CHAPTER 6

What We Mean When We Call Political Science a Science: Ambiguity and Certainty in the Pursuit of Knowledge

Political science *is* a science. Don't be confused by the variety of research schools, the diversity of hypotheses, the dearth of experiments, and the inability to draw binding conclusions from analysis. Don't be put off by the absence of universally accepted general laws of politics. Certainly, you have no reason to assert that a science of politics is impossible. Following Weber and Geertz, you might contend that the social sciences need to include methods not found in the physical sciences, but that is not a reason to deny the label "science" to the study of politics.

The criteria of science are rather straightforward. To do science, we must assume that the world outside our minds is real and that we may know it. How we know it is critical. In science, claims to knowledge are based on the public criteria of evidence and reason. Meehl accentuates "reproducibility, degree of quantification, and conceptual neatness" (1986, 316), and Gellner lists "accurate observation, testing, mathematicisation, shared conceptual currency, the abstention from transcedence or circularity, and perhaps others" (1985, 119–120). To the extent that political scientists present claims to knowledge about politics that fit these criteria, they do science.

To relate political science to other disciplines, you must develop a properly nuanced view of science as it is actually pursued. Following the pathbreaking study of Thomas Kuhn, *The Structure of Scientific Revolutions* (1962), we have come to recognize the conceptual, social, and political factors that condition all of the sciences. Nonrational factors also enter into the process by which knowledge is claimed and tested. Who controls power in a laboratory or in an academic journal, for example, always affects the certification of claims to knowledge. However, these factors join with but do not replace reason and evidence. Furthermore, disciplines vary in the extent to which they are characterized by widely accepted claims to knowledge and in the extent to which they possess universal laws. Both conceptual confusion and conceptual neatness appear in all fields. A plethora of hypotheses and theories, a relative absence of unequivocal experiments and other demonstrations of empirical claims, and a dearth of formal logics characterize many, if not all, fields; such traits are neither unique to political science in particular nor to the social sciences in general. As a result, all disciplines are necessarily limited in the claims that they make.

ALL SCIENCES SEEK ORDER IN "THE MULTIPLICITY OF IMMEDIATE SENSE EXPERIENCES"

The effort to make sense of the world "out there" is pivotal to all fields of knowledge that bear the title "science." Each attempt to describe and explain an event or pattern of events addresses the question of the relationship between our minds and the apparent confusion in the world outside ourselves.

Recall the passage that guides Max Weber's philosophy of social science, which I cited in Chapter 3. Life "confronts us," notes Weber, as "an infinite multiplicity of successively and coexistently emerging and disappearing events both 'within' and 'outside' ourselves" (cited in Eldridge 1980, 11). Although he directs this view to social phenomena, there is no reason to accept his limitation. Indeed, the same language appears in the work of Albert Einstein. He defines the "experiences given to us" or the "multiplicity [or variety] of immediate (sense) experiences."

> [It is] an infinite plane on which the separate and diverse sense experiences or observations that clamor for our attention are laid out, like so many separate points. It does indeed represent the "totality of empirical fact" . . . or "totality of sense experiences." In themselves the points on this plane are bewildering, a universe of elements, a veritable "labyrinth of sense impressions," of which, moreover, we never can be completely sure that they are not "the result of an illusion or an hallucination" (Holton 1986, 31–32).

Consider the rush of news events, the kaleidoscope of sense impressions, and the mixed chorus of sounds that surround us. Scientists must confront the question of how we can know the complicated world in which we live.

Nature Exists, Scientists Seek to Know It

Scientists accept the reality of that world and strive to devise ways to understand it. They disagree over methods and whether there are limits on our ability to analyze, but they accept the claim that nature exists. It is not possible to do science while maintaining that every effort to know the world is an artificial and, therefore, fundamentally doomed attempt to assert an intellectual order over a chaotic universe.

We stretch and push our minds to discover the order that exists in the world outside our minds. Einstein defines science as the effort to build intellectual edifices that correspond to the order in the universe. I will continue with Holton's presentation of Einstein's views: "In fact the ultimate aim of science can be defined in this manner: 'Science is the attempt to make the chaotic diversity of our sense-experience correspond to a logically uniform [unified] system of thought.' The chaotic diversity of 'facts' is mastered by erecting a structure of thought on it that points to relations and order" (1986, 32). In this view, we strive to create a set of logically bound propositions that mirror the regularities of the universe itself. Analysis is the effort to create a model of the world in order to predict and explain particular elements of it.

Think back to the contrast between Marx and Weber that we explored in the third and fourth chapters. Marx's theory implies a position that is even more optimistic than that of Einstein. Not only does he see order in the universe that we strive to uncover, but he insists that he has discovered the logic of social, economic, and political development. His theory, Marx maintains, is exactly in line with reality. Furthermore, it demonstrates that our lives will change for the better as we proceed to socialism and communism. In essence, Marx claims to know the world exactly and to know that it will end wonderfully; you cannot be more optimistic than that. But Weber is much less confident about science's ability to uncover the order in nature. He offers ideal-types, claiming that each concrete case will more or less resemble the order erected by our minds. Weber offers "shafts of light," which he sometimes develops into precise hypotheses and policy recommendations. Fundamentally, though, all the approaches to political science that we have examined accept the reality of the political world. Einstein, Marx, Weber, and other social and physical scientists agree that we may, indeed, know the world around us.

As noted earlier, it is impossible to do science without assuming that the natural world exists and that it is amenable to our efforts to analyze it. The reports of scientists, even those who are self-conscious about the limits of scientific knowledge, reiterate this point. Take, for example, the views of biologist Stephen Jay Gould: "If I didn't believe that in working with these snails I was really finding out something about nature, I couldn't keep going. I'd like to be honest enough to admit that everything that I'm doing is filtered through my psychological presuppositions, my cultural vices. . . . *The truth value of a statement has to do with the nature of the world, and there I do take the notion that you can test and you can refute*" (1988, 147, emphasis added).

Notice the imagery, passion, and purpose in the comments of Mitchell Feigenbaum, a mathematician and leader of the newly developing field of chaos theory: "I truly want to know how to describe the clouds. . . . Somehow the wondrous promise of the earth is that there are things beautiful in it, things wondrous and alluring, and by virtue of your trade you want to understand them" (cited in Gleick 1987, 187). Feigenbaum seeks not just another interpretation but the accurate description of that portion of the world that interests him.

Paul Meehl summarizes the scientist's response to the question of the reality of the outside world: "As to realism, I have never met any scientist who, when doing science, held to a phenomenalist or idealist view; and I cannot force myself to take a nonrealist view seriously even when I work at it. So I begin with the presupposition that the external world is really there, there is a difference between the world and my view of it, and the business of science is to get my view in harmony with what the world really is to the extent that is possible" (1986, 322).

Any effort to do science assumes that the world out there is something other than an extension of the analyst's thoughts. This does not mean that our minds do not structure how we perceive. When we do science—political science or any other variety—we create abstractions, knowing that our concepts, hypotheses, and theories are our inventions and knowing that there is a world out there against which they are to be tested, no matter how difficult and equivocal the results of the tests may be.

THERE IS NO KNOWLEDGE WITHOUT EMOTIONS: THE ROLE OF PASSION IN SCIENCE

Do not suppose that scientists approach the effort to know the world in a cold, dispassionate frame of mind. Numerous reports demonstrate that no research occurs without emotion; passion drives the effort to uncover the order in the world. Sometimes, our passions lead us astray.

At other times, they direct us to powerful insights. Not only are you entitled to your political preferences, your personal goals, and your fascination with the world of politics, without them you may not be able to pursue the task of knowing the world. Without passion, there is no knowledge.

A prime illustration is a study of scientists involved in the early stages of the exploration of space. Ian Mitroff analyzed forty-two of the top scientists in the Apollo moon project in order "to study the nature and function of the commitment of scientists to their pet hypotheses in the face of possibly disconfirming evidence" (1974, 581). They were an elite group: Two held a Nobel Prize, six were members of the National Academy of Science, all but four had doctorates, thirteen were major editors of leading scientific journals in the field, and nearly all were employed at prestigious universities or research laboratories (1974, 584).

In the interviews, the scientists openly displayed deep and strong emotions and a powerful competitive drive. Mitroff cites the following typical description: "X is so committed to the idea that the moon is Q that you could literally take the moon apart piece by piece, ship it back to Earth, reassemble it in X's backyard and shove the whole thing . . . and X would still continue to believe that the moon is Q. X's belief in Q is unshakeable. He refuses to listen to reason or to evidence. I no longer regard him as a scientist. He's so hopped up on the idea of Q that I think that he's unbalanced" (1974, 586). These views really did not place X outside the boundaries of science; after all, he was part of the research group, and Mitroff presents the report of his opponent. In fact, those scientists most strongly attached to their own ideas were judged to be the most creative and the most outstanding in the group (Mitroff 1974, 586). Furthermore, all the scientists agreed that emotional commitment is essential to science. Numerous examples of this claim fill Mitroff's article:

> **Scientist A**. Commitment, even extreme commitment, such as bias, has a role to play in science and it can serve science well. . . . We must be emotionally committed to the things we do energetically. No one is able to do anything with liberal energy if there is no emotion connected with it.
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> **Scientist B**. Most of the scientists I know have theories and are looking for data to support them; they're not sorting impersonally through the data looking for a theory to fit the data. You've got to make a clear distinction between not being objective and cheating. A good scientist will not be above changing his theory if he gets a preponderance of evidence that doesn't support it, but basically he's looking to defend it.

Without [emotional] commitment one wouldn't have the energy, the drive to press forward, sometimes against extremely difficult odds. . . .

Scientist D. One thing that spurs a scientist on is competition, warding off attacks against what you've published. . . .

Scientist G. Science is an intensely personal enterprise. Every scientific idea needs a personal representative who will defend and nourish that idea so that it won't suffer a premature death (Mitroff 1974, 588–589).

Notice how far these statements take us from the mythical scientist, coldly sifting through the evidence or applying a mathematical formula. Doing science takes deep personal commitment. We drive ourselves and need passions and emotions to do the work.

Do not suppose that Mitroff's group of astronomers, physicists, and engineers involved in the Apollo mission is unique. Emotional commitments to ideas, competition, and passion shine through every report that I have read of how scientists work. The following selections are taken from Lewis Wolpert and Alison Richards's volume entitled, *A Passion for Science* (1988), which gathers together the reports of scientists about their careers.

I was so excited by these quite new ideas that I was almost stopping people in the street to tell them. Perhaps it is, above all, the thrill of the ideas which binds scientists together, it is the passion which drives them and enables them to survive (Lewis Wolpert quoted in Wolpert and Richards 1988, 9).

The intellectual gratification is much less than the expected reward that I'll have when I see my buddies the next time, and tell them "Look here man, I found this!" This is what I like best about science. I'm always thinking about the papers you see. Even before I have found something, I'm already thinking of the opening phrase of the paper in which I will describe this discovery (molecular biologist Gunther Stent quoted in Wolpert and Richards 1988, 115).

It's playing a game either against one's colleagues or somebody who's written a book you think might be silly or, perhaps, wiser than oneself; but particularly it's a game against nature, against the way things are. And you try to win—there's a certain competitive streak here, I think—against reality itself (neuropsychologist Richard Gregory quoted in Wolpert and Richardson 1988, 195).

Feel the joy in the expression of the physicist Leo Kadanoff: "It's an experience like no other experience I can describe, the best thing that can happen to a scientist, realizing that something that's happened in his or her mind exactly corresponds to something that happens in nature" (cited in Gleick 1987, 189). Excitement, pleasure, the strength of an advocate, pride, anger, the drive that comes from conflict, the effort to defeat nature, and other similar emotions motivate scientists.

Notice, too, the importance of daring in Holton's description of how Einstein proposed to develop theories about "the multiplicity of immediate [sense] experiences." "Rising out of an area just above a portion of the chaos of observables *E*, there is an arrow-tipped arch reaching to the very top of the whole scheme. It symbolizes what under various circumstances could be a bold leap, a 'widely speculative' attempt, a 'groping constructive attempt,' or a desperate proposal, made when one has despaired of finding other roads. There, high above the infinite plane of *E*, is suspended a well-delimited entity labeled 'A, system of axioms' " (1986, 32–33).

Only someone bold enough to apply his or her mind to the "chaos of observables" can devise a coherent system of propositions that matches the order inherent in the universe. This involves acrobatics, not only rules and procedures. Holton cites other adjectives that Einstein used for this effort—"inspiration," "guess," "hunch" (1986, 33). "But the leap to the top of the schema symbolizes precisely the precious moment of great energy, the response to the motivation of 'wonder' and the 'passion of comprehension' . . . which can come from the encounter with the chaotic *E*" (1986, 33).

No scientist has developed a more comprehensive and systematic theory of the world outside himself than Einstein. In his own words, this effort required passion and feats of intellectual daring as much as it did the application of reason and evidence.

Emotion drives science. In order to do science, you must dare to know the world; dreams of glory, competition, the drive to conquer, and other powerful emotions urge the scientist to take the challenge.

THERE IS NO KNOWLEDGE WITHOUT REASON AND EVIDENCE

To do science, rules of logic and evidence that are outside your control must limit your passion. Scientists obviously must test their claims to know the world, and though they may work in private, they must present their knowledge in public. They know that their hunches, dreams, and competitive needs do not provide compelling reasons for others to accept their claims to know the world. But reason and evidence do.

As you saw in the last chapter, the willingness to test and accept the falsification of one's hypothesis defines science. Numerous philosophers of science and scientists themselves exclude from their field any school of analysis whose proponents act as if their propositions could never be falsified. "Again, the crucial idea is that to hold a claim in an objective manner, I must be prepared to subject it to the widest possible range of observational tests" (Brown 1987, 203). David Hull, a biologist who uses the tools of philosophy and sociology as well as his own observations and understanding of his specialty, makes this point:

> Nearly all the views that we tend to dismiss as not being "scientific" because they are not "falsifiable" are actually quite falsifiable. The problem is that their proponents are not interested. . . . If a theory handles a wide range of phenomena and only a few anomalies crop up, a scientist can afford to set these counterinstances by the side for the moment. Perhaps the data are in error or possibly a slight modification can account for them. However, if through time enough phenomena turn out to be sufficiently recalcitrant, he or she might well be led to abandon the theory altogether (1988, 81).

Holton presents Einstein's frame of mind, which joins reason and evidence with passion at the frontiers of science: "Below this upper layer of a few grand laws lies a layer of experimental facts—not the latest news from the laboratory, but hard-won, well-established, aged-in-the-bottle results. . . . But between these two solid levels is the uncertain and shifting region of concepts, theories, and recent findings. They deserve to be looked at skeptically; they are man-made, limited, fallible, and if necessary, disposable" (1986, 13). Einstein does not argue that you should let your passions and hunches run wild. No matter how strongly you are committed to your hypotheses, you must approach these ideas with a willingness to abandon them.

However much scientists may seek to advance their own ideas, they remain constrained by the rules of logic and evidence, standards that lie beyond their control. Gould is particularly sensitive to the importance of selfish motives in science, and, therefore, he insists on the need to check oneself: "Science is done by human beings who are after status, wealth, and power, like everybody else. That's why I advocate self scrutiny. I say, if you don't scrutinize yourself carefully, and you really think that you are just objectively depicting the world, then you're self-deluding. The capacity for self-delusion is amazing" (1988, 147). Hull would argue with those who maintain that power and career interests, not empirical tests, determine claims to knowledge in evolutionary biology: "Regardless of the impression that one might get from reading the recent literature in the sociology of knowledge, scientists really do make extensive observations and run exhaustive and exhausting experiments. All this effort is not mythical behavior designed to camouflage the causal factors that are actually operative in their arriving at their conclusions" (1988, 342).

Sciences and Scientists Vary in the Extent to Which They Use Theoretical and Empirical Tests

In James Gleick's account of the development of chaos theory, he emphasizes the basic difference between the theorists' and empiricists' approaches to research in the physical sciences. "Theorists conduct experiments with their brains. Experimenters have to use their hands, too. Theorists are thinkers, experimenters are craftsmen. The theorist needs no accomplice. The experimenter has to muster graduate students, cajole machinists, flatter lab assistants" (1987, 125). Both organize their work to develop and test efforts and thereby know the world. And both seek compelling reasons to accept or reject hypotheses.

Einstein was a theorist, not an experimentalist, preferring the precision of mathematical proofs to the vagaries of empirical tests. "The criterion is simply this: 'The theory must not contradict empirical fact' " (Einstein, cited in Holton 1986, 40–41). This position rests on the principle that cognition precedes observation. "We must be able to tell how nature functions, know the natural laws at least in practical terms, before we can claim to have observed" (Einstein, cited in Holton 1986, 41). Einstein did not, therefore, place much store in the role of experiments in physics or in the work of experimental physicists:

> Einstein's attitude was perhaps best expressed in a remark reported to me by one of his colleagues in Berlin: "Einstein once told me in the lab: 'You make experiments and I make theories. Do you know the difference? A theory is something nobody believes except the person who made it, while an experiment is something everybody believes except the person who made it' " (Holton 1986, 13).

As a result, Einstein would ignore empirical findings that ran counter to his theory: "Even though the 'experimental facts' seem clearly to favor the theory of his opponents, Einstein found the limited scope and ad hoc character of their theories more significant and objectionable than the apparent disagreement between his own theory and the new results of experimental measurements" (Holton 1986, 71). In Einstein's view, test results, even predictions of heretofore unknown events, cannot determine the truth value of a theory. Although his fame spread when he was acclaimed for having predicted an eclipse as a derivation of his general theory of relativity, he himself was much less impressed by his feat:

> Einstein . . . though pleased by the eclipse results, gave them little weight as evidence for his theory. According to his student, Ilse Rosenthal-Schneider, after showing her a cable he received from Arthur Eddington about the measurements, Einstein remarked "But I knew that the theory is correct." When she asked what he would have done if the prediction had not been confirmed, he said "Then I would have been sorry for the dear Lord—the theory *is* correct." Later, he wrote: "I do not by any means find the chief significance of the general theory of relativity in the fact that it has predicted a few minute observable facts, but rather in the simplicity of its foundation and in its logical consistency (Brush 1989, 1125, emphasis in original).

Einstein offers theoretical scope and logical rigor (especially as expressed in mathematical form) more than—perhaps even rather than—the results of empirical tests as compelling reasons to accept his theory.

Nobel Prize winner Sheldon Glashow reiterates Einstein's position about the centrality of theoretical criteria for claims to knowledge: "Our hypotheses may be wrong and our speculations idle, but the uniqueness and simplicity of our scheme are reasons enough to be taken seriously" (cited in Holton 1986, 172). For him, the ability to solve existing problems in distinctive ways and the clarity with which the hypotheses relate to each other guard against "illusion and hallucination." This position also allows the scientist to ignore results of empirical tests that run counter to the expectations of his or her theory. Above all, theoretical physicists insist on the primacy of reason over evidence in the efforts to analyze the world.

The way in which theorists view the relationship between theory and evidence is significant. They assert that when the set of hypotheses conforms to the rules of logic and mathematics and does not run counter to what we know about the world, we have reason to proceed as if it were true. They maintain that a theory develops by the internal unfolding of its own logic. The vagaries of any and all empirical tests, they reason, imply that we should not put great store in the ability of experiments to assess the explanatory power of our theories.

Rational choice theorists emphasize the importance of deductive logic in their analysis. Riker's theory of political coalitions, for instance, illustrates one of many applications of the mathematical theory of games to political analysis. These political scientists also present empirical evidence to illustrate but not test or substantiate their analyses. Doubting the reliability and validity of empirical evidence, rational choice theorists use the power of mathematical logics to test their claims to knowledge.

Others, in physics and other sciences, insist that the effort to know the world must confront its object directly. Keep in mind Abraham

Kaplan's admonition: Seek to know this world, not one that is solely the creation of our minds. Geertz makes a similar point: "Coherence cannot be the major test of validity for a cultural description. Cultural systems must have a minimal degree of coherence, else we could not call them systems; and, by observation they normally have a good degree more. But there is nothing so coherent as a paranoid's delusion or a swindler's story" (1973, 17–18). The real world that we seek to analyze is too complex for simple formulations that are not grounded in empirical tests. Psychologists and biologists are especially known for the use of carefully designed observations. Anthony Epstein, who discovered the first virus known to cause cancer in humans—the Epstein-Barr virus—spent years grappling with puzzling results in the laboratory. He posited his discovery only after exhausting all available alternative hypotheses. An experimenter, not a theoretician, Epstein describes how he works: "I think just sort of messing about is the answer. You've got to keep messing about at the bench. You see how to change this just a little bit . . . and you want to tinker with something. . . . I'm not sure it's right in astrophysics or big science, but it's very important in certain kinds of biological work. It means registering inside yourself minute changes, tiny things which may have a big influence" (1988, 164–166). In Hull's description of the critical role of laboratory experiments for biologists, he notes that psychologists and biologists use empirical evidence to guide claims to knowledge more frequently than they use mathematical proofs.

As you know, most research in political science accepts the centrality of empirical evidence. Political scientists examine survey data, organize demanding empirical tests of their hypotheses, and conduct fieldwork in order to directly observe the world of politics. Many of them, no matter what their research school, use the same statistical methods to evaluate their hypotheses.

Scientists Seek to Check but Not Inhibit Their Work

There is always tension between the scientist's claim to know the world—the passion that drives research—and the need to test the claim. Each scholar seeks a balance among these contradictory demands. He or she must wriggle off the hook of negative tests and perhaps concede failure. Successful scientists view both practices as obligations.

Because we can never be absolutely certain about our claims to knowledge, we must guard against the premature rejection of smart ideas. Indeed, the more committed we are to the view that we have uncovered the order of the universe, the more right we have to suspend disbelief, assume that our hypotheses are accurate, and proceed to derive further implications from them (Lakatos 1974; Van Fraasen 1980 and 1986). Holton discusses Einstein's views that balance the pull toward skepticism and emotional distance with the drive to say, "I know." "Hence in this early and usually private stage of theorizing the researcher has to grant himself a freedom, the right of 'suspension of disbelief,' a moratorium of premature attempts at falsification. . . . Though the very idea is contrary to the naive picture of the scientist, it is an essential part of the scientific imagination" (1986, 38). Consider how theoretical physicist Michael Berry uses mathematics in his work:

> As the physicist Richard Feynman said, "A great many more things are known than can be proved." And while a physicist wants to be right, he doesn't want rigour to turn into rigour mortis. And if you're trying to create some rather elaborate edifice, and calling on lots of branches of mathematics, you just can't afford to be perfectly rigorous with every step. It's as though, for example, a printer would, in printing a book, have to insist that every letter was absolutely perfectly formed, that there wasn't a shadow of a smudge anywhere. You'd never get beyond the first line if you did that (1988, 46).

Hull, too, balances the need to wriggle off the hook of negative test results and the need to accept defeat. "As Darwin himself remarked, he was a master wriggler. Any scientist who is incapable of wriggling a bit will never succeed in science, but there are limits to wriggling. If it becomes too pervasive, the scientist ceases to be a 'scientist' " (1988, 280–281). Successful scientists necessarily defend their ideas against efforts to deny them. The need to test and the need to wriggle underline the contradictory demands of reason, evidence, commitment, and passion in seeking order in the world outside ourselves.

IN SCIENCE, NONRATIONAL FACTORS INFLUENCE BUT DO NOT DETERMINE CLAIMS TO KNOWLEDGE

As we move away from the myth that equates science and absolute truth, we can better understand that nonrational and even irrational factors influence the decision to accept or reject a claim to knowledge. Certainly, scientists make mistakes. But more importantly, the psychological requirements of the process of doing research and the limitations of their minds lead them to produce less than ideal studies. Passion, the drive to uncover nature's secrets, and conflict with intellectual opponents provide the psychological commitment to work long hours and solve research puzzles. Political interests, the demands of careers, and social pressures affect research, as well. Thus, nonrational and irrational factors play exceedingly important roles in science.

Conceptual Schema and Cognitive Limitations Influence but Do Not Determine Knowledge Claims

All perceptions of the world require concepts, and all concepts are embedded in wider frameworks of understandings. As a result, all research reflects these intellectual structures, which Kuhn (1962) labeled paradigms. Groups of scholars at work on a topic reach consensus on the questions to be asked, the proposed answers to be tested, the standards for evidence, and the techniques of gathering data. Containing both conceptual and theoretical elements, the paradigms offer the puzzles that guide future research. Normal science adds to the precision of the paradigm, filling in the blanks in the accepted understanding of the world while ruling out competing questions, hypotheses, and methods. Paradigms shield the analyst from alternative points of view.

There is no basis for the naive view that truth is simply out there, calling attention to itself. Rather, the analyst must perceive, analyze, and judge. From this claim, David Faust derives two critical points: "Scientists, along with other individuals, evidence cognitive limitations that lead to frequent judgment error and that set surprisingly harsh restrictions on the capacity to manage complex information and make decisions." Because we cannot eliminate the role of the scientist, we must understand how the mind is limited in its ability to know the world (Faust 1984, 3; see also Brown 1987, 157–167). At the same time, he says, science is not the creation of the analyst, having no relationship to the natural world: "To admit this does not necessitate the retreat into fictionalism that some have chosen—the view that scientific knowledge reflects nothing but the inner workings and imaginations of minds, and that the external world contributes nothing to what is known" (Faust 1984, 4). Faust's book specifies these limitations and offers mechanisms to maximize the role of rational factors in decisions about claims to knowledge.

Intellectual paradigms and the cognitive limitations of scientists highlight the nonrational elements that are always present in research. The paradigms inhibit the scientist's range of vision. The need to judge, the tendency to overvalue the first piece of evidence, and other cognitive characteristics that scientists share with other humans necessarily distort their ability to observe and analyze the world. Such principles lead students of political attitudes and behavior to structure their analyses around the individual's perceptions. Attitudes contain affective, intellectual, and intentional components. And what applies to political attitudes holds for attitudes toward scientific subjects, as well. Affect always accompanies intellectual judgment; both nonrational and rational factors influence all analyses.

Because Science Is a Social Phenomenon, Power, Interests, and Status Affect but Do Not Determine Claims to Knowledge

Kuhn's study of scientific revolutions (1962) drew initial attention to the role of social and political factors in the creation and defense of claims to knowledge. The philosopher Paul Feyerabend (1975) and some sociologists of science suggest a fundamental revision of how we understand science by arguing that these factors impede and even doom all efforts to know the world. Most students of science accept the limited point that politics and social pressures affect claims to knowledge but not the sweeping position that only these factors determine what stands as science.

The revisionists maintain that nonrational factors, not the impersonal standards of reason and evidence, determine what knowledge is. Sociologists of science have entered laboratories in biology, chemistry, and physics and returned to report the central role played by power and social pressures in the development and certification of truth claims. Donald T. Campbell has summarized the conclusions that may be drawn from such studies (1986, 112–113), along the following lines:

1. Nonrational factors distort analyses, inducing some scholars to accept "false" ideas, but they also influence scientists to accept ideas as "true."
2. There are no empirical or logical tests with absolutely clear results. "The Quine-Duhem observation on the equivocality of 'factual' (experimental, observational) falsifications or confirmations of theoretical predictions is an unavoidable predicament in all scientific belief change and allows for extrascientific beliefs and preferences to influence the inevitably discretionary judgments involved. No certain proof, logical or observational, is ever available or socially compelling" (Campbell 1986, 113). In *no* science, social or otherwise, is it possible to devise experiments or mathematical proofs that compel the acceptance or rejection of claims to knowledge.
3. If we accept the second point and the principle that no theory is ever more than temporarily true, the truth value of a claim cannot be the reason that it was accepted as true. Truth does

not guide research toward itself. Rather, it is ephemeral, always subject to nonrational judgments.

4. Political power within scientific professions frequently affects the acceptance of ideas as true. Because some professors control access to jobs in laboratories, appointments to university faculties and research institutes, allocation of research funds, and editorial decisions on publications, many find it difficult to oppose their analyses and conclusions. As a result, power determines judgments.
5. "Scientists do not live up to the so-called norms of science, for example, of neutrality, objectivity, and the sharing of all information" (Campbell 1986, 113).
6. In the laboratory, "social persuasion and selection" affect the outcome of research results. The views of one's colleagues, not only the power of the head of the research group, help determine whether hypotheses are confirmed or rejected.

Campbell reports that the revisionist sociologists of science believe the distinctions between science and nonscience are artificial and temporary, and nonrational factors draw scientists to reject or accept claims to knowledge. He concludes that the work of the revisionists leads them to claim that "history and current practice in both astronomy and astrology should be studied with equal trust and respect" (1986, 114). In this view, then, the decision as to what constitutes scientific knowledge reflects social and political pressures; it does not derive from analyses based on impersonal and rational evaluations.

This view takes us back to the question of the nature of reality. Campbell summarizes two recent studies of life in the laboratory, seeking to determine whether the results of research decide questions of knowledge. He notes that the authors of both argue:

> The production of scientific belief assertions of truth claims is shown by participant observation and ethnographic research to be a process in which order is imposed upon a chaotic welter of inconsistent and inconclusive observations through quasi-conspiratorial social negotiations. Thus when life in the scientific laboratory is examined in detail, the factual proof that might be expected never appears. Ambiguity, equivocality, and discretionary judgment pervade. A point at which Nature intrudes and says yes or no to theory is never encountered. This research experience increases doubts about the reputed objectivity of science (Campbell 1986, 118).

This perspective fosters the belief that scientists impose an artificial order on an essentially chaotic reality. The kind of order they propose reflects the needs of their careers and the social pressures of the laboratory, rather than any notion of objective truth.

Note the radical extension of the revisionists' argument: Knowledge is what a community of people, especially those in powerful positions (in regard to professorships, journals, grants), define it to be. There are no absolute standards, no objective facts, no definitive rules of logic. Personal, careerist, and social interests, as well as—and perhaps even more than—intellectual factors, dominate decisions about what science and knowledge are.

I do not accept the revisionists' view of science. In my opinion, doing science combines nonrational, irrational, and rational elements, but there is no reason to maintain that all analyses are equally and only based on subjective, political, careerist, and other such factors. Conceptual biases and intellectual weaknesses do not make all claims to know the world equally false. Faust admonishes us not to retreat into the view of science as fiction. As Newton-Smith responded to Kuhn: "Even if there is no rationally grounded algorithm to guide our decisions [about theories] there may none the less be rational considerations which it is relevant to appeal to in justifying our decisions" (1981, 116). Our passions vary in the extent to which they drive us to push on with powerful and weak ideas. And the fact that the decisions of scientists—like those of other human beings—are affected by the demands of those with power, the perceived needs of self-interest and careers, and other social effects does not mean that rational criteria have no place in the establishment of truth claims. Hull makes this point in a particularly telling fashion: "Scientists form social groups. They cooperate with each other, compete, build on each other's work, sometimes give credit where it is due. Studying the world in which we live is a social process, but from this platitude it does *not* follow that our knowledge of the world is socially determined. It might be,but it need not be" (1988, 15, emphasis in original). We must not exaggerate the importance of conceptual blinders, power, interests, and status in science. Indeed, although the relative importance of these factors necessarily varies, rarely do the nonrational factors completely displace reason and evidence.

Furthermore, there is a fundamental difficulty with the radical extension of the revisionists' account of science, such as the argument that Feyerabend (1975) offers. On what grounds should you accept their claims? If you find them convincing because they are in accord with your experience or because of the power of their logic or the wealth of their empirical detail, then reason and evidence underpin your decision. But this negates their very argument. If the power and status of the revisionists convince you, then they are in the awkward position of making reasoned claims to knowledge but having them accepted only because of social persuasion and arm-twisting—and they are not likely to be satisfied with this outcome. Furthermore, to the extent that they hold no social, economic, or political power over you (which is likely to be true for most readers of this book), they can convince you only on rational grounds. Hence, your acceptance must be based on rational grounds. So, whether we accept or reject the claims of the revisionists, we must use reason and evidence to assess their claims to knowledge. All analyses, *including* those of the revisionists, must be amenable to rational tests (Putnam 1981; Newton-Smith 1981; Brown 1987).

As a result, there is good reason to reject the revisionist sociology of science. Even though Campbell applauds their research, he rejects the radical implications of their position. He provides several instances in which scientists changed their minds and accepted new ideas in the face of social, political, and intellectual pressures to maintain the orthodox position. Noting that the revisionists accept the claim that "the natural world has [at least] a small . . . role in the construction of scientific knowledge," Campbell devises ways to maximize that role, using the findings of these sociologists to enhance the "fallible, corrigible, presumptive, and contingent" nature of these claims to knowledge. Indeed, he defines science by these very criteria (Campbell 1986, 115).

In all sciences, the accumulation of knowledge involves both a deep emotional commitment to a way of seeing the world and tests of that vision. It requires the ability to know when the vision has failed, while realizing that there need be no definitive tests that will ever absolutely certify propositions. Regardless of the importance of nonrational factors in the assessment of hypotheses and theories, they are always tempered and limited by rational criteria. To do science, we must make our vision of the world comprehensible to others. Ultimately, science revolves around knowledge that is accepted or rejected on grounds of reason and evidence, as well as nonrational and irrational criteria.

AMBIGUITY AND CERTAINTY IN SCIENCE

By now, you should recognize the need to discard two critical myths about science: Science is truth, and science is power. Both statements are false because they are exaggerations. The search to uncover and posit the order in nature is always limited by the methods and theories used and by the social and psychological processes inherent in the activities of the scientist. No study is perfect, but that does not mean that all studies are equally and fatally flawed. The process of research involves disagreement as well as agreement, conflict as well as cooperation. Using reason and evidence as best we can to evaluate claims to knowledge, we sometimes produce powerful analyses. Rarely, however, are they so powerful that they compel others to accept them.

In All Sciences, Research at the Frontiers of Knowledge Is Characterized by Alternative Approaches, Contrasting Methods, and Sharp Disagreements

Sciences vary in the extent to which they exhibit consensus over what constitutes knowledge. It is important not to exaggerate the extent to which paradigms structure research within an entire field. In reality, competing schools of analysis divide all disciplines, and dissension always characterizes work at the frontiers of knowledge. Furthermore, no two scientists ever agree on everything. Inevitably, dissension, as well as consensus, is a constant feature of all fields, not only political science.

Stephen Cole offers strong evidence for refuting the proposition that there is a hierarchy of sciences that sharply distinguishes between the physical and social sciences. Using the same standards of science that I presented at the start of this chapter, he examines whether there are clear and systematic differences with regard to the development of theory, quantification, cognitive consensus, predictability, rate of obsolescence, and rate of growth. According to Cole, disciplines at the top of such a hierarchy—physics and associated fields, in most people's minds—would be characterized by highly developed theories, high levels of codification of knowledge, "ideas expressed in mathematical language," much agreement on research findings and methods to test hypotheses, and other characteristics that indicate the absence of diversity and the presence of consensus on what constitutes knowledge (1983, 113). He notes that most persons who accept the existence of a hierarchy of sciences place the social sciences far from the top, expecting them to be characterized by the absence of most or all of these traits. To test for differences in current research, he studies scientific journals and the process by which proposals for grants are evaluated at the National Science Foundation (NSF). He also examines citations in text books for variations in the presence of codified knowledge. Cole's work is one of the few systematic efforts to assess the hierarchy of the sciences.

At the research frontier, Cole's study finds scientists of all kinds using diverse methods and hypotheses, with little consensus on what stands for accepted claims to knowledge. In the journals of psychology, sociology, geology, and chemistry, he locates no significant differences in the level of intellectual agreement, measured as the tendency to cite "a relatively small number of papers and authors" (Cole 1983, 121). He expands this test by studying the process of peer review for research grants at the NSF, looking at decisions in ten fields: algebra, anthropology, biochemistry, chemical dynamics, ecology, economics, fluid mechanics, geophysics, meteorology, and solid-state physics. His examination reveals that all fields are characterized by a relatively high measure of disagreement and that the two social sciences, economics and anthropology, had the lowest level of disagreement on proposals (1983, 123). Additional tests of the levels of consensus further corroborate the claim that all sciences display much disagreement over methods, findings, and the prospects of research proposals. Similarly, Cole identifies no significant differences across the journals of the disciplines in the tendency to cite recent, rather than older, studies; research in the physical sciences is no more characterized by an "immediacy effect" than is work in sociology and economics (1983, 126–127). Developing the general significance of his findings, Cole maintains that "science at the research frontier is a great deal less rational and predictable than our imagery suggests" (1983, 131). Furthermore, he points to a growing body of evidence that indicates that scientists have a very poor record of predicting which ideas at the research frontier will be successful and which will fail (1983, 132). Disagreements about the methods, hypotheses, theories, and results of current research and the likelihood of future success apply to *all* the sciences, and diversity and dissension are constant features of current research.

The same descriptions fill reports on the state of cosmology, a subfield of physics. In an article written for the "Science Times" section of the *New York Times,* entitled "If Theory Is Right, Most of the Universe Is Missing," William J. Broad writes:

> According to astrophysicists, calculations show that the sum of all the known dust, planets, comets, asteroids, stars, pulsars and quasars now accounts for about 1 percent of the matter that theory says ought to make up the universe—that is, unless there is a flaw in current understanding of the laws of nature. . . .
>
> The depth of the mystery is illustrated by the odd suspects now being put forward to account for the invisible mass: Giant slush balls, swarms of black holes, cosmic rocks, and exotic new types of hypothetical subatomic particles with names like photinos and winos (1984, C1, C5).

In his book *Science à la Mode,* cosmologist Tony Rothman provides another characterization of the research frontier of his field: "We have presented the reader with a number of cosmological models: isotropic, closed, open, anisotropic inhomogenous, halls or mirrors, Variable-*G*, with constants, without constants, Big Bang, and Steady-State, Inflation, and Quantum. Each has its attractive features and each has its failings; undoubtedly the reader has a headache. In the end we must simply say that we don't know. But before the end, we should remind the reader that the Garden of Cosmological Delights is very large" (1989, 28). Confusion and disarray—in the form of competing hypotheses, theories, and schools of analysis—are constant and conspicuous features of research frontiers. But they provide their fields with a sumptuous array of intellectual fare.

Cole's research also uncovers differences among the sciences regarding the extent to which they possess a shared core of knowledge. Because text books codify what scholars claim to know about a field, Cole compares texts in physics, chemistry, and sociology. He measures the extent of codification by examining the age and number of references that are cited. The older the references are and the smaller their number is, Cole reasons, the higher the level of intellectual agreement in the science would be. His results show that the studies cited in physics and chemistry are far older and fewer than those in sociology. The sociology texts contained an average of 800 references, three-fourths of which were published after 1959, and the physics texts averaged 74 references, only 3 percent of which appeared for the first time after 1959 (Cole 1983, 134). Interestingly, texts in political science resemble those in sociology. As an exercise for this chapter, I examined two recent textbooks on U.S. politics (Janda, Berry, and Goldman 1989, and Keefe et al. 1989). One contained 339 references, 75 percent of which were published after 1969. In the other, there were 419 references, 83 percent appearing for the first time in 1970 or after. Such tests indicate that physics and chemistry have a stable core of knowledge and that political science and sociology do not. Thus, sciences appear to differ only with regard to the presence of a research core.

Disagreement at the research frontier reappears in David Hull's work, reaffirming that each discipline is a cacophonous orchestra. Indeed, the sound of music comes only from the work of small clusters, "cliques" of scholars. As a result, Hull cautions against the use of phrases like the "scientific community" or "physicists." "No such groups exist. They are worse than useless fictions; they are highly misleading" (Hull 1988, 22). Working on shared problems with shared methods, he says, cliques form research clusters that divide the broader discipline.

At the same time, Hull insists that we not exaggerate the cohesion of the cliques:

> Although members of scientific research groups frequently go to great lengths to hide their disagreements both from each other and from outsiders, they still exist. The preceding observations apply even to the smallest, most highly integrated research groups (or cliques). Such diversity only increases when the groups in question are larger and more amorphous. In short, cooperation in science does not require total agreement, nor does agreement necessarily imply extensive cooperation. Pick any two members of a research group and they will agree with each other on all but a few of the issues relevant to their research. However, it does not follow from this sort of pair-wise agreement that there is one set (or even cluster) of propositions about which all members of the group agree (1988, 113).

Examining patterns of rejection and acceptance in the journal *Systematic Zoology* for evidence of conflict and cohesion, Hull uncovers very little proof that members of each clique are particularly kind to each other and especially harsh on others (1988, 335). Not only is dissension apparent at the level of entire disciplines but it abounds within research clusters as well.

As a result, you should not suppose that any one scientist sits down at the desk to create a "seamless web" or "net" of theory. It may be possible to construct a set of tightly related propositions that join a body of research, but rarely does the work of any one scholar involve the full net itself. Indeed, Hull suggests that we have used the wrong imagery, and he proposes that "patchwork nets" should replace "seamless webs" and "nets."

> Although there are areas of systematicity in any scientist's conceptual system, few scientists succeed in forming a totally seamless worldview. Areas are partially dependent, partially independent. When one expands this picture to include other scientists, conceptual development in science becomes even patchier. . . . No two scientists are ever in total agreement with each other even in their areas of most concentrated investigation. . . .
>
> If one is interested in science as a process, conceptual systems cannot be viewed as seamless wholes. . . . A more appropriate picture is scientists casting their patchwork nets, one after the other, retrieving them, reworking them piecemeal on the basis of the most recent fit, and then casting them again. . . .
>
> According to the logical empiricist analysis of science, scientific theories are totally explicit, perfectly precise inferential systems. . . . But scientific theories as they function in science are much cruder. . . . Frequently it is very difficult to decide which observation statements follow from a particular theory without worrying about deriving a particular observation

> from one theory and its contradictory from another (Hull 1988, 493–494).

Hull describes the consensus over methods, hypotheses, and results that exists (though never all the time and for all purposes) within small clusters of scholars. Another researcher, James Gleick, depicts physicists, biologists, mathematicians, meteorologists, and others cutting divisions into their traditional disciplines as they formed a research cluster to examine chaos: "Those who recognized chaos in the early days agonized over how to shape their thoughts and findings into publishable form. Work fell between disciplines—for example, too abstract for physicists yet too experimental for mathematicians" (1987, 37–38).

The descriptions above should remind you of the differences among political scientists who study turnout, the conflict among those who analyze demonstrations and political violence, and the divisions among the other political scientists whose research you have observed in the previous chapters. In this and all scientific disciplines, conflict and dissension at the frontiers of knowledge are organized by the consensus that prevails among members of the same research cluster.

Where There Are Passion, Power, and Disagreement, We Find Conflict

Unconventional analyses, explanations, and theories usually encounter opposition. Indeed, given the strength of intellectual paradigms, the psychological difficulty of assimilating information that is markedly different, and the exhortation that scientists be skeptical about new claims to knowledge, it would be unreasonable to expect any other response to a radically new analysis. Each effort to advance an unorthodox perspective encounters opposition, and when scientists persist in this effort, conflict ensues.

The history of science is populated with scholars whose work was ignored for long periods of time but who persevered against powerful odds and finally succeeded. Another selection from the *New York Times,* entitled "A Psychiatrist Who Wouldn't Take No for an Answer" illustrates this point:

> For years, Aaron T. Beck had to struggle. In the 1970s he published his own journal to, as he puts it, "bootleg" reports of his studies that other psychiatric journals rejected. He wrote a textbook to get the word out about the discipline he called "cognitive therapy."
>
> "He really was a pariah," says Dr. Beck's colleague Ruth Greenberg. "Talk about people who stuck to an idea when other people had no use for it!" . . .

> The approach is so straightforward in shunning traditional complexities that Dr. Beck says it befuddled many psychiatrists. "Analysts view me as a behaviorist and behaviorists view me as an analyst" (Greenberg 1981, C1, C2).

Anthony Epstein describes the difficulties he faced in the pursuit of clues that led him to conclude that a virus causes cancer in humans: "It was a tremendous uphill battle from the very beginning. First of all conventional virologists in those days would not accept that something seen in the electron microscope was a virus" (1988, 163).

Holding to the accuracy of his view, Epstein persisted: "Well, I knew I was right, so I really didn't care." But in order to convince others, he needed reason and evidence. Epstein prefers experiments, so he sought but was refused permission to test his hypotheses in laboratories in Britain. "I remember that Yvonne Barr was very upset that in fact we had to send it abroad, to America . . . in order to pursue these further studies and get some independent confirmation of our . . . findings" (Epstein 1988, 164). Scientists with revolutionary claims always encounter powerful opposition.

Where research clusters form, they benefit from the research and political successes of their adherents. Once again, I will cite Gleick's work on chaos theory to illustrate a point:

> Every scientist who turned to chaos early had a story to tell of discouragement or open hostility. Graduate students were warned that their careers could be jeopardized if they wrote theses in an untested discipline. . . . A particle physicist . . . might begin playing with it on his own . . . but would feel that he could never tell his colleagues about it. Older professors felt they were suffering a kind of midlife crisis, gambling on a line of research that many colleagues were likely to misunderstand and resent. . . .
>
> To some the difficulty of communicating the new ideas and the ferocious resistance from traditional quarters showed how revolutionary the new science was. . . .
>
> As the chaos specialists spread, some departments frowned on these somewhat deviant scholars; others advertised for more. Some journals established unwritten rules against submissions on chaos; other journals came forth to handle chaos exclusively. . . . By the middle of the eighties a process of academic diffusion had brought chaos specialists into influential positions within university bureaucracies (1987, 37–38).

New questions and answers, especially those that challenge existing approaches, engender active opposition. Research success requires political success, and political success requires research success—the two go hand in hand.

Passion and controversy filled the media during the spring and summer of 1989 when two chemists claimed to have discovered "fusion in a flask" or "cold fusion." It is impossible to exaggerate the significance of their claims: Developing the engineering consequences of their discovery would solve all of our energy problems. At the same time, their successes would have destroyed theories long held dear by almost all physicists. As a result, their first reports received enormous media attention and rabid opposition from physicists. Indeed, the conflict spread to involve leaders of the national associations of both chemists and physicists. Consider this report from the battlefront:

> In mid-April, at a meeting of the American Chemical Society in Dallas, chemists cheered the work of their colleagues, Drs. B. Stanley Pons . . . and Martin Fleischmann. . . .
>
> After years of expensive failure by physicists, "it appears that chemists may have come to the rescue," the society's president, Dr. Clayton F. Callis, told an enthusiastic crowd.
>
> Last week, the other team struck back.
>
> At a meeting of the American Physical Society in Baltimore, physicists smugly reported that, try as they might, some of the best laboratories in the country have failed to replicate the cold fusion experiment. Physicists applauded when Dr. Steven E. Koonin said the phenomenon was a result of "the incompetence and delusion of Pons and Fleischmann" (Johnson 1989, E6).

A year later, the controversy no longer involved physicists in battle against chemists, and the media no longer kept score on whether the breakthrough had occurred. Pons, Fleischmann, and others interested in cold fusion were still at work trying to provide compelling evidence to support their claims.

The disciplines, fields, research schools, clusters, and cliques of science have different levels of conflict and, as these reports emphasize, conflicts do abound. But as Kuhn points out, most research falls within the category of normal science and does not challenge accepted schools of analysis. Such studies naturally are not greeted with hostility and do not generate conflict. And many, if not most, challenges to orthodox wisdom fail. Only the successful scientists get to tell their tales for only the successful combine adequate reason, evidence, and political support.

Where There Are Power, Dissension, and Conflict, We Find Ambiguous Conclusions

It should be evident by now that political science is not the only science in which claims to knowledge are more or less compelling. Nowhere are there simple rules that may be applied in order to certify knowledge. The existing rules allow us to assess claims to knowledge, but they do not provide definitive evaluations. But all scientists must accept and reject propositions. In the words of Scientist G, quoted in Mitroff's study of the Apollo researchers:

> In every real scientific problem I've ever seen, the evidence by itself never settled anything because two scientists of different outlook could both take the same evidence and reach entirely different conclusions. You eventually settle the differences, but not because of the evidence itself but because you develop a preference for one set of assumptions over the other. How you do this is not clear . . .
>
> I've learned by now that you never completely prove or disprove anything; you just make it more or less probable with the best of what means you've got at the time (1974, 588–589).

Hull reiterates this position time and again, emphasizing one of the central theses of this chapter:

> Reason, argument, and evidence are supposed to decide controversies in science, but when scientists have to make choices, evidence is never totally determinate, nor arguments overwhelmingly convincing. More than one alternative is not just possible but also plausible. The appropriate conclusion to such realizations is not that anything goes. There has to be some middle ground between the one and only possible answer and total arbitrariness (1988, 288).
>
> In any instance of apparent falsification, too many alternative sources of error are not only possible but plausible. If one could be absolutely sure that a particular observation is veridical and that no modification elsewhere can save a particular hypothesis, then single-minded attention to falsification would be justified. However, in the real world, scientists must balance confirming instances against apparent disconfirmations and make their decisions accordingly (1988, 342–343).

These points apply not only to work in physics and biology but to political science in particular and the social sciences in general. We seek to know the world, while accepting that there are no simple techniques to ensure the brilliance of our analysis and no rules to certify the reliability and generalizability of our claims. But there are rules to guide analyses, allowing us to reach reasoned choices among alternatives. And over time, there are successes. "We have in [the scientific] community the tradition of argument and counter-argument . . . and the success of science gives us reason to rely on the element of judgment that is inevitably involved in resolving these disputes" (Newton-Smith 1981, 117). Science is a successful and necessarily limited effort to know the world.

Where Science Takes Us and Where It Does Not: The Limits of Knowledge at the University

These studies produce a complex and realistic view of science. This scientific mode of reasoning has produced enormous intellectual and technological successes. Reason and evidence always matter, even if nonrational factors affect the effort to know the world. And science is more than the manipulation of knowledge in the defense and pursuit of power. A discipline that merits the claim of science produces knowledge, even if it does not produce absolute truth. All efforts to understand the world are aided and limited by the questions asked, the theories and hypotheses offered, and the methods used to test the claims to knowledge. In addition, all disciplines are characterized by alternative schools of analysis, by dissension, and by conflict. Every scientist faces the problem that reason and evidence rarely compel acceptance or rejection of a particular hypothesis. And all recognize that their analyses can provide only partial understandings of the world. In science, using reason and evidence to create public knowledge, we know that we may be wrong even as we strive to be right. We seek not to be a science but to do science.

In the process, we confront powerful frustrations. Recall Weber's discussion of science, which we examined in the second chapter. Using the image of the "disenchantment of the world," Weber remarks that when we assume that everything may be known, as we do in science, we place a weighty burden on our own shoulders. Because we expect to know more and more, whatever we know now is only temporarily true. Each success is only a step toward the next success, which, in turn, is a step toward the next, and so on, forever. Scientists never die "old and satiated with life," as the Bible describes Abraham. Rather, we must expect that our claims to know the world, as partial as they may be, will become outmoded. We devote ourselves to uncovering the order in the universe, knowing that we can have no more than limited successes. Indeed, given the descriptions presented in this chapter, we know that our efforts will meet disagreement, sometimes even strident opposition. But to do science, we must either ignore or overcome these frustrations.

Don't let these pressures and limitations block your studies. "Of course, the history of science is a history of flux. Of course, our current theories are doomed. Of course, in so far as truth (strictly speaking) is concerned all theories stand together. For they are all false. But admitting that the historical scene is a flux does not mean that nothing is preserved or that there is no progress" (Newton-Smith 1981, 260). We continue to seek to know the world because we *need* to know it and because that is what we do at the university. We also strive to learn in order to improve the world and to limit the harm that might happen in the future.

The tools of reason and evidence define our work and lives at the university. Only when we use them can we know the world in ways that produce public, as opposed to private, knowledge. These activities do not define our sum and substance as humans, but they guide our efforts to know the world beyond the university's walls.

At the same time, you should remember that we are not only creatures of the university, of science, and of reason and evidence. Weber fears that the rational calculations of science illustrate the spread of instrumental value rational orientations to action in every aspect of our lives. But Weber errs. There is no reason to maintain that emotions and commitments to absolute values are not important in the lives of most people or even in the lives of scientists. We have not become "clerks," and there is little basis to the claim that we are all rational maximizers of our personal interests at all times and in all circumstances. Max Weber to the contrary, you need not be reduced to a rational calculator, coldly sifting through the evidence, in order to do science. Indeed, to do science you must engage your passion as well as your reason.

POLITICAL SCIENCE AS A SCIENCE

Political scientists agree that there is a "real world" of politics and that we strive to analyze it. They display a shared language of research, which emphasizes conceptual neatness and the analysis of empirical evidence. When you do political science, you should specify hypotheses, properly define concepts and variables, and locate the hypotheses in logically coherent arguments or theories. Where appropriate, you should do fieldwork, and if you have reliable and valid data, you should display supporting numbers, graphs, tables, and equations. At the same time, you know that work in political science varies in the extent to which it possesses tightly knit logics and makes use of the full powers of statistical analyses.

Agreement over the language of analysis gives way to disagreement over approaches to analysis. Looked at as a whole, the study of turnout is characterized by four competing research schools: Wolfinger and Rosenstone's hypotheses, which draw on political attitudes; Powell's combination of structural and attitudinal variables; Jackman's emphasis on structural variables linked to assumptions about voters that are rooted in rational choice theory; and Piven and Cloward's structural analysis, which utilizes Marxist theory. Taken together, they do not provide a coherent answer to the question of how to explain variation in the level of turnout. There is, as well, disagreement over the analysis of electoral behavior. In the Michigan model, party identification is basic to the analysis, yet Rose and McAllister, I, and numerous other political scientists have offered many reasons to dismiss studies that use this variable. Rational choice theorists have offered new hypotheses for electoral behavior that contradict propositions taken from structural analyses, as well as models based on political attitudes. As Hull notes with regard to the term "physicists," it makes little sense to speak of "political scientists" as such, given the diversity of this field.

The cacophony of theoretical dissension gives way to the distinct choruses of separate research clusters. From the perspective of each approach, we are given explanations of turnout, electoral behavior, political violence, and a host of other subjects with varied amounts of theoretical power and empirical evidence to support them. No hypothesis is so weak that it denies the utility of its theoretical approach or causes members of the research cluster that offers it to abandon their theory. And no hypothesis is so strong as to compel its acceptance. Research occurs within clusters of scientists who share language, method, and argument and pay little regard to alternative schools of analysis.

Note as well that such research clusters usually examine different subject matters, further dividing political science. Wolfinger and Rosenstone reflect a long-standing orientation among students of political attitudes that emphasizes the uniqueness of U.S. politics; indeed, it even defines "American Politics" as a separate field. Following the logic of Marxist theory, Piven and Cloward locate the special characteristics of U.S. politics under a broader concept, implying the need for cross-national analyses. Both Powell's and Jackman's approaches, which use the explanatory power of structural variables, also emphasize the need for cross-national analysis. Students of voting behavior also disagree over the utility of carving along geographical lines. And for a long time, proponents of the Michigan model (and other studies emphasizing political attitudes) examined voting in the United States to the near exclusion of all other cases, while analyses employing the structural perspectives (especially the emphasis on social class) focused on voting in other democracies. Differences in subject matter, as well as methodology and theory, separate the research clusters of political science.

In political science, as in other disciplines, dissension is frequently accompanied by conflict. The clash between Wolfinger and Rosenstone and Piven and Cloward over how best to increase the level of turnout in the United States is typical. Different recommendations to political candidates would flow from Marxist theorists, rational choice proponents, and students of electoral behavior that use the Michigan model. And deep differences separate rational actor, Marxist, Weberian, and deprived actor models of political violence. In our field, interest in current events and the pull of political ideologies further exacerbate these conflicts. Inevitably, as political scientists compete for professional appointments, research grants, and influence over government officials, members of research clusters come into conflict with each.

Do not expect to find absolute truth in political science. As in all sciences, knowledge in this field is limited by the methods employed, the questions asked, and the hypotheses offered. This does not mean that political science is a hallucination; rather, it implies that you must be very careful about how you use the theories of political science to engineer changes in the world around you. Knowledge is relative to the theoretical approaches used.

DOING POLITICAL SCIENCE

Strive to know the political world: Engage your passions, check them with reason and evidence, and push ahead with the work. When you do that, you do political science.

In political science, you take leaps of intellectual daring, and you take them in public. Success does not come to the timid. The more sweeping your hypotheses are and the more tightly drawn the threads of your theories are, the stronger your claims will be and the more reason you will have to test them. Do not be intimidated by the thought that politics is "oh so complicated." This fear will tie you down and limit your daring.

Present your hypotheses and test them. As tougher tests are applied to your ideas, you will find added reasons to hold them and an increased ability to convince others.

Do not fix your sights on current events. Analyzing only the immediate and the familiar inhibits knowledge. It does not make for expertise. An important reason why so many people were surprised by the revolutions in Eastern Europe during 1989 was that political scientists had not been working on the broad problem of revolution in Communist systems. Too many students of these political systems busied themselves with day-to-day events, especially the nuances of politics among the leaders of the various Communist parties, and too few examined the conditions that lead to revolution in authoritarian regimes. Do not limit yourself to the analysis of the most recent election or war. For example, keep in mind that, as political scientists, we are interested in the determinants of turnout in the United States as part of the general analysis of political participation and that we care about the changes in Eastern Europe because it helps us to analyze the general question of revolution. Allow your mind to soar to questions of theoretical importance.

Strive to control the influence of your political preferences on your analysis. Do not give them up, but use the tools of analysis to keep you from being led by your dreams alone. In a field like political science, in which everyone has views about the subject matter and in which there are relatively few theories, insufficient data, and few compelling claims to knowledge, it is all too easy to allow your wishes to replace reason and evidence. The best way to keep your political preferences in check is to purposefully and consciously increase the role of theory and observation in your analysis.

Push yourself to follow the implications of your hypotheses. Tie your claims to other propositions, and increase the net and strength of your ideas. Follow through on a decision to use only a defined set of explanatory variables by exploring the logical implications and empirical outcomes of this choice. Hull is probably right when he cautions us not to expect seamless webs of proposition. Still, weaving patchwork nets takes the willingness to push your thread through coarse material.

Consider when it is time to discard critical assumptions. I have argued that rational choice theorists have been too quick to modify their assumptions and that Marxists and exponents of the Michigan school of electoral analysis have been too slow. Where do you stand on these questions?

The use of mathematical reasoning enhances your ability to elaborate your ideas. Indeed, Einstein, rational choice theorists, and many others imply that only the use of mathematical proofs permits the development of these implications. And it is clearly true that precision about evidence demands appropriate measures and quantification. If you intend to examine the world out there as it exists, you must make sure that your indicators are both reliable and valid. Where appropriate, you should do fieldwork. You also need to master the relevant statistical techniques that facilitate data analysis. These points are crucial.

Shift the level of analysis from the particular to the more general so that you enhance your ability to speak to questions of theory. I have argued that we cannot explain cases without reference to theories, but do not suppose that you must offer universal laws of politics. Strive, instead, to advance hypotheses that apply to types of political systems.

Tackle big questions. Look at the problem of the social sciences in the context of all the sciences. Why does so much research occur at the frontier? Why do so many citations refer to recent work and not to the classics that compose a core of knowledge? In the absence of well-developed theories and in the presence of pervasive interest in current problems, you should not be surprised that political science, like its related disciplines in the social sciences, stands apart from the other sciences. Contemplate, too, the issue of explanation in the social sciences. Weber reiterated a widely held principle when he insisted that the way to study politics, economics, and society derives from the unique characteristics of humans. This position demands that you focus on the purposeful action of humans, and it suggests that explanations in the social sciences should include motives as explanatory variables. Many of the analyses you examined accept this point. Indeed, most of you began this book with that assumption in mind, and many may still retain it. Is there reason to jettison this principle? I would urge you to study the means by which political scientists can develop techniques to address the unique properties of humans. Consider whether the search for motives has inhibited the ability of political scientists to offer powerful hypotheses, which explain by offering statistical patterns and theoretical principles but no motives. Above all, as you strive to analyze politics, strive, as well, to examine the basic questions of the discipline itself.