Gauquelin in order to suggest that through the use of statistical techniques astrology is at least *verifiable*. Hence the verification principle does not mark astrology as pseudoscience.

Because the predictions of astrologers are generally vague, a Popperian would assert that the real problem with astrology is that it is not falsifiable: astrologers cannot make predictions which if unfulfilled would lead them to give up their theory. Hence because it is unfalsifiable, astrology is unscientific.

But the doctrine of falsifiability faces serious problems as described by Duhem [4], Quine [21], and Lakatos [15]. Popper himself noticed early that no observation ever guarantees falsification: a theory can always be retained by introducing or modifying auxiliary hypotheses, and even observation statements are not incorrigible ([17], p. 50). Methodological decisions about what can be tampered with are required to block the escape from falsification. However, Lakatos has persuasively argued that making such decisions in advance of tests is arbitrary and may often lead to overhasty rejection of a sound theory which *ought* to be be saved by anti-falsificationist stratagems ([15], ~~pp. 112 ff.~~). Falsification only occurs when a better theory comes along. Then falsifiability is only a matter of replaceability by another theory, and since astrology is in principle replaceable by another theory, falsifiability provides no criterion for rejecting astrology as pseudoscientific. We saw in the discussion of Gauquelin that astrology can be used to make predictions about statistical regularities, but the non-existence of these regularities does not falsify astrology; but here astrology does not appear worse than the best of scientific theories, which also resist falsification until alternative theories arise.[2]

Astrology cannot be condemned as pseudoscientific on the grounds proposed by verificationists, falsificationists, or Bok and Jerome. But undoubtedly astrology today faces a great many unsolved problems ([32], ch. 5). One is the negative result found by Gauquelin concerning careers and signs. Another is the problem of the precession of the equinoxes, which astrologers generally take into account when heralding the "Age of Aquarius" but totally neglect when figuring their charts. Astrologers do not always agree on the significance of the three planets, Neptune, Uranus and Pluto, that were discovered since Ptolemy. Studies of twins do not show similarities of personality and fate that astrology would suggest. Nor does astrology make sense of mass disasters, where numerous individuals with very different horoscopes come to similar ends.

But problems such as these do not in themselves show that astrology is either false or pseudoscientific. Even the best theories face unsolved problems throughout their history. To get a criterion demarcating astrology from science, we need to consider it in a wider historical and social context.

A demarcation criterion requires a matrix of three elements: theory, community, historical context. Under the first heading, "theory", fall familiar matters of structure, prediction, explanation and problem solving. We might also include the issue raised by Bok and Jerome about whether the theory has a physical foundation. Previous demarcationists have concentrated on this theoretical element, evident in the concern of the verification and ~~falsification principles with~~ prediction. But we have seen that this approach is not sufficient for characterizing astrology as pseudoscientific.

We must also consider the *community* of advocates of the theory, in this case the community of practitioners of astrology. Several questions are important here. First, are the practitioners in agreement on the principles of the theory and on how to go about solving problems which the theory faces? Second, do they care, that is, are they concerned about explaining anomalies and comparing the success of their theory to the record of other theories? Third, are the practitioners actively involved in attempts at confirming and disconfirming their theory?

The question about comparing the success of a theory with that of other theories introduces the third element of the matrix, historical context. The historical work of Kuhn and others has shown that in general a theory is rejected only when (1) it has faced anomalies over a long period of time and (2) it has been challenged by another theory. Hence under the heading of historical context we must consider two factors relevant to demarcation: the record of a theory over time in explaining new facts and dealing with anomalies, and the availability of alternative theories.

We can now propose the following principle of demarcation:

> A theory or discipline which purports to be scientific is *pseudoscientific* if and only if:
>
> 1 it has been less progressive than alternative theories over a long period of time, and faces many unsolved problems; but
>
> 2 the community of practitioners makes little attempt to develop the theory towards solutions of the problems, shows no concern for attempts to evaluate the theory in relation to others, and is selective in considering confirmations and disconfirmations.

Progressiveness is a matter of the success of the theory in adding to its set of facts explained and problems solved ([15], p. 118; cf. [26], p. 83).

This principle captures, I believe, what is most importantly unscientific about astrology. First, astrology is dramatically unprogressive, in that it has changed little and has added nothing to its explanatory power since the time of Ptolemy. Second, problems such as the precession of equinoxes are outstanding. Third, there are alternative theories of personality and behavior available: one need not be an uncritical advocate of behaviorist, Freudian, or Gestalt theories to see that since the nineteenth century psychological theories have been expanding to deal with many of the phenomena which astrology explains in terms of heavenly influences. The important point is not that any of these psychological theories is established or true, only that they are growing alternatives to a long-static astrology. Fourth and finally, the community of astrologers is generally unconcerned with advancing astrology to deal with outstanding problems or with evaluating the theory in relation to others.[3] For these reasons, my criterion marks astrology as pseudoscientific.*

This demarcation criterion differs from those implicit in Lakatos and

* Since writing this paper, Thagard has offered a revised account of pseudoscience in chapter 9 of his book *Computational Philosophy of Science* (Cambridge, Mass.: MIT Press, 1988). This revised account is discussed in our commentary on chapter 1.

Kuhn. Lakatos has said that what makes a series of theories constituting a research program scientific is that it is progressive: each theory in the series has greater corroborated content than its predecessor ([15], p. 118). While I agree with Lakatos that progressiveness is a central notion here, it is not sufficient to distinguish science from pseudoscience. We should not brand a nonprogressive discipline as pseudoscientific unless it is being maintained against more progressive alternatives. Kuhn's discussion of astrology focuses on a different aspect of my criterion. He says that what makes astrology unscientific is the absence of the paradigm-dominated puzzle solving activity characteristic of what he calls normal science ([14], p. 9). But as Watkins has suggested, astrologers are in some respects model normal scientists: they concern themselves with solving puzzles at the level of individual horoscopes, unconcerned with the foundations of their general theory or paradigm ([30], p. 32). Hence that feature of normal science does not distinguish science from pseudoscience. What makes astrology pseudoscientific is not that it lacks periods of Kuhnian normal science, but that its proponents adopt uncritical attitudes of "normal" scientists despite the existence of more progressive alternative theories. (Note that I am not agreeing with Popper [18] that Kuhn's normal scientists are unscientific; they can become unscientific only when an alternative paradigm has been developed.) However, if one looks not at the puzzle solving at the level of particular astrological predictions, but at the level of theoretical problems such as the precession of the equinoxes, there is some agreement between my criterion and Kuhn's; astrologers do not have a paradigm-induced confidence about solving theoretical problems.

Of course, the criterion is intended to have applications beyond astrology. I think that discussion would show that the criterion marks as pseudoscientific such practices as witchcraft and pyramidology, while leaving contemporary physics, chemistry and biology unthreatened. The current fad of biorhythms, implausibly based like astrology on date of birth, cannot be branded as pseudoscientific because we lack alternative theories giving more detailed accounts of cyclical variations in human beings, although much research is in progress.[4]

One interesting consequence of the above criterion is that a theory can be scientific at one time but pseudoscientific at another. In the time of Ptolemy or even Kepler, astrology had few alternatives in the explanation of human personality and behavior. Existing alternatives were scarcely more sophisticated or corroborated than astrology. Hence astrology should be judged as not pseudoscientific in classical or Renaissance times, even though it is pseudoscientific today. Astrology was not simply a perverse sideline of Ptolemy and Kepler, but part of their scientific activity, even if a physicist involved with astrology today should be looked at askance. Only when the historical and social aspects of science are neglected does it become plausible that pseudoscience is an unchanging category. Rationality is not a property of ideas eternally: ideas, like actions, can be rational at one time but irrational at others. Hence relativizing the science/pseudoscience distinction to historical periods is a desirable result.

But there remains a challenging historical problem. According to my criterion, astrology only became pseudoscientific with the rise of modern psychology in the nineteenth century. But astrology was already virtually excised from scientific circles by the beginning of the eighteenth. How could this be? The simple answer is that a theory can take on the appearance of an unpromising project well before it deserves the label of pseudoscience. The Copernican revolution and the mechanism of Newton, Descartes and Hobbes undermined the plausibility of astrology.[5] Lynn Thorndike [27] has described how the Newtonian theory pushed aside what had been accepted as a universal natural law, that inferiors such as inhabitants of earth are ruled and governed by superiors such as the stars and the planets. William Stahlman [23] has described how the immense growth of science in the seventeenth century contrasted with the stagnation of astrology. These developments provided good reasons for discarding astrology as a promising pursuit, but they were not yet enough to brand it as pseudoscientific, or even to refute it.

Because of its social aspect, my criterion might suggest a kind of cultural relativism. Suppose there is an isolated group of astrologers in the jungles of South America, practicing their art with no awareness of alternatives. Are we to say that astrology is *for them* scientific? Or, going in the other direction, should we count as alternative theories ones which are available to extraterrestrial beings, or which someday will be conceived? This wide construal of "alternative" would have the result that our best current theories are probably pseudoscientific. These two questions employ, respectively, a too narrow and a too broad view of alternatives. By an alternative theory I mean one generally available in the world. This assumes first that there is some kind of communication network to which a community has, or should have, access. Second, it assumes that the onus is on individuals and communities to find out about alternatives. I would argue (perhaps against Kuhn) that this second assumption is a general feature of rationality; it is at least sufficient to preclude ostrichism as a defense against being judged pseudoscientific.

In conclusion, I would like to say why I think the question of what constitutes a pseudoscience is important. Unlike the logical positivists, I am not grinding an anti-metaphysical ax, and unlike Popper, I am not grinding an anti-Freudian or anti-Marxian one.[6] My concern is social: society faces the twin problems of lack of public concern with the advancement of science, and lack of public concern with the important ethical issues now arising in science and technology, for example around the topic of genetic engineering. One reason for this dual lack of concern is the wide popularity of pseudoscience and the occult among the general public. Elucidation of how science differs from pseudoscience is the philosophical side of an attempt to overcome public neglect of genuine science.[7]

Notes

1. However, astrology would doubtlessly have many fewer supporters if horoscopes tended less toward compliments and pleasant predictions and more toward the kind of analysis included in the following satirical horoscope from the December, 1977, issue of *Mother Jones*: VIRGO (Aug. 23–Sept. 22). You are the logical type and hate disorder. This nitpicking is sickening to your friends. You are cold and unemotional and sometimes fall asleep while making love. Virgos make good bus drivers.

2. For an account of the comparative evaluation of theories, see [26].

3. There appear to be a few exceptions; see [32].

4. The fad of biorhythms, now assuming a place beside astrology in the popular press, must be distinguished from the very interesting work of Frank Brown and others on biological rhythms. For a survey, see [5].

5. Plausibility is in part a matter of a hypothesis being of an appropriate *kind*, and is relevant even to the acceptance of a theory. See [26], p. 90, and [25].

6. On psychoanalysis see [3]. I would argue that Cioffi neglects the question of alternatives to psychoanalysis and the question of its progressiveness.

7. I am grateful to Dan Hausman and Elias Baumgarten for comments.

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MICHAEL RUSE

Creation-Science Is Not Science

In December 1981 I appeared as an expert witness for the plaintiffs and the American Civil Liberties Union (ACLU) in their successful challenge of Arkansas Act 590, which demanded that teachers give "balanced treatment" to "creation-science" and evolutionary ideas.[1] My presence occasioned some surprise, for I am an historian and philosopher of science. In this essay, I do not intend to apologize for either my existence or my calling, nor do I intend to relive past victories[2]; rather, I want to explain why a philosopher and historian of science finds the teaching of "creation-science" in science classrooms offensive.

Obviously, the crux of the issue—the center of the plaintiffs' case—is the status of creation-science. Its advocates claim that it is genuine science and may, therefore, be legitimately and properly taught in the public schools. Its detractors claim that it is not genuine science but a form of religion—dogmatic Biblical literalism by another name. Which is it, and who is to decide?

It is somewhat easier to describe who should participate in decisions on this issue. On the one hand, one naturally appeals to the authority of religious people and theologians. Does creation-science fit the accepted definitions of a religion? (In Arkansas, the ACLU produced theologians who said that indeed it did.) One also appeals to the authority of scientists. Does creation-science fit current definitions of science? (In Arkansas, the ACLU produced scientists who said that indeed it did not.)[3]

Having, as it were, appealed to the practitioners—theologians and scientists—a link still seems to be missing. Someone is needed to talk at a more theoretical level about the nature of science—any science—and then show that creation-science simply does not fit the part. As a philosopher and an historian, it is my job to look at science, and to ask precisely those questions about defining characteristics.

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What Is Science?

It is simply not possible to give a neat definition—specifying necessary and sufficient characteristics—which separates all and only those things that have ever been called "science." The concept "science" is not as easily definable as, for example, the concept "triangle." Science is a phenomenon that has developed through the ages—dragging itself apart from religion, philosophy, superstition, and other bodies of human opinion and belief.[4]

What we call "science" today is a reasonably striking and distinctive set of claims, which have a number of characteristic features. As with most things in life, some items fall on the borderline between science and nonscience (e.g., perhaps Freudian psychoanalytic theory). But it is possible to state positively that, for example, physics and chemistry are sciences, and Plato's Theory of Forms and Swedenborgian theology are not.[5]

In looking for defining features, the obvious place to start is with science's most striking aspect—it is an empirical enterprise about the real world of sensation. This is not to say that science refers only to observable entities. Every mature science contains unobservables, like electrons and genes, but ultimately, they refer to the world around us. Science attempts to understand this empirical world. What is the basis for this understanding? Surveying science and the history of science today, one thing stands out: science involves a search for order. More specifically, science looks for unbroken, blind, natural regularities *(laws)*. Things in the world do not happen in just any old way. They follow set paths, and science tries to capture this fact. Bodies of science, therefore, known variously as "theories" or "paradigms" or "sets of models," are collections of laws.[6]

Thus, in Newtonian physics we find Newton's three laws of motion, the law of gravitational attraction, Kepler's laws of planetary motion, a so forth. Similarly, for instance, in population genetics we find the H Weinberg law. However, when we turn to something like philosop do not find the same appeal to empirical law. Plato's Theory of Fo only indirectly refers to this world. Analogously, religion does not insist on unbroken law. Indeed, religious beliefs frequently allow or suppose events outside law or else events that violate law (miracles). Jesus feeding the 5,000 with the loaves and fishes was one such event. This is not to say that religion is false, but it does say that religion is not science. When the loaves and fishes multiplied to a sufficiency to feed so many people, things happened that did not obey natural law, and hence the feeding of the 5,000 is an event beyond the ken of science.[7]

A major part of the scientific enterprise involves the use of law to effect *explanation*. One tries to show why things are as they are—and how they fall beneath or follow from law (together perhaps with certain specified initial conditions). Why, for example, does a cannon ball go in a parabola and not in a circle? Because of the constraints of Newton's laws. Why do two blue-eyed parents always have blue-eyed children? Because this trait obeys Mendel's first law, given the particular way in which the genes control eye-color. A scientific explanation must appeal to law and must show that what is being explained had to occur. The explanation excludes those things that did not happen.[8]

The other side of explanation is *prediction*. The laws indicate what is going to happen: that the ball will go in a parabola, that the child will be blue-eyed. In science, as well as in futurology, one can also, as it were, predict backwards. Using laws, one infers that a particular, hitherto-unknown phenomenon or event took place in the past. Thus, for instance, one might use the laws of physics to infer back to some eclipse of the sun reported in ancient writings.

Closely connected with the twin notions of explanation and prediction comes *testability*. A genuine scientific theory lays itself open to check against the real world: the scientist can see if the inferences made in explanation and prediction actually obtain in nature. Does the chemical reaction proceed as suspected? In Young's double slit experiment, does one find the bands of light and dark predicted by the wave theory? Do the continents show the expected after-effects of drift?

Testability is a two-way process. The researcher looks for some positive evidence, for *confirmation*. No one will take seriously a scientific theory that has no empirical support (although obviously a younger theory is liable to be less well-supported than an older theory). Conversely, a theory must be open to possible refutation. If the facts speak against a theory, then it must go. A body of science must be *falsifiable*. For example, Kepler's laws could have been false: if a planet were discovered going in squares, then the laws would have been shown to be incorrect. However, in distinguishing science from nonscience, no amount of empirical evidence can disprove, for example, the Kantian philosophical claim that one ought to treat people as ends rather than means. Similarly, Catholic religious claims about transubstantiation (the changing of the bread and wine into the body and blood of Christ) are unfalsifiable.[9]

Science is *tentative*. Ultimately, a scientist must be prepared to reject his theory. Unfortunately, not all scientists are prepared to do in practice what they promise to do in theory; but the weaknesses of individuals are counterbalanced by the fact that, as a group, scientists do give up theories that fail to answer to new or reconsidered evidence. In the last 30 years, for example, geologists have reversed their strong convictions that the continents never move.

Scientists do not, of course, immediately throw their theories away as soon as any counter-evidence arrives. If a theory is powerful and successful, then some problems will be tolerated, but scientists must be prepared to change their minds in the face of the empirical evidence. In this regard, the scientists differ from both the philosophers and the theologians. Nothing in the real world would make the Kantian change his mind, and the Catholic is equally dogmatic, despite any empirical evidence about the stability of bread and wine. Such evidence is simply considered irrelevant.[10]

Some other features of science should also be mentioned, for instance, the urge for simplicity and unification; however, I have now listed the major characteristics. Good science—like good philosophy and good religion—presupposes an attitude that one might describe as professional *integrity*. A scientist should not cheat or falsify data or quote out of context or do any other thing that is intellectually dishonest. Of course, as always, some individuals fail; but science as a whole disapproves of such actions. Indeed, when transgressors are detected, they are usually expelled from the community. Science depends on honesty in the realm of ideas. One may cheat on one's taxes; one may not fiddle the data.[11]

■ | Creation-Science Considered

How does creation-science fit the criteria of science listed in the previous section? By "creation-science" in this context, I refer not just to the definition given in Act 590, but to the whole body of literature which goes by that name. The doctrine includes the claims that the universe is very young (6,000 to 20,000 years), that everything started instantaneously, that human beings had ancestry separate from apes, and that a monstrous flood once engulfed the entire earth.[12]

LAWS—NATURAL REGULARITIES

Science is about unbroken, natural regularity. It does not admit miracles. It is clear, therefore, that again and again, creation-science invokes happenings and causes outside of law. For instance, the only reasonable inference from Act 590 (certainly the inference that was accepted in the Arkansas court) is that for creation-science the origin of the universe and life in it is not bound by law. Whereas the definition of creation-science includes the unqualified phrase "sudden creation of the universe, energy and life from nothing," the definition of evolution specifically includes the qualification that its view of origins is "naturalistic." Because "naturalistic" means "subject to empirical law," the deliberate omission of such a term in the characterization of creation-science means that no laws were involved.

In confirmation of this inference, we can find identical claims in the writings of creation scientists: for instance, the following passage from Duane T. Gish's popular work *Evolution—The Fossils Say No!*

> CREATION. By creation we mean the bringing into being of the basic kinds of plants and animals by the process of sudden, or fiat, creation described in the first two chapters of Genesis. Here we find the creation by God of the plants and animals, each commanded to reproduce after its own kind using processes which were essentially instantaneous.
>
> We do not know how God created, what processes He used, *for God used processes which are not now operating anywhere in the natural universe.* This is why we refer to divine creation as special creation. We cannot discover by scientific investigations anything about the creative processes used by God.[13]

By Gish's own admission, we are not dealing with science. Similar sentiments can be found in *The Genesis Flood* by John Whitcomb, Jr., and Henry M. Morris:

> But during the period of Creation, God was introducing order and organization and energization into the universe in a very high degree, even to life itself! *It is thus quite plain that the processes used by God in creation were utterly different from the processes which now operate in the universe!* The Creation was a unique period, entirely incommensurate with this present world. This is plainly emphasized and reemphasized in the divine revelation which God has given us concerning Creation, which concludes with these words: 'And the heavens and the earth were *finished*, and *all* the host of them. And on the seventh day God *finished* His work which He had made; and He *rested* on the seventh day from *all His work* which He had made. And God blessed the seventh day, and hallowed it; because that in it He *rested* from *all* his work which God had created and made.' In view of these strong and repeated assertions, is it not the height of presumption for man to attempt to study Creation in terms of present processes?[14]

Creation scientists generally acknowledge this work to be *the* seminal contribution that led to the growth of the creation-science movement. Morris, in particular, is the father figure of creation-science and Gish his chief lieutenant.

Creation scientists also break with law in many other instances. The creationists believe that the Flood, for example, could not have just occurred through blind regularities. As Whitcomb and Morris make very clear, certain supernatural interventions were necessary to bring about the Flood.[15] Similarly, in order to ensure the survival of at least some organisms, God had to busy himself and break through law.

EXPLANATION AND PREDICTION

Given the crucial role that law plays for the scientist in these processes, neither explanation nor prediction is possible where no law exists.

Thus, explanation and prediction simply cannot even be attempted when one deals with creation-science accounts either of origins or of the Flood.

Even against the broader vistas of biology, creation-science is inadequate. Scientific explanation/prediction must lead to the thing being explained/predicted, showing why that thing obtains and not other things. Why does the ball go in a parabola? Why does it not describe a circle? Take an important and pervasive biological phenomenon, namely, "homologies," the isomorphisms between the bones of different animals. These similarities were recognized as pervasive facets of nature even before Darwin published *On the Origin of Species*. Why are the bones in the forelimbs of men, horses, whales, and birds all so similar, even though the functions are quite different? Evolutionists explain homologies naturally and easily, as a result of common descent. Creationists can give no explanation, and make no predictions. All they can offer is the disingenuous comment that homology signifies nothing, because classification is all man-made and arbitrary anyway. Is it arbitrary that man is not classified with the birds?[16] Why are Darwin's finches distributed in the way that we find on the Galapagos? Why are there 14 separate species of this little bird, scattered over a small group of islands in the Pacific on the equator? On those rare occasions when Darwin's finches do fly into the pages of creation-science, it is claimed either that they are all the same species (false), or that they are a case of degeneration from one "kind" created back at the beginning of life.[17] Apart from the fact that "kind" is a term of classification to be found only in Genesis, this is no explanation. How could such a division of the finches have occurred, given the short span that the creationists allow since the Creation? And, in any case, Darwin's finches are anything but degenerates. Different species of finch have entirely different sorts of beaks, adapted for different foodstuffs—evolution of the most sophisticated type.[18]

TESTABILITY, CONFIRMATION, AND FALSIFIABILITY

Testability, confirmation, and falsifiability are no better treated by creation-science. A scientific theory must provide more than just after-the-fact explanations of things that one already knows. One must push out into the frontiers of new knowledge, trying to predict new facts, and risking the theory against the discovery of possible falsifying information. One cannot simply work at a secondary level, constantly protecting one's views against threat: forever inventing *ad hoc* hypotheses to save one's core assumptions.

Creation scientists do little or nothing by way of genuine test. Indeed, the most striking thing about the whole body of creation-science literature is the virtual absence of *any* experimental or observational work by creation scientists. Almost invariably, the creationists work exclusively with the discoveries and claims of evolutionists, twisting the conclusions to their own ends. Argument proceeds by showing evolution (specifically Darwinism) wrong, rather than by showing Creationism right.

However, this way of proceeding—what the creationists refer to as the "two model approach"—is simply a fallacious form of argument. The views of people like Fred Hoyle and N. C. Wickramasinghe, who believe that life comes from outer space, are neither creationist nor truly evolutionist.[19] Denying evolution in no way proves Creationism. And, even if a more straightforward either/or between evolution and Creationism existed, the perpetually negative approach is just not the way that science proceeds. One must find one's own evidence in favor of one's position, just as physicists, chemists, and biologists do.

Do creation scientists ever actually expose their theories and ideas to test? Even if they do, when new counter-empirical evidence is discovered, creation scientists appear to pull back, refusing to allow their position to be falsified.

Consider, for instance, the classic case of the "missing link"—namely, that between man and his ancestors. The creationists say that there are no plausible bridging organisms whatsoever. Thus, this super-gap between man and all other animals (alive or dead) supposedly underlines the creationists' contention that man and apes have separate ancestry. But what about the australopithecines, organisms that paleontologists have, for most of this century, claimed are plausible human ancestors? With respect, argue the creationists, australopithecines are not links, because they had apelike brains, they *walked* like apes, and they used their knuckles for support, just like gorillas. Hence, the gap remains.[20]

However, such a conclusion can be maintained only by blatant disregard of the empirical evidence. *Australopithecus afarensis* was a creature with a brain the size of that of an ape which walked upright.[21] Yet the creationists do not concede defeat. They then argue that the *Australopithecus afarensis* is like an orangutan.[22] In short, nothing apparently makes the creationists change their minds, or allows their views to be tested, lest they be falsified.

TENTATIVENESS

Creation-science is not science because there is absolutely no way in which creationists will budge from their position. Indeed, the leading organization of creation-science, The Creation Research Society (with 500 full members, all of whom must have an advanced degree in a scientific/technological area), demands that its members sign a statement affirming that they take the Bible as literally true.[23] Unfortunately, an organization cannot require such a condition of membership, and then claim to be a scientific organization. Science must be open to change, however confident one may feel at present. Fanatical dogmatism is just not acceptable.

INTEGRITY

Creation scientists use any fallacy in the logic books to achieve their ends. Most particularly, apart from grossly distorting evolutionists' positions, the creation-scientists frequently use inappropriate or incomplete quotations. They take the words of some eminent evolutionist, and attempt to make him or her say exactly the opposite to that intended. For instance, in *Creation: The Facts of Life*, author Gary E. Parker constantly refers to "noted Harvard geneticist" Richard Lewontin as claiming that the hand and the eye are the best evidence of God's design.[24] Can this reference really be true? Has the author of *The Genetic Basis of Evolutionary Change*[25] really foresworn Darwin for Moses? In fact, when one looks at Lewontin's writings, one finds that he says that *before Darwin*, people believed the hand and the eye to be the effect of direct design. Today, scientists believe that such features were produced by the natural process of evolution through natural selection; but, a reader learns nothing of this from Parker's book.

What are the essential features of science? Does creation-science have any, all, or none of these features? My answer to this is none. By every mark of what constitutes science, creation-science fails. And, although it has not been my direct purpose to show its true nature, it is surely there for all to see. Miracles brought about by an intervening supervising force speak of only one thing. Creation "science" is actually dogmatic religious Fundamentalism. To regard it as otherwise is an insult to the scientist, as well as to the believer who sees creation-science as a blasphemous distortion of God-given reason. I believe that creation-science should not be taught in the public schools because creation-science is not science.

Notes

1. In fact, Act 590 demanded that *if* one teach[es] evolution, *then* one must also teach creation-science. Presumably a teacher could have stayed away from origins entirely—albeit with large gaps in some courses.

2. For a brief personal account of my experiences, see Michael Ruse, "A Philosopher at the Monkey Trial," *New Scientist* (1982): 317–319.

3. Judge William Overton's ruling on the constitutionality (or, rather, unconstitutionality) of Act 590 gives a fair and full account of the various claims made by theologians (including historians and sociologists of religion) and scientists.

4. In my book, *The Darwinian Revolution: Science Red in Tooth and Claw* (Chicago, IL: University of Chicago Press, 1979), I look at the way science was breaking apart from religion in the 19th century.

5. What follows is drawn from a number of basic books in the philosophy of science, including R. B. Braithwaite, *Scientific Explanation* (Cambridge, England:

Cambridge University Press, 1953); Karl R. Popper, *The Logic of Scientific Discovery* (London: Hutchinson, 1959); E. Nagel, *The Structure of Science* (London: Routledge and Kegan Paul, 1961); Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago, IL: University of Chicago Press, 1962); and C. G. Hempel, *Philosophy of Natural Science* (Englewood Cliffs, NJ: Prentice-Hall, 1966). The discussion is the same as what I provided for the plaintiffs in a number of position papers. It also formed the basis of my testimony in court, and, as can be seen from Judge Overton's ruling, was accepted by the court virtually verbatim.

6. One sometimes sees a distinction drawn between "theory" and "model." At the level of this discussion, it is not necessary to discuss specific details. I consider various uses of these terms in my book, *Darwinism Defended: A Guide to the Evolution Controversies* (Reading, MA: Addison-Wesley, 1982).

7. For more on science and miracles, especially with respect to evolutionary questions, see my *Darwinian Revolution, op. cit.*

8. The exact relationship between laws and what they explain has been a matter of much debate. Today, I think most would agree that the connection must be fairly tight—the thing being explained should follow. For more on explanation in biology see Michael Ruse, *The Philosophy of Biology* (London: Hutchinson, 1973); and David L. Hull, *Philosophy of Biological Science* (Englewood Cliffs, NJ: Prentice-Hall, 1974). A popular thesis is that explanation of laws involves deduction from other laws. A theory is a body of laws bound in this way: a so-called "hypothetico-deductive" system.

9. Falsifiability today has a high profile in the philosophical and scientific literature. Many scientists, especially, agree with Karl Popper, who has argued that falsifiability is *the* criterion demarcating science from non-science (see especially his *Logic of Scientific Discovery*). My position is that falsifiability is an important part, but only one part, of a spectrum of features required to demarcate science from non-science. For more on this point, see my *Is Science Sexist? And Other Problems in the Biomedical Sciences* (Dordrecht, Holland: D. Reidel Publishing Company, 1981).

10. At the Arkansas trial, in talking of the tentativeness of science, I drew an analogy in testimony between science and the law. In a criminal trial, one tries to establish guilt "beyond a reasonable doubt." If this can be done, then the criminal is convicted. But, if new evidence is ever discovered that might prove the convicted person innocent, cases can always be reopened. In science, too, scientists make decisions less formally but just as strongly—and get on with business, but cases (theories) can be reopened.

11. Of course, the scientist as citizen may run into problems here!

12. The key definitions in Arkansas Act 590, requiring "balanced treatment" in the public schools, are found in Section 4 [of the Act]. Section 4(a) does not specify exactly how old the earth is supposed to be, but in court a span of 6,000 to 20,000 years emerged in testimony.

The fullest account of the creation-science position is given in Henry M. Morris, ed., *Scientific Creationism* (San Diego, CA: Creation-Life Publishers, 1974).

13. Duane T. Gish, *Evolution—The Fossils Say No!* (San Diego, CA: Creation-Life Publishers, 1973), pp. 22–25, his italics.

14. John Whitcomb, Jr., and Henry M. Morris, *The Genesis Flood* (Philadelphia, PA: Presbyterian and Reformed Publishing Company, 1961), pp. 223–224, their italics.

15. *Ibid*, p. 76.

16. See Morris, *op. cit.*, pp. 71–72, and my discussion in *Darwinism Defended*, *op. cit.*

17. For instance, in John N. Moore and H. S. Slusher, *Biology: A Search for Order in Complexity* (Grand Rapids, MI: Zondervan, 1977).

18. D. Lack, *Darwin's Finches* (Cambridge, England: Cambridge University Press, 1947).

19. Fred Hoyle and N. C. Wickramasinghe, *Evolution from Space* (London: Dent, 1981).

20. Morris, *op. cit.*, p. 173.

21. Donald Johanson and M. Edey, *Lucy: The Beginnings of Humankind* (New York, NY: Simon and Schuster, 1981).

22. Gary E. Parker, *Creation: The Facts of Life* (San Diego, CA: Creation-Life Publishers, 1979), p. 113.

23. For details of these statements, see [footnote] 7 in Judge Overton's ruling.

24. Parker, *op. cit.* See, for instance, pp. 55 and 144. The latter passage is worth quoting in full:

> Then there's 'the marvelous fit of organisms to the environment,' the special adaptations of cleaner fish, woodpeckers, bombardier beetles, etc., etc.,—what Darwin called 'Difficulties with the Theory,' and what Harvard's Lewontin (1978) called 'the chief evidence of a Supreme Designer.' Because of their 'perfection of structure,' he says, organisms 'appear to have been carefully and artfully designed.'

The pertinent article by Richard Lewontin is "Adaptation," *Scientific American* (September 1978).

25. Richard C. Lewontin, *The Genetic Basis of Evolutionary Change* (New York, NY: Columbia University Press, 1974).

LARRY LAUDAN

Commentary: Science at the Bar—Causes for Concern

In the wake of the decision in the Arkansas Creationism trial *(McLean v. Arkansas)*[1] the friends of science are apt to be relishing the outcome. The creationists quite clearly made a botch of their case and there can be little doubt that the Arkansas decision may, at least for a time, blunt legislative pressure to enact similar laws in other states. Once the dust has settled, however, the trial in general and Judge William R. Overton's ruling in particular may come back to haunt us; for, although the verdict itself is probably to be commended, it was reached for all the wrong reasons and by a chain of argument which is hopelessly suspect. Indeed, the ruling rests on a host of misrepresentations of what science is and how it works.

The heart of Judge Overton's Opinion is a formulation of "the essential characteristics of science." These characteristics serve as touchstones for contrasting evolutionary theory with Creationism; they lead Judge Overton ultimately to the claim, specious in its own right, that since Creationism is not "science," it must be religion. The Opinion offers five essential properties that demarcate scientific knowledge from other things: "(1) It is guided by natural law; (2) it has to be explanatory by reference to natural law; (3) it is testable against the empirical world; (4) its conclusions are tentative, i.e., are not necessarily the final word; and (5) it is falsifiable."

These fall naturally into two families: properties (1) and (2) have to do with lawlikeness and explanatory ability; the other three properties have to do with the fallibility and testability of scientific claims. I shall deal with the second set of issues first, because it is there that the most egregious errors of fact and judgment are to be found.

At various key points in the Opinion, Creationism is charged with being untestable, dogmatic (and thus non-tentative), and unfalsifiable. All

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three charges are of dubious merit. For instance, to make the inter-linked claims that Creationism is neither falsifiable nor testable is to assert that Creationism makes no empirical assertions whatever. That is surely false. Creationists make a wide range of testable assertions about empirical matters of fact. Thus, as Judge Overton himself grants (apparently without seeing its implications), the creationists say that the earth is of very recent origin (say 6,000 to 20,000 years old); they argue that most of the geological features of the earth's surface are diluvial in character (i.e., products of the postulated worldwide Noachian deluge); they are committed to a large number of factual historical claims with which the Old Testament is replete; they assert the limited variability of species. They are committed to the view that, since animals and man were created at the same time, the human fossil record must be paleontologically co-extensive with the record of lower animals. It is fair to say that no one has shown how to reconcile such claims with the available evidence—evidence which speaks persuasively to a long earth history, among other things.

In brief, these claims are testable, they have been tested, and they have failed those tests. Unfortunately, the logic of the Opinion's analysis precludes saying any of the above. By arguing that the tenets of Creationism are neither testable nor falsifiable, Judge Overton (like those scientists who similarly charge Creationism with being untestable) deprives science of its strongest argument against Creationism. Indeed, if any doctrine in the history of science has ever been falsified, it is the set of claims associated with "creation-science." Asserting that Creationism makes no empirical claims plays directly, if inadvertently, into the hands of the creationists by immunizing their ideology from empirical confrontation. The correct way to combat Creationism is to confute the empirical claims it does make, not to pretend that it makes no such claims at all.

It is true, of course, that some tenets of Creationism are not testable in isolation (e.g., the claim that man emerged by a direct supernatural act of creation). But that scarcely makes Creationism "unscientific." It is now widely acknowledged that many scientific claims are not testable in isolation, but only when embedded in a larger system of statements, some of whose consequences can be submitted to test.

Judge Overton's third worry about Creationism centers on the issue of revisability. Over and over again, he finds Creationism and its advocates "unscientific" because they have "refuse[d] to change it regardless of the evidence developed during the course of the[ir] investigation." In point of fact, the charge is mistaken. If the claims of modern-day creationists are compared with those of their nineteenth-century counterparts, significant shifts in orientation and assertion are evident. One of the most visible opponents of Creationism, Stephen Gould, concedes that creationists have modified their views about the amount of variability allowed at the level of species change. Creationists do, in short, change their minds from time to time. Doubtless they would credit these shifts to their efforts to adjust their views to newly emerging evidence, in what they imagine to be a scientifically respectable way.

Perhaps what Judge Overton had in mind was the fact that some of Creationism's core assumptions (e.g., that there was a Noachian flood, that man did not evolve from lower animals, or that God created the world) seem closed off from any serious modification. But historical and sociological researches on science strongly suggest that the scientists of any epoch likewise regard some of their beliefs as so fundamental as not to be open to repudiation or negotiation. Would Newton, for instance, have been tentative about the claim that there were forces in the world? Are quantum mechanicians willing to contemplate giving up the uncertainty relation? Are physicists willing to specify circumstances under which they would give up energy conservation? Numerous historians and philosophers of science (e.g., Kuhn, Mitroff, Feyerabend, Lakatos) have documented the existence of a certain degree of dogmatism about core commitments in scientific research and have argued that such dogmatism plays a constructive role in promoting the aims of science. I am not denying that there may be subtle but important differences between the dogmatism of scientists and that exhibited by many creationists; but one does not even begin to get at those differences by pretending that science is characterized by an uncompromising open-mindedness.

Even worse, the *ad hominem* charge of dogmatism against Creationism egregiously confuses doctrines with the proponents of those doctrines. Since no law mandates that creationists should be invited into the classroom, it is quite irrelevant whether they themselves are close-minded. The Arkansas statute proposed that Creationism be taught, not that creationists should teach it. What counts is the epistemic status of Creationism, not the cognitive idiosyncrasies of the creationists. Because many of the theses of Creationism are testable, the mind set of creationists has no bearing in law or in fact on the merits of Creationism.

What about the other pair of essential characteristics which the *McLean* Opinion cites, namely, that science is a matter of natural law and explainable by natural law? I find the formulation in the Opinion to be rather fuzzy; but the general idea appears to be that it is inappropriate and unscientific to postulate the existence of any process or fact which cannot be explained in terms of some known scientific laws—for instance, the creationists' assertion that there are outer limits to the change of species "cannot be explained by natural law." Earlier in the Opinion, Judge Overton also writes "there is no scientific explanation for these limits which is guided by natural law," and thus concludes that such limits are unscientific. Still later, remarking on the hypothesis of the Noachian flood, he says: "A worldwide flood as an explanation of the world's geology is not the product of natural law, nor can its occurrence be explained by natural law." Quite how Judge Overton knows that a worldwide flood "cannot" be explained by the laws of science is left opaque; and even if we did not know how to reduce a universal flood to the familiar laws of physics, this requirement is an altogether inappropriate standard for ascertaining whether a claim is scientific. For centuries scientists have recognized a difference between establishing the existence of a phenomenon and explaining that phenomenon in a lawlike way. Our ultimate goal, no doubt, is to do both. But to suggest, as the *McLean* Opinion does repeatedly, that an existence claim (e.g., there was a worldwide flood) is unscientific until we have found the laws on which the alleged phenomenon depends is simply outrageous. Galileo and Newton took themselves to have established the existence of gravitational phenomena, long before anyone was able to give a causal or explanatory account of gravitation. Darwin took himself to have established the existence of natural selection almost a half-century before geneticists were able to lay out the laws of heredity on which natural selection depended. If we took the *McLean* Opinion criterion seriously, we should have to say that Newton and Darwin were unscientific; and, to take an example from our own time, it would follow that plate tectonics is unscientific because we have not yet identified the laws of physics and chemistry which account for the dynamics of crustal motion.

The real objection to such creationist claims as that of the (relative) invariability of species is not that such invariability has not been explained *by* scientific laws, but rather that the evidence for invariability is less robust than the evidence for its contrary, variability. But to say as much requires renunciation of the Opinion's other charge—to wit, that Creationism is not testable.

I could continue with this tale of woeful fallacies in the Arkansas ruling, but that is hardly necessary. What is worrisome is that the Opinion's line of reasoning—which neatly coincides with the predominant tactic among scientists who have entered the public fray on this issue—leaves many loopholes for the creationists to exploit. As numerous authors have shown, the requirements of testability, revisability, and falsifiability are exceedingly *weak* requirements. Leaving aside the fact that (as I pointed out above) it can be argued that Creationism already satisfies these requirements, it would be easy for a creationist to say the following: "I will abandon my views if we find a living specimen of a species intermediate between man and apes." It is, of course, extremely unlikely that such an individual will be discovered. But, in that statement the creationist would satisfy, in one fell swoop, all the formal requirements of testability, falsifiability, and revisability. If we set very weak standards for scientific status—and, let there be no mistake, I believe that all of the Opinion's last three criteria fall in this category—then it will be quite simple for Creationism to qualify as "scientific."

Rather than taking on the creationists obliquely and in wholesale fashion by suggesting that what they are doing is "unscientific" *tout court* (which is doubly silly because few authors can even agree on what makes an activity scientific), we should confront their claims directly and in piecemeal fashion by asking what evidence and arguments can be marshalled for and against each of them. The core issue is not whether Creationism satisfies some undemanding and highly controversial definitions of what is scientific; the real question is whether the existing evidence provides stronger arguments for evolutionary theory than for Creationism. Once that question is settled, we will know what belongs in the classroom and what does not. Debating the scientific status of Creationism (especially when "science" is construed in such an unfortunate manner) is a red herring that diverts attention away from the issues that should concern us.

Some defenders of the scientific orthodoxy will probably say that my reservations are just nitpicking ones, and that—at least to a first order of approximation—Judge Overton has correctly identified what is fishy about Creationism. The apologists for science, such as the editor of *The Skeptical Inquirer*, have already objected to those who criticize this whitewash of science "on arcane, semantic grounds . . . [drawn] from the most remote reaches of the academic philosophy of science."[2] But let us be clear about what is at stake. In setting out in the *McLean* Opinion to characterize the "essential" nature of science, Judge Overton was explicitly venturing into philosophical terrain. His *obiter dicta* are about as remote from well-founded opinion in the philosophy of science as Creationism is from respectable geology. It simply will not do for the defenders of science to invoke philosophy of science when it suits them (e.g., their much-loved principle of falsifiability comes directly from the philosopher Karl Popper) and to dismiss it as "arcane" and "remote" when it does not. However noble the motivation, bad philosophy makes for bad law.

The victory in the Arkansas case was hollow, for it was achieved only at the expense of perpetuating and canonizing a false stereotype of what science is and how it works. If it goes unchallenged by the scientific community, it will raise grave doubts about that community's intellectual integrity. No one familiar with the issues can really believe that anything important was settled through anachronistic efforts to revive a variety of discredited criteria for distinguishing between the scientific and the nonscientific. Fifty years ago, Clarence Darrow asked, *à propos* the Scopes trial, "Isn't it difficult to realize that a trial of this kind is possible in the twentieth century in the United States of America?" We can raise that question anew, with the added irony that, this time, the pro-science forces are defending a philosophy of science which is, in its way, every bit as outmoded as the "science" of the creationists.

Notes

1. *McLean v. Arkansas Board of Education*, 529 F. Supp. 1255 (E.D. Ark. 1982). For the text of the law, the decision, and essays by participants in the trial, see 7 *Science, Technology, and Human Values* 40 (Summer 1982), and also *Creationism, Science, and the Law* [:*The Arkansas Case*, ed. Marcel C. La Follette (Cambridge, Mass.: MIT Press, 1983).]

2. "The Creationist Threat: Science Finally Awakens," *The Skeptical Inquirer* 3 (Spring 1982): 2–5.

MICHAEL RUSE

Response to the Commentary: Pro Judice

As always, my friend Larry Laudan writes in an entertaining and provocative manner, but, in his complaint against Judge William Overton's ruling in *McLean v. Arkansas*,[1] Laudan is hopelessly wide of the mark. Laudan's outrage centers on the criteria for the demarcation of science which Judge Overton adopted, and the judge's conclusion that, evaluated by these criteria, creation-science fails as science. I shall respond directly to this concern—after making three preliminary remarks.

First, although Judge Overton does not need defense from me or anyone else, as one who participated in the Arkansas trial, I must go on record as saying that I was enormously impressed by his handling of the case. His written judgment is a first-class piece of reasoning. With cause, many have criticized the State of Arkansas for passing the "Creation-Science Act," but we should not ignore that, to the state's credit, Judge Overton was born, raised, and educated in Arkansas.

Second, Judge Overton, like everyone else, was fully aware that proof that something is not science is not the same as proof that it is religion. The issue of what constitutes science arose because the creationists claim that their ideas qualify as genuine science rather than as fundamentalist religion. The attorneys developing the American Civil Liberties Union (ACLU) case believed it important to show that creation-science is not genuine science. Of course, this demonstration does raise the question of what creation-science really is. The plaintiffs claimed that creation-science always was (and still is) religion. The plaintiffs' lawyers went beyond the negative argument (against science) to make the positive case (for religion). They provided considerable evidence for the religious nature of creation-science, including such things as the creationists' explicit reliance on the Bible in their various writings. Such arguments seem about as

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strong as one could wish, and they were duly noted by Judge Overton and used in support of his ruling. It seems a little unfair, in the context, therefore, to accuse him of "specious" argumentation. He did not adopt the naïve dichotomy of "science or religion but nothing else."

Third, *whatever the merits* of the plaintiffs' case, the kinds of conclusions and strategies apparently favored by Laudan are simply not strong enough for legal purposes. His strategy would *require arguing* that creation-science is weak science and therefore ought not to be taught:

> The core issue is not whether Creationism satisfies some undemanding and highly controversial definitions of what is scientific; the real question is whether the existing evidence provides stronger arguments for evolutionary theory than for Creationism. Once that question is settled, we will know what belongs in the classroom and what does not.[2]

Unfortunately, the U.S. Constitution does not bar the teaching of weak science. What it bars (through the Establishment Clause of the First Amendment) is the teaching of religion. The plaintiffs' tactic was to show that creation-science is less than weak or bad science. It is not science at all.

Turning now to the main issue, I see three questions that must be addressed. Using the five criteria listed by Judge Overton, can one distinguish science from non-science? Assuming a positive answer to the first question, does creation-science fail as genuine science when it is judged by these criteria? And, assuming a positive answer to the second, does the Opinion in *McLean* make this case?

The first question has certainly tied philosophers of science in knots in recent years. Simple criteria that supposedly give a clear answer to every case—for example, Karl Popper's single stipulation of falsifiability[3]—will not do. Nevertheless, although there may be many grey areas, white does seem to be white and black does seem to be black. Less metaphorically, something like psychoanalytic theory may or may not be science, but there do appear to be clear-cut cases of real science and of real non-science. For instance, an explanation of the fact that my son has blue eyes, given that both parents have blue eyes, done in terms of dominant and recessive genes and with an appeal to Mendel's first law, is scientific. The Catholic doctrine of transubstantiation (i.e., that in the Mass the bread and wine turn into the body and blood of Christ) is not scientific.

Furthermore, the five cited criteria of demarcation do a good job of distinguishing the Mendelian example from the Catholic example. Law and explanation through law come into the first example. They do not enter the second. We can test the first example, rejecting it if necessary. In this sense, it is tentative, in that something empirical might change our minds. The case of transubstantiation is different. God may have His own laws, but neither scientist nor priest can tell us about those which turn bread and wine into flesh and blood. There is no explanation through law. No empirical evidence is pertinent to the miracle. Nor would the believer be swayed by any empirical facts. Microscopic examination of the Host is considered irrelevant. In this sense, the doctrine is certainly not tentative.

One pair of examples certainly do not make for a definitive case, but at least they do suggest that Judge Overton's criteria are not quite as irrelevant as Laudan's critique implies. What about the types of objections (to the criteria) that Laudan does or could make? As far as the use of law is concerned, he might complain that scientists themselves have certainly not always been that particular about reference to law. For instance, consider the following claim by Charles Lyell in his *Principles of Geology* (1830/3): "We are not, however, contending that a real departure from the antecedent course of physical events cannot be traced in the introduction of man."[4] All scholars agree that in this statement Lyell was going beyond law. The coming of man required special divine intervention. Yet, surely the *Principles* as a whole qualify as a contribution to science.

Two replies are open: either one agrees that the case of Lyell shows that science has sometimes mingled law with non-law; or one argues that Lyell (and others) mingled science and non-science (specifically, religion at this point). My inclination is to argue the latter. Insofar as Lyell acted as scientist, he appealed only to law. A century and a half ago, people were not as conscientious as today about separating science and religion. However, even if one argues the former alternative—that some science has allowed place for non-lawbound events—this hardly makes Laudan's case. Science, like most human cultural phenomena, has evolved. What was allowable in the early nineteenth century is not necessarily allowable in the late twentieth century. Specifically, science today does not break with law. And this is what counts for us. We want criteria of science for today, not for yesterday. (Before I am accused of making my case by fiat, let me challenge Laudan to find one point within the modern geological theory of plate tectonics where appeal is made to miracles, that is, to breaks with law. Of course, saying that science appeals to law is not asserting that we know all of the laws. But, who said that we did? Not Judge Overton in his Opinion.)

What about the criterion of tentativeness, which involves a willingness to test and reject if necessary? Laudan objects that real science is hardly all that tentative: "[H]istorical and sociological researches on science strongly suggest that the scientists of any epoch likewise regard some of their beliefs as so fundamental as not to be open to repudiation or negotiation."[5]

It cannot be denied that scientists do sometimes—frequently—hang on to their views, even if not everything meshes precisely with the real world. Nevertheless, such tenacity can be exaggerated. Scientists, even Newtonians, have been known to change their minds. Although I would not want to say that the empirical evidence is all-decisive, it plays a major role in such mind changes. As an example, consider a major revolution of our own time, namely that which occurred in geology. When I was an undergraduate in 1960, students were taught that continents do not move. Ten years later, they were told that they do move. Where is the dogmatism here? Furthermore, it was the new empirical evidence—e.g., about the nature of the sea-bed—which persuaded geologists. In short, although science may not be as open-minded as Karl Popper thinks it is, it is not as close-minded as, say, Thomas Kuhn[6] thinks it is.

Let me move on to the second and third questions, the status of creation-science and Judge Overton's treatment of the problem. The slightest acquaintance with the creation-science literature and Creationism movement shows that creation-science fails abysmally as science. Consider the following passage, written by one of the leading creationists, Duane T. Gish, in *Evolution: The Fossils Say No!*:

> CREATION. By creation we mean the bringing into being by a supernatural Creator of the basic kinds of plants and animals by the process of sudden, or fiat, creation.
>
> We do not know how the Creator created, what processes He used, *for He used processes which are not now operating anywhere in the natural universe.* This is why we refer to creation as Special Creation. We cannot discover by scientific investigations anything about the creative processes used by the Creator.[7]

The following similar passage was written by Henry M. Morris, who is considered to be the founder of the creation-science movement:

> . . . it is . . . quite impossible to determine anything about Creation through a study of present processes, because present processes are not created in character. If man wishes to know anything about Creation (the time of Creation, the duration of Creation, the order of Creation, the methods of Creation, or anything else) his sole source of true information is that of divine revelation. God was there when it happened. We were not there . . . therefore, we are completely limited to what God has seen fit to tell us, and this information is in His written Word. This is our textbook on the science of Creation![8]

By their own words, therefore, creation-scientists admit that they appeal to phenomena not covered or explicable by any laws that humans can grasp as laws. It is not simply that the pertinent laws are not yet known. Creative processes stand outside law as humans know it (or could know it) on Earth—at least there is no way that scientists can know laws breaking (or transcending) Mendel's laws through observation and experiment. Even if God did use His own laws, they are necessarily veiled from us forever in this life, because Genesis says nothing of them.

Furthermore, there is nothing tentative or empirically checkable about the central claims of creation-science. Creationists admit as much when they join the Creation Research Society (the leading organization of the movement). As a condition of membership applicants must sign a document specifying that they now believe and will continue to believe:

> (1) The Bible is the written Word of God, and because we believe it to be inspired throughout, all of its assertions are historically and scientifically true in all of the original autographs. To the student of nature, this means that the account of origins in Genesis is a factual presentation of simple historical truths. (2) All basic types of living things, including man, were made by direct creative acts of God during Creation Week as described in Genesis. Whatever biological changes have occurred since Creation have accomplished only changes within the original created kinds. (3) The great Flood described in Genesis, commonly referred to as the Noachian Deluge, was an historical event, worldwide in its extent and effect. (4) Finally, we are an organization of Christian men of science, who accept Jesus Christ as our Lord and Savior. The account of the special creation of Adam and Eve as one man and one woman, and their subsequent fall into sin, is the basis for our belief in the necessity of a Savior for all mankind. Therefore, salvation can come only thru accepting Jesus Christ as our Savior.[9]

It is difficult to imagine evolutionists signing a comparable statement, that they will never deviate from the literal text of Charles Darwin's *On the Origin of Species*. The non-scientific nature of creation-science is evident for all to see, as is also its religious nature. Moreover, the quotes I have used above were all used by Judge Overton, in the *McLean* Opinion, to make exactly the points I have just made. Creation-science is not genuine science, and Judge Overton showed this.

Finally, what about Laudan's claim that some parts of creation-science (e.g., claims about the Flood) are falsifiable and that other parts (e.g., about the originally created "kinds") are revisable? Such parts are not falsifiable or revisable in a way indicative of genuine science. Creation-science is not like physics, which exists as part of humanity's common cultural heritage and domain. It exists solely in the imaginations and writing of a relatively small group of people. Their publications (and stated intentions) show that, for example, there is no way they will relinquish belief in the Flood, whatever the evidence.[10] In this sense, their doctrines are truly unfalsifiable.

Furthermore, any revisions are not genuine revisions, but exploitations of the gross ambiguities in the creationists' own position. In the matter of origins, for example, some elasticity could be perceived in the creationist position, given the conflicting claims that the possibility of (degenerative) change within the originally created "kinds." Unfortunately, any open-mindedness soon proves illusory; for creationists have no real idea about what God is supposed to have created in the beginning, except that man was a separate species. They rely solely on the Book of Genesis:

> And God said, Let the waters bring forth abundantly the moving creature that hath life, and the fowl that may fly above the earth in the open firmament of heaven.
>
> And God created great whales, and every living creature that moveth, which the waters brought forth abundantly, after their kind, and every winged fowl after his kind: and God saw that it was good.
>
> And God blessed them, saying Be fruitful, and multiply, and fill the waters in the seas, and let fowl multiply in the earth.
>
> And the evening and the morning were the fifth day.
>
> And God said, Let the earth bring forth the living creature after his kind, cattle, and creeping thing, and beast of the earth after his kind: and it was so.
>
> And God made the beast of the earth after his kind, and cattle after their kind, and everything that creepeth upon the earth after his kind: and God saw that it was good.[11]

But the *definition* of "kind," what it really is, leaves creationists as mystified as it does evolutionists. For example, creationist Duane Gish makes this statement ~~on the subject~~:

> [W]e have defined a basic kind as including all of those variants which have been derived from a single stock. . . . We cannot always be sure, however, what constitutes a separate kind. The division into kinds is easier the more the divergence observed. It is obvious, for example, that among invertebrates the protozoa, sponges, jellyfish, worms, snails, trilobites, lobsters, and bees are all different kinds. Among the vertebrates, the fishes, amphibians, reptiles, birds, and mammals are obviously different basic kinds.
>
> Among the reptiles, the turtles, crocodiles, dinosaurs, pterosaurs (flying reptiles), and ichthyosaurs (aquatic reptiles) would be placed in different kinds.

> Each one of these major groups of reptiles could be further subdivided into the basic kinds within each.
>
> Within the mammalian class, duck-billed platypus, bats, hedgehogs, rats, rabbits, dogs, cats, lemurs, monkeys, apes, and men are easily assignable to different basic kinds. Among the apes, the gibbons, orangutans, chimpanzees, and gorillas would each be included in a different basic kind.[12]

Apparently, a "kind" can be anything from humans (one species) to trilobites (literally thousands of species). The term is flabby to the point of inconsistency. Because humans are mammals, if one claims (as creationists do) that evolution can occur within but not across kinds, then humans could have evolved from common mammalian stock—but because humans themselves are kinds such evolution is impossible.

In brief, there is no true resemblance between the creationists' treatment of their concept of "kind" and the openness expected of scientists. Nothing can be said in favor of creation-science or its inventors. Overton's judgment emerges unscathed by Laudan's complaints.

Notes

1. For the text of Judge Overton's Opinion, see 7 *Science, Technology, and Human Values* 40 (Summer 1982): 28–42; and *Creationism, Science, and the Law* [Cambridge, Mass.: MIT Press, 1983].

2. Larry Laudan, "Commentary: Science at the Bar—Causes for Concern," [p. 52 this volume].

3. Karl Popper, *The Logic of Scientific Discovery* (London: Hutchinson, 1959).

4. Charles Lyell, *Principles of Geology*, Volume I (London: John Murray, 1830), p. 162.

5. Laudan, *op. cit.*, [p. 50 this volume].

6. Thomas Kuhn, *The Structure of Scientific Revolutions* (Chicago, IL: University of Chicago Press, 1962).

7. Duane Gish, *Evolution: The Fossils Say No!*, 3rd edition (San Diego, CA: Creation-Life Publishers, 1979), p. 40 (his italics).

8. Henry M. Morris, *Studies in the Bible and Science* (Philadelphia, PA: Presbyterian and Reformed Publishing Company, 1966), p. 114.

9. Application form for the Creation Research Society, reprinted in Plaintiffs' trial briefs, *McLean v. Arkansas* (1981).

10. See, for instance, Henry M. Morris, *Scientific Creationism* (San Diego, CA: Creation-Life Publishers, 1974); and my own detailed discussion in Michael Ruse, *Darwinism Defended: A Guide to the Evolution Controversies* (Reading, MA: Addison-Wesley, 1982).

11. Genesis, Book I, Verses 20–25.

12. Gish, *op. cit.*, pp. 34–35.

1 | COMMENTARY

1 | Commentary

1.1 | Popper's Demarcation Criterion

In "Science: Conjectures and Refutations," Sir Karl Popper explains how he came to formulate his falsifiability criterion for the scientific status of a theory. He recognized that it was not enough to use the so-called empirical (or inductive) method of generalizing from observation and experience, for by this standard astrology might well qualify as genuine science. So why, Popper wondered, were Freudian psychoanalysis, Adlerian "individual psychology," and the Marxist theory of history more like astrology than astronomy, more like myth than science?

His answer came from noting that, while proponents of these disciplines found confirming evidence for their theories at every turn, they made no predictions that could be disconfirmed by evidence. With deliberate irony, Popper describes "the incessant stream of confirmations, of observations which 'verified' the theories in question" (5). Moreover, it seemed to Popper as though just about anything, even apparent counterevidence, could be explained in Freudian or Adlerian or Marxist terms. In marked contrast to this were certain features characterizing one of the most important physical theories of this century. Popper recounts how impressed he was by the bold prediction of the bending of starlight near the surface of the sun made by Einstein's general theory of relativity. This prediction was verified by two astronomical expeditions to observe the total solar eclipse of 29 May 1919—one to Brazil, the other to the west coast of Africa—organized by the British cosmologist Sir Arthur Eddington. Photographic plates produced during these expeditions revealed that starlight was indeed deflected by the sun by an amount very close to Einstein's prediction of 1.75 seconds of arc. This crucial observation led to the overthrow of Newton's theory of gravity by Einstein's general theory of relativity.[1]

Unlike Marx's theory of history and Adler's theory of the inferiority complex, Einstein's theory ran a serious risk of refutation by predicting the result of an observational test *before* the test was made. Popper sees this possibility of refutation by observation and experiment as the hallmark of genuine science. Agreement with known facts, or the ability to explain known facts, is not enough to make a theory scientific. Whereas the Marxists and Adlerians saw confirmation of their theories everywhere and recognized nothing that their theories could not explain, Einstein's theory is refutable because, by its very nature, it is incompatible with certain possible results of observation—it is open to falsifying tests. Popper insists that in order to be scientific a theory must take a risk by predicting something new. Thus, Popper advocates falsifiability (testability), not verifiability (confirmability), as the demarcation criterion for distinguishing science from pseudoscience.

FALSIFIABILITY

Despite its simplicity and initial plausibility, there is much that is unclear and controversial about Popper's demarcation criterion. Part of the unclarity arises because Popper shifts back and forth between two different notions—between *falsifiability* as a logical property of statements (requiring that scientific statements logically imply at least one testable prediction) and *falsifiability* as a term prescribing how scientists should act. According to Popper, scientists should test their theories by trying to refute them; when a prediction disagrees with observation and experiment, they should abandon their theories as refuted. Falsifiability in this second, prescriptive sense implies falsifiability in the first sense, for it is only by making testable predictions that a theory—made up of scientific statements—can be refuted. But the implication does not hold in the other direction. It is perfectly possible for a theory such as Marxism to imply at least one testable prediction (say, that all socialist revolutions will occur among the proletariat of industrialized capitalist nations) and yet, when the prediction turns out to be false (because the Russian and Chinese revolutions occurred in societies that were preindustrial and feudal), the adherents of the theory refuse to regard it as refuted and strive to explain away the anomaly. Thus, a theory that is scientific in Popper's (first) logical sense might be judged pseudoscientific in Popper's (second) methodological sense because of the behavior of its proponents.

Many philosophers have criticized the prescriptive, methodological aspect of Popper's demarcation criterion. They argue that abandoning a theory the instant it makes a false prediction would rule out too much good science. (This criticism, made by Kuhn and Lakatos, among others, will be discussed later.) Some philosophers also object to the first sense of Popper's falsifiability criterion, that falsifiability is a logical property of scientific statements, on the grounds that it is too weak. Take any statement, however implausible or crazy it may sound, and conjoin it with a respectable scientific theory. The crazy statement, *C*, might be the claim that aliens visited the earth during the Pleistocene era and removed all traces of their visit before departing. Although *C* is not a tautology, it makes no testable predictions. The respectable scientific theory, *T*, could be from any field whatever—geology, chemistry, physics, or astronomy. The conjunction, *(T & C)*, makes lots of testable predictions since its logical consequences include all the predictions made by *T* alone. Thus, *(T & C)* satisfies Popper's falsifiability criterion. The moral is clear: having testable consequences is a very weak requirement. At best, perhaps, it is a necessary condition for genuine science, and many statements that satisfy it are not part of science. Thus, presumably, Popper was not claiming that all falsifiable statements are scientific; he was merely claiming that in order to be scientific, a statement must be falsifiable.[2]

POPPER AND THE THEORY OF EVOLUTION

Popper claimed for several decades that the principle of natural selection in Darwin's theory of evolution fails to satisfy his falsifiability criterion (that is, falsifiability as a logical property of statements) and thus, in some important sense, that it is not scientific but "metaphysical." Popper recanted this belief, appropriately enough, when he delivered the first Darwin Lecture at Darwin College, Cambridge University, in 1977.[3] This is important for three reasons. First, it illustrates how difficult it can be to decide whether or not a component of a scientific theory is falsifiable. Second, it illustrates the complexity of Popper's position, since Popper never condemned the whole of Darwin's theory as a pseudoscience even when he judged that an important part of that theory could not be falsified. Third, it sheds some light—if just a little—on the position of creationists who, much to Popper's dismay, have appealed to Popper's (pre-1977) writings for support in their crusade against the theory of evolution.

Before his recantation, Popper expressed reservations about Darwin's theory by saying that "Darwinism is not a testable scientific theory, but a *metaphysical research programme*—a possible framework for testable scientific theories."[4] What Popper meant by this is that Darwin's theory, when expressed in very general terms as a group of claims about heredity, random mutation, and differential survival, does not make any predictions about which species (or indeed whether any species) will evolve. Popper thought that prediction and explanation seem to occur because we forget that adaptation or fitness is implicitly defined in terms of survival. Thus, while it may seem as if we have explained why a particular species now thrives by saying that it adapted to its environment, Popper judged this to be no explanation at all. Rather, he said that the claim that a species now living has adapted to its environment is "almost tautological," that is, true by definition.[5]

Popper's charge that the phrase "the survival of the fittest" is tantamount to a tautology (that to survive *is* to be fittest) has been repeated by creationists such as Henry Morris, who have then denied that evolutionary theory as whole is either empirical or testable.[6] But even if a theory includes some elements that are true by definition or untestable for some other reason, it hardly follows that the theory as a whole or specific versions of it are untestable. Indeed, Popper regarded Darwinism as similar in this regard to atomism and field theory. In his view, these are all metaphysical generalizations that make no predictions and hence are untestable. Nonetheless, they are of great scientific value because they give rise to specific theories that *are* testable and have been tested. So when Popper judges a proposition to be unfalsifiable and "metaphysical," he is not claiming that the proposition has no scientific value, nor is he asserting that any theory associated with it is pseudoscientific.

But is "the survival of the fittest" a tautology as has been charged? One problem in assessing this accusation is that "the survival of the fittest" is a phrase, not a proposition, and only propositions can be tautologies.[7] What is needed is a precise statement of the allegedly tautologous proposition. Is the proposition in question a definition of *fitness* (or *relative adaptiveness*) in terms of the probability of reproductive success? Or is it the historical claim that the traits in current populations are the result of natural selection (i.e., selection of the fittest ancestral variants)? In his Darwin Lecture, Popper opted for the latter and then noted, correctly, that it is an empirical matter whether natural selection or some other mechanism (such as genetic drift) is responsible for the traits we now find in a population of organisms. Thus, Popper conceded that the principle of natural selection is falsifiable and testable.

Before concluding this section, there is one further small matter concerning Darwin's theory and Popper's criterion of falsifiability. Sometimes the claim is made (often, but not always, by creationists) that Darwin's theory (and, presumably, other sciences such as paleontology, geology, and cosmology) are unscientific because they are, at least in part, historical. Evolutionary theory, we are told, makes claims about historical events, many of which occurred before the advent of any human observers on this planet. Historical events are unique and unrepeatable. Therefore, critics conclude, Darwin's theory cannot be tested or refuted.[8] This argument, as Popper himself has emphasized, is invalid: its conclusion does not follow from its premises.[9] Claims about historical events, even events that occurred millions of years ago can be tested (and thus, in principle, refuted) by using them to make predictions about the evidence we should find now if the historical claims are true: cometary collisions with the earth leave craters and abnormally high concentrations of iridium in the surrounding rocks; animals and plants leave fossils; the "big bang" still resonates in the form of background microwave radiation in space.

1.2 | Kuhn's Criticisms of Popper

One of the many people who have challenged Popper's appeal to falsifiability as a demarcation criterion is Thomas Kuhn. In his book, *The Structure of Scientific Revolutions* (1962), Kuhn insisted that if we are to arrive at an adequate characterization of science, close attention must be paid to its history: a proper philosophy of science should reflect the history of science. On this account, philosophy of science ought to describe the way scientists actually behave and the way that science has evolved over time.

Doing so shows that not all scientific activity is of the same kind. Kuhn thinks that scientific activity falls into two distinct types: normal science and extraordinary (or revolutionary) science.

NORMAL SCIENCE AND PUZZLE SOLVING

During periods of normal science, scientists take for granted the major theories of their day and content themselves with what Kuhn calls puzzle solving. In some respects, the puzzle-solving aspect of normal science is like trying to do the exercises at the back of a physics or chemistry textbook. The aim of practicing scientists is not to call into question Newtonian mechanics or the laws of thermodynamics, but rather to see whether they can solve problems by using these accepted theories in conjunction with other assumptions and models. Just as failure to get the right answer to an exercise is regarded as a failure of the student, not of the theory, so, too, failure to solve a puzzle during a period of normal science is considered the fault of the scientist using the theory, not the fault of the theory itself. Only very rarely, during periods of extraordinary science, do scientists deliberately question the received theories of their day and attempt to refute them. Typically such periods of extraordinary science arise because of repeated failures to solve puzzles. If a theory is refuted, then it must be replaced by another theory that is at least as general in scope. Science, like nature, abhors a vacuum: scientists will give up a global theory only when they have an even better theory to adopt in its place. When such a replacement occurs, we have a scientific revolution. (For a much fuller discussion of Kuhn's views on scientific revolutions, see chapter 2, "Rationality, Objectivity, and Values in Science," below.)

Kuhn agrees with Popper and many other philosophers of science that astrology is a pseudoscience. In this, as in many other cases, Popper's criterion of demarcation (severity of testing) leads to the same verdict as Kuhn's criterion (puzzle solving). But Kuhn rejects Popper's demarcation criterion and with it Popper's explanation of why astrology is pseudoscientific. Popper insists that by formulating their accounts in suitably vague terms, astrologers are able to "explain away anything that might have been a refutation of the theory" (8). For Popper, this emphasis on confirmation and avoidance of testability or falsification marks the difference between pseudoscience and science. Kuhn's account of why astrology is a pseudoscience is quite different from Popper's. Kuhn points out that astrology was finally abandoned by scientists around the middle of the seventeenth century, mainly as a consequence of the Copernican revolution. But throughout its history, astrology was notoriously unreliable and its predictions often failed. Interestingly, these frequent failures were never given as a reason for thinking that astrology is false until after astrology had been abandoned. During its heyday, astrology was regarded rather as medicine and meteorology once were—as an imprecise study of an enormously complex subject. In any case, astrology can scarcely be reckoned a nonscience simply because it made predictions that turned out to be false. Still, Kuhn insists, astrology never was a science. Although astrologers use rules of thumb to cast horoscopes, astrology has no central theory and no puzzle-solving tradition of a sort characterizing normal science. Thus astrology was, and remains, at best a craft and not a science.

SCIENTIFIC REVOLUTIONS

In "Logic of Discovery or Psychology of Research?" Kuhn gives several examples of scientific revolutions: the overthrow of Newton's theory of gravity by Einstein's general theory of relativity; the replacement of the phlogiston theory by Lavoisier's new chemical theory (in which the addition of oxygen, not the release of phlogiston, is responsible for the burning of metals in air); the experimental confirmation of Lee and Yang's theory that the weak interaction—the nuclear process responsible for the release of electrons during radioactive decay—does not conserve parity. One of Kuhn's main criticisms of Popper is that sincere attempts to refute theories are quite rare in science. Such attempts are usually confined to the periods of extraordinary science that immediately precede scientific revolutions. Thus, according to Kuhn, Popper's falsifiability account of science fails to describe normal science. If falsifiability were the criterion marking off science from pseudoscience, then genuine science as it is done most of the time, being normal and not extraordinary, would be improperly classified as pseudoscientific.

As we have seen, Kuhn rejects Popper's falsifiability criterion as an account of normal science; but how well does it fit those episodes of extraordinary science (scientific revolutions) in which large-scale theories are refuted and replaced? According to Kuhn, another flaw in Popper's historically insensitive treatment is that in some scientific revolutions—Kuhn gives the Copernican revolution as an example—the old theory (Ptolemy's geocentric theory) was replaced by the new theory (Copernicus's heliocentric theory) *before* the old theory was refuted. For example, Galileo's telescopic observations of the phases of Venus, the moons of Jupiter, and the motion of sunspots were made at least sixty years after the publication of Copernicus's *De Revolutionibus* (1543) and only after Galileo had become a convinced Copernican. Arguably, Ptolemy's theory (in which the earth is stationary at the center of the universe) was decisively refuted only when Newton's theory of mechanics and gravity was accepted. (Newton's *Principia* was published in 1687.) Newton's theory showed that it was physically impossible for the entire heavens to rotate around the earth's north-south axis. When Copernicus proposed his new theory, most astronomers thought that the Ptolemaic theory could solve all its problems by adjusting a few parameters. Hardly anyone thought that Ptolemy's theory had been severely tested and found irreparably wanting. Here again, Kuhn argues, Popper's account of science does not fit the history of science. There is more to science and being scientific than falsifiability and testing.

1.3 | Lakatos and Scientific Research Programmes

In "Science and Pseudoscience," Imre Lakatos notes that genuine *scientia* (knowledge) cannot be marked off from impostors simply in terms of the number of people who believe it or how strongly they believe it. The worst of pseudoscience has, in the past, commanded dogged assent from large numbers of intellectuals. Nor can we rest a criterion of demarcation on the commonplace assertion that genuine science is supported by the observable facts. For, Lakatos asks, how could this criterion be justified? Like Kuhn and Popper, Lakatos agrees that no scientific theory can be deduced from observational and experimental facts. When scientists such as Newton and Ampère claimed that their theories were not *hypotheses* but proven *truths* because they were deduced from experiments and observations, they were simply wrong.

Why All Theories Are Unprovable

We can appreciate Lakatos's point by considering a single example: Newton's theory of gravitation. Newton's theory says that *every* particle of matter in the universe attracts every other particle with a force according to an inverse square law. Newton's theory is a universal generalization that applies to every particle of matter, anywhere in the universe, at any time. But however numerous they might be, our observations of planets, falling bodies, and projectiles concern only a finite number of bodies during finite amounts of time. So the scope of Newton's theory vastly exceeds the scope of the evidence. It is possible that all our observations are correct, and yet Newton's theory is false because some bodies not yet observed violate the inverse square law. Since "All *F*s are *G*" cannot be deduced from "Some *F*s are *G*," it cannot be true that Newton's theory can be proven by logically deducing it from the evidence. As Lakatos points out, this prevents us from claiming that scientific theories, unlike pseudoscientific theories, can be proven from observational facts. The truth is that no theory can be deduced from such facts. *All* theories are unprovable, scientific and unscientific alike.

Why All Theories Are Improbable

While conceding that scientific theories cannot be proven, most people still believe that theories can be made more probable by evidence. Lakatos follows Popper in denying that any theory can be made probable by any amount of evidence. Popper's argument for this controversial claim rests on the analysis of the objective probability of statements given by inductive logicians.[10] Consider a card randomly drawn from a standard deck of fifty-two cards. What is the probability that the card selected is the ten of hearts? Obviously, the answer is $1/52$. There are fifty-two possibilities, each of which is equally likely and only one of which would render true the statement "This card is the ten of hearts." Now consider a scientific theory that, like Newton's theory of gravitation, is universal. The number of things to which Newton's theory applies is, presumably, infinite. Imagine that we name each of these things by numbering them 1, 2, 3, . . . , *n*, ... There are infinitely many ways the world could be, each equally probable.

> 1 obeys Newton's theory, but none of the others do.
> 1 and 2 obey Newton's theory, but none of the others do.
> 1, 2, and 3 obey Newton's theory, but none of the others do.
> . . . .
> All bodies (1, 2, 3, . . . , *n*, . . .) obey Newton's theory.

Since these possibilities are infinite in number, and each of them has the same probability, the probability of any one of them must be 0.[11] But only one, the last one, represents the way the world would be if Newton's theory were true. So the probability of Newton's theory (and any other universal generalization) must be 0.

Now one might think that, even if the initial probability of a theory must be 0, the probability of the theory when it has been confirmed by evidence will be greater than 0. As it turns out, the probability calculus denies this. Let our theory be T, and let our evidence for T be E. We are interested in $P(T/E)$, the probability of T given our evidence E. Bayes's theorem (which follows logically from the axioms of the probability calculus) tells us that this probability is:

$$P(T/E) = \frac{P(E/T) \times P(T)}{P(E)}.$$

If the initial probability of T—that is, $P(T)$—is 0, then $P(T/E)$ must also be 0.[12] Thus, no theory can increase in objective probability, regardless of the amount of evidence for it. For this reason, Lakatos joins Popper in regarding all theories, whether scientific or not, as equally unprovable and equally improbable.

THE METHODOLOGY OF SCIENTIFIC RESEARCH PROGRAMMES

The failure to specify demarcation criteria along the intuitively attractive lines of "whatever is proved or made probable by evidence" might suggest returning to the Popperian model. But like Kuhn (and Thagard after him), Lakatos rejects Popper's falsifiability criterion as a solution to the demarcation problem. Scientists rarely specify in advance of observation and experiment those results that, if found, would refute their theories. At best, such results would be regarded as anomalous or recalcitrant, not as genuine refutations. Even when they are first proposed, some theories are (or are thought to be) inconsistent with the known data. Newton's gravitational theory is a good example. By his own admission, Newton was unable to reconcile his theory with the known orbit of the earth's moon. (This anomaly was later cleared up by Alexis Clairault who found a mistake in Newton's calculations.[13]) But Newton did not immediately abandon his theory as refuted. Later, after the discovery of the planet Uranus (by William Herschel in 1781), it was noted that Uranus did not move precisely as Newton's theory predicted. Again, scientists did not abandon the inverse square law; rather, they postulated another planet, as yet unobserved, which was perturbing the orbit of Uranus. This hypothetical new planet was eventually discovered and given the name Neptune.[14]

In order to make sense of the ways in which scientists protect their theories from refutation, Lakatos proposes that scientific theories be regarded as having three components: a hard core, a protective belt, and a positive heuristic. The hard core of Newton's theory consists of his three laws of motion plus the inverse square law of gravitational attraction. These are basic postulates that scientists were extremely reluctant to give up. The protective belt consists of many auxiliary hypotheses such as assumptions about the number and the masses of the planets. The positive heuristic tells scientists how to solve problems using the theory and how to respond to anomalies by revising the protective belt. Lakatos proposes that we stop thinking of scientific theories as frozen in time but instead regard theories as historically extended scientific research programmes. The Newtonian research programme covered several centuries. For much of its history it was progressive. Why? Because in dealing with anomalies and other problems, the Newtonian programme continued to predict novel facts.

According to Lakatos, Popper is wrong in thinking that a crucial experiment can (or should) instantly refute a theory. As Kuhn has shown, the actual history of science teaches us otherwise: genuine scientific progress (as opposed to degenerating science or pseudoscience) is not simply a matter of one theory remaining unrefuted while others are falsified. But Lakatos is equally critical of Kuhn for suggesting that scientific revolutions are largely irrational affairs, dependent on a kind of group psychology. Were Kuhn right, there would be no objective way of marking off scientific progress from scientific regress or decay. Instead, Lakatos suggests that scientific change occurs as the result of competition between rival research programmes. If one programme is progressive (because it continues to predict novel facts), and if its rival is degenerating, then most scientists will, rationally, switch their allegiance. In this way, progressive research programmes replace degenerating ones.

1.4 | Thagard on Why Astrology Is a Pseudoscience

Paul Thagard takes up Lakatos's notion of scientific theories as research programmes, and develops it into an explicit criterion for demarcating science from pseudoscience. In "Why Astrology Is a Pseudoscience," Thagard surveys several different proposals for a demarcation criterion that would explain why astrology is a pseudoscience and finds each of them deficient. In light of the alchemical and occult origins of chemistry and medicine, one cannot uncritically cite astrology's origin in magic as what makes it a pseudoscience. (Indeed, this merely postpones the question, Why is magic not itself a genuine science?) Nor can the supposed immunity from testing, verification, or falsification be what makes astrology a pseudoscience. As Thagard notes, some astrological claims (about the influence of planetary positions at the time of one's birth on one's personality and future career, for example) are testable. Moreover, Thagard agrees with Kuhn and Lakatos that abandoning a theory the moment one of its predictions failed would be irrational. Many of our best scientific theories have been modified in the light of failed predictions and recalcitrant observations. Hasty rejection would nip too many good theories in the bud, before they had the chance to grow and blossom. (The ambiguity of falsification and the Duhem-Quine thesis are discussed further in chapter 3, "The Duhem-Quine Thesis and Underdetermination.")

Contrary to Kuhn, Thagard claims that modern astrology does indeed present a number of unsolved problems (such as accommodating the precession of the equinoxes and planets that were discovered many centuries after Ptolemy's death). This undercuts the Kuhnian proposal that astrology fails as a science simply because it is not a paradigm-dominated discipline of problem solving. Against Lakatos, Thagard suggests that lack of progress is not by itself a sufficient condition of pseudoscience, since it might be nonprogressive only in periods when it faces no progressive competitors. Despite these differences, however, Thagard agrees with Kuhn that judgments about the scientific status of a theory or discipline must involve both a social and a historical dimension, and he agrees with Lakatos that progress is necessary for genuine science.

THAGARD'S DEFINITION OF PSEUDOSCIENCE

In light of his criticisms of Kuhn and Lakatos, Thagard proposes two conditions that are necessary and sufficient for a theory or discipline to be pseudoscientific.

> A theory or discipline which purports to be scientific is *pseudoscientific* if and only if:
>
> 1 it has been less progressive than alternative theories over a long period of time, and faces many unsolved problems; but
>
> 2 the community of practitioners makes little attempt to develop the theory towards solutions of the problems, shows no concern for attempts to evaluate the theory in relation to others, and is selective in considering confirmations and disconfirmations. (33)

According to these conditions, astrology is pseudoscientific in part because it has not changed much since the time of Ptolemy. Unsolved problems have accumulated, and as Thagard notes, we now have (since the nineteenth century) psychological theories that do a better job of explaining and predicting human behavior. Despite this competition from psychology, astrologers have shown little interest in improving their theory or in evaluating it with respect to rivals.

Thagard concludes by isolating a number of interesting logical (and, to some, startling) consequences of his demarcation criterion. One might view the acceptability of these consequences as a measure of the plausibility of his proposal. First, some current fads, such as pyramidology and biorhythms, would not be considered pseudosciences because, at the moment, they lack serious competitors. Second, a theory can be scientific at one time and pseudoscientific at a later time; being scientific is not an unchanging property of a theory. Third, Thagard concludes that astrology used to be a genuine science but became pseudoscientific only when modern psychology arose in the late nineteenth century. If this is correct, then those scientists (the vast majority) who rejected astrology as pseudoscientific in the eighteenth century were being irrational.

THAGARD'S LATER THOUGHTS ABOUT PSEUDOSCIENCE

Because of objections to his demarcation principle for pseudoscience, especially the objection that nothing can be a pseudoscience unless it has competitors, Thagard has changed his views. In his book *Computational Philosophy of Science* (1988) he gives up trying to provide necessary and sufficient conditions for pseudoscience. Instead, he offers contrasting profiles of genuine science and pseudoscience. Relative progressiveness and a concern with confirmation and disconfirmation are still presented as hallmarks of science (and their absence is still associated with pseudoscience), but Thagard no longer claims that science must always possess these features or that pseudoscience must necessarily lack them. Thagard also introduces two new criteria for pseudoscience. One of these criteria is that pseudoscientific theories are often highly complex and riddled with ad hoc hypotheses. This provides some grounds for judging a doctrine pseudoscientific on its content, even if it currently has no scientific competitors.

Thagard's second new criterion concerns the sort of reasoning employed by many practitioners of pseudoscience, such as astrologers, namely, reasoning based on resemblances. Instead of testing causal claims by looking for statistical correlations, pseudoscientists are often content to rest their beliefs on superficial analogies. Traditional astrology is full of this sort of "resemblance thinking." For example, the planet Mars often has a reddish appearance, and so astrologers associate it with blood, war, and aggression. From this they conclude that Mars causes (or, at least, has a tendency to cause) aggressive personalities in people born at the appropriate time. In a similar way believers in folk medicine recommend turmeric as a treatment for jaundice and powdered rhinoceros horn as a cure for impotence.

As Thagard recognizes, not all pseudosciences employ resemblance thinking, and some pseudosciences employ reasoning based on statistical correlations that mimics, to some extent, reasoning found in the genuine sciences. Proponents of biorhythms, for example, rest much of their case on alleged correlations as do Velikovsky and von Daniken when they appeal to common elements in ancient myths to support their astronomical theories. Thus, in Thagard's revised account of pseudoscience, none of the elements mentioned—using resemblance thinking, refusing to seek confirmations and disconfirmations, ignoring alternative theories, trafficking in ad hoc hypotheses, sticking with theories that fail to progress—is a necessary feature of pseudoscience, and genuine sciences might, from time to time, share one or two of these features. But, Thagard claims, pseudosciences usually have most of these features and genuine sciences nearly always lack most of them. Thus, the difference between science and pseudoscience is a matter of degree rather than kind, although Thagard remains convinced that the difference of degree is usually large and obvious.

1.5 | Creation-Science and the Arkansas Trial

The search for demarcation criteria is not simply a curiosity to entertain armchair intellectuals or a pastime for students in philosophy of science.

Consider, for instance, the 1982 case of *McLean v. Arkansas Board of Education.*[15] At issue in the case was the constitutionality of Arkansas Act 590, which required teachers to give "balanced treatment" to both evolutionary theory and creationism in the biology classes taught in public schools. Act 590 describes "evolution-science" and "creation-science" as competing scientific models of the origin of species and offers the following definition for creation-science:

> "Creation-science" means the scientific evidences [*sic*] for creation and inferences from those scientific evidences. Creation-science includes the scientific evidences and related inferences that indicate: (1) Sudden creation of the universe, energy, and life from nothing; (2) The insufficiency of mutation and natural selection in bringing about development of all living kinds from a single organism; (3) Changes only within fixed limits of originally created kinds of plants and animals; (4) Separate ancestry for man and apes; (5) Explanation of the earth's geology by catastrophism, including the occurrence of a worldwide flood; and (6) A relatively recent inception of the earth and living kinds.[16]

JUDGE OVERTON'S OPINION

The task of the presiding judge, William Overton, was to decide whether Act 590 violates the Constitution of the United States. He reasoned that Act 590 is consistent with the Constitution only if the act satisfies the Establishment Clause of the First Amendment, which says that "Congress shall make no law respecting an establishment of religion, or prohibiting the free exercise thereof." The Supreme Court of the United States has for many years applied the articles of the Bill of Rights (the first ten amendments to the Constitution) not only to federal legislation but also to the laws passed by individual states. The Supreme Court's interpretation of the Establishment Clause has evolved into a three-part test for the constitutionality of any legislation involving religion. It was this three-part test that Judge Overton applied to Arkansas's Act 590. Failing any one of these three parts is sufficient to render a piece of legislation unconstitutional. Here is the test:

> First, the statute must have a secular purpose; second, its principal or primary effect must be one that neither advances nor inhibits religion . . . ; finally, the statute must not foster "an excessive government entanglement with religion."[17]

Judge Overton thought it clear that Act 590 was passed by the Arkansas General Assembly with the specific intention of advancing religion, and that fact alone—the lack of a secular purpose—would suffice to invalidate the statute. But Judge Overton wanted to show that Act 590 also fails the second and third parts of the three-part test. In order to show that Act 590 fails the second part, it is necessary to show that the statute either advances or inhibits religion as its "principal or primary effect." To accomplish this, Judge Overton thought it necessary to establish that creation-science is not a genuine science. For, as he argued (at the end of part IV(D) of his Opinion), "Since creation-science is not science, the conclusion is inescapable that the *only* real effect of Act 590 is the advancement of religion."[18]

Thus Judge Overton entered the philosophical debate over the criteria for genuine science. He sought guidance from expert witnesses, especially from a philosopher of biology, Michael Ruse.[19] It was primarily Ruse who developed the five characteristics that Overton lists as essential (necessary conditions) for genuine science:

1 it is guided by natural law;
2 it has to be explanatory by reference to natural law;
3 it is testable against the empirical world;
4 its conclusions are tentative, i.e., they are not necessarily the final word; and
5 it is falsifiable.

Ruse on the Status of Creation-Science

In "Creation-Science Is Not Science" Ruse defends the five items on Overton's list and argues that creation-science satisfies none of them. Ruse sees an intimate connection between items (1) and (2) on the list: it is only because scientific theories posit natural laws that the theories are able to explain; genuinely to explain something is to show why, given the relevant circumstances, it had to happen, and that requires an appeal to laws. (For more on explanation and laws, see chapter 6, "Models of Explanation," and chapter 7, "Laws of Nature.") Since creation-science posits acts of creation that are miraculous and unlawlike, Ruse concludes that it is not scientific. He also points out that creation-scientists make few if any testable predictions. Most of the time, creationists content themselves with describing the evidence in ways that are consistent with their doctrines. For example, creationists regard the common pattern of bones in the forelimbs of humans, bats, whales, and other mammals as an instance of God's design plan for mammals, but they offer no reason why this particular pattern exists rather than some other pattern or several different patterns. Evolutionists follow Darwin in explaining the pattern as the result of common descent: because all mammals have descended from a common ancestor, they share a common anatomical structure. Ruse concludes his case against creation-science by noting that most creationist research aims at trying to find flaws in evolutionary theory rather than making testable predictions based on the creationists' own theory. Modern creationists are dogmatic (not tentative) about their fundamental beliefs and show little or no interest in trying to falsify them.

LAUDAN'S CRITICISMS OF RUSE

In his "Commentary: Science at the Bar—Causes for Concern," Larry Laudan chastizes Ruse for perpetuating a view of science that, he claims, both of them know to be false. Laudan denies that philosophers of science would accept any list of characteristics as capturing the essence of science. Why, for example, should explanation by means of laws be regarded as a necessary condition for a theory to be scientific? Many theories begin by describing a new phenomenon, and only later, if at all, explain the phenomenon in a lawlike way. For example, Galileo discovered that all bodies released near the surface of the earth fall with the same acceleration but offered no explanation for this. Similarly, Newton claimed to have "deduced" the universal law of gravitation "from the phenomena" but accepted action at a distance as ultimately unexplainable.[20] Indeed, if one accepts the deductive-nomological model of explanation, according to which scientific explanations are deductive arguments with at least one statement of a law in their premises (see chapter 6, "Models of Explanation"), then in any such explanation there will remain, at least provisionally, something that is not explained, namely the premises that do the explaining. So Laudan rejects item (2) from the Ruse-Overton list as too strong.

Laudan also criticizes items (3) and (5) as being too weak, since, he argues, they are all too easily satisfied. Any theory, even a theory like creation-science that posits a divine creator, implies something about the observable world. For example, many creationists claim that all living things were created at the same time fewer than 50,000 years ago and that a worldwide flood caused many of the geological features now observed on the earth. As Laudan sees it, the flaw with creationism is not that such claims are untestable or unfalsifiable but rather that they have been tested and falsified.[21] Ruse responds to these and other criticisms by Laudan in the final piece in this chapter, "Response to the Commentary: *Pro Judice*."[22]

1.6 | Summary

In this chapter, we have explored a number of attempts to demarcate science from pseudoscience. But the results have been curiously inconclusive. Most scientists and philosophers of science readily agree that such things as pyramidology and creation-science are not genuine sciences, but there is no consensus on why this is so. Like obscenity, most people are able to recognize pseudoscience when they encounter it but find it much harder to explain why what they have encountered is pseudoscientific. The stress on explanation is important here. What we are seeking, as philosophers of science, is not just a handy way of detecting pseudoscience (on the basis, say, of a majority vote of the National Academy of Sciences) but a philosophically informative account of what makes a discipline genuinely scientific.

Despite the defects of his own demarcation criterion—falsifiability—Popper deserves credit for disposing of one tempting answer to the demarcation problem. No appeal to confirming evidence, by itself, is going to distinguish genuine science from its counterfeit. Inventing an elaborate hypothesis that is consistent with the known facts is just too easy. Popper's fruitful idea was to seek the demarcation between science and pseudoscience, not in confirmation, but in falsification. The hallmark of true science is its willingness to make testable predictions. If the predictions fail, then the theory should be abandoned as false. Unfortunately, Popper's simple idea does not work. As Lakatos and Thagard explain, falsifiability is both too weak and too strong. It is too weak because it would allow as scientific any number of claims that are testable in principle but that are, by no stretch of the imagination, scientific. It is too strong because it would rule out as unscientific many of the best theories in the history of science. Few scientists give up their theories simply because they have come into conflict with observation and experiment. Instead, they either look for a flaw in the data, or they modify their theories. The rejection of a theory simply because is disagrees with the facts (or what are taken to be facts) is the exception rather than the rule in science.

In differing ways, Kuhn, Lakatos, and Thagard each propose that there is a historical and a social dimension to judgments concerning the scientific status of a theory. All three insist that when we ask of a theory "Is it genuinely scientific?" it is a mistake to look at the theory as if it were a snapshot, caught at an instant of time. Rather, they argue, we have to consider how the theory has developed, especially how the theory has been modified to deal with new problems and recalcitrant data. For Kuhn, this means seeing the theory as part of a larger whole—what Kuhn calls a *paradigm*. (For more on Kuhn's notion of a paradigm see chapter 2, "Rationality, Objectivity, and Values in Science.") Thagard adopts Lakatos's notion of a scientific research programme in order to define a demarcation criterion. On this approach, roughly speaking, a theory is pseudoscientific if the research programme with which it is associated has been less progressive over time than has its rivals. As suggested in our discussion of Thagard's proposal, this comparative-progress definition of pseudoscience has a number of startling consequences. For example, some modern fads, such as pyramidology, might fail to qualify as pseudosciences simply because, at the moment, they lack competitors. Because of these defects, Thagard has avoided giving necessary and sufficient conditions for pseudoscience in his more recent writings.

As the debate between Ruse and Laudan concerning the status of creation-science makes clear, judgments about pseudosciences often depend on detailed considerations of the nature of law, explanation, confirmation, and falsification. It is highly unlikely that any simple-minded, one- or two-sentence definition of science will yield a plausible demarcation criterion that we can use to label and condemn as pseudoscientific those theories (and their advocates) that fail to meet the standards of good science. Ultimately, discriminating between science and its counterfeit depends on a detailed understanding of how science works. Despite the variety and complexity of the many different theories and activities that are, by common consent, genuinely scientific, are there general principles concerning explanation, confirmation, testing, and the like that these theories and activities share? In the rest of our book, some important attempts to answer this question will be explained and evaluated. Thus, what follows can be seen as an attempt to answer the questions left unanswered in this first chapter.

Notes

1. Newton's theory also predicts the bending of starlight if light rays are regarded as a stream of particles traveling at the speed of light. Because inertial mass is exactly equal to gravitational mass, the orbit of any object moving around the sun depends only on the velocity of the moving object, not on its mass. (The same thing is true of bodies near the surface of the earth. If you throw two objects with the same velocity in the same direction, then they will follow the same path regardless of their mass.) Thus, we do not have to know the mass of the light particles in order to calculate how they will move when close to the sun. But, Newton's theory predicts an amount of bending which is only half of that predicted by Einstein. The difference arises because Einstein's theory entails that the gravitational field close to the sun is slightly stronger than in Newton's theory. Thus, it is not that Einstein's theory predicted a kind of effect, the bending of starlight, that Newton's theory did not. Rather, both theories gave competing predictions of its magnitude, and Einstein's prediction was more nearly right. Interestingly, observations made during some later eclipses (1929, 1947) found deviations that were higher than those predicted by Einstein. But more recent observations are in closer agreement with Einstein's theory, and none of the observations agrees with Newton's. (The issue of whether theories such as Newton's theory of gravity can be conclusively refuted is discussed in chapter 3, "The Duhem-Quine Thesis and Underdetermination.") Eddington's role in this episode is controversial because he threw out as biased one set of observations that agreed with Newton's prediction. For details about the difficulties of making the eclipse observations and competing interpretations of Eddington's behavior, see John Earman and Clark Glymour, "Relativity and Eclipses: The British Eclipse Expeditions of 1919 and

Their Predecessors," *Historical Studies in the Physical Sciences* 11 (1980): 49–85; Deborah Mayo, "Novel Evidence and Severe Tests," *Philosophy of Science* 58 (1991): 523–52; and Harry Collins and Trevor Pinch, *The Golem: What Everyone Should Know about Science* (Cambridge: Cambridge University Press, 1993).

2. It is tempting to try to rule out cases such as *(T & C)* by requiring not merely that the theory as a whole make testable predictions but that each individual component of the theory also make testable predictions. See chapter 3 for a discussion of whether any significant scientific theory could meet this additional requirement.

3. See Karl R. Popper, "Natural Selection and the Emergence of Mind," *Dialectica* 32 (1978): 339–55.

4. Karl R. Popper, "Autobiography of Karl Popper," in *The Philosophy of Karl Popper*, ed. Paul A. Schilpp (La Salle, Ill.: Open Court, 1974), 1: 134.

5. *The Philosophy of Karl Popper*, 1: 137. For excellent discussions of the tautology problem, see chapter 2 of Elliott Sober's *The Nature of Selection* (Chicago, Ill.: University of Chicago Press, 1984), and chapter 4, "The Structure of the Theory of Natural Selection," in Robert N. Brandon's *Adaptation and Environment* (Princeton, N.J.: Princeton University Press, 1990).

6. Thus, somewhat paradoxically given the Arkansas trial (discussed later in this commentary), one of the themes in creationist literature is that creationism and evolutionary theory are both equally *un*scientific because neither makes testable predictions. Needless to say, this position is hard to reconcile with another creationist theme, namely, that evolutionary theory has been significantly disconfirmed by a variety of evidence.

7. Another problem is that tautologies, strictly speaking, are propositions that are true solely in virtue of their logical form. Presumably, the issue is not whether some biological statement is a tautology but whether it is analytic. Analytic statements, on one characterization of analyticity, are statements that are true solely in virtue of the meanings of the words and symbols used to express them. In chapter 3, there is an extended discussion of Quine's thesis that no line can be drawn, even in principle, between statements that are analytic and those that are not. If Quine is right, then the charge of being tautologous evaporates.

8. See, for example, the authors quoted in Beverly Halstead, "Popper: Good Philosophy, Bad Science?" *New Scientist* (17 July 1980): 215–17.

9. Karl R. Popper, "Letter on Evolution," *New Scientist* (21 August 1980): 611.

10. The argument that follows is a simplified version of the one given in Appendix *vii of Karl R. Popper, *The Logic of Scientific Discovery* (New York: Basic Books, 1959), 363–77.

11. The possible hypotheses enumerated in the text—exactly one particle obeys Newton's theory, exactly two particles obey Newton's theory, etc.—are exclusive: if any one of them is true, then all the others must be false. If each hypothesis has the same prior probability, p, and there are n of them, then the probability that at least one of the hypotheses is true is $n \times p$. (See axiom 3, the special addition rule, in "Bayes's Theorem and the Axioms of Probability Theory" in the commentary on chapter 5.) Since $n \times p$ is a probability, it cannot be greater than

1. So, if n is infinite, p cannot be finite. Thus, p must be 0. The derivation of this result depends on assuming that each hypothesis has the same prior probability, something that Bayesians deny. For this and other Bayesian criticisms of Popper's argument, see Colin Howson, "Must the Logical Probability of Laws Be Zero?" *British Journal for the Philosophy of Science* 35 (1973): 153–63.

12. For a fuller discussion of this and other applications of Bayes's theorem to issues in confirmation, see chapter 5, "Confirmation and Relevance: Bayesian Approaches."

13. As Newton realized, the main irregularities in the motion of the moon are due to the attraction of the sun. The force exerted on the moon by the sun is a rather large fraction (1/89 at new and full moon) of the force exerted by the earth. As in all such three-body problems, no exact solution of Newton's equations is possible. Because the moon is close to the earth, even small perturbations are easily observed. This requires the calculations to be extended down to very small terms. Initially, Clairault's calculations yielded a rate of precession of the moon's apogee of 20 degrees per year, only half the real amount. At first, Clairault speculated that Newton's inverse-square law gravitational formula was incorrect for small distances and should be supplemented by an extra term varying as the inverse fourth power of the distance. But on extending his calculations to include higher order terms that had been neglected in his original approximation, Clairault found that his first result was doubled. So Newton's theory was vindicated. For more on the problem of the moon, see Anton Pannekoek, *A History of Astronomy*, rpt. (1951; New York: Dover Publications, 1990) ch. 30.

14. See Morton Grosser, *The Discovery of Neptune* (Cambridge, Mass.: Harvard University Press, 1962). For a lively criticism of the oft-repeated claim (by Popper, Lakatos, and others) that Newton's theory was prima facie falsified by the discovery of perturbations in the orbit of Uranus, see Greg Bamford, "Popper and His Commentators on the Discovery of Neptune: A Close Shave for the Law of Gravitation?" *Studies in History and Philosophy of Science* 27 (1996): 207–32.

15. Judge Overton's opinion in this case is reprinted in *Science* 215 (1982): 934–43, in *Science, Technology, and Human Values* 7 No. 40 (1982): 28–42, and in Michael Ruse, ed., *But Is It Science?* (Buffalo, N.Y.: Prometheus Books, 1988).

16. *Balanced Treatment for Creation-Science and Evolution-Science Act* (1981), 73d General Assembly, State of Arkansas, Act 590 sec. 4; reprinted in *Science, Technology, and Human Values* 7 No. 40 (1982): 11.

17. William R. Overton, "Opinion in *McLean v. Arkansas*," *Science, Technology, and Human Values* 7 No. 40 (1982): 29.

18. For a criticism of this inference and other aspects of Overton's opinion, see Philip L. Quinn, "The Philosopher of Science as Expert Witness," in *Science and Reality*, ed. J. T. Cushing, C. F. Delaney, and G. M. Gutting (Notre Dame, Ind.: University of Notre Dame Press, 1984), 32–53. As Quinn explains in a later article, he agrees with Overton's conclusion that Arkansas Act 590 is unconstitutional because it lacks a secular purpose. What he criticizes is Overton's attempt to show that Act 590 has the advancement of religion as its primary effect because, as it is alleged, creation-science fails each of the five conditions on Ruse's list deemed necessary for genuine science. Like Laudan, Quinn argues that each of Ruse's

conditions is either not necessary for genuine science (because some bona fide sciences lack it) or, when properly interpreted, is possessed by creation-science. According to Quinn, the proper thing to say about creation-science is not that we can show that it is not science but that, at best, it is dreadful science. See Philip L. Quinn, "Creationism, Methodology, and Politics," in Michael Ruse, ed., *But Is It Science?* 395–99.

19. For an entertaining account of Ruse's participation in the Arkansas trial and a transcript of his testimony, see Michael Ruse, ed., *But Is It Science?* 13–35, 287–306.

20. In the General Scholium of the *Principia*, added to the second edition of 1713, Newton wrote: "But hitherto I have not been able to discover the cause of those properties of gravity [i.e., the proportionality of gravitational force to the quantity of matter and its variation with the inverse square of distance], and I frame no hypotheses [*hypotheses non fingo*]; for whatever is not deduced from the phenomena is to be called an hypothesis; and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy. In this philosophy particular propositions are inferred from the phenomena, and afterwards rendered general by induction." Isaac Newton, *Philosophiae naturalis principia mathematica*, vol. 2, trans. A. Motte, rev. F. Cajori (Berkeley: University of California Press, 1934), 547. Throughout the *Principia*, Newton argues that gravity cannot be explained by any mechanism acting by direct contact, as Descartes and the Cartesians had hypothesized. This seems to leave only two choices, both of which Newton entertained in his writings: either gravity is due to the direct action of God, or it is caused by an aether, itself composed of particles between which forces act at a distance across empty space. Many philosophers of science, notably Duhem and Popper, have criticized Newton's claim that his own theory can be "deduced from the phenomena." See "Duhem's Critique of Inductivism: The Attack on Newtonian Method," in the commentary on chapter 3.

21. Elsewhere, Laudan has argued that the wide diversity of scientific beliefs and activities and the failure of the attempts by Popper, Thagard, and others to solve the demarcation problem make it unlikely that we will ever find a demarcation criterion in the form of necessary conditions for genuine science. See Larry Laudan, "The Demise of the Demarcation Problem," in *Physics, Philosophy, and Psychoanalysis*, ed. R. S. Cohen and L. Laudan (Dordrecht, Netherlands: D. Reidel, 1983), 111–28.

22. The debate between Ruse and Laudan is continued in Larry Laudan, "More on Creationism," *Science, Technology, and Human Values* 8 (Winter 1983): 36–38, and Michael Ruse, "The Academic as Expert Witness," *Science, Technology, and Human Values* 11 (Spring 1986): 68–73. These and other relevant articles are conveniently reprinted in Michael Ruse, ed., *But Is It Science?*