Science, Technology, and Society I
Reading I

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for Prof. Leah
SocTec 1
Theme 1 Reading

Paradigms Lost: Tackling the Unanswered Mysteries of Modern Science
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FAITH, HOPE, AND ASPERITY

BELIEF SYSTEMS, SCIENCE, AND THE INVENTION OF REALITY

WORLD VIEWS IN COLLISION

On the night of February 24, 1987, Canadian astronomer Ian Shelton was looking through the telescope at the Las Campanas Observatory in Chile; what he saw became *the* scientific event of the decade in the astronomical world. On that night, Shelton became the first to see the star Sanduleak −69° 202 come to the end of its cosmic tether in that most spectacular of celestial fireworks displays, a supernova. According to current astrophysical wisdom, such events occur when the hydrogen that fuels the thermonuclear furnaces of stars a little bigger than our

sun runs out, allowing the contracting force of gravity to gain the upper hand over the expanding forces of thermal radiation. The star's mass then collapses in on itself until the pressures build to the point where the star literally blows its top, scattering most of its mass into the interstellar void, leaving behind a small, rapidly spinning ball consisting solely of neutrons at an incredibly high density. In fact, so dense is the material of such a "neutron star" that one cubic inch of it would weigh more than a billion tons, and a pinhead's worth several million. Although many supernovas have been seen in distant galaxies, the importance of supernova 1987A was twofold: It was the first time that astronomers had extensive observations of a star before it became suicidal, and it happened in the Large Magellanic Cloud, a galaxy "only" 170,000 light-years distant—essentially next door on the astronomical scale of things. While supernovas have been observed from Earth for centuries, going back at least as far as the Chinese accounts of what is now the Crab Nebula in A.D. 1054, observation of their neutron star residue dates back only a few years and constitutes one of the major science stories of the 1960s. Since the discovery of these neutron stars or, as they as more colloquially termed, *pulsars* (for "pulsating radio sources") serves as an admirable case study of the ways of science in the late twentieth century, let's climb into a time machine and go back to those exciting days to retrace the steps leading to this momentous discovery.

The story begins in 1965 with the decision by Jocelyn Bell, a young woman from Northern Ireland, to seek a doctorate at Cambridge University in the then-new field of radioastronomy. As Bell (now Jocelyn Bell Burnell) tells it, she had become fascinated with astronomy as a young girl when her architect father was hired to design the observatory in the small Irish town of Armagh. Unfortunately, even then she saw that a necessary condition for successful pursuit of the astronomer's nocturnal art is to have a night owl's constitution, easily being able to interchange the normal hours for sleeping and working. Despite her passion for the stars, in the 1950s her constitutional need for a good night's sleep at the normal hours looked like a fatal obstacle to any budding astronomical aspirations. But as luck would have it, this was the time when Martin Ryle of Cambridge was developing one of the first telescopes devoted to searching the skies in the radio rather than visible light part of the electromagnetic spectrum. Since the best time for "seeing" at these frequencies is during the daylight hours, Cambridge was the place for her, and off she went armed with an undergraduate degree in physics to work for her Ph.D. in a group led by Anthony Hewish.

One of the most sacred rules of academic institutions everywhere is that the graduate students perform the slave labor, the Cambridge Institute of Theoretical Astronomy being a staunch upholder of this venerable principle. Consequently, Bell spent her first two years as a graduate student wielding a 20-pound sledgehammer, helping to construct the radiotelescope that she would later use to gather the material for her doctoral dissertation. Following completion of the telescope in 1967, team leader Hewish assigned Bell the thesis topic of measuring the angular diameter of radio galaxies *(quasars)* from the way their signals "twinkled" when seen from Earth due to the solar wind of material emitted from the Sun. Her job was to operate the telescope singlehanded and analyze the output until she accumulated enough data for a respectable thesis. Since the telescope spewed out 96 feet of three-track paper each day and covered the entire sky in four days, Bell's data analysis activity was hardly less energy-intensive than building the telescope itself, involving as it did eyeballing the telescope record and separating the wheat of true twinkling signals from the chaff of French television, military radar, aircraft altimeters, and other Earth-based sources of interference. The telescope was turned on in July 1967 and, not surprisingly, by October she was already 1,000 feet of chart paper behind. It was at this point that the fun, both galactic and earthly, began.

In one of the 400 feet of chart readings produced with each scan of the sky, Bell noticed that there was about half an inch of what she termed "scruff" that resisted classification. She saw that the scruff was neither twinkling or man-made interference, and then recalled having seen similar patterns before on another record from the same part of the sky. Furthermore, she noticed that the mysterious signals seemed to be appearing periodically on sidereal time of twenty-three hours, fifty-six minutes, i.e., the time needed for a given location on Earth to return to the same position relative to the fixed stars (the sidereal day is four minutes shorter than the terrestrial day due to the Earth's orbital motion about the Sun).

At this juncture Bell discussed the signals with Hewish, and they decided to look at them again on a faster recorder that would allow them to pick out more detail. This recorder was occupied at the moment, so they had to wait until mid-November to make the new reading. As so often happens in life, just when you want a taxi (or a cop) there's not one to be found anywhere; astronomical anomalies are similar, and Bell had to wait several weeks before she could reacquire the odd signal. Imagine her surprise when she finally found it again and discovered that it was pulsating at the metronomic rate of almost *exactly* 1⅓ seconds. She immediately phoned Hewish, who promptly dismissed the signals as man-made in light of their extreme regularity. However, an Earth-based source would keep terrestrial time, not sidereal, casting a very dark shadow over Hewish's offhand conclusion. But the fastest variable star then known had a period of one third of a *day*, and it was difficult to conceive of what kind of star would rotate in little more than a second.

The first attempt to reconcile these conflicting facts was to conjecture that the observations were radar signals bouncing off the Moon, or a satellite in an odd orbit. But such an explanation didn't wash, and since only astronomers and the stars keep sidereal time, Hewish thought that perhaps some other observatory had a program under way that would account for the unusual signals. His queries to other radioastronomers turned up no such program. The next trial explanation was the LGM Hypothesis, postulating that the signals were intelligent communications from "little green men." As a test of this conjecture, Hewish calculated the Doppler shift of the pulses assuming that the LGM would be on a planet, and that the planet's orbital movement around its star would create a clustering of the pulses as the planet moved toward Earth and a spacing-out of the signals as it moved away. This explanation also came a cropper when the only Doppler shift noted was that due to the Earth's motion around our sun. At this point, theory gave way to another observation, which definitively settled the matter.

Just before leaving for her Christmas holiday in December 1967, Bell was working late one night analyzing a record from a different part of the sky. She noticed some more scruff that looked remarkably similar to that of the LGM signal. As serendipity would have it, the telescope was due to scan that part of the sky again that very night, and she luckily got a strong read-

ing showing an extremely regular train of pulses coming in at the rate of about 1¼ seconds per pulse. Since another rule of graduate student life is that you don't telephone your professor at 3 A.M. (at least you don't if you value finishing your degree program), Bell just dropped the recording on Hewish's desk with a note asking him to keep the recorder going over the vacation period, and left for her holiday. Hewish himself then made a recording in mid-January confirming the second source, thereby removing the LGM hypothesis from further consideration on the grounds that it was extremely unlikely that there could be two groups of LGM trying to signal us on different frequencies at the same time. So when Bell returned from her Christmas break, she had two important problems to deal with: (1) there was more than one pulsar, and (2) it was time to start writing up a thesis describing her original work on the angular diameter of quasars (although it ultimately contained an appendix describing the pulsar observations, too).

Forced into accepting that the sources of these pulses were some sort of stellar phenomena, Hewish, Bell, and three others from the Cambridge team coauthored the first paper on the subject, which was published in February 1968, and which vacillated between identifying the sources as neutron stars and as white dwarfs, the kind of object our own sun will contract into a few billion years from now. Six months later, the astrophysical community accepted Thomas Gold's interpretation that they were neutron stars as being the only plausible explanation fitting all the observations. This proposal followed up a theoretical suggestion that Fritz Zwicky and Walter Baade made in 1934. The general picture of how a neutron star acts to produce the observations seen by Bell and Hewish is shown in Figure 1.1. While the scientific excitement ended here, the story was still far from over.

In 1974 the Nobel Committee awarded its prize in physics for the first time to astronomers, citing Martin Ryle and Anthony Hewish for their "decisive work in the discovery of pulsars." Not a word was said about the actual discoverer of pulsars, Jocelyn Bell! Shortly after the award ceremony in December, another member of the Cambridge astronomical group, Fred Hoyle, said in a speech in Montreal that Bell's findings had been kept secret for six months while her supervisors "were busily pinching the discovery from the girl, or that was what it

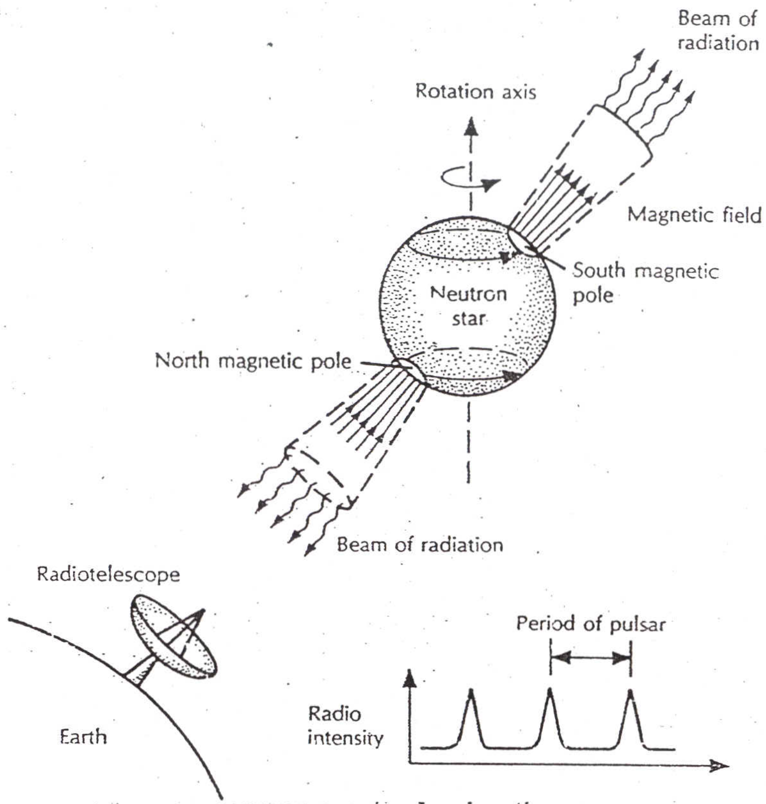

FIGURE 1.1 *A pulsar in action*

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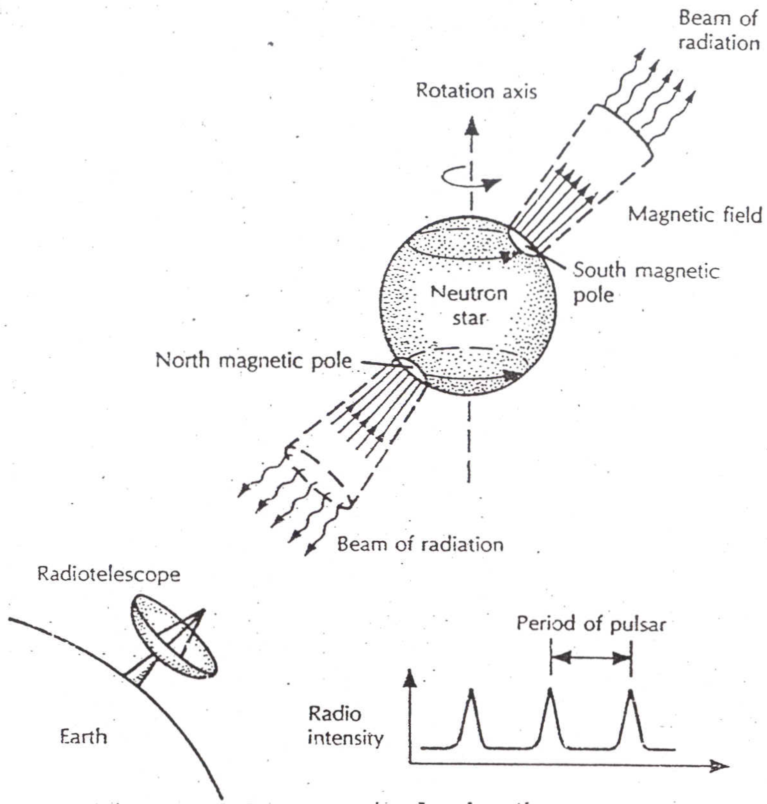
by not putting this story on film, showing an upset, slightly bookish Jane Fonda or Meryl Streep look-alike publicly denouncing a suave, but faintly sinister, James Mason–ish professor on the steps of the Stockholm City Hall for casting her and her contribution aside in pursuit of personal fame and glory. Unfortunately for Hollywood, real life as usual had quite a different ending in mind. In response to the various claims and counterclaims, Jocelyn Bell had the last word when she stated that Hoyle "has overstated the case so as to be incorrect." But still, given the proclivity of the film industry for warping and distorting reality in pursuit of art and entertainment, not to mention hard cash, maybe there's hope yet for realization of my vision. In any case, the entire pulsar episode serves as a sterling example of the bright side of the folkways, mores, and byways of contemporary scientific life. For a look at the dark side, let's return to our time machine and go back a few more years to examine another tempest in the astrophysical teapot.

In the writings of Plato and Herodotus we find the assertion that the Sun now rises where it once set. How could they make such a bizarre claim? And why do so many cultures have legends of global floods, manna from heaven, darkness on the Earth, and other such strange phenomena? In 1950 the Macmillan Publishing Company put out the volume *Worlds in Collision* by a Russian-born psychoanalyst, Immanuel Velikovsky, who purported to explain these and many other phenomena as the result of a series of celestial cataclysms taking place during historical times. This book so enraged the scientific community that Macmillan, under pressure of a boycott of its textbook division, handed the best-selling project over to Doubleday and fired the editor responsible for dealing with the manuscript. It's instructive to examine Velikovsky's claims and methods as an example of the sort of thing that sends the scientific establishment into apoplectic fits.

The gist of Velikovsky's argument is that a large comet was expelled from Jupiter sometime around the year 1500 B.C. This comet passed very close to us, with its tail touching the Earth and causing a rain of petroleum, as well as darkening the sky for several days with its dust and debris. In addition, the Earth's rotation rate was slowed down by the comet, resulting in earthquakes, hurricanes, tidal waves, and a variety of other dra-



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matic environmental shenanigans. Electrical discharges between the Earth and the comet caused a reversal of the Earth's magnetic field, the polar regions shifted, and the Earth's axis of rotation was altered, resulting in a change in the order of the seasons. Furthermore, the Earth was pushed into a larger orbit, lengthening the year to 360 days.

Velikovsky correlates this first pass of the comet with the Exodus of the Israelites from Egypt, claiming that the plagues of blood, vermin, and hail noted in the Bible were the result of the Earth's contact with the comet's tail. He also explains the parting of the waters of the Red Sea as being due to the stopping of the Earth's rotation, and that the manna from heaven sustaining the Israelites in the desert was composed of carbohydrates from the comet. *Worlds in Collision* then asserts a second passage of the comet fifty-two years later, this time interfering with the Earth's rotation just at the time when Joshua commanded the Sun to stand still. And what does Velikovsky say about the identity of this celestial molester? He claims that the comet is now what we call the planet Venus! But the story doesn't end there.

In Velikovsky's scenario there was another close cometary encounter around the year 800 B.C., this time with the planet Mars. This near collision knocked Mars out of its orbit, bringing it close to the Earth on at least three occasions. These near misses shifted the Earth's orbit even further away from the Sun, bringing about the current year of 365¼ days. At this point, all three planets settled into their current positions, thus folding up the tent on Velikovsky's celestial circus.

One might well inquire as to what kinds of arguments and methods Velikovsky employed to explain these catastrophic goings-on. Fundamentally, *Worlds in Collision* is based upon ancient manuscripts, legends, and traditions. In a later volume, *Earth in Upheaval,* he cites evidence such as the existence of coal beds in Antarctica, rock formations with reversed magnetic polarity, fossil beds containing animals from both desert and forest, as well as other geological and paleontological facts. The cometary origin of Venus also gave rise to Velikovsky's speculations that Venus was hot and that the material for the comet had originally been expelled from Jupiter, leaving behind what we now know as the giant Red Spot.

It probably goes without saying that mainline astronomers, geologists, astrophysicists, and paleontologists speak with one

loud voice in their condemnation of both Velikovsky's methods and his conclusions. While his work represents an imposing piece of sustained scholarship, there are just too many inconsistencies in far too much of his historical, archaeological, astronomical, and physical data to take the arguments seriously. For instance, while it did turn out that Venus was scorchingly hot, just as Velikovsky had predicted, this is almost certainly due to an atmospheric "greenhouse effect" and not to any kind of cometary origin. Furthermore, the atmosphere of Venus is almost totally devoid of the hydrocarbons that Velikovsky claimed would be found as its main constituents. Moreover, the surface of Venus appears to be over 1 billion years old, instead of just a few thousand years as predicted by Velikovsky. For these reasons and many more, Velikovsky's vision of the solar system has now been relegated to that corner of the scientific attic where sit ancient astronauts, the Piltdown man, phrenologists, astrologers, and all the other playmates of the pseudoscientist.

Despite the truly devastating holes in his theory, Velikovsky died in November 1979 convinced that he had been the victor in his war against the Brahmins of science. And, in fact, his ideas live on to this day in some circles. In our quest here to uncover the essence of what constitutes "scientific" knowledge, it's worth taking a moment to examine the pulsar and *Worlds in Collision* theories as antipodes of the spectrum of what is commonly termed scientific research.

At first glance, there appear to be a number of similarities between the work of Bell and Hewish on pulsars and that of Velikovsky: unexplained astronomical phenomena, conjectures and refutations of various theoretical explanations, a physically unobservable explanation interpreted to fit the observations—even a public controversy over some sociological aspects of the way the world of science goes about distributing its accolades. With these points of contact, why is it that the scientific community chose to reward Hewish with its highest honor, the Nobel Prize, while at the same time vilifying Velikovsky and dismissing him as what could charitably be termed a misguided crank? Just what was it *exactly* about the pulsar work that made it the height of respectability and was so obviously lacking in the efforts of Velikovsky?

The long and proper answer to the question will occupy us for

much of the remainder of this chapter; the short answer is that by common consensus in the scientific community, certain standards have been set for what constitutes acceptable evidence and methods, with the pulsar work adhering to them while Velikovsky's did not. The central point for us in this volume is the degree to which those commonly accepted standards generate real rather than virtual knowledge of the universe *in itself*. Put another way, do the methods and standards of science produce a brand of knowledge that is somehow more certain or of higher intrinsic pedigree than the methods and standards of other seekers after truth like Velikovsky? The first step toward a resolution of this overarching question is to address a different question: Just what does constitute the practice of "science" as that term is commonly used in today's world?

DID YOU SAY SCIENCE?

Back in the days when I still attended cocktail parties, the most awkward situations always arose at those odd moments when the music stopped and social convention dictated that I make some feeble effort to "mix." Generally at these times, life conspired to place me next to some slightly frenetic, upwardly mobile yuppie type suffering from an overdose of adolescent enthusiasm for drinking deeply from the brackish waters of life, not to mention our host's bar. Inevitably such encounters began with the question "What do you do?" Resisting the temptation to reply, "Ah, yes, the eternal question," or give some other equally sophomoric response, in the early going I used to answer honestly that "I'm a mathematician." The reactions to this bit of ill-advised candor fell into one of two categories: a petulant pout followed by the curious compliment that "I was always terrible in math," or what was even worse, a bright smile and the remark "Oh, you'd love my uncle. He's an accountant." Being a slow learner, I needed some time to realize that such frank confessions of professional perversion were not the road to success on the cocktail-and-corn-chip circuit. So I began experimenting with other, less esoteric replies: "I'm an electrical engineer, a chemist, an agronomist ["What's *that?*"], a scientist." The results could hardly have been worse if I'd claimed to have been a psychiatrist, an undertaker, or, heaven forbid, some back-slapping politico type. Finally, I hit upon the winning solution of just

saying that I was an unemployed tennis coach, at which point my Social Interaction Index shot up like a Minuteman missile. But the sad conclusion to be reached from this very statistically insignificant sample is that there is a wide variety of gross misconceptions and nontrivial misunderstandings floating around, even among the educated public, as to the nature of both scientists and the ways in which they spend their days (and nights!).

Trying to distill the essence out of the aforementioned encounters, I eventually came to the surprising realization that the term *science* seems to be used interchangeably in general conversation in at least three quite distinct ways:

Science =
- a set of *facts* and a set of *theories* that explain the facts
- a particular *approach*, the scientific method
- whatever's being done by *institutions* carrying on "scientific" activity

As a general rule, the nonscientific public usually opts for the third interpretation, occasionally the first, but virtually never the second—just the opposite ordering from that given by the scientific community itself. It's no wonder C. P. Snow could develop a lengthy essay on the "two cultures."

The fundamental misunderstanding on the public's part of what constitutes a "scientific" activity gives rise to an array of subsidiary misperceptions about the goals of science and the way scientists go about their business of trying to achieve them. Let me list just a few of the more important popular fictions:

- *The primary goal of science is the accumulation of facts.* Unfortunately, the mere cataloguing of data is not enough; we also require some overall organizing principles and a relationship between these principles and the data. Actually, for scientists the more reliable a fact is, the more trivial and unimportant it becomes. For instance, the atomic weight of carbon can confidently be given as 12.011 atomic units. Yet this fact is basically just a curiosity until it's correlated with similar facts about the other chemical elements, using the laws and theories of chemistry and physics.
- *Science distorts reality and can't do justice to the fullness of human experience.* Every human undertaking must somehow pick and choose as to what aspects of reality to omit in order to probe other aspects of the world. In this regard science is no differ-

ent from religion, art, literature, mysticism, or any of its other competitors in the reality-generation business.

- *Scientific knowledge is truth.* Science is not in the business of providing ultimate explanations. Every scientific law or theory is subject to modification; there are no universal, absolute, unchangeable "truths" in science.
- *Science is concerned primarily with solving practical and social problems.* I can't think of a single statement about science that could be further from the actual case. For most scientists, science is a game played for understanding, not for obtaining practical information about how to build a better radio, mix more nutritious dog food or iron out the wrinkles of middle-aged dowagers. In fact, this "science = technology" misperception is so pervasive that it merits a few additional words all its own.

Some time back, I had the enervating experience of working for a man who suffered from the delusion that doing science meant finding answers to practical problems posed by industrialists, government policymakers, and other dreamers, schemers, and so-called men of affairs. One conversation that I ruefully recall involved my temerarious claim that if you focus attention on finding well-defined answers, then you're not doing research, at least not scientific research. Research involves ideas, not answers. In my view, what counted was developing a deep understanding of the question itself; whatever "answers" there might be would then follow as corollaries of this insight into the real nature of the question. A solution itself is not the ultimate goal; what's important is understanding why an answer is possible at all, and why it takes the form that it does. The point I was making was that technological advancement and the acquisition of scientific knowledge have only the feeblest points of contact with each other. Technology is primarily engineering, and new technologies come more from fighting with physical reality than from scientific theories. Besides, it's not clear that new technologies give us a better *understanding* of nature anyway, e.g., modern medicine vis-à-vis Chinese acupuncture.

The moral of the foregoing little tale is that even many people who practice under the rubric of what in the vernacular is called a scientist hold to a view of science and scientific work that at best falls into the third category noted earlier, which we might compactly describe as "the General Electric Syndrome." That is, if GE is doing it, it must be science. Well if GE is doing it, it probably *isn't* science, at least not the kind of science that most members of the global scientific community would recognize. It may be high-grade technology or world-class engineering or even pathbreaking developmental research, but definitely not science. I hasten to point out here that this observation is in no way intended to minimize the truly outstanding and genuine scientific work that *is* carried out at places like GE, IBM, Bell Labs, Exxon, and so on. But it's not the real science going on in these corporate research labs that members of the public have in mind when they think of, say, IBM. What comes to mind is computers, typewriters, and all the other office paraphernalia that carries the IBM logo and that people use in their day-to-day affairs. The development of these gadgets is the main business of such an institution, and that development is definitely not science; it's technology. Now let's get back on course and examine just what it is that *does* constitute science as it's seen by the scientists themselves.

Paradoxically, scientists usually think of science as one area of life in which ideologies play no role. Nevertheless, there is a collection of beliefs and ideals about the practice of science that the scientific community clings to with such universal tenacity that it's difficult to describe it as anything other than an ideology—the ideology of science. The scientific ideology is a mixture of logical, historical, and sociological ideals about how science should operate in a Panglossian world, and rests upon the following pillars:

- *The logical structure of science:* This pillar represents what many of us learn in our early schooling about the procedures followed in science. Here we find the sequence:

Observations/Facts
↓
Hypothesis
↓
Experiment
↓
Laws
↓
Theory

To many, this diagram represents the essence of what we think of as *the scientific method.* Observations give rise to conjectures and hypotheses, which in turn are checked out by performing

experiments. If the experiments don't confirm the hypothesis, then new hypotheses are formed, just as in the pulsar work described earlier. Those hypotheses that survive are encapsulated into empirical relationships, or laws, which in turn are embedded in larger explanatory theories. It is this sequence of steps that's been the focus of most of the philosophical analyses of the process of science, as we shall discuss in detail later. However, to the practicing scientist there is much more to the scientific enterprise than mere philosophy.

- *Verifiability of claims:* Science is a public undertaking with many filters that a claim must pass through before it's accepted as part of the current conventional wisdom. Two of the most important are the refereeing process for scientific articles and the repeatability of experimental results. Before a reputable scientific journal will publish a research announcement, it's sent out for review to other workers in the field, not only as insurance that the results are correct, but also to substantiate their significance within the framework of current knowledge in the area. In a similar manner, published work is supposed to report all the details of the investigator's experimental setup so that any interested party can, in principle, repeat the experiment and try to replicate the reported results. Thus, in the utopian world where the scientific ideology reigns, refereeing and repeatability keep the scientific process (and the scientist) honest.
- *Peer review:* The modern scientist is in much the same situation as the artisan of the Renaissance, at least when it comes to needing a patron to finance pursuit of the muse. The only difference is that nowadays everyone has the same patron—the federal government. As a result, most funds are allocated by federal agencies, making liberal use of the so-called peer review process. This involves committees of experts from the various fields getting together and recommending to the funding agencies those projects and those scholars whose work they feel merits support. According to the ideology, this process ensures that money is channeled to those ideas, institutions, and individuals showing the clearest evidence of being able to do something productive with it.

Given the highly egalitarian, logical, meritocratic nature of the scientific ideology, it comes as no surprise that many scientists accept it as at least a very close approximation to the way

science really is. I'll defer detailed consideration of this point to a later section. At the moment let me just remark that a neutral skeptic would almost certainly raise an eyebrow or two over the rather obvious fact that the conventional ideology focuses entirely upon the *process* of science, leaving aside all considerations of the motives and needs of the scientists themselves. The degree to which this omission casts a cloud over the rosy picture painted above will occupy our attention throughout the book. For now, let's stick to the scheme above and turn the spotlight on the cognitive structure of science, in an attempt to get back to the questions of just what kind of knowledge the process of science is able to offer us about the nature of the world as it is, and whether that kind of knowledge is in some way superior to any other kind.

THE NATURAL PHILOSOPHER'S STONES

The issue before the house for the next couple of sections is consideration of the dual questions:

> Do scientific theories in any sense tell us about the way the world *is?*
>
> Does science have anything like a *method* for creating and/or evaluating theories?

Since all theories must necessarily be expressed in some kind of language (natural, symbolic, mathematical), the first question takes us into the province of the philosophy of language as a tool for representing reality. The second question deals more with science per se, forcing us to confront the natural query "What's so special about science?" In other words, why should we believe that scientific knowledge is any more correct or reliable than any other sort? So our short-term objectives are to explore the question marks in the following diagram:

Scientific theory $\overset{?}{\rightleftarrows}$ Objective reality

↑ ?

Scientific methods

To address these two foundational question marks, it will be necessary for us to dip briefly into the work of several twentieth-

experiments. If the experiments don't confirm the hypothesis, then new hypotheses are formed, just as in the pulsar work described earlier. Those hypotheses that survive are encapsulated into empirical relationships, or laws, which in turn are embedded in larger explanatory theories. It is this sequence of steps that's been the focus of most of the philosophical analyses of the process of science, as we shall discuss in detail later. However, to the practicing scientist there is much more to the scientific enterprise than mere philosophy.

- *Verifiability of claims:* Science is a public undertaking with many filters that a claim must pass through before it's accepted as part of the current conventional wisdom. Two of the most important are the refereeing process for scientific articles and the repeatability of experimental results. Before a reputable scientific journal will publish a research announcement, it's sent out for review to other workers in the field, not only as insurance that the results are correct, but also to substantiate their significance within the framework of current knowledge in the area. In a similar manner, published work is supposed to report all the details of the investigator's experimental setup so that any interested party can, in principle, repeat the experiment and try to replicate the reported results. Thus, in the utopian world where the scientific ideology reigns, refereeing and repeatability keep the scientific process (and the scientist) honest.
- *Peer review:* The modern scientist is in much the same situation as the artisan of the Renaissance, at least when it comes to needing a patron to finance pursuit of the muse. The only difference is that nowadays everyone has the same patron—the federal government. As a result, most funds are allocated by federal agencies, making liberal use of the so-called peer review process. This involves committees of experts from the various fields getting together and recommending to the funding agencies those projects and those scholars whose work they feel merits support. According to the ideology, this process ensures that money is channeled to those ideas, institutions, and individuals showing the clearest evidence of being able to do something productive with it.

Given the highly egalitarian, logical, meritocratic nature of the scientific ideology, it comes as no surprise that many scientists accept it as at least a very close approximation to the way

science really is. I'll defer detailed consideration of this point to a later section. At the moment let me just remark that a neutral skeptic would almost certainly raise an eyebrow or two over the rather obvious fact that the conventional ideology focuses entirely upon the *process* of science, leaving aside all considerations of the motives and needs of the scientists themselves. The degree to which this omission casts a cloud over the rosy picture painted above will occupy our attention throughout the book. For now, let's stick to the scheme above and turn the spotlight on the cognitive structure of science, in an attempt to get back to the questions of just what kind of knowledge the process of science is able to offer us about the nature of the world as it is, and whether that kind of knowledge is in some way superior to any other kind.

THE NATURAL PHILOSOPHER'S STONES

The issue before the house for the next couple of sections is consideration of the dual questions:

> Do scientific theories in any sense tell us about the way the world *is?*
>
> Does science have anything like a *method* for creating and/or evaluating theories?

Since all theories must necessarily be expressed in some kind of language (natural, symbolic, mathematical), the first question takes us into the province of the philosophy of language as a tool for representing reality. The second question deals more with science per se, forcing us to confront the natural query "What's so special about science?" In other words, why should we believe that scientific knowledge is any more correct or reliable than any other sort? So our short-term objectives are to explore the question marks in the following diagram:

$$\text{Scientific theory} \overset{?}{\rightleftarrows} \text{Objective reality}$$

$$\uparrow ?$$

$$\text{Scientific methods}$$

To address these two foundational question marks, it will be necessary for us to dip briefly into the work of several twentieth-

grasp when he developed *logos* into "logic" by use of the process of deduction. One of the main uses of myths as outlined above is to provide an explanation of how real-world events work. In everyday speech, an "explanation" is usually taken to be the answer to a question that begins "Why?" Such answers inevitably begin with "Because," and the question and answer together constitute what we generally call a statement of *cause and effect.* Thus, "Why is the sky blue?" is answered with "Because the air molecules absorb all frequencies of visible light except those in the blue part of the spectrum." And "Why does water boil at 100°C (at sea level)?" is answered by "Because at that temperature the thermal motion of the water molecules is able to overcome the external atmospheric pressure"—cause and effect, stimulus-response. The method of logical deduction is Aristotle's theoretical, or some might say mathematical, counterpart to the explanation of physical happenings by cause and effect.

In his *Physics,* Aristotle attempted to combine the purely logical method of deduction with his ideas about the nature of physical reality in order to draw conclusions about the way the world really works. In Aristotle's view physical matter was composed of three things: qualities, form, and spirit. He felt that there was only one kind of matter, which could take many forms, the fundamental forms being air, earth, fire, and water. Because these four fundamental forms were not elements in any sense in which we might understand that term, they could be transformed into each other. To illustrate, this scheme gave rise to what today we might term Aristotle's version of the hydrologic cycle: The Sun's heat changes water into air; heat rises, so the heat in this air pulls the rest of it up to the skies; the heat then leaves the vapor, which becomes progressively more watery again, and this process results in cloud formation. There ensues a positive feedback effect in which the more watery the cloud, the more the water drives away its opposite, the heat. Thus, the cloud gets colder and contracts. The contraction then restores true wateriness to the water, which falls as rain or, if the cloud's heat has now fallen below the freezing point, hail or snow. So we see here the relentless chain of cause and effect being employed to "explain" the observed behavior of water, air, heat, rain, and snow. What's amazing about the whole setup is how all the wrong reasons somehow combine to produce something remarkably close to the way things really do work!

For almost two thousand years Aristotelian logic and physics served as the "science" of the time, explaining various aspects of nature, body, and mind by logical consequences of assumptions of the foregoing type about the nature of matter. Oddly enough, despite Aristotle's main occupation as an observational biologist, the biggest flaw in his entire world picture was that he advocated no experiments or even use of observations to serve as a check on the validity of his underlying premises. Basically, his was an epistemology in which one inferred specific instances (conclusions) from general observations (premises). It was not until the work of Francis Bacon in the seventeenth century that someone had the courage to challenge the authority of Aristotle and suggest turning the situation around, i.e., trying to infer general instances from specific observations.

Bacon's argument was that if one wants to come to grips with the way the world really is, it's necessary to begin the investigation with the facts of life rather than prejudices about what those facts might be. Thus followed the principle of *induction,* whereby conclusions about future events are drawn on the basis of repeated past observations. Such an approach is just what we might come to expect from a man who was not only a philosopher, but also a lawyer who rose to the post of lord chancellor of England before being dismissed for taking a bribe (an indication, perhaps, that the current dubious ethical state of the legal, financial, and political professions are not late-twentieth-century aberrations, after all). In Bacon's view of things, if we observe the Sun rising in the east for fifty consecutive days, then we can predict that it will rise in the east on day 51. And the longer we observe such regular behavior, the more confidently we can speak about its continuation. In a nutshell, this is the method of induction—lots of individual observations eventually resulting in the inductive leap to a general conclusion.

On the one hand, it's satisfying to have a method that takes into account what Nature is actually doing; on the other hand, why should such a procedure provide reliable information about the way things work? On what grounds can I be certain that every time I put water into my ice-cube trays and leave them in the freezer for a few hours I'll soon have ice for my scotch on the rocks? Just because it's always happened this way before, does that give me any assurance that today's drink will have the customary satisfying "clink"? The short answer is that there's

absolutely no justification at all for my concluding that I'll soon be enjoying a scotch on the rocks and not a scotch and water. This is the Problem of Induction: Why should induction work? Why is it a reliable guide to the future?

To illustrate the Problem of Induction, consider the following exchange:

WOMAN: Professor, professor. You must help me. My husband uses an inductive argument to justify the use of inductive arguments.

PROFESSOR HUME: That's terrible. How long has he acted this way?

WOMAN: As long as I can remember.

HUME: Then why didn't you see me sooner?

WOMAN: I would have, but we needed (the conclusions of) the inductive arguments.

HUME: I'm afraid I need them too.

Philosophers beginning with Hume have grappled with this problem, and I'll consider some of their conclusions in the next section. For now we leave it as a gaping hole in the attempt to repair the difficulties in Aristotle by introducing actual observations into the creation of a world view.

Galileo and Newton are the last two supporting actors in our cursory sketch of developments leading up to the modern era of scientific "truth." Galileo was a contemporary of Francis Bacon, and although there appears to be no record of direct contact between the two, there is a clear connection between the idea of Nature as the arbiter of what's what as advocated by Bacon, and Galileo's refinement of the idea by instituting the notion of a *controlled experiment.* In effect, Galileo said that if you have a theory about how some phenomenon works, you must construct an experiment in which all the variables except the one you're interested in are controllable. Then, by fixing the controlled variables, you can measure the variable of interest, thereby checking your theoretical hypothesis against the supreme court of observation. Thus follows the oft-recounted legend (for which there's not a shred of documentary evidence) of his experiment of dropping two different weights from the Leaning Tower of Pisa, and measuring their respective rates of fall as a "laboratory test" of the hypothesis that objects fall at a uniform rate in the absence of air resistance, irrespective of their mass.

Newton added the idea of the description of nature in *mathematical* terms—the keystone in the arch of scientific knowledge whose foundations were laid by Aristotle. More than his remarkable experimental results in optics, mechanics, and chemistry, Newton's legacy as writ large in his *Principia* is the idea of what we would today call the *mathematical model.* Newton showed not only how to "encode" Bacon and Galileo's world of observation into mathematical form, but also invented the method (calculus) for using the mathematical machinery to grind out theorems that could be "decoded" into new implied statements about Nature. The essence of this procedure is depicted in Figure 1.2, where the physical system to be modeled (e.g., the solar system, an electrical circuit, or whatever) is on the left, while the formal mathematical system that represents it appears on the right. Also on the left is our earlier notion of causality, represented as a property of the physical system in which certain parts of the system exert influences "causing" things to happen elsewhere in the system. The term *implication* is used on the right to represent either the process of Aristotelian deduction or that of Baconian induction as the means of proving mathematical statements to be logically correct. These statements are usually called *theorems* and follow from axioms and the above logical rules of inference. The set of implications is the logical counterpart of the physical causality noted on the left side of the diagram. These implied statements are then *interpreted*—i.e., decoded—into assertions about the way the material system really *is.*

With the ideas of deduction, induction, observation, and experiment welded together by the symbolic formalism of mathematics, the stage is now set for a brief account of the alphabet by which modern science tries to inscribe the secrets of nature. The main letters in this alphabet are facts/observations, laws, theories, and models. Let's take a look at what each of these concepts means in the context of modern science.

In Dickens's tale *Hard Times,* the schoolmaster Thomas Gradgrind opens the story with the statement "Now, what I want is, Facts. Teach these boys and girls nothing but Facts. Facts alone are wanted in life. Plant nothing else, and root out everything else. You can only form the minds of reasoning animals upon Facts: nothing else will ever be of any service to them. . . . Stick to Facts, Sir!" While Gradgrind is hardly a role model of the

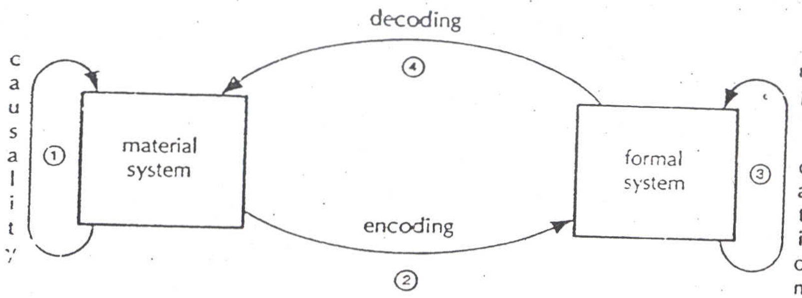

FIGURE 1.2 *Newton's scheme for mathematical modeling*

kindly, scholarly schoolmaster, his view forms the starting point of what many think of as constituting "reality": the world we can see, touch, smell, and hear; the world of Facts. But this commonsense view is only the starting point for a scientific investigation of Nature's scheme of things. As noted earlier, isolated facts are useless curiosities until they are put together with other facts into some kind of pattern. This requires the development of laws.

Suppose we do the following experiment: Take a long cylinder with a movable piston and fill it with gas (e.g., one of the cylinders in the motor of your car). Imagine now that we move the piston to various positions, and for each position measure the pressure that the enclosed gas exerts upon the walls of the cylinder. Further, suppose that after performing many such measurements, we note that whenever the volume of the cylinder is decreased by a certain fraction, the pressure increases by the same fraction; similarly, if we increase the volume by a fraction A by letting the piston rise, we find that the pressure decreases by the same amount A. By an inductive argument, after many repetitions of this experiment we would eventually conjecture (hypothesize) that there is a direct relationship between the pressure and the volume of the gas in the cylinder. Specifically, we would probably assert that the pressure P is inversely proportional to the volume V. And if we were mathematically inclined, we would compactly write this relationship as $PV = k$, where k is a constant determined by the nature of the particular gas and the units of measurement being used. This relationship is an example of what is called an *empirical law*. The law enables us to summarize a large number of individual facts (the results of the individual experiments) in one general statement.

The characteristic properties of laws of the foregoing type are that they:

1. are about *kinds* of events (experiments involving the pressures and volumes of gases in cylinders), not about any singular event (a particular experiment with a particular cylinder using a particular gas);
2. show a *functional* relationship between two or more kinds of events;
3. are supported by a large amount of *experimental data* containing little or no disconfirming evidence;
4. are applicable to *different events* (other types of gases and/or cylinders).

It's important to observe here that there are many different types of laws, not all of which are scientific. The reader might like to try to distinguish among the following in regard to their scientific character: parking regulations, the Ten Commandments, the Law of Conservation of Energy, the Law of the Excluded Middle.

Useful as it is, the above pressure-volume relationship (Boyle's Law) still doesn't tell us *why* an increase in pressure is linked with a decrease in volume. For this we need a *theory* of gases. An explanation for Boyle's Law can be obtained only if we invoke the atomic nature of the gas, and think of it as being composed of a large number of little "billiard balls" randomly moving about, occasionally colliding with each other and with the walls of the cylinder. Newtonian mechanics describes the motion of each such ball, and by combining their individual motions we can in principle calculate the pressure on the container walls by determining how many balls are colliding with the walls at each instant, and the strength of each such collision. With this picture in mind, it's easy to see why when the volume of the cylinder is halved, the pressure doubles. Since the cylinder's surface area has been cut in half, the likelihood that a randomly moving ball will collide with the wall doubles. Newton's laws of mechanical motion in the context of this gas situation form the basis for what is termed the Kinetic Theory of Gases, a framework that enables us to *explain* Boyle's Law.

The characteristic feature of a theory is that it offers a means of relating the laws describing a class of events to a framework and a set of principles described in terms differing from those

used for the laws. Thus, the Kinetic Theory of Gases doesn't make use of the idea of pressure or volume at all, but only the notion of a particle, together with its associated mass and velocity. We obtain an explanation of Boyle's Law by deriving the law from the principles (Newton's laws of motion).

The idea of the gas molecules as little billiard balls flying about inside the cylinder also illustrates the notion of a *model* of a physical situation or, more precisely, a *physical* model as contrasted with a *formal,* or *mathematical,* model. No one takes seriously the idea that the gas molecules really are hard little inelastic spheres, but this turns out to be a very useful picture upon which to let common sense feed in order to generate intuitions about how the physical system will act under various circumstances. The same technique is employed in other types of physical models, as, for instance, in the use of scale models of cars and aircraft in wind tunnels to test for various sorts of aerodynamic properties. In these situations, many aspects of the real car or plane are neglected so that attention can be paid solely to the aerodynamic properties. Similarly, in the gas example many real properties of the gas, like its reactivity, color, temperature, and so forth, are neglected to study its pressure-volume relationship. Facts, laws, models, and theories—such are the tools that the scientist uses to prospect for the gold of reality in the mountainous doings of Nature. Figure 1.3 depicts the interconnections between these landmarks on the terrain of science.

Depending upon your inclination, there are several different philosophical positions that can be taken as to whether the nuggets of reality that turn up in the scientist's prospecting pan are fool's gold or the mother lode. In the philosopher's game, each of these positions is associated with a particular philosophical point of view, or "-ism," the most important for our purposes being:

- *Realism:* Realists believe that there is an objective reality "out there" independent of ourselves. This reality exists solely by virtue of how the world is, and it is in principle discoverable by application of the methods of science. I think it's fair to say that this is the position to which most working scientists subscribe. They believe in the possibility of determining whether or not a theory is indeed *really* true or false. Indicative of this position is the outcome of a straw poll taken recently in a small university department of physics consisting of eleven faculty members, ten of whom claimed that what they were describing with their symbols and equations was objective reality. As one of them remarked, "Otherwise, what's the use?"
- *Instrumentalism:* This school clings to the belief that theories are neither true nor false, but have the status only of instruments or calculating devices for predicting the results of measurements. Basically, this amounts to the belief that the only things that are genuinely real are the results of observations, i.e., Gradgrindian Facts. A typical statement along these lines comes from the engineer Rudolf Kalman, who remarks in the context of mathematical model building: "[Prejudice] means assumptions unrelated to data, independent of data; assumptions which cannot be (or simply are not) checked against the data." In light of the engineer's hunger for any solution that "works," perhaps such an extreme position is acceptable in engineering, but it's hard to see how it can be defended on any other than pragmatic grounds. As we'll see later in the book, the same problem arises at a much deeper level than mere practical engineering when one passes to foundational questions of epistemology in quantum mechanics. There, too, the principal defense of instrumentalism is that "it works."
- *Relativism:* In this increasingly popular position, truth is no longer a relationship between a theory and an independent re-

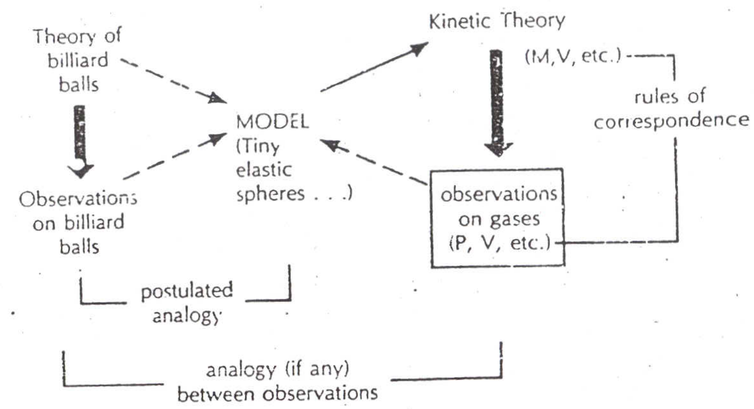

FIGURE 1.3 *Observations, laws, theories, and models*

ality, but rather depends at least in part on something like the social perspective of the person holding the theory. Thus, for a relativist as one passes from age to age, or from society to society, or from theory to theory, what's true changes. In this view it's not what is taken to be true that changes; *au contraire*, what changes is literally truth itself.

So reality is out there, in here, or what your measuring instruments (senses) tell you it is—take your pick! In an attempt to tell us how to weight the odds, philosophers of science have expended inordinate amounts of energy, thought, and heated verbiage in pursuit of the elusive essence of the process of science as a vehicle for unmasking the imposters on the "-ism" list. We can summarize their Herculean task as:

THE FUNDAMENTAL QUESTION OF THE PHILOSOPHY OF SCIENCE

Do scientists proceed as they do because there are objective reasons for doing so, or do we call those procedures *reasonable* merely because a certain group sanctions them?

To dig deeper into the ways science *might* be able to vindicate the creed of the realists and gain a glimpse of their nirvana of objective reality, there's no choice but to step into the twentieth century and look a little harder at the logical structure of science as seen by the philosophers. While most practicing scientists, not to mention laymen, find the discussion of such matters irksome, they are inescapable and cannot be ignored in a work such as this. Besides, as David Hawkins wisely noted, "Those who most ignore, least escape." So with this credo as our battle cry, let's briefly consider what the philosophers have to say about the correlation between the *praxis* and the *theoria* of science and their connection with any kind of objective reality.

RATIONALITY FOR REALISTS

If Plato's Academy in Athens served as the geographical focal point for Greek philosophy and its view of the world, then its twentieth-century counterpart can only be a small seminar room in the Mathematics Department of the University of Vienna,

where a group of physicists, mathematicians, and philosophers met every Thursday evening for several years in the 1920s and 1930s to debate the relationship between the theories of science and objective reality. This group, christened the Vienna Circle in 1929, eventually came to what amounts to the instrumentalist position that the only meaningful statements that can be made are those for which we can give a definite prescription (method, algorithm) for their verification. Thus, use of a word like "yellow" would be equivalent to specifying a procedure for verifying that any particular object possessed the property of being yellow. In this way, the *meaning* or *reality* of "yellow" became equivalent to the statement of the procedure for its verification. This, in essence, forms the basis for the notorious Verification Principle, which lay at the heart of the school of *logical positivism,* the term later given to the philosophy expounded by the Vienna Circle. But to understand this blend of empiricism and logic, it's necessary to go back a few years and look at the work of another Viennese philosopher of the time, Ludwig Wittgenstein.

WITTGENSTEIN, LOGIC, AND LANGUAGE

For ordinary men, the middle of a battlefield with bullets flying and bombs bursting amid cries of human pain and agony is hardly the kind of place in which to engage in contemplative philosophical speculation. But Ludwig Wittgenstein was no ordinary man, and during the course of his valiant service with the Austrian Army during World War I, he developed ideas about the relationship of thoughts expressed in language to the actual state of affairs in the world, ideas that were later enshrined in the pages of his classic work *Tractatus Logico-philosophicus.* The basic tenet of this seminal volume, containing the only ideas of Wittgenstein's published during his lifetime, is that there must be something in common between the structure of a sentence and the structure of the fact that the sentence asserts. In this view, representation of the world in thought is made possible by logic, but the propositions of logic do not in and of themselves represent any actual state of the world. Thus, logic was necessary but not sufficient to describe any kind of objective reality. However, for Wittgenstein logic did reveal which states were theoretically possible, reflecting his underlying belief

that reality was at least consistent—e.g., if the statement "Water boils at 100°C at sea level" is true, then the statement "Water does not boil at 100°C at sea level" cannot also be true.

Wittgenstein illustrated these ideas by what he called a "picture theory" of language, in which he compared logical propositions to pictures. A picture can represent some physical state using certain types of symbols; language can do likewise but with a different set of symbols. The picture bears some relationship to the physical reality that it represents. So, for example, if we see a human face in a photograph, the nose may appear in the center of the face both in physical reality and in the picture. However, if the picture is by Salvador Dali we might find the nose appearing in some quite different location, or not at all. Of course we might try to clarify the relationship between the picture and the object—for example, by introducing color or perspective—but such an attempt at clarification only gives rise to another picture, which itself will require additional analysis. At some stage the essence of the picture has to be understood directly, or we fall into an infinite regress.

In the picture theory of language, propositions making up the language are thought of as analogous to a series of pictures. Furthermore, since Wittgenstein assumes that the logical structure of language mirrors the logical structure of reality, the language "pictures" represent *possible* states of the world. It follows that linguistic statements are meaningful when they can, in principle, be correlated with the world. Actual observation of the world will then tell if they are true or false. To illustrate, we can meaningfully say that "the United Nations is in New York," but it is meaningless to state that "is United the New in York Nations." Of course, different logical rules (grammars) could be developed in which the latter statement is meaningful, but within the context of conventional English grammar it has no logical structure at all. So the main claim of the picture theory—namely, that there must be something in common between the logical structure of the language and the structure of the fact that it asserts—cannot really be "said" in terms of the language being used to make the statement; it can only be "shown." This conclusion gave rise to Wittgenstein's famous metaphor in the penultimate section of the *Tractatus:*

> My propositions serve as elucidations in the following way: anyone who understands me eventually recognizes them as nonsensical, when he has used them—as steps—to climb up beyond them. (He must, so to speak, throw away the ladder after he has climbed up it.) He must transcend these propositions, and then he will see the world aright.

So Wittgenstein's punch line is that the sense of the relationship between reality and its description in language cannot be expressed in language.

Thus ended Wittgenstein's "early period" studies on the interplay of logic, language, and reality. The essence of his ideas can be summarized in the following steps:

1. There is a world that we want to describe.
2. We try to describe it in some language, scientific, mathematical, or otherwise.
3. There is a problem about whether what we say about the world corresponds to the way the world really is.
4. We want to know the true nature of the correspondence between what we say and the way things are, but we can only use language itself to describe that correspondence.
5. Words of a language can never express the desired correspondence, and we must take recourse merely to *showing* it, i.e., using the picture theory, since otherwise we would fall into the infinite regress of descriptions of descriptions of descriptions . . .

At Step 5 we come to one of the most famous statements in all of philosophy, with which Wittgenstein concluded the *Tractatus:* "What we cannot speak about we must pass over in silence."

It's easy to see how Wittgenstein's exploration of the interplay of language, logic, and observation of the world would appeal to the members of the Vienna Circle, with their concerns about constructing a coherent philosophy of science from an amalgamation of logic and empirical epistemology. And indeed the *Tractatus* did serve as a point of departure for many of their deliberations, with several members of the circle in regular contact with Wittgenstein in Vienna, although Wittgenstein himself seems never to have participated in the Thursday night discussions. As an ironic twist, while the Vienna Circle was busy putting together the tenets of logical positivism using Wittgenstein's work as a basis, Wittgenstein himself was in the process of undermining the entire effort by the development of his ideas on the rules of language.

* * *

Remember those old IQ tests where some sequence of numbers is given and you're supposed to pick the "right" continuation of the sequence as a demonstration of your smarts? This kind of problem lies at the heart of what started to bother Wittgenstein about his picture theory of language, ultimately resulting in his repudiation of the entire idea. Consider the following simple example. Suppose the initial sequence is {1, 2, 4, 8} and you're asked, what's the "natural" or "right" continuation. On those absurd high-school IQ and College Board tests, the examiners would probably give full credit only if you answered with the sequence {16, 32, 64, 128}. Presumably this is the "correct" answer because you're supposed to recognize that each term in the original sequence is twice as large as its predecessor. Now there's no doubt that this is one logically defensible reason for guessing that the right continuation is one that extends this pattern. But there can be other continuations that, depending upon the context, would be equally logical and correct. For instance, if the context were the high-school football stadium rather than the examination room, then the most logical continuation might be {1, 2, 4, 8} → {"Who do we appreciate?"}. Or even in the examination room you might think of continuing with {9, 11, 15}, a pattern that reflects the jumps in the original sequence. The point is that in the absence of context, i.e., additional information, there's just no such thing as a "natural" continuation of the sequence. The reader will recognize this situation as just another illustration of the Problem of Induction stated earlier, and it's just this kind of difficulty that began to trouble Wittgenstein after the *Tractatus*.

Following the First World War, Wittgenstein spent time as a high-school teacher in village schools in Austria, where it is rumored he taught some of his pupils about the Liar Paradox ("This sentence is false"). By all accounts he was very popular with the students, but was eventually run out of the village by their parents, most likely on account of his homosexuality and inability to relate to the concerns of the peasant families in the regions where he worked. In any case, during this time he began to become dissatisfied with his picture theory of language, since it gave no clear-cut answer to questions like "Why should we see the principles of logic to be true, even though it's not possible to express the reasons in words?" (Because we can only "show" their truth, not "say" it.) Or "Is there some kind of underlying logical structure either to the world or to our thought systems that somehow can be held responsible for the apparent self-evidence of the propositions of logic?" In other words, is there a set of rules for organizing sense experiences that is fixed within our brains, but that we cannot articulate even though we all follow these rules automatically when we "see" in the same way and when we talk to each other?

In his later work Wittgenstein considered this kind of question, coming to the unhappy conclusion that there could be no underlying logical structure to the world to which our minds must adhere, or vice versa. In the final analysis, he claimed that the propositions of logic reflect the rules of language, and these are known to us by our use of language in everyday life and by linguistic experience. Consequently, Wittgenstein's solution as to why the right continuation of the sequence {1, 2, 4, 8} is {16, 32, 64, 128} and not {"Who do we appreciate?"} is that we know how to go on "in the same way" because we share a form of life. Thus the continuation is dictated by sociological considerations, and bears no contact with any kind of objective reality for number sequences. He then concluded that there are no private rules; rules are the property of a social group. Hence, Wittgenstein gave a "sociological" solution to the Problem of Induction by concerning himself not with how we could be certain *in principle* about the continuation, but rather with how we come to be certain about it *in practice*. The implication of all this for science is that science rests upon a foundation of taken-for-granted reality, a crucial aspect of the relativist school of scientific thought. We'll come back to this relativistic notion of scientific reality later, but for now let's return briefly to the Vienna Circle and its attempts to use the early Wittgenstein to clarify the meaning of language, thereby trying to uncover the "realness" of scientific propositions about the world.

THE LOGICAL POSITIVISTS AND VERIFICATION

In his account of the evolution of knowledge, Auguste Comte identified three stages of development: (1) the *theological*, in which reality is comprehended in terms of the conflicts and creations of gods and spirits; (2) the *metaphysical*, in which there is the use of abstractions and generalities; (3) the *positivistic*,

which relies upon the quantitative description of sensory phenomena. The Vienna Circle was interested in formalizing the last stage by marrying Comte's quantification of empirical observations and data with the logical structure of language and its relationship to the physical world as outlined by Wittgenstein. The result was the philosophy of logical positivism, whose core element was the Verification Principle discussed earlier. For the logical positivists, there were only two sorts of statements or propositions: analytic statements and those that could be empirically verified. Only the latter had meaning, with analytic statements being either tautologies or literally meaningless.

The basic difficulty with the positivist approach is the Problem of Induction: General empirical statements just cannot be verified. For example, if I make the empirical claim that the Sun will rise in the east tomorrow on the grounds that it always has risen there up to now, the Problem of Induction prevents me from offering an empirical procedure for verifying this claim. Consequently, according to the positivist's creed my statement is meaningless, and certainly not scientific. Also, the Verification Principle had difficulties in verifying things like the wave function in quantum mechanics and, in general, failed to make a clear-cut distinction between meaningfulness and meaninglessness, thus coming up empty as a criterion for meaning or reality. As the source of this difficulty is the Problem of Induction, what could be more natural than to try to get around it by the simple expedient of rejecting the use of induction altogether? Enter Karl Popper and the idea of falsification.

POPPER, CONJECTURES, AND REFUTATIONS

Popper, the son of a Viennese lawyer, was originally interested in developing methods for separating scientific statements from pseudoscience. He also took an active part in the discussions of the Vienna Circle, whose members at first thought Popper shared their interest in meaning, a misunderstanding that was soon cleared up. While still a teen-ager, Popper recognized that no amount of supporting data will ever be sufficient to confirm a hypothesis, but all it takes to refute it is one piece of negative evidence. So, for instance, if I hypothesize that all Ferraris are red, no matter how many red Ferraris I see, the Problem of Induction will still prevent me from stating with certainty that

this is the color of *all* Ferraris. However, all I need do is go to the Ferrari factory in Maranello and see that there is even one white car being built, and I can then confidently assert that my original hypothesis is false. This chain of argument constitutes what Popperians call the method of *falsification,* and forms the heart of Popper's view as to how science, as opposed to pseudoscience, is to be carried out. In his own words, "The criterion of the scientific status of a theory is its falsifiability, or refutability, or testability."

Popper is a realist and believes that there is an objective reality out there that science can acquire increasingly accurate information about. His method is conjecture and refutation: We make a hypothesis and then look for evidence to falsify it. For Popper, one theory of a given situation is to be preferred to another if there are more potential observations that can refute the theory than can refute its competitor. In other words, the more statements that could be refuted by direct observation a theory makes, the better the theory is. The classic example is the hypothesis that the Earth's orbit around the Sun is circular, as compared to the hypothesis (theory) that it is an ellipse with the circular orbit as just a special case. Since there are more potential observations that will falsify, or refute, the circular hypothesis, the theory that the orbit is circular would have more empirical content for Popper. To understand clearly the distinction between Popper's views and those of the logical positivists, it's instructive to examine the comparison given in Table 1.1.

While Popper seems to have banished the Problem of Induction from the philosophical banquet table, his conjectures-and-refutations methodology is not without a few flaws of its own. The most difficult obstacle is what is known as the Problem of Auxiliary Hypotheses. To illustrate, let's go back to the red Ferrari problem. If I happen to see a white Ferrari on the road, thereby refuting my original contention, the "red Ferrari" hypothesis can always be resurrected by adding some new background condition to the situation, such as "It wasn't really a Ferrari, but a Lamborghini," or "It was a red car that had only been painted white," and so on. Following this line of attack, any theory in trouble can always be saved by the introduction of suitable auxiliary hypotheses, since it may then be claimed that the original assertion wasn't wrong; the error was in one of the background assumptions.

POSITIVISTS		POPPER
	IDEAS	
	that is	
DESIGNATIONS		STATEMENTS
or TERMS		*or* PROPOSITIONS
or CONCEPTS		*or* THEORIES
	may be formulated in	
WORDS		ASSERTIONS
	which may be	
MEANINGFUL		TRUE
	and their	
MEANING		TRUTH
	may be reduced by way of	
DEFINITIONS		DERIVATIONS
	to that of	
UNDEFINED		PRIMITIVE
CONCEPTS		PROPOSITIONS
The attempt to establish (rather than reduce) by these means their		
MEANING		TRUTH
	leads to an infinite regress	

TABLE 1.1 *Logical positivism versus Popper*

Popper's ideas place great emphasis upon scientific method. He is telling scientists about how they *ought* to behave, neglecting entirely how they actually do behave in practice. The hard facts are that very few scientists, if any, spend much time looking for data or trying to develop experiments that would falsify their hypotheses—just the opposite, in fact. This commonplace observation leads us into consideration of the way social conventions and ideas determine what we take to be scientific truth, a position that Popper himself ultimately came around to acknowledging in connection with his original problem of distinguishing science from pseudoscience. He finally concluded that if we want to know whether or not a theory is scientific, we should look and see how it is handled by people, rather than consider its logical structure—a position remarkably similar to that arrived at by Wittgenstein in his deliberations on many of the same issues.

LAKATOS AND SCIENTIFIC RESEARCH PROGRAMS

An important way station on the road from the purely realist position of the positivists and early Popper to the completely relativistic stance of today's Kuhnians, as discussed in the next section, is the work of the Hungarian educator and philosopher Imré Lakatos. After serving in the anti-Nazi resistance during World War II, Lakatos became a high-ranking official in the Ministry of Education, later fleeing to the West during the Hungarian uprising of 1956. At this time Lakatos went to England, where he began work on his Ph.D. thesis at Cambridge on the theme of mathematical discovery. This novel work, presented in the form of a dialogue centering on the proof of Leonhard Euler's famous formula relating the number of faces, vertices, and edges of a polyhedron, led Lakatos to a deeper interest in the question of the "dynamics" of theories. Thus he went one step further than Popper and the positivists by centering attention not just on the structure of scientific theories, but also upon how they change. The vehicle for this study was what Lakatos termed a *scientific research program* (SRP).

For Lakatos, an SRP is a *sequence* of theories in which certain methodological rules are followed. The primary components of an SRP are:

- *The hard core*—an inviolate cluster of hypotheses at the center of the program
- *The protective belt*—a set of auxiliary hypotheses
- *The negative heuristic*—assumptions underlying the hard core that are not to be questioned
- *The positive heuristic*—a set of suggestions or hints saying how the SRP is to be altered

A good example of the kind of SRP that Lakatos had in mind is the Ptolemaic view of the solar system, in which the Earth sits at the center with the various planets moving about on orbits that are described as complicated epicycles. These curves are just the path traced out by a fixed point on, say, the rim of a coin as you roll it along the top of a flat table. Coins of different

sizes give rise to different epicycles, and Ptolemaic theory used combinations of these curves to describe the planetary orbits. The hard core of the Ptolemaic program is the geocentric hypothesis, together with the necessity of the planetary orbits being given by epicycles. The protective belt consists of the details of the various types of epicycles, while the positive heuristic would consist of a plan for developing increasingly sophisticated models of the planetary system. Note that this positive heuristic is not a vague, general set of principles, but a quite specific set of procedures giving definite advice on how to proceed, including instructions on how to handle anomalies.

On the positive side of the ledger, Lakatos's ideas were an improvement over Popper's since they acknowledged the social dimensions of science. In this sense they served as forerunners to the ideas of Kuhn. Furthermore, the Lakatos vision of what constitutes scientific truth had the virtue of showing that no particular research program is unambiguously to be preferred to any other. In this way, the SRPs opened the door for the anarchical views of Paul Feyerabend, which we'll look at in a moment. Also to his credit, Lakatos discerned two important facts about scientific procedure: (1) scientists have sufficient faith in the hard core that anomalies are explained away, and (2) scientists have general ideas about how one should try to cope with anomalies (the positive heuristic).

As to liabilities of SRPs, there are many, not the least of which is that the choice between two SRPs for Lakatos is no easier than the choice between two theories for Popper. The assessment of which of two programs to prefer eventually comes down to a situation analogous to having Donald Trump and Harry Helmsley tossing pennies off the top of the World Trade Center, the title Grand Real Estate Baron of Manhattan being awarded to the one whose penny lands first; it's a meaningless game without a criterion that they can employ to see who will reign as king of the towers. But there is no operational way for them to decide whose penny lands first without invoking outside agents, i.e., additional information outside the two "programs." Lakatos's SRPs had other drawbacks as well.

There were great difficulties in coming to agreement as to just what constitutes the hard core of an SRP in any specific situation. For instance, Newton's view of planetary motion used the inverse-square law of gravitational attraction as an inviolate hypothesis, i.e., as part of the hard core of Newtonian mechanics.

Yet in considering the motion of the planet Uranus, both George Airy and Friedrich Bessel suggested modifying the inverse-square law to account for the observations, while Urbain Jean-Joseph Leverrier and John Adams suggested keeping the law and explaining the motion by the presence of a hitherto-unobserved celestial body (which turned out to be the planet Neptune). Similarly, before the Theory of Relativity was promulgated in 1905, some suggested modifying the inverse-square law to account for aberrations in the perihelion of the planet Mercury. In fact, the *Encyclopaedia Britannica* (1910 edition) stated that the gravitational law should have the exponent 2.0000001612 instead of 2 to make things come out right! So even in that most solid of scientific bastions, Newtonian mechanics, there were heated disagreements as to what should and should not be in the hard core. A final difficulty for Lakatos is that the idea of the positive heuristic is hopelessly vague. This part of the program is supposed to tell us what to do to modify the program but, in fact, emerges during the course of the research. As a result, it says nothing about what one is supposed to do to carry out an investigation successfully.

Lakatos's vision of the scientific enterprise is far richer than Popper's in that his notion of heuristics directs attention to important aspects of scientific practice not stressed by Popper at all. Nevertheless, the difficulties with his SRPs cast aspersions on the kinds of views of scientific "reality" that can be expected from any such program.

So we see the various attempts by Wittgenstein & Co. to provide a solid, logical foundation, or *method,* for the scientific pursuit of knowledge all come to one bad end or another. Dare we entertain the idea that perhaps there is no method? Well, Paul Feyerabend not only entertains the notion, he insists upon it.

FEYERABEND: THERE AIN'T NO METHOD

In studies of scientific method, there are two principal branches:

A. Rules or techniques to use in the discovery of theories
B. Rules for the objective evaluation of rival theories

The Vienna Circle claimed that only B was the legitimate province of the philosophy of science; Paul Feyerabend denies that there is any valid distinction between the two.

In *Against Method,* his famous manifesto for scientific anarchy, Feyerabend states his basic theme in the following way:

"No set of rules can ever be found to guide the scientist in his choice of theories, and to imagine there is such a set is to impede progress. The only principle that does not impede progress is *anything goes* [italics added]." Feyerabend is claiming that there is no such thing as a scientific method. His argument is that science is just one tradition among many, and is privileged neither in terms of methods nor in terms of results. He goes on to advocate removing science from its pedestal and trying to create a society in which all traditions have equal access to power and education. Among the traditions he suggests giving equal weight with science are astrology, witchcraft, mysticism, and folk medicine! If this all sounds like the grumblings of a failed scientist to you, it's perhaps worth noting that Feyerabend did at one time study physics and astronomy.

Feyerabend was also active in the Berkeley Free Speech Movement, and became interested in the so-called alternative society ideas bandied about in the 1960s. But he eventually redeems himself by confessing that he doesn't have the seriousness of purpose of a true anarchist and would like to be remembered as a "flippant Dadaist."

The central thesis of incommensurability of theories brought out in such stark fashion by Feyerabend takes us from the ideas of realism and the work of Wittgenstein and the Vienna Circle clear across town to relativism and the offbeat ideas of Feyerabend. Despite their shade of lunacy, the visions of Feyerabend contain just enough good sense to suggest there's something worthwhile lurking at their core. This kernel of sense hiding in the flamboyant noise is the notion that there are many methods and ways of coming to scientific truth, and what is taken to be true at any moment is more a matter of social convention in the scientific community than it is a product of logical methods and procedures. Recognition of this startling fact constitutes the theme song for Thomas Kuhn, whose ideas about paradigms in science lie at the heart of what is by far the most talked-about view of the scientific enterprise in the second half of this century.

BUDDY, CAN YOU PARADIGM?

Julian Bigelow, an electrical engineer who helped John von Neumann build the Johnniac computer at the Institute for Advanced Study in Princeton in the early 1950s, tells a story about how when he drove down from Cambridge, Massachusetts, to be interviewed by von Neumann for the job, he met with the great man at his home in Princeton. As the story goes, there was a large dog romping on the lawn, and as von Neumann opened the door to let Bigelow in, the dog ran into the house and started running from room to room, sniffing everything in sight in the manner commonly practiced by dogs everywhere. Busy in their discussion, neither von Neumann nor Bigelow paid much attention to these canine antics for quite awhile, but finally von Neumann's curiosity overcame his courtly Central European manners and he asked Bigelow if he always traveled with his dog. Bigelow replied, "It's not my dog. I thought it was yours." Such are the presuppositions that pervade every aspect of human activity, science (and scientists) being no exception. And it's exactly these kinds of presuppositions that constitute the nucleus of the idea underpinning Thomas Kuhn's notion of a scientific *paradigm*.

In 1947 Kuhn, a young professor at Harvard, was asked to organize a set of lectures on the origins of seventeenth-century mechanics. As preparation, he began tracing the subject back to its roots in Aristotle's *Physics*, being struck time and again by the total and complete wrongheadedness of Aristotle's ideas. As noted earlier, Aristotle held that all matter was composed of spirit, form, and qualities, the qualities being air, earth, fire, and water. Kuhn wondered how such a brilliant and deep thinker, a man who had single-handedly invented the deductive method, could have been so flatly wrong about so many things involving the nature of the physical world. Then, as Kuhn recounts it, one hot summer day the answer came to him in a flash while he was poring over ancient texts in the library: Look at the universe through Aristotle's eyes! Instead of trying to squeeze Aristotle's view of things into a modern framework of atoms, molecules, quantum levels, and so forth, put yourself in Aristotle's position, give yourself the prevailing world view of Aristotle's time, and lo and behold, all will be light. For instance, if you adopt Aristotle's world view, one of the presuppositions is that every body seeks the location where by its nature it belongs. With this presumption, what could be more natural than to think of material bodies as having spirits, so that "heavenly" bodies of airlike quality rise, while the spirit of "earthly" bodies causes them to fall?

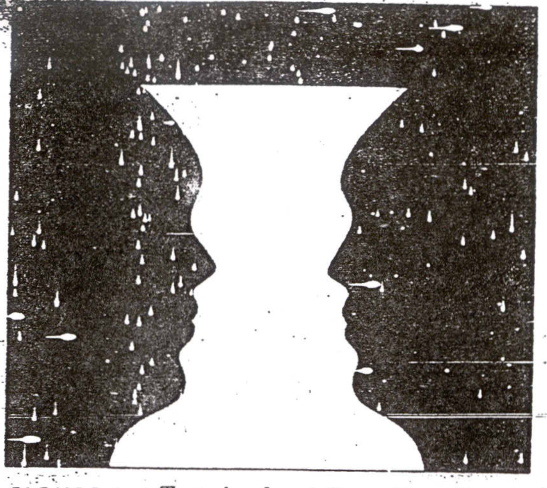

FIGURE 1.4 *Two visual gestalts or "paradigms"*

This stroke of inspiration resulted in Kuhn's developing the idea that every scientist works within a distinctive paradigm, a kind of intellectual gestalt that colors the way Nature is perceived. The situation is vaguely analogous to the picture in Figure 1.4, where one way of looking shows what appears to be two men face to face in profile, while another way shows a flower vase.

According to Kuhn's thesis as presented in his enormously influential 1962 book *The Structure of Scientific Revolutions,* scientists, just like the rest of humanity, carry out their day-to-day affairs within a framework of presuppositions about what constitutes a problem, a solution, and a method. Such a background of shared assumptions makes up a paradigm, and at any given time a particular scientific community will have a prevailing paradigm that shapes and directs work in the field. Since people become so attached to their paradigms, Kuhn claims that scientific revolutions involve bloodshed on the same order of magnitude as that commonly seen in political revolutions, the only difference being that the blood is now intellectual rather than liquid—but no less real! In both cases the argument is that the underlying issues are not rational but emotional, and are settled not by logic, syllogisms, and appeals to reason, but by irrational factors like group affiliation and majority or "mob" rule. As Kuhn states it: "There is no standard higher than the assent of

the relevant community. The transfer of allegiance from one paradigm to another is a conversion experience that cannot be forced." With these ideas in mind, just what constitutes a paradigm anyway, at least as that term is used by Kuhn? The answer is not easy, and Kuhn has come in for plenty of criticism for the vagueness of the notion. But the basic concept can be made clear by the following map-making analogy.

Let's imagine scientific knowledge of the world as being the *terra incognita* of the ancient geographers and map makers. In this context, a paradigm can be thought of as a crude sort of map in which territories are outlined, but not too accurately, with only major landmarks like large rivers, prominent mountains, and the like appearing. From time to time, explorers venture into this ill-defined territory and come back with accounts of native villages, desert regions, minor rivers, and so on, which are then dutifully entered on the map. Often such new information is inconsistent with what was reported from earlier expeditions, so it's periodically necessary to redraw the map totally in accordance with the current best estimate of how things stand in the unknown territory. Furthermore, there is not just one map maker but many, each with a different set of sources and data on the lie of the land. As a result there are a number of competing maps of the same region, and the adventurous explorer has to make a choice of which map he will believe before embarking upon an expedition to the "New World." Generally, the explorer will choose the old, reliable firm of map makers, at least until gossip and reports from the Explorers Society show too many discrepancies between the standard maps and what has actually been observed. As these discrepancies accumulate, eventually the explorers shift their allegiance to a new firm of map makers whose pictures of the territory seem more in line with the reports of the returning adventurers.

This exploration fable gives a fair picture of the birth and death of a scientific paradigm. Kuhn realized that revolutionary changes in science overturning old theories are not in fact the normal process of science, nor do theories start small and grow more and more general as claimed by Bacon, nor can they ever be axiomatized as asserted by Newton. Rather, for most scientists major paradigms are like a pair of spectacles that they put on in order to solve puzzles. Occasionally a paradigm shift takes

place when the spectacles get smashed, and they then put on a new pair that transforms everything into new shapes, sizes, and colors. Once this shift takes place, a new generation of scientists is brought up wearing the new glasses and accepting the new vision of "truth." Through these new glasses, scientists see a whole new set of puzzles to be solved in the process of carrying out what Kuhn called *normal science*.

The paradigms have great practical value for the scientist just as maps have value for the explorer: Without them no one would know where to look or how to plan an experiment (expedition) and collect data. This observation brings out the crucial point that there is no such thing as an "empirical" observation or fact; we always see by interpretation, and the interpretation we use is given by the prevailing paradigm of the moment. In other words, the observations and experiments of science are made on the basis of theories and hypotheses contained within the prevailing paradigm. As Einstein put it, "The theory [read paradigm] tells you what you can observe." According to Kuhn's paradigmatic view of scientific activity, the job of normal science is to fill in the gaps in the map given by the current paradigm, and it's only seldom, and with great difficulty, that the map gets redrawn when the normal scientists (explorers) turn up so much data not fitting into the old map that the map begins to collapse into a morass of inconsistencies. But what happens during these times of paradigm crisis?

Imagine we are at the initial stages of such a crisis, where the old paradigm can't account for certain anomalies, strange observations, and the like. Two new theories emerge, which offer different explanations for these aberrations. These theories represent different maps or sets of spectacles, i.e., different realities. After a period of competition, one of these theories begins to gain the acceptance of the scientific community. The reasons may not be objective at all, but may revolve about matters like simplicity, elegance, the social position of the theory's adherents, government science policies, and so forth. This support leads to experiments that then "corroborate" the theory, and the more evidence that accumulates, the more supporters the theory gathers, especially among the young Turks in the scientific community. Soon "reality" begins to take on the look of the new theory, and scientists universally begin to see and test for certain features of this reality and ignore others.

But what if the community had given its initial support to the other, competing theory? According to Kuhn, in that event "reality" would have taken a quite different turn, and the scientific view of the world would have been seen through that pair of spectacles rather than the first. This means that there is no such thing as scientific "progress," at least not in the sense that one paradigm builds upon its predecessor. Rather, the new paradigm turns in an entirely different direction, and as much knowledge is lost with the abandonment of the old paradigm as is gained from the new. Now we "know" a *different* universe.

If Kuhn's thesis is true, then it also destroys one of the main pillars of the scientific method, since the whole idea of a scientific experiment rests upon the assumption that the observer can be essentially separate from the experimental apparatus that tests the theory. Kuhn contends that the observer, his theory, and his equipment are all essentially an expression of a point of view, and the results of the experimental test must be an expression of that point of view as well. This position effectively asserts that science is not objective, but at the same time we know that science is not totally subjective either, since paradigms are eventually overthrown. So we're back to consideration of the central question: What is the relationship of the scientist to the universe he observes?

The most revolutionary aspect of Kuhn's claims is that they entirely omit things like knowledge, truth, and external reality. In fact, Kuhn states that in science truth is an entirely optional and gratuitous concept. As he puts it, "Does it really help to imagine that there is some one full, objective, true account of nature and that the proper measure of scientific achievement is the extent to which it brings us closer to that ultimate goal?" I think most practicing scientists would say that yes, such a belief helps a hell of a lot! But apparently Kuhn doesn't think so, since he says that there's no way for science to get hold of the "truth" anyway, so you can't measure scientific progress as getting closer to the way things are in themselves. Returning to the map-making analogy, Kuhn's claim is tantamount to the belief that not only are there many map makers, each emphasizing different aspects of the territory, but that it is in principle impossible ever to produce a complete map of the entire region. So you can't judge a map by how close it comes to this ideal Platonic map, since such a map is literally undrawable. In some ways this line of argument is reminiscent of Wittgenstein's claim that lan-

guage cannot describe the intrinsic logical structure of the world.

Just like the revolutions they describe, Kuhn's arguments were met with fierce opposition from the philosophical community, although he was a minor saint to humanists since he seemed to be putting the human being back into the scientific enterprise. One of Kuhn's sharpest critics has been the philosopher Dudley Shapere, who complained that Kuhn was a relativist denying the objectivity and rationality of science. Shapere felt that science according to Kuhn is nothing more than a series of fads dressed up to look presentable, and offered the counterargument that even though we may be wearing rose-colored glasses, there's still a lot that shines through unaffected. The colors may be skewed, but other qualities like shape, size, and texture come through loud and clear. In short, the glasses may distort our view of reality but they don't create it—a staunch realist position.

Another criticism of Kuhn's ideas is that he places too little emphasis upon the social determinants of scientific revolutions. On the one hand, Kuhn argues that a paradigm shift takes place when there's an accumulation of anomalies; on the other hand, he says an anomaly can be ignored to preserve the paradigm. Question: At what point does a mass of discrepancies become irritating enough to bring about a paradigm shift? Kuhn offers little help in addressing this dilemma.

While Kuhn denies the label of an "irrationalist," he does assert that there are no methods or methodological rules for creating or evaluating scientific theories. His argument is that only propagandizing plays a role in changing allegiances from one paradigm to another. What makes reasons for theory change "good" is that they are generally accepted by the community, and if you want to be a member of that community it behooves you to operate within the framework of this system of reasons. As an immediate consequence, we find Kuhn's statement that rival paradigms cannot really be compared, although he does offer what we might term a Fivefold Way for characterizing the features of a good theory. Kuhn's way consists of the following points stating that a good theory must be

- *Accurate:* Consequences of the theory should be in agreement with experiment.

- *Consistent:* The theory should contain no internal contradictions and, moreover, it should be consistent with currently accepted theories applicable to related aspects of Nature.
- *Broad:* The scope of the theory's consequences should extend beyond the particular observations, laws, or subtheories that it was created to explain.
- *Simple:* It should bring order to phenomena that without it would be individually isolated.
- *Fruitful:* The theory should disclose new phenomena or previously unobserved relationships.

Kuhn's claim is that these criteria offer the shared basis for theory choice, but that there is no possible way of giving a justification for this selection of criteria.

To compare Kuhn with Feyerabend, Kuhn says there are rules (the Fivefold Way) for theory choice, but their application may be problematic and they cannot be given objective justification. Feyerabend says there are no rules whatsoever but, like Kuhn, rests much of his case on the existence of incommensurable theories.

We can also compare Kuhn with Popper and Lakatos by noting that, roughly speaking,

Paradigm = Hard core + Positive heuristic

enabling us to connect Lakatos's SRPs to the notion of a paradigm. As far as Popper is concerned, his central themes of conjecture, test, refutation, are also present in Kuhn's world, but only during the course of practicing normal science. Popper's contention that there is no rationale for the introduction of new conjectures in science, but only for the exposure of such conjectures to falsifying tests, is basically similar to Kuhn's claim that there is no rationale for the introduction of a new paradigm, but only for the attempt to "articulate" the paradigm and make it deal successfully with anomalies. The point of divergence between Kuhn and Popper arises when it comes time to shift from one paradigm to another. Popper believes this can and should (and is) done rationally, logically, and with little fuss; Kuhn says this method may be fine in the abstract, but *real* science just doesn't work that way.

With Kuhn we have come to the end of the line as far as contemporary views on the ways science operates both to form and

to validate its view of the world. Since the path from Wittgenstein to Kuhn has been a complicated one filled with lots of switchbacks and strange meanderings, in the next section I'll try to summarize the competing positions as well as briefly reexamine our original question: How real is scientific reality?

PHILOSOPHICALLY SPEAKING

When embarking upon this whirlwind tour of twentieth-century philosophy of science, our point of departure was to explore the two basic issues: What is the connection between scientific theories (language) and objective reality, and does science have any special sort of procedures or methods for either generating new theories or evaluating competing ones? Note again here the important point that when we use the term *method* in this setting, we're referring to a method for generating theories and not to the more common concept of the "scientific method" as constituting the potentially infinite sequence hypothesis → experiment → hypothesis . . . These questions led us to divide beliefs on the nature of reality into three categories:

- *Realism* = Objective reality exists.
- *Instrumentalism* = Reality is the readings noted on measuring instruments.
- *Relativism* = Reality is what the community says it is.

We also saw that beliefs as to whether or not there's method in the madness of science determine one's position as a rationalist or an irrationalist, with rationalists believing in method, irrationalists not. The various philosophers and philosophical schools took differing views on these matters, and to expound them occupied a lot more time and space than I'd intended, but necessarily so. Consequently, before going on to consider what the practicing scientists themselves, as well as competing ideologies, have to say about these matters, I have tried to summarize the story so far in Table 1.2. As the table shows, the overwhelming conclusion of the philosophers is that, as Einstein said, "it's all relative." But we saw earlier that ten out of eleven everyday physicists supported the idea of an objective reality "out there" that their equations were describing. To address this paradox, let's quickly hear from the laboratory instead of the ivory tower

SCHOOL	REALITY BELIEF	METHOD	ARGUMENT
Wittgenstein I	realism	rationalist	picture language
Wittgenstein II	relativism	irrationalist	language rules
logical positivists	instrumentalism	rationalist	verification principle
Popper	realism	rationalist	falsification
Lakatos	relativism	rationalist	SRPs
Feyerabend	relativism	irrationalist	"anything goes"
Kuhn	relativism	rationalist	paradigms

TABLE 1.2 *The battle of the philosopher kings*

and listen to what the players rather than the Monday morning quarterbacks have to say about the whole business.

In 1979 the Institute for Advanced Study in Princeton held a celebration to honor the one hundredth anniversary of the birth of Einstein, the institute's first and most celebrated resident genius. To plan for this celebration, a committee was formed at the institute to arrange a program and invite scholars from around the world to participate. Just as Caesar divided all Gaul into three parts, the IAS committee decided to organize the Einstein centennial similarly, focusing on Einstein's science, the historical genesis of his ideas, and, finally, the philosophical impact of his work. As Freeman Dyson tells it, the committee solicited names and put together lists of scholars who could be invited in each of the three areas. The committee was personally acquainted with almost everyone on the list of scientists. As to the historians, the committee didn't know them personally but at least had heard of most of them and knew of their work. But when it came to the philosophers of science, Dyson remarks that the committee was not only unfamiliar with them personally, but had never even heard the names of most of them! More than any abstract argument could ever hope to show, this little episode conveys the level of contact between the activities of the working scientist and the arguments of the philosopher: It is exactly zero! In Dyson's words, "There's a whole culture of philosophy out there somewhere with which we have no contacts at all. . . . there's really little contact between what we call science and what these philosophers of science are doing—whatever that is."

Dyson's observation serves to unravel the contradiction noted

a moment ago between the beliefs of scientists and those of philosophers. As far as most practicing scientists are concerned, there's nothing more dangerous than a philosopher in the grip of a theory. In fact, there appears to be something of an unrequited love affair between the scientists and philosophers, in which the scientists by and large spend their days ignoring the attempts by the philosophers to press their attentions upon them. As an indicator of the state of affairs, the physicist Murray Gell-Mann at all times carries with him a doctor's prescription forbidding him to argue with philosophers on the grounds that it could be dangerous to his health!

So we come to the perhaps not so surprising conclusion that if you want to know about how scientists really think and work, you'll get no help from a philosopher of science. However, if your concerns go beyond what scientists do and encompass the broader issues of the *significance* of what they do and its relationship to other knowledge-generating mechanisms, then, as noted before, a consideration of matters philosophical is unavoidable. Most of our stories in this volume center upon what scientists are really doing, but in each one of them there is a strong undercurrent of philosophical presupposition conditioning the interpretation of the results. The reader should try to keep these deeper issues in mind as we go along, as a guide to evaluating the myriad competing arguments.

While philosophical factors probably are honored more in the breach than in the practice of science, sociological pressures are another matter. Science is not yet done by impersonal, uninvolved machines, but by real, live, thinking and feeling human beings, and it's impossible for this fact not to have some impact upon the way science proceeds to its conclusions about the way the universe functions. Let's take a few pages to consider the sociology of science rather than its philosophy, as another avenue to walk down on our way to learning about the way science comes to what it sees as "truth."

A TALE OF TWO SUICIDES

Ludwig Boltzmann and Paul Kammerer were both professors at the University of Vienna in the early part of this century; they were both popular with their students and held in great esteem

by their colleagues; and they both committed suicide. While perhaps extreme in the outcome, these two cases serve as examples of one aspect of the way scientific truth is determined at least as much by the social climate of the times as by the dictates of reason and logic alone.

Boltzmann, a physicist, is perhaps best remembered for his work in thermodynamics and the connections he discovered between the theory of heat and the more general issues of randomness and order. He is today credited with having introduced the notion of *entropy* as a measure of the disorder present in a collection of objects of any sort, an idea that later served as the basis for the theory of information, which turned out to be so crucial to the development of modern communications technology. In fact the formula $S = k \log W$, expressing the entropy S as being proportional to the logarithm of W, the number of possible states that a system can assume, is engraved on Boltzmann's tombstone in Vienna's Zentralfriedhof, a fitting memorial to the importance of this fundamental idea. In this expression, the constant of proportionality k is even today termed *Boltzmann's constant* in recognition of this magnificent achievement. But at the time he was carrying out this pioneering work, the achievement was anything but magnificent, at least if one was listening to the leading scientists of the day.

Boltzmann's problem was that his theory of heat involved an assemblage of atoms moving according to the usual rules of Newtonian mechanics. He used this concept of an atom as a particle of matter to construct his theory of heat as a statistical property emerging out of the overall motion of these atoms. Note that this idea was put forth around the turn of the century, several years before the work of Ernest Rutherford, J. J. Thomson, and Niels Bohr gave the concept of an atom its modern birth. As a result of his atomistic speculations, Boltzmann came into heated conflict with several of the giants of the scientific community, most notably his Viennese colleague Ernst Mach and the German physical chemist Wilhelm Ostwald, who argued forcefully against the idea of the atom. Ostwald, in particular, preferred a theory of heat based upon the notion of energy rather than matter. Depressed by the acrimony of this opposition, as well as his failing eyesight and what he thought of as the decline of his mental faculties, Boltzmann took his life in Duino, Italy, on September 5, 1906.

Tragically, Boltzmann's suicide took place almost coterminously with the work by Thomson and Rutherford in Britain that would lead to a complete vindication of his ideas. So here we have a textbook illustration of how the social climate of the scientific community, as well as the influence of two great men, acted to delay introduction of what ended up being a major contribution to our way of thinking about the way the world works. Now let's move the clock forward almost exactly twenty years and examine the case of another Viennese professor as illustration of how these same social forces can work to rid science of equally controversial, but this time erroneous, ideas.

Paul Kammerer was a professor of biology at the University of Vienna in the 1920s. Accounts credit him with an almost magical skill at breeding amphibians and other types of animals. They also note that he was an ardent socialist and crusader for the political causes of what today we would term the liberal left. Given this combination of scientific and political leanings, it's perhaps not surprising that Kammerer supported the idea that acquired characteristics can be pass on to offspring, i.e., Lamarckian inheritance. For ideologues bent upon improving the human race, the idea that behavioral traits like learning, altruism, and the like can be acquired holds great appeal. So it was for Kammerer, too, and he set out to prove the idea with his now infamous experiments on the midwife toads.

Generally these toads breed on land, with the male lacking the so-called nuptial pads of the male members of other species of toads that breed in the water. These pads are rough patches on the hands of the male that he uses to grab on to the back of the slippery female during the course of mating in water. Kammerer's experiment involved forcing the midwife toad to breed in water for several generations, his claimed results being that such toads then developed the nuptial pads characteristic of their naturally water-breeding cousins. The supporters of Kammerer focused upon this experiment as clear-cut evidence for Lamarckism; opponents remained highly doubtful and requested a closer look at the evidence.

These experiments with the midwife toad came under heavy attack from naturalists in both Europe and America, especially William Bateson in England and Kingsley Noble in New York. On a visit to Vienna in 1923, Bateson saw Kammerer's last remaining specimen of a midwife toad with nuptial pads and later asked to reexamine it in his own lab. Kammerer replied that it could not be sent from Vienna. At the same time, Noble was having doubts about some of the particulars of the physical structure of Kammerer's claimed nuptial pads, and visited Vienna in 1926 to examine the last specimen personally. His results, published later that year in *Nature,* claimed that the so-called pads were nothing more than black markings made with India ink.

At the time of Noble's report, Kammerer was preparing to leave Vienna for a position at Moscow University as head of a new laboratory in Lamarckian biology. Noble's *Nature* article appeared on August 7, 1926. In a letter of September 22 to the Soviet Academy of Sciences, Kammerer wrote that he had examined Noble's claims and found them to be totally accurate. He went on to protest his ignorance of how the inking had been done, but acknowledged that his experimental conclusions about Lamarckism were baseless. After withdrawing from the post in Moscow, the letter concluded with the poignant statement "I am not in a position to endure this wrecking of my life's work, and I hope that I shall gather together enough courage and strength to put an end of my wrecked life tomorrow." And, in fact, during a walk in the Wienerwald the next day, Kammerer shot himself in the head. This was another extreme example of scientific peer-group pressure and its sometimes tragic effect upon the lives of scientists deviating from the group norms. Only this time the pressure acted to discredit wrong results rather than to suppress correct ones.

The tales of these two Viennese professors serve to underscore the sometimes dramatic influence that the social component of science plays in establishing what we take to be the scientific "truth" of the moment. These social factors operate within the scientific community itself as well as in the outside world, shaping not only the way scientific activity is carried out but also the manner in which certain ideas, like Boltzmann's, are buried while others thrive. One of the pioneers in studying these social determinants, at least inside science itself, is the sociologist of science Robert K. Merton, who in 1942 identified a small set of what he termed *norms* characterizing the scientific enterprise. Roughly speaking, in modern terms we can give Merton's norms as:

- *Originality:* Scientific results should always be original, i.e., novel. Studies that add nothing new to what is already known are not part of science.
- *Detachment:* Scientists undertake their work with no motive other than the advancement of knowledge. They should have no personal axes to grind insofar as the results of their work go, and they should have no psychological commitment to a particular point of view. The impersonal style of most scientific communications is a direct consequence of this norm.
- *Universality:* Claims and arguments should be given weight according to their intrinsic merits alone, and should not depend upon religious, social, ethnic, or personal factors surrounding the individuals who make them. In short, there are no privileged sources of scientific knowledge.
- *Skepticism:* No scientific statements of fact should be taken on faith. All claims should be carefully scrutinized for invalid arguments and errors of fact, and any such mistakes should be made public immediately. To put it simply, scientists should trust no one, at least not when it comes to claims of scientific truth.
- *Public accessibility:* All scientific knowledge should be freely available to anyone. Thus, results of research are not the private property of the scientist, but are public goods that should be transmitted immediately to the community of science. This norm lies at the heart of debates as to whether or not engaging in classified military research is scientifically ethical.

Anyone involved with the way scientific practice actually works will immediately recognize that these prescriptions are violated every day of the week in both trivial and not so trivial ways, serving the same role in science that general laws serve for society at large. There's nothing particularly disturbing about this gap between theory and practice, just as the fact that human beings jaywalk, rob banks, and drive their cars too fast is not really news either. What is disturbing, to some anyway, is what appears to be an increasing incidence of such violations of the spirit of science, at least as it's embodied in these norms. Such an increased pace of corner cutting in science seems especially evident in the last decade or so, certainly aided and abetted by science's Faustian bargain with government funding agencies. Nevertheless, the Mertonian norms are still the ethos to

which the community of scientists subscribes, and form the heart of the code by which the behavior of most scientists is judged by their peers. And in exactly this way the norms make their contribution to the way scientists think, hence to what they ultimately come to accept as the way things are. But these factors working inside the scientific community are not the only social components influencing the work of science. Of equal importance are the forces affecting science from the outside, especially in today's mass-media-saturated and cash-hungry world.

In his 1971 State of the Union address, President Richard M. Nixon declared that the time had come for the country to wage war on cancer, with the "same kind of concentrated effort that split the atom and took man to the Moon. . . ." This pronouncement led to an avalanche of money pouring into the nation's cancer research laboratories, and resulted not only in a war on cancer but also in a war among the various research establishments for a generous hunk of the federal government's cancer war chest. One of the foot soldiers in both of these conflicts was William T. Summerlin, a young skin specialist at the prestigious Sloan-Kettering Institute for Cancer Research in New York City.

Amid the high-pressure political climate surrounding cancer research and the feverish hustling and grantsmanship, in March 1973 Summerlin applied for a five-year federal research grant from the American Cancer Society to pursue his special interest in skin grafts and immunology. In particular, Summerlin felt that he was on the track of developing procedures whereby skin treated by his technique could be transplanted without rejection. Thinking that a little favorable publicity never hurt the case of a relatively obscure, but ambitious, young researcher, Summerlin presented an outline of his work in progress at a science writers' convention. The results were predictable: a three-column headline the next day in *The New York Times* declaring LAB DISCOVERY MAY AID TRANSPLANTS. Summerlin was on his way, or so it seemed.

During the course of the next year, while Summerlin traveled the country presenting seminars and lectures on his work, colleagues were finding it increasingly difficult to confirm his results by independent experiments. In fact, even workers in

Summerlin's own laboratory at Sloan-Kettering were unable to reproduce the claimed properties of the specially treated "Summerlin skin," leading to a showdown between Summerlin and Sloan-Kettering Director Dr. Robert A. Good in March 1974. On his way to this fateful meeting, Summerlin pulled out a black felt-tip pen and hurriedly inked in some dark patches on the white mice he was bringing as evidence for his claims. At the time Good didn't notice the Summerlin embellishments, and it was only when the mice were returned to the lab assistant that Summerlin's "help" was discovered. The assistant immediately reported the matter to his boss, at which point Summerlin was instantly suspended. While he denyed any wrongdoing, asserting that he had inked in the skin grafts on the mice only to make them more easily identifiable, Summerlin's credibility was shattered by the incident, along with the credibility of his supposed technique for skin grafts.

Interestingly enough, the Summerlin episode bears some strange similarities to that of Kammerer and the midwife toads, although without the same tragic suicidal ending. The point in raising these cases here is not so much the issue of whether or not Kammerer or Summerlin was really guilty of fraud, but rather to illustrate the degree to which forces outside the world of science, in this case the federal research-funding establishment and the public at large, contribute to creating a climate that can drive scientists to manufacture and/or artificially enhance what they claim are "the facts." And money is not the only such pressure. Political considerations, especially those involving what is often termed "human nature," can and do play a dramatic role in influencing what's scientifically "right." A good illustration of this kind of effect was the controversy over social Darwinism in the first half of the century, a debate about which we shall have much more to say later when we consider its modern incarnation: the Sociobiology Problem. In this context, it may even be safe to say that the real issue is the conflict between the norms of science, as exemplified by Merton's list, and the "norms" of politics as encoded in the ideologies of certain political movements (in the case of sociobiology, Marxism).

The foregoing stories barely scratch the surface of the many ways in which sociological considerations shape what science thinks of as being true, with many far more detailed accounts noted under "To Dig Deeper" in this volume. For our purposes

here, the main consideration is the manner in which these social factors influence the way science validates its claims and comes to a consensus on a given issue. The heart of the difficulty is that knowledge is underdetermined. Thus, there are always many different theories, each of which can give a plausible account of the available facts. So how are we to choose one and let the others go? The basic problem is encapsulated in the remark of the philosopher Willard Van Orman Quine, who noted that "any statement can be held true, come what may, if we make drastic enough adjustments elsewhere in the system." One natural place to make these drastic adjustments is in the cultural background to the problem, thereby creating a climate in which only one or at most a few of the contending theories can survive. Again, we will see ample evidence of this kind of "cultural imperialism" in the raging sociobiology debate covered in Chapter Three.

As to arrival at a consensus, the key factor is the Mertonian norm relating to the public character of scientific knowledge. The rule that scientific information is communicated explicitly and unambiguously influences both the form and the content of knowledge that is labeled "scientific." For example, this norm goes a long way toward accounting for why experimental verification involving neutral instrumentation occupies such a hallowed position in science, as well as the great value attached to quantitative observation and expression of results in mathematical form. All of these features contribute to the public accessibility of the information and the reproducibility of the results, at least in principle. One need only consider other fields like literature or the arts, where such a norm is not the norm, to see some of the ways in which scientific knowledge differs in significant ways from these other forms of reality representation.

Since we'll see many concrete instances of these sociological factors entering into the stories that follow, there's no need to belabor the point here in the abstract. For now, it's of somewhat more interest to look at some of the knowledge-generating devices that make some pretense to a degree of scientific character, in their goals if not their methods. With the above ideas as prelude, the reader should be in a better position to distinguish those groups doing what we would now term science from those practicing at the fringe.

We began this chapter with the dual stories of Jocelyn Bell and Immanuel Velikovsky, noting their positions at opposite

ends of the spectrum of what's currently held to be "good science." We are finally in a position to give the long answer to the question posed earlier about why Velikovsky's work has been relegated to the dustbin of pseudoscience, while Bell's was rewarded with the Nobel Prize for physics (although not to her).

ON THE FRINGE OR AT THE CUTTING EDGE?

As editor of a scientific journal, I'm regularly faced with the unpleasant task of telling potential contributors that their papers are not suitable for publication. Generally the reasons are the usual ones: trivial or nonexistent results, poor writing, work outside the scope of the journal, and so on. However, occasionally I get a paper that I don't even bother to send out for the customary refereeing process, rejecting it out of hand. Such papers are the bane of the editor of almost every scientific publication, and every editor soon becomes sensitized to their telltale aroma of nonsense masquerading as science. Since my own journal is devoted to mathematics, papers of this sort tend to involve such well-known impossibilities as squaring the circle, trisecting an angle, and doubling the cube, although they occasionally address famous outstanding problems like Fermat's Last Theorem or the Riemann Hypothesis (in which case I'm compelled to look at them seriously, even though there's not yet been one that was correct). Luckily for me, mathematics is an area where it's difficult to try to dress up such pseudoscience in respectable clothes and not have it show. Certainly my colleagues in biology, medicine, and the social sciences must have it much worse in this regard. But just what is it about this kind of paper that immediately stamps it as pseudoscience to the trained (and jaundiced) scientific eye? To answer this puzzling query, let's briefly recall what's been learned so far about the actual practice of science in today's world.

Our deliberations up to now allow us to summarize compactly the *practice* as opposed to the *philosophy* of science in the following two principles:

A. There is an ideology of science consisting of a *cognitive structure* (facts → hypothesis → experiment → laws → theory), together with the processes of *verification* and *peer review*.

B. Science is a *social activity*, with the standards for what constitutes good science determined by the norms of a particular community.

With these facts of modern scientific life in mind, let me now offer a short checklist of "sights and sounds" (and smells) for detecting pseudoscience. If you're reading a paper and catch the whiff of even one of the items on this list, be assured that the author is dealing in pseudoscience, at least by the standards prevailing in today's world of science. For the following list I am indebted to the outstanding work *Science and Unreason* by Michael and Daisie Radner, to which I direct the reader's attention for a far more extensive account of the whole culture of pseudoscience and pseudoscientists.

HALLMARKS OF PSEUDOSCIENCE

- *Anachronistic thinking:* Cranks and pseudoscientists often revert to outmoded theories that were discarded by the scientific community years, or even centuries, ago as being inadequate. This is in contrast to the usual notion of crackpot theories as being novel, original, offbeat, daring, and imaginative. Good examples of this kind of crankishness are the creationists, who link their objections to evolution to catastrophism, claiming that geological evidence supports the catastrophic rather than uniformitarian view of the kind of geological activity they associate with evolution. The argument is anachronistic insofar as it presents the uniformitarianism-catastrophism dichotomy as if it were still a live debate.
- *Seeking mysteries:* Scientists do not set out in their work to look for anomalies. Max Planck wasn't looking for trouble when he carried out his radiation emission experiments and Michelson and Morley certainly were not expecting problems when they devised their experiment to test for the luminiferous ether. Furthermore, scientists do not reject one theory in favor of another solely because the new theory explains the anomalous event. On the other hand, there's an entire school of pseudoscience devoted to enigmas and mysteries, be they the Bermuda Triangle, UFOs, yetis, spontaneous combustion, or other even more offbeat phenomena. The basic principle underlying such searches seems to be that "there are more things in heaven and earth than are dreamt of in your philosophy," cou-

[illegible] the methodological principle that anything that *can* be seen as a mystery *ought* to be seen as one.

- *Appeals to myths:* Cranks often use the following pattern of reasoning: Start with a myth from ancient times and take it as an account of actual occurrences; devise a hypothesis that explains the events by postulating conditions that obtained at that time but that no longer hold; consider the myth as providing evidence for support of the hypothesis; argue that the hypothesis is *confirmed* by the myth as well as by geological, paleontological, or archaeological evidence. This is a pattern of circular reasoning that is absent from the blackboards and laboratories of science.
- *A Casual approach to evidence:* Pseudoscientists often have the attitude that sheer quantity of evidence makes up for any deficiency in the quality of the individual pieces. Further, pseudoscientists are loath ever to weed out their evidence, and even when an experiment or study has been shown to be questionable, it is never dropped from the list of confirming evidence.
- *Irrefutable hypotheses:* Given any hypothesis, we can always ask what it would take to produce evidence against it. If nothing conceivable could speak against the hypothesis, then it has no claim to be labeled scientific. Pseudoscience is riddled with hypotheses of this sort. The prime example of such a hypothesis is creationism; it's just plain not possible to falsify the creationist model of the world, as we'll see in the next chapter.
- *Spurious similarities:* Cranks often argue that the principles that underlie their theories are already part of legitimate science, and see themselves not so much as revolutionaries but more as the poor cousins of science. For example, the study of biorhythms tries to piggyback upon legitimate studies carried out on circadian rhythms and other chemical and electrical oscillators known to be present in the human body. The basic pseudoscience claim in this area is that there is a similarity between the views of the biorhythm theorists and those of the biological researchers, and therefore biorhythms are consistent with current biological thought.
- *Explanation by scenario:* It's commonplace in science to offer scenarios for explanation of certain phenomena, such as the origin of life or the extinction of the dinosaurs, when we don't have enough data to reconstruct the exact circumstances of the process. However, in science such scenarios must be consistent with known laws and principles, at least implicitly. Pseudoscience engages in explanation by scenario *alone,* i.e., by mere scenario without proper backing from known laws and theories. A prime offender in this regard is the work of Velikovsky, who states that Venus's near collision with the Earth caused the Earth to flip over and reverse its magnetic poles. Velikovsky offers no mechanism by which this cosmic event could have taken place, and the basic principle of deducing consequences from general principles is totally ignored in his "explanation" of such phenomena.

- *Research by literary interpretation:* Pseudoscientists frequently reveal themselves by their handling of the scientific literature. They regard any statement by any scientist as being open to interpretation, just as in literature and the arts, and such statements can then be used against other scientists. They focus upon the words, not on the underlying facts and reasons for the statements that appear in the scientific literature. In this regard, the pseudoscientists act like lawyers gathering precedents and using these as arguments, rather than attending to what has actually been communicated.
- *Refusal to revise:* Cranks and crackpots pride themselves on never having been shown to be wrong. It's for this reason that the experienced scientific hand never, under any circumstances, enters into dialogue with a pseudoscientist. But immunity to criticism is no proof of success in science, for there are many ways to fend off attacks: Write only vacuous material replete with tautologies; make sure your statements are so vague that criticism can never get a foothold; simply refuse to acknowledge whatever criticism you do receive. A variant of this last ploy is a favorite technique of pseudoscientists: They always reply to criticism, but never revise their position in light of it. They see scientific debate not as a mechanism for scientific progress but as an exercise in rhetorical combat. Again the creationists serve as sterling testimony to the power of this principle.

The major defense of pseudoscience is summed up in the statement "Anything is possible," the pseudoscientific version of Feyerabend's philosophical theme song "Anything Goes." Earlier we considered the question of competition between models

and theories and drew up a few ground rules by which the competition is generally carried out in legitimate scientific circles. Let's look at how pseudoscientists, with their "Anything is possible" shield, enter into such competition.

In the competition among theories, the pseudoscientist makes the following claim: "Our theories ought to be allowed into the competition because they may become available alternatives in the future. Scientists have been known to change their minds on the matter of what is and is not impossible, and they are likely to do so again. So who's to say what tomorrow's available alternatives may be?" In other words, anything is possible! The fact that a theory may become an available alternative in the future does not constitute a reason for entering it in the competition today. Every competitor now must be an available alternative now. The pseudoscientist suggests that we may as well throw away the current scientific framework since it will eventually have to be replaced anyhow.

By referring to a future but as-yet-unknown state of science, the cranks are in effect refusing to participate in the competition. This would be all right if they didn't at the same time insist on entering the race. It's as if one entered the Monaco Grand Prix with a jet-propelled car and insisted on being allowed to compete because, after all, someday the rules may be changed to make it a jet car race!

The pseudoscientists also worm their way into the competition by putting the burden of proof on the other side. They declare that it's up to the scientific community to prove their theory wrong, and that the theory must be taken seriously if the community cannot do so. The obvious logical flaw is the assumption that failing to prove a theory impossible is the same thing as proving it possible. While the principle of innocent till proven guilty may be used in Anglo-Saxon courts of law, scientific debate is not such a court. The reason why pseudoscientists think they can put the burden of proof on the scientists can be traced to a mistaken notion of what constitutes a legitimate entry in the debate. They think that the scientific method places a duty on the scientific community to consider *all* proposed ideas that are not logically self-contradictory. In their view, to ignore any idea is to be prejudiced.

Finally, we note that the pseudoscientists often act as if the arguments supporting their theory were peripheral to the theory. Science is defined in terms of *how* and *why* we know something, not *what* we know. Thus, the pseudoscientists fail to see that what makes a theory a serious contender is not just the theory, but the theory plus the arguments that support it. Cranks think that somehow the theory stands on its own, and that the only measure of its merit for entering the competition is its degree of daring and novelty. Hence, they think the scientific community has only two choices: admit their theory into the competition or else prove it to be wrong. However, when it comes to defending a theory or model in scientific debate, without high-quality supporting evidence and a solid conceptual scheme, there's just no time, room, or patience for the "Anything is possible" antics of pseudoscience.

As a postscript on the pseudoscientists, it's of interest to ask why the ideas of many pseudoscientists like Velikovsky are so popular. While it is true that Velikovsky's concepts are a little simpler than those used by modern astronomers and paleontologists, his real advantage is that they are so much easier to visualize mentally and come to terms with. In short, they appeal to what John Q. Public would call common sense. Unfortunately, neither the world nor science is as simple as naïve common sense would have us believe. For example, what kind of peasant cunning would suggest that energy levels in atoms can come only in discrete packages? Common sense would say that if you can walk up stairs one step at a time, then you can also stroll up a ramp to get to the same place. But modern physics says no: Change of energy levels can occur only in discrete steps. The more developed a scientific specialty becomes, the less reliable common sense is as a guide. In fact, there are aspects of science that are just plain contrary to common sense, like the staircase example just noted. The point to keep in mind is that most beliefs being promoted as alternatives to science are deliberately calculated to fit smoothly into what common sense suggests is the way things *should* be, as well as the way to solve all our problems. Within these comforting world views, we have no problems of our own—everything that happens to us does so because of bad aspects of Jupiter, the work of the devil, or the will of superior beings from Andromeda. At root, these beliefs are a measure of the degree of disappointment with which the general public greets the revelations of modern science. The average man wants complete, easy-to-understand, clear-cut answers, when all

that science has to offer is arcane, difficult-to-follow ifs, ands, buts, or maybes.

Belief systems outside science come in many forms, some of them covered by the general umbrella of pseudoscience. By far the most interesting and important alternative to a scientific ordering of the world is that provided by the principles and tenets of organized religion. From the beginnings of Western science in the Middle Ages, there has been a sort of (not always undeclared) guerrilla war waged between the Church and the scientific community on the matter of which is the keeper of true knowledge about the nature of the cosmos. In the next section we will examine this conflict as our final statement about the alternative realities that we use to shape and interpret our daily lives.

THE PULPIT AND THE LAB

A few years ago Daysi Fernandez, a mother of three living on welfare in New York City, bought a lottery ticket that came up a winner, returning almost $3 million, a tidy profit on a $4 investment. Little did Mrs. Fernandez realize that in her good fortune she would become embroiled in a classic case pitting the claims of science against those of religion. As the story goes, Mrs. Fernandez had asked a young friend, John Pando, to purchase lottery tickets for her. Pando, a staunch believer in the power of prayer, thought that the chances of success for one of the tickets would be greatly enhanced if he asked for the divine intervention of Saint Eleggua. Apparently Mrs. Fernandez was sympathetic to his beliefs, for he claimed that she had promised to give him half the proceeds if any of the tickets struck gold. If you've already guessed the punch line of this story, you're just a bit ahead of me.

One of Mrs. Fernandez's tickets was drawn to the tune of $2,877,203.30, but she refused to fork over the promised half of the pie to Pando. In the tried and true American fashion for dealing with such slights, Pando's immediate response was to file a lawsuit against her, in an attempt also to gain entry to the Millionaires' Club. Mrs. Fernandez argued that the agreement was illegal and/or unenforceable on a number of grounds, including the fact that John Pando was a minor under the age of

eighteen. After hearing the competing arguments, Judge Edward Greenfield of the New York County Supreme Court ruled on the matter.

The judge found in favor of Pando on most of the points, including the matter of age, but came up with a final verdict in favor of Mrs. Fernandez on the grounds that it was impossible in a court of law to prove that "faith and prayers brought about a miracle and caused the defendant to win." In other words, Pando hadn't proved that Saint Eleggua had rigged the lottery to point the finger of fate at Mrs. Fernandez. As far as it goes, this seems a defensible statement. But what is open to serious debate is the reasons given by the judge for denying Pando a share of the fortune.

Judge Greenfield in effect assumed a priori that religious beliefs are not amenable to scientific testing. As part of his decision, the judge also stated that rainmaking by cloud seeding would qualify for payment, but that the production of rain by dances, chants, and the other tricks of the medicine man's trade would not. Thus, the Fernandez case opens up for further inspection the age-old question of where a belief system stops and science begins.

In the Reality Game, religion has always been science's toughest opponent, perhaps because there are so many surface similarities between the actual practice of science and the practice of most major religions. Let's take mathematics as an example. Here we have a field that emphasizes detachment from worldly objects, a secret language comprehensible only to the initiated, a lengthy period of preparation for the "priesthood," holy missions (famous unsolved problems) to which members of the faith devote their entire lives, a rigid and somewhat arbitrary code to which all practitioners swear allegiance, and so on. These features are present in most of the sciences as well, and bear a striking similarity to the surface characteristics of many religions. Both scientific and religious models of the world direct attention to particular patterns in events and restructure how one sees the world. But at a deeper level there are substantial differences between the religious view and that of science.

Let's consider some of the major areas in which science and religion differ:

- *Language:* The language of science is primarily directed toward prediction, explanation, and control; religion, on the other hand, is an expression of commitment, ethical dedication, and existential life orientation. So even though there are superficial similarities at the syntactic level, the semantic content of scientific and religious languages are poles apart.
- *Reality:* In religion, beliefs concerning the nature of reality are presupposed. This is just the opposite of the realist view of science, which is directed toward discovering reality. Thus religion must give up any claims to truth, at least with respect to any facts external to one's own commitment. In this regard, the reality content of most religious beliefs is much the same as in the myths considered earlier. Fundamentally, what we have in science is a basic belief that the universe is understandable using rational arguments, experimental observations, even divine inspirations, but no acts of blind faith. This is a viewpoint that is not necessarily shared by many religions.
- *Models:* While both scientific and religious models are analogical, and used as organizing images for interpreting life experiences, religious models also serve to express and evoke distinctive attitudes, as well as to encourage allegiance to a way of life and adherence to policies of action. The imagery of religious models elicits self-commitment and a measure of ethical dedication. These are features completely anathematic to the role of models in science. In religion the motto is "Live by these rules, think our way, and you'll see that it works." The contrast with the traditional ideology of science is clear.
- *Paradigms:* In the discussion of paradigms, we saw that scientific paradigms were subject to a variety of constraints like simplicity, falsification, the influence of theory on observation, and so forth. *All* of these features are absent in the selection of a religious paradigm.
- *Methods:* In science there is a set of procedures to get at the scheme of things: observation, hypothesis, experiment; in religion there is a method, too—divine enlightenment. However, the religious method is not repeatable, nor is it necessarily available to every interested investigator.

Table 1.3 displays a comparative chart of the different ways of science and religion. How are we to divine what this table is trying to convey about the respective abilities of science and religion to tell us anything useful about ourselves and the universe

ISSUE	RELIGION	SCIENCE
subject matter	God and humankind	phenomena of Nature
information source	revealed word, holy books	observations, experiments
objective of study	purpose and plan	mechanisms
language	everyday speech	mathematics
method	literary interpretations	measurement and analysis
results	moral imperatives	explanations
validation	personal experience	replication, testing
limitations	mechanisms unexplained	no goals or values
community	church	scientific establishment

TABLE 1.3 *Religion compared with science*

we inhabit? It seems that there are at least three possible answers to this classic conundrum:

1. *Two realms:* Science and religion have different spheres of jurisdiction.
2. *Concordance:* Religious and scientific explanations of Nature can be brought together on the same plane.
3. *Partial views:* Science and religion each illuminate the same reality (whatever *that* might be), but from different perspectives.

To my mind, only the last possibility makes any sense whatsoever. The first leads to the all too depressing territorial disputes of the kind that so much blood has been shed over through the years, while the second is self-defeating since scientific views are always changing. As a result, a theology that attaches itself to one scientific family today will surely be an orphan tomorrow.

With the above considerations on religion under our belt, we see that both pseudoscience and religion provide alternate reality-structuring procedures radically different in character from those employed in science. It's of interest to ponder why there is such a diverse mixture of nonscientific knowledge, especially in view of the claims of virtually every sect that its own brand of medicine is uniformly most powerful.

My view on this matter is quite simply that neither science nor

...udoscience gives a product that is satisfactory to ...omers; the wares are just not attractive enough. In some ... the beliefs are not useful in the way that people want to employ them. For example, many people have a deep-seated psychological need for security and turn to conventional religion for myths of all-powerful and beneficent Beings who will attend to this need for protection. Science, with its mysterious and potentially threatening pronouncements about black holes, the "heat death" of the universe, evolution from lower beings, nuclear holocaust, and so on, offers anything but comfort to such primal needs and, as a result, loses customers to the competition. Basically, beliefs thrive because they are useful, and the plain fact is that there is more than one kind of usefulness.

To the practicing scientist, the foregoing observations come as a sobering if not threatening conclusion, since they seem to put in jeopardy the conventional wisdom that the road to truth lies in the "objective" tools of science, not the subjective, romantic notions of believers and crusaders. But if we accept Feyerabend's arguments of alternative and equally valid belief systems, then we are inexorably led full circle back to the position that there are many alternative realities, not just within science itself but outside as well, and the particular brand of reality we select is dictated as much by our psychological needs of the moment as by any sort of rational choice. In the final analysis, there are no complete answers but only more questions, with science providing procedures for addressing certain important and interesting classes of such questions.

INTO THE COURTROOM OF BELIEFS

The British philosopher John Locke appears to have been the first to use the word "science" in anything like its modern meaning when he equated "scientifical" with certainty and demonstration of knowledge about the physical world. In the chapters that follow, we will be out to question the degree to which science delivers on these lofty aims. Our dual philosophical themes center about the eternal puzzles: What is real, and what is our relationship as human beings to this reality? In the course of attempting to shed light on these Bobbsey Twins of philosophical speculation, I have chosen the vehicle of a courtroom metaphor within which the competing scientific (and sometimes pseudoscientific and/or religious) parties can plead their case. My reasons for this setting are best summed up in the remark by Henry Bauer that "where eminent men disagree violently, and both sides present their cases as proven, we can be rather sure that certainty is not in fact available, and that the matter is not technical but rather trans-scientific. It is a dispute over probabilities, values, desirability, *not* over facts." The only factor that characterizes science as a whole is that, in the long run, untruths are weeded out and what remains becomes more reliable. Thus, just as in economics where Adam Smith's Invisible Hand guides the flow of events into progressive channels, in science we have the Invisible Boot, which acts to kick out those ideas, theories, and beliefs that don't prove useful to enough people over a sufficiently long period of time.

I leave it to the reader to be the final judge of whether or not "scientism" (I promise that this will be my last -ism) establishes a case for its underlying thesis that "science = truth." But succeed or fail, I hope that as we go through the various case studies in scientific conflict that follow, the reader will not only get some basic grasp of the ideas themselves, but even more important will discover that these ideas are genuinely *worth* an attempt to understand them. Only by acquiring a deeper feeling for the processes as well as the results of science will it be possible to assess its merits effectively as a reality-generation activity. So now that the anthems have been sung, the pledges of allegiance given, and the witnesses called, the court is ready to hear the first case in the continuing litigation between science and Nature. Let the opening arguments proceed!