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Clinical Psychology and the Philosophy of Science

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William O'Donohue
Department of Psychology
University of Nevada
Reno, NV
USA

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Chapter 1
Introduction

The motivation for this volume is simple. For a variety of reasons, clinical psychologists have long shown considerable interest in the philosophy of science. When logical positivism gained currency in the 1930s, psychologists were among the most avid readers of what these philosophers had to say about science. Part of the critique of Skinner's radical behaviorism and thus behavior therapy was that it relied on and thus was logically dependent on, the truth of logical positivism—a claim decisively refuted both historically and logically by Smith (1986) in his important *Behaviorism and Logical Positivism*: *A Reassessment of the Alliance*.

With the rise of logical positivism in the beginning of the twentieth century, Anglo-American philosophy of science began as a philosophy of physics. The logical positivist, Carnap's (1986) textbook can serve as an exemplar. Its title is *Philosophical Foundations of Physics*, but the subtitle, *An Introduction to the Philosophy of Science*, is intended to reassure the biologist or psychologist that most of the book is also directly relevant to them—if only they are willing to adopt (some would call it "ape") the practices and views of the physicist. Psychology then was ultimately to be reduced to physics. It can be fairly said that during the first 50 years of the beginnings of the philosophy of science when cases from the social sciences were cited, it was only to show how underdeveloped or unscientific these disciplines were. For example, when Sir Popper (1934) proposed his demarcation criterion, two of his prime examples of unfalsifiable, pseudoscientific theories were those of Freud and Adler. And when his student Imre Lakatos discussed progressive research programs, he declared it an advantage of his methodology that it did not fit much of the ongoing work in social science. When Kuhn's (1962) *Structure of Scientific Revolutions* later threw the logical positivist view of science into doubt—including their heavy reliance on physics, psychologists were again among the first to read and discuss the new perspective on science

"Something's happening here and you don't know what it is, do you, Mr. Jones?"
—Bob Dylan

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[see O'Donohue (1993)]. However, Kuhn's approach was only a bit kinder to psychology—his view was that maybe social science could be considered to be a part of science: It was just in a pre-paradigmatic state.

It is important to point out that the influence of philosophy of science was beyond the meta-questions about their discipline—prominent clinical psychologists such as Ellis (1989) claimed that although initially the philosophy of the Stoics influenced his views on the role of cognition in a person's emotional life, he claimed later it was the view of the Sir Karl Popper's student, the philosopher of science and rationality, Bartley (1984) that influenced his latter views on cognition and disputing irrational cognition. It seems philosophers of science and cognitive therapists share an important interest: What exactly is rational belief formation?

But by now even the staunchest "Unitarian" would have to admit that the physics paradigm must at least be supplemented if we are to have a philosophy of all the sciences. To cite two very simple examples: early twentieth century philosophers of science not only concentrated on physics, for the most part, they focused on mechanics and relativity theory (see, for example, Mach, Carnap, Reichenbach). Here, their conception of science as physical geometry (a deductive axiomatic system linked to experience through correspondence rules) was not implausible, but even the attempt to extend this approach to the rest of physics, particularly to quantum mechanics, generated difficulties. Quantum mechanics is a statistical theory, and some of the events it deals with, such as the disintegration of a radioactive nucleus, occur with only a low probability yet on the logical positivist view of explanation (which worked very well for deterministic relativity theory), to explain an event was to show that given appropriate antecedent conditions, it was to be *expected*, that is, *its probability was greater than 0.5*. The dilemma was clear—either one had to conclude that explanation in quantum mechanics was not genuinely scientific *or* one had to modify one's model of scientific explanation.

A second example: In physical science, theoretical progress is generally accompanied by the characterization of homogeneous *natural kinds* (roughly, a natural grouping rather than an artificial one). The evolution of the concept of "chemical element" is a particularly clear example. The vague, ordinary language notion of "sulfur," which referred to its color and smell, was replaced by Dalton's characterization of sulfur in terms of its atomic weight. A century later when it was discovered that not all atoms of sulfur have the same weight, one moved to a single defining property that they all did share, namely the number of protons in the nucleus. All atoms of any element are identical with respect to atomic number and it is this property that is basic to all chemical theory.

But the evolution of the concept of species in biology proceeded quite differently. It is the essence of Darwinian Theory that members of a biological species *not* be identical, for without variation, there can be no differential selection. The fact that taxonomists use cluster concepts instead of simple definitions that provide a few necessary and sufficient conditions is not a sign of theoretical immaturity. Again, the philosopher's model must be modified, unless one wants to ignore the achievements of biology.

I want to extend the general philosopher of science's repertoire into some of the foundational problems of psychology. Thus, my first major question is: How, if at all, must our philosophical accounts of explanation, prediction, and growth of scientific knowledge be modified if we are to make sense of psychological inquiry? I also want to inform psychologists about the most important aspects of the philosophy of science. So, my second major question is: How, if at all, can philosophy of science be helpful to psychologists?

Popper's (1934) work is nearly 80 years old, while Kuhn's (1962) work is still nearly a half century old, and the philosophy of science has grown into a highly complex and ramified subject matter that is no long readily accessible to psychologists. Most psychologists are by now aware (to varying degrees) that logical positivism is obsolete, that Kuhn's work is dated, and even that dramatic new developments in science studies are now at hand. But the pluralistic, almost tumultuous, state of the philosophy of science stands as a serious impediment to the non-philosopher who wishes to stay abreast of recent developments. My aim in this book, then, is to present an overview of current schools of thought in the philosophy of science that will be accessible to non-specialists and that will demonstrate the bearing of these schools of thought.

In setting out a synoptic overview of contemporary philosophy of science for psychologists, I am fully aware that post-positivists' philosophy of science has no coherent picture of science to offer the reader. This need not be disturbing. Science is itself a highly complex and ramified enterprise, and any psychologists will have to admit that much the same can be said of the field of psychology. If Nozick (1983) is correct that "knowing the world involves seeing the different ways it can be viewed," then knowing science must entail, at least for now, a similar pluralism of perspectives. The analogous conclusion for psychology in particular seems difficult to escape. It remains possible, of course, that some coherent picture of science will eventually emerge from the current plethora of views. In any case, pluralism does not entail a complete lack of agreement and I will discuss important points of consensus. For the time being, the even-handed pluralism that I attempt in this book can be considered a pluralism of convenience—there is little point in waiting for a complete consensus to emerge in philosophy of science before presenting post-positivist developments to the psychological readership. But the possibility should be borne in mind that the reigning pluralism in philosophy of science, like that in psychology, may reflect an irreducible complexity in science itself. Regardless of one's theoretical affinities, it may be that to know science is to know the different ways it can be viewed.

Special Topic: What are Philosophical Methods?

Interestingly, philosophers (who tend to explicate and criticize just about everything) have not clearly articulated what their methods are. What are the tools philosophers use to solve their identified problems? And as in any discipline there would be deep debates regarding this. Philosophy is broadly divided into two schools: *analytic philosophy* (the approach taken here) and generally most influential in the United States and English-speaking countries; and *continental philosophy*—which generally relies on the methods of literary criticism and is most influential on the European continent, particularly in France and Germany. Continental philosophy will be discussed in the penultimate chapter on post-modernism. (Joke told by analytic philosophers—and there are not a lot of jokes told among them—continental philosophy is like continental breakfasts and Continental Airlines—it leaves you unsatisfied).

And it is also important to note that the methods of the philosopher are quite different than the methods of the psychologist. This is largely due to the fact that they are attempting to resolve different *kinds* of problems. Psychologists often are attempting to answer empirical problems—problems about the world. Philosophers are often attempting to address linguistic problems—problems about language such as words and arguments. Philosophers mainly rely on three kinds of methods: (1) exegesis; (2) argumentation; and (3) conceptual analysis.

Exegesis

Exegesis means "faithful interpretation." Philosophers, much more than psychologists, take seriously the problem of constructing a complete and accurate exposition of what an author or speaker is claiming. This is in direct opposition of the (illegitimate) rhetorical critique of twisting the meaning of something you want to criticize. For example, here is an example of bad exegesis:

1. Skinner claimed that humans don't think.
2. *I am a human and I think*.
3. Therefore, Skinner is wrong.

There is nothing wrong with the premise 2 but the problem lies with premise 1: if one reads Skinner even half way carefully one would see that he never said that humans don't think—in fact, he explicitly says the opposite many times [see, for example, O'Donohue and Ferguson (2001); Chap. 6, "Skinner on Cognition"].

Beyond sloppy reading or motivated distortion for rhetorical ends, exegesis can be difficult. Here are some of the key issues:

1. Sometimes, verbal agents are none too clear themselves and thus interpreting what they are saying is quite difficult.
2. Sometimes, the issues are just complex, and thus, faithful interpretation is also complex. For example, what exactly the U.S. Constitution means, which rights it gives to the people, which to the states, and which to the federal government has been debated over 200 years and often only partially resolved by two interpretations being presented to the Supreme Court and narrow votes determining the prevailing interpretation. Constitutionally (note the reflexive issue), the Supreme Court's full-time job is the exegesis of the Constitution.

3. Sometimes, there can be multiple readings, and each interpretation has a certain amount of evidence in support of it—exegesis can sometimes attempt to arrive at a "deeper meaning" or some sort of synthesis, or several sets of arguments in support of one kind of reading versus another.
4. Sometimes, there is just a lot to interpret and questions arise about what is relevant to a certain questions and what is not. Skinner, to continue our example, wrote a dozen or so books and questions can be asked whether his account of say, reinforcement changes across these or, more basically, even what parts of these texts are even relevant to this issue.
5. Sometimes, thinkers change their minds and earlier claims are no longer held by them, and at times it can be difficult to identify these changes and their precise starting points.
6. Sometimes, the verbal agent is of a different historical period, language, or culture and thus uses words differently or is addressing a different kind of audience. Exegesis can attempt to understand the role of these kinds of variables on the meaning of a text.

Suffice it to say that one method that philosophers' use is to attempt to correctly and deeply interpret the precise meaning(s) of a sample of verbal behavior. Philosophers view this sort of analysis and commentary as very important scholarly work. Thus, to attempt to accurately summarize, for example, what Skinner actually did claim regarding cognition is thought of as an important intellectual endeavor or, to use a second example, changes in the concept of reinforcement from John Watson to B.F. Skinner to more contemporary accounts. Sometimes, again, philosophers can find that accurate interpretation of verbal material can clear up questions and problems. For example, it can answer questions such as "Does this author contradict himself?"; "How is his or her account different from some other account of the same or similar material?" (e.g., How does Skinner's account of cognition differ from Watson's?); "What precisely are the arguments the author put forth in favor of some position—and how sound are these?"

Logic and Arguments

Another key method relied on by analytic philosophers is to offer arguments and then to analyze the soundness of these. First, a few technical terms:

- A *valid* argument is one in which the conclusion validly follows from the premises. The truth of the premises guarantees (entails) the truth of the premises.
- A *sound* argument is one that is both valid (see definition above), and the premises are all true.

Let us examine a few examples to clarify further:

1. All men are mortal.
2. *Socrates is a man*.
3. Therefore, Socrates is mortal.

This argument is sound. Its premises are true, and due to a logical rule of *modus ponens*, it is relying on a valid logical inference rule to deduce the conclusion from the premises.

Here is a valid argument but not a sound one (because premise 2 is false).

1. All men are mortal.
2. *My cat Sprinkles is a man.*
3. Therefore, Sprinkles is mortal.

But note something interesting here. The conclusion is actually true! However, the philosopher is not particularly impressed regarding this, because we got to truth in an illegitimate manner. We used a false premise to do so. Just as a researcher in psychology is not impressed with the following situation: "Women are more frequently depressed than men. How do I know this?—I just have a strong intuition that this is true." Although the claim is true—research does reveal that women are depressed more frequently than men—the research psychologist wants belief formation to follow certain rules—it is legitimate for them to make conclusions from well-designed research but not from intuition. Similarly, for the philosopher, it is legitimate to from beliefs from sound arguments but not from other ways.

Analytic philosophers attempt to distill long passages of prose into more succinct premises and conclusions and then to examine the soundness of these arguments. Sometimes again, they simultaneously analyze competing arguments about a certain topic (for example, for the morality of abortion versus those arguments against the morality of abortion) and attempt to make fair appraisals of which one has fewer problems. Sometimes, this is called the *dialectical method* in which argument breeds counterargument and so on.

Philosophers also use *counterexamples and possible worlds* to criticize arguments. Finding a counterexample to an argument involves imagining a possible world in which the premises are true but the conclusion is false. The possibility of this world shows the conclusion as false. For example, suppose a carnivore presents this argument:

1. If it tastes good, then it is moral to eat it.
2. *Meat tastes good.*
3. Therefore, it is moral to eat meat.

A philosopher can criticize this argument looking for a counterargument by enlisting a possible world:

1. Suppose baby arms taste delicious (this is a possible world—maybe even this world).
2. But if it tastes good, then it is moral to eat it (this is using the carnivore's previously stated premise).
3. *But we know that it is immoral to eat baby arms.*
4. Therefore, the premise that "If it tastes good, then it is moral to eat it" is false.

Thus, philosophers generally are not interested in claims by themselves—these are almost always seen as problematic and perhaps dogmatic. Philosophers are interested in *argument*s—a series of claims some of which (premises) are used to support (perhaps, entail) another statement, the conclusion. They want to do proper exegesis and faithfully capture arguments and then to put these in the form of arguments and then to evaluate these.

It is important to note that often secondary arguments are needed to support the premises of the first argument. Here is an example:

1. Abortion is the killing of human fetuses.
2. Human fetuses are innocent human life.
3. *If something involves killing innocent human life, then it is morally wrong*.
4. Thus, abortion is morally wrong.

One way of criticizing this argument is to attack premise 2. The claim is that fetuses are not human life; thus to sustain the principle argument, the rational agent would now need to construct a further argument supporting premise 2. For example,

1. Human fetuses are formed at the moment of conception.
2. At the moment of conception, fetuses have all that is necessary to develop into a full human being.
3. If something has all that is necessary to develop into a full human being, then it is *currently human life*.
4. Therefore, fetuses are human life.

In all likelihood, the critic would again point out problems in one of these premises—necessitating a further argument, and so on. This proliferation and "nesting" of arguments is one of the reason why philosophy becomes quite complex. Also, it is complex because there is usually a simultaneous critique of the opponent's arguments. In our example, the pro-lifer would not just be playing defense but also criticizing the arguments offered by the pro-choice individuals. Despite all this complexity, the view is that this is the epitome of rationality—arguments are explicated, criticized, and the fair observer can make judgments that are best defended. This system of argument and counterargument is called the *dialectical method.*

Summary

Philosophers attempt to succinctly explicate arguments and then criticize these. This is often complicated business. First, it relies on the previous method of exegesis—the philosopher wants to make sure they are faithfully capturing the claims. Second, it then involves looking at the logic of the argument—are valid inference rules being used. Third, it looks at the truth of the premises.

Conceptual Analysis

Conceptual analysis involves exploring the meaning of a word or concept. Note that this is related to the other two methods: exegesis and argumentation. Both of these methods will produce words or concepts and questions can be raised about the meanings of these and the clarity of these meanings. Conceptual analysis (sometimes called philosophical analysis or conceptual explication) is meant to clarify this word or concept or to perhaps draw attention to problematic aspects of the concept. Here is a reasonable definition found in Wikipedia:

> Conceptual analysis consists primarily in breaking down or analyzing concepts into their constituent parts in order to gain knowledge or a better understanding of a particular philosophical issue in which the concept is involved. For example, the problem of free will in philosophy involves various key concepts, including the concepts of freedom, moral responsibility, determinism, ability, etc. The method of conceptual analysis tends to approach such a problem by breaking down the key concepts pertaining to the problem and seeing how they interact.

Wittgenstein (2009, pp. 232) famously said,

> "The confusion and barrenness of psychology is not to be explained by calling it a 'young science'; its state is not comparable with that of physics, for instance, in its beginnings. For in psychology there are experimental methods and conceptual confusion. The existence of the experimental method makes us think we have the means of solving the problems that trouble us; though problem and method pass one another by."

The notion is that science has both empirical problems—which can be resolved by appropriate empirical research methods—but it also has conceptual problems in which these traditional methods are largely impotent (we will see that the philosopher of science, Larry Laudan also agrees with this).

Plato was one of the first philosophers to engage in this sort of activity. In the Socratic dialogs, Socrates raised critical questions about the accounts of such key concepts as knowledge, justice, and the good. Accounts of these concepts were offered and Socrates poked holes in these—often ending in the position that concepts we thought we understood, we in fact do not understand because our explication of their properties was found to have serious problems.

Let me take an example of conceptual analysis in psychology to illustrate this. Psychologists use the term "homophobia." They have devised scales that purportedly measure this construct; they have suggested that it is a bad thing that needs to be rooted out and changed. They even have suggested that it might even be a kind of mental disorder [see, for example, O'Donohue and Caselles (1993)]. However, the philosopher can ask, "Exactly what is meant by 'homophobia'—what are its properties and boundaries?" The term literally means something along the lines of "fear of homosexuals" but is this really its meaning—when one examines actual usage by psychologists one finds that its usage more closely follows "a dislike or hatred of homosexuals." Does "homophobia" have two meanings—a hatred and an irrational fear? Are these two means related—for example, is it still proper to call it fear because all fear is based on hatred (is this empirical premise true?)? To dig a bit deeper, phobia actually means an *irrational* fear. Is the fear of homosexuality always irrational? Can a person have a dislike of homosexuality that is rational and legitimate? What about a person who faithfully practices their religion—say an Orthodox Rabbi or Catholic Mother Theresa—can't the Rabbi or Mother Teresa dislike or fear homosexuality because their religion teaches them that is a sin? Isn't there some evidence of rationality here (although we may disagree with the religious teaching), as it seems somewhat plausible to allow folks to follow their religious teachings? On what grounds can psychologists claim expertise in ethics and theology in order to claim that all dislike of homosexuality is problematic? O'Donohue and Casselles (1993) have somewhat ironically concluded, "the construct of homophobia, as it is usually used, makes an illegitimately pejorative evaluation of certain open and debatable value positions, much like the former disease construct of homosexuality."

One more quick example, what about the construct "cultural sensitivity" (O'Donohue 2005)? This concept is in the ethical code of psychologists and is even required course content in the training of clinical psychologists. But what exactly does this phrase mean? What are the key properties of cultural sensitivity? How would one know it when one sees it? Am I being culturally sensitive now? Don't we need to know these conceptual issues before we can devise a scale that measures it? And don't we have to first know how to measure it before we can try to change it?

What is meant by culture? Is Asian-American really a culture?—or are there serious anthropological problems in lumping Japanese Americans with Chinese Americans? The same questions regarding the legitimacy of lumping can be raised about Hispanic Americans (from Mexico, Brazil, and Puerto Rico?) and Native Americans (Piute, Crow, Cherokee, Inuit?). Is gay the same kind of culture as being an Aleutian Islander—or do sexual categories and geographical categories possess important differences? If being gay is a culture—a sexual culture, then do men who like blondes with big breasts also form a cultural group? What does acculturation do to culture status—how does it (purportedly) lessen it and how can we measure this purported dilution? What are the facts about each culture and what evidence supports these claims—for example, What do we actually know about "the Hispanic culture?" These are the types of hard questions that philosophers ask in an attempt to clarify and deepen an the understanding of a construct.

Moreover, what is meant by "sensitivity"? Is it the kind of construct that is categorical—one is either sensitive or not—or are there degrees of sensitive—one can be a little sensitive, medium sensitive, or a whole lot sensitive? Is being sensitive purely in the eyes of the member of the cultural group—or is it an objective concept; and thus cultural members can be wrong when they claim they feel that x has been insensitive to them? How does one actually appraise if a therapist is being sensitive to a client in therapy? How wide is its scope—does it cover brushing one's teeth for example? Am I in any way being insensitive to you now (e.g., is my writing in English insensitive to those who do not speak English as a first language?) When you express your answer to this question—how would you being sensitive to me and my culture? Does being sensitive to say an African-American differ in any way from being sensitive to an Irish American, if so, how? Does cultural sensitivity have some inadvertent negative implications? Martin Luther King in his classic "I Have a Dream Speech" stated:

> I have a dream that my four children will one day live in a nation where they will not be judged by the color of their skin but by the content of their character.

But does cultural sensitivity at least partially involve judging folks by the color of their skin and thus is inconsistent with Dr. King's vision? Again, philosophers do not take construct unquestioningly—instead, they ask hard questions to explore the construct and to expose problems that need to be remedied.

Summary

The list of candidates for conceptual explication can go on and on—what is meant by the concept of mental disorder; the concept of depression; the concept of intelligence, the concept of emotional avoidance? Have the properties of each of these constructs been well explicated and have boundary conditions been clearly established? Are there ambiguities or paradoxes? The philosopher spends time on words and tries to ask hard, revealing questions to make sure we understand what we mean when we use these words.

However, a note of caution, one does not want to be trivial in conceptual analysis—when President Clinton was cornered and seen as possibly perjuring himself when he stated about Monica Lewinsky, "there's nothing going on between us." He told the grand jury:

"It depends on what the meaning of the word 'is' is. If the—if he—if 'is' means is and never has been, that is not—that is one thing. If it means there is none, that was a completely true statement…Now, if someone had asked me on that day, are you having any kind of sexual relations with Ms. Lewinksy, that is, asked me a question in the present tense, I would have said no and it would have been completely true." Under normal circumstances, we do not need to do a conceptual analysis of the word "is."

One final point: Conceptual analysis involves some vision of what an explication might look like. Wittgenstein famously argued that all words do not have simple properties. The concept "brother" may be simply explicated quite simply—as a male sibling. But what about more complex concepts such as "games"? Wittgenstein (2009) stated:

> Consider, for example, the proceedings that we call "games." I mean board-games, card-games, ball-games, Olympic games, and so on. What is common to them all?

- Don't say: "There must be something common, or they would not be called 'games'"—but look and see whether there is anything common to all.
- For if you look at them, you will not see something that is common to all, but similarities, relationships, and a whole series of them at that. To repeat: don't think, but look!

Look, for example, at board-games, with their multifarious relationships. Now pass to card-games; here, you find many correspondences with the first group, but many common features drop out, and others appear. When we pass next to ball-games, much that is common is retained, but much is lost.

- Are they all “amusing”? Compare chess with noughts and crosses. Or is there always winning and losing, or competition between players? Think of patience. In ball-games, there is winning and losing; but when a child throws his ball at the wall and catches it again, this feature has disappeared. Look at the parts played by skill and luck; and at the difference between skill in chess and skill in tennis.

Think now of games like ring-a-ring-a-roses; here is the element of amusement, but how many other characteristic features have disappeared!

Thus, it may be the case that our conceptual analysis reveals a complexity which Wittgenstein called “family resemblances.” We must not have the simple model that we will always find simple necessary and sufficient properties.

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Chapter 2
The Major Problems of the Philosophy of Science and Clinical Psychology

Some individuals reject philosophy and philosophical analysis because they claim there simply are no problems to be examined—there is nothing there. For example, if one thinks that it is just absolutely clear what is morally right and what is morally wrong, and why these have this sort of status—then, it would seem to follow that there is then no need to examine questions regarding morality. In this view, there simply is no problem to be explored. Similarly with science and the philosophy of science, if one thinks it is obvious what science is, what the scientific method is, how to distinguish between good science and bad science, what is special about science, what is the logic of science, and what a cause is; whether observation is theory neutral or not, and etc., one need not rise to the meta-perspective and worry about and attempt to resolve these problems.

It is part of the originating thesis of this book that there are indeed real problems in the philosophy of science and particularly real problems with respect to these questions and psychology. Philosophers, in general, abhor dogmatic assertions—they will argue that rationality necessitates not bald claims but arguments and evidence. Thus, I believe that I (and others) am not falsely problematizing when we worry about these issues. There are several concerns that support this notion:

1. Not everyone agrees on the answers to these questions. When there is such disagreement, one can ask, "Who is right and who is wrong?" When asked what are the unique properties of science and the scientific method, both philosophers of science and psychologists have given quite different and incompatible answers (O'Donohue and Halsey 1997). O'Donohue and Halsey (1997) found that Freud, Skinner, and Rogers (founder of client-centered therapy) all thought their methods were scientific—although these methods were quite different from each other. What are the merits of these individual answers? It appears that there is indeed a problem here because folks disagree, each of their competing claims has some initial plausibility, and these questions seem to have important implications, for example, how one conducts "research" and what counts as "evidence" to provide warrant for one's professional decisions.

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2. Some have argued that psychology has been much slower in progressing—discovering laws and answering empirical questions—than the "hard" science such as physics and chemistry (see for example Meehl 1978). This seems like a reasonable worry—it is a different experience reading a physics textbook than a psychology textbook. In a physics textbook, one encounters universal laws (and many of these) that are very precise (they make point predictions). While reading a psychology textbook, one sees few, if any universal laws, and little such precision. Instead, one finds competing viewpoints that are rather vague. Why is this the case? Is psychology doing something wrong—perhaps has a wrongheaded view of what research methods ought to be—that accounts for this differential progressiveness? Are psychologists less talented—we are still waiting for our Newton and Darwin to make dramatic scientific progress? Is the subject matter just more difficult? One can view this differential progress as a meta-question that needs to be thought about. A hope can be that if we straighten out these meta-issues, psychology can make more progress in its attempt to solve its problems
3. Within the field, there can be competing claims about the scientific qualities of competing programs. Cognitive behaviorists can claim that psychoanalysis is simply not scientific. A few quick examples are as follows: Those in applied behavior analysis can claim that the cognitivists make unwarranted inferences to cognitive states when simple, environmental events provide more parsimonious explanations. Skinnerians can be critiqued for relying on a rather novel and thus initially strange vision of science—single-subject experimental designs. These debates at the meta-level need to be sorted out as they have an impact on the appraisal of evidence and thus an impact on what therapies deserved to be called "evidence based" and those that do not.
4. There are also important sub-problems that plague psychologists in their work, and they seek some resolution of these. For example, a psychologist may wonder, if I am trying to discover the causes of say, depression, I wonder about the following: What exactly is a "cause"? The Scottish philosopher Hume (1737) stated:

> When we look about us towards external objects, and consider the operation of causes, we are never able, in a single instance, to discover any power or necessary connexion; any quality, which binds the effect to the cause, and renders the one an infallible consequence of the other. There is required a medium, which may enable the mind to draw such an inference, if indeed it be drawn by reasoning and argument. What that medium is, I must confess, passes my comprehension; and it is incumbent on those to produce it, who assert that it really exists, and is the origin of all our conclusions concerning matter of fact. This question I propose as much for the sake of information, as with an intention of raising difficulties. I cannot find, I cannot imagine any such reasoning. But I keep my mind still open to instruction, if any one will vouchsafe to bestow it upon me.

Can an entity like "socioeconomic status" be a cause—or is it too abstract? Does one have to operationalize it and tie it to more concrete events that have a more substantive impact on the push and pulls of a person's life? Can age be a cause—which is actually only a direct measurement of how many times the earth has gone around the sun? If so, what does it mean to "operationalize" a construct? And then how does one conduct research to show causation—we all know correlation does not entail causation, but what sort of information it does? This is not to belabor this particular example, but it is to show that in working with an individual problem, specific meta-questions can arise and the sophisticated psychologist may need tools and an understanding of past work on these questions to better resolve these questions. Again, the view that there are simply no philosophical problems seems wrong and naïve.

Some Key Questions in Philosophy of Science

So, if we have convinced the reader that indeed, psychology has meta-questions that are real and that are even important, let us now turn to a brief listing of these. This is not go into detail on each of these at this points—and the purpose is certainly not to answer these (if only!)—but rather the purpose of this list is so the reader can gain a synoptic view of the major questions in the philosophy of science. This is based on the notion that a comprehensive vision of the whole of the terrain as well as the key features of this terrain can be information. One can see that there are a large number of these questions that there are a variety of different kinds of questions, and one can also wonder how these questions relate to one another.

Major Questions

1. What is science and more specifically what is the scientific method? Are psychologists' views of the scientific method coherent, comprehensive, and good? How do these relate to important views of the scientific method found within the philosophy of science? Are psychologists developing their own indigenous views of science or are they borrowing and importing those of philosophers of science? If the latter, which views because as we shall see philosophers of science have very different views?
2. Relatedly, what is pseudoscience? Do any theories in psychology deserve this (negative) appellation? Popper, for example, thought psychoanalytic theories as examples of pseudoscience? Herbert et al. (2000) and others have called EMDR as pseudoscience. Are these folks right? What else might qualify?
3. Is there a "second demarcation problem"—between the natural sciences and the social sciences? Is psychology just different from say physics that different research methods, different standards of evidence, different kinds of evidence, and different kinds of regularities (e.g., fewer universal laws and more probabilistic ones) are found? If there is human free will, how does it impact the project of scientist? What about human cognition—we can react to an experimenter's hypothesis where atoms cannot—how does this change the project of science, if at all?
4. Does science grow (i.e., is it cumulative)? If it does grow, how does it grow? Has psychology shown such cumulative progress? If so, how? Does the history of psychology show cumulative progress from say, Freud, to Rogers, to

Beck, or are there simply sharp disjunctures that really do not build on one another? We certainly often like what is current and think of it as "progress," but what we like now really is not built brick by brick on what preceded it before. Part of the worry here is, in the future, will there be another sharp disjuncture and our favored views will now be on the ash heap of history.

5. Is the philosophy of science normative or descriptive? Do its claims simply describe what scientists do or have done, or instead do they describe what scientists *ought* to do? If they are descriptive, should we worry that to date, philosophers of science have paid little attention to the scientific behavior of psychologists—they have not sampled our behavior to include in their descriptive claims about the behavior of scientists. So, if they have not paid much attention to us, do their descriptive claims apply to us (a generalization question)? Or are we *sui generis*—unique—and if they were to pay attention to us, they would learn important lessons in the variability of what counts as science? Finally, if claims from the philosophy of science care normative—they tell us what scientists ought to do—should we as psychologists pay more attention to these and make sure our research behavior conforms to these?
6. Is observation theory-laden? Does a Freudian see clients in different ways than a Skinnerian? If observation is theory-laden, how can observations really be (independent) tests of theories? Or is observation "objective," that is, independent of the theory that spawns it? Is there such a thing as "dust-bowl empiricism" in which an unbiased scientist simply observes and builds an account "bottom-up" not influenced by any prior conceptions? Can psychologists claim "objectivity" for their research?
7. Do scientific theories give us an actual picture of reality, or are these merely verbal constructions, or ideas, that have a problematic and indirect relationship to the actual world? Alternatively, does science do neither of these but simply provide us with "rules of thumb," that is, practical rules that "work"—help us navigate our world, or something else? Thus, are the products of science "useful fictions" or clean direct pictures of reality?
8. What is scientific explanation? If a scientist wants to provide an explanation of autism—what does it mean to "explain" something? Is an explanation a deductive argument, in which the explanations are the premises—and contain at least one scientific law—and the thing to be explained in the conclusion [as the philosopher of science Hempel (1965) would suggest]? Can there be more than one explanation of the same thing? Are there deeper versus more superficial explanations?
9. How does one appraise or evaluate a scientific theory? Are two different theories of the same phenomenon even comparable? How does one fairly and comprehensively evaluate say, psychoanalysis? What counts as something in its favor—internal coherence?; case studies of its successes?; its scope (how many things it attempts to explain)?; the clarity of its claims and constructs?; its longevity? Are any of these dimensions more important than others—if so what is the ranking of importance? Moreover, if we view a cognitive-behavioral account as its competitor, does part of any sound appraisal process involve an evaluation of its theoretical competitors—if so, how do we do

a side-by-side comparison (and is this even possible)—or do these simply so unique that it is like comparing apples and oranges?

10. How does research "support" a theory? Is this fundamentally an inductive support? How much do diverse pieces of research support a theory? Do we tally confirmations (whatever these are) and subtract falsifications? How does one weigh research that is negative with research that is positive? How does one weigh tests that could have been conducted but have not?
11. What is a scientific law? Are there different kinds of laws—say, universal laws (covers all objects, all the time) and probabilistic laws (true only some of the time)? Does psychology have any universal laws? If not, why not? Why does psychology seem to have many fewer of these than other sciences such as physics and chemistry?
12. How do categorization and taxonomies work in science? Are there universal principles of categorization? Should taxonomies rely only on natural kinds? The DSM seems to be a different sort of category system than the periodic table of elements. In what ways do these differ? What is the import of these differences?
13. Are there crucial experiments (strong inference)? That is, when there are two competing claims, can one do an experiment that decisively falsifies one? Platt (1964) famously claimed there is and others (e.g., McFall 1991) have claimed that psychology should utilize strong inference, so we can make progress—by fairly pitting competing claims against one another and ruling one out. However, O'Donohue and Buchanan (2001) have claimed that due to what is known as the Duhem–Quine problem (more to follow) implies that strong inference and crucial experiments are not possible.
14. What is causation? Hume has argued that we can never observe causation. We can observe constant conjunction—one event inevitably following another—but this is not causation. So, what exactly is causation? Are there different kinds of causes (Aristotle thought that there were four distinct types—material causes, formal causes, efficient causes, and final causes). Rachlin (1998) has suggested that different theories in psychology emphasize different kinds of causes—specifically that behaviorism emphasizes final causes, while other theories emphasize efficient causes.
15. Does science assume determinism—that all things are caused—and thus, is it inconsistent with notions of human free will? If it does, what are the implications of this? Skinner (2002), for example, wrote a controversial book called *Beyond Freedom and Dignity* in which he claimed that science does assume a deterministic view of the universe, and thus, notions such as "freedom" and "dignity" just do not make sense. However, this is quite a problem as our criminal system and our government assume that free will exists—for example, one needs guilty intent in order to commit a crime.
16. Are there limits to science? Can science give answers to ethical questions, for example? Hempel (1965) argued that it could not:

> Let us assume, then, that faced with a moral decision, we are able to call upon the Laplacean demon as a consultant. What help might we get from him? Suppose that we have to choose one of several alternative courses of action open to us and that we want to know which of these we

> ought to follow. The demon would then be able to tell us, for any contemplated choice, what its consequences would be for the future course of the universe, down to the most minute detail, however, remote in space and time. But, having done this for each of the alternative courses of action under consideration, the demon would have completed this task; he would have given us all the information that an ideal science might provide under the circumstances. And yet he would not have resolved our moral problem, for this requires a decision as to which of the several alternative sets of consequences mapped out by the demon as attainable to us is best; which of them we ought to bring about. And the burden of the decision would still fall upon our shoulders; it's we who would have to commit ourselves to an unconditional (absolute) judgment of value by singling out one of the set of consequences as superior to the alternatives (pp. 88–89).

If science does have such limits—what methods do we use to generate knowledge in these domains? How do we validly make these ethical decisions? How does the American Psychological Association make the knowledge claims it does in its Ethical Code?

17. What is the role, if any, of values, ideology, and vested interests in science? Are these illegitimate or a necessary component of science? If necessary, how should these be handled? Who rules science? Is science related to political oppression—is it, for example, a way that the privileged (say white males) exploit the underprivileged? Is it a way that the secular exploit the religious? Is science appropriately democratic?
18. What is the role of rhetoric and persuasion in science? Do rhetorical skills play a role in science? Does this make science a bit like marketing and Madison Avenue? Is this bad?
19. What are the importance of problems, problem choice, and problem statements in science? Scientists work on a number of problems, how does one choose among these? Are some problems better to work on than others? Are some problems trivial?

Again, the purpose of this rather lengthy list is not to supply you with the answers but to give you an overview of some of the main problems of the philosophy of science. Before we know answers, we need to know what the questions are. The remainder of this book will address many of these questions—however, this is an introductory book and we cannot get too deep into any one question, and some we will have to give very short shrift. However, the reader is encouraged to go to the primary literature for a deeper treatment of these questions.

Special Topic: The Relationship Between the History of Science and the Philosophy of Science

At first glance, these may seem to be two entirely separate questions and thus two separate disciplines: the questions "What has actually happened in science?" (What actually is the historical record?) and "How can one resolve the meta-questions or meta-problems that arise in science?" seem to be two very different questions. However, a moment's reflection will see that these two questions are actually interconnected in important ways.

This interrelationship can first be seen because the historian of science must answer—either implicitly or explicitly—some fundamental philosophical questions before she can even begin her historical studies. First, the historian must make some antecedent judgments about what constitutes science—but these are based on (perhaps inchoate) philosophical judgments. Should the historian study Newton but not Freud, what about Shakespeare, Einstein, and Mesmer? What about a bit more recently—creation science? The answer to these questions has to be partly normative and philosophical—the historian must commit herself to a philosophical position about what constitutes science, to define the domain and boundaries of the domain of investigation so she can begin her inquiries. To give another quick example, if the historian claims that scientific progress is made when say, Ptolemaic theory of the planetary motions was superseded by the Copernican, then in doing this, she has again made normative philosophical commitment—to what properties must be shown in scientific progress—parsimony, increases precision, wider scope, etc. Thus, data from the history of science are to some extent "theory-laden"—they are based on philosophical assumptions (perhaps explicit, perhaps not; perhaps examined, perhaps not).

Note that the philosopher of science is in the same position. He has to make historical commitments in his arguments about science. He will need to argue something along the lines, "My normative, philosophical account is correct, in that it is consistent with these past episodes in the history of science."

There are a couple of additional points. First, historians reflect on their methodology; this reflection over history's meta-questions is called *historiography*, and perhaps, its most fundamental questions are: What are valid historical methods? How does one fairly go about gathering historical data to create a fair, comprehensive historical record? What inferences can be validly made from this data?

Historians recognize there are several mistakes one can make. First, the historical record can be incomplete. But for the historian of science, this is a huge problem. Even after antecedent decisions are made about what constitutes science, the amount of historical data seems daunting. Certainly, a historian cannot look at all of science—all the research conducted by physicists, chemists, biologists, astronomers, biochemists, etc., is simply too large to be examined. Even trying to construct the historical record of a small sub-discipline can be daunting—imagine trying to collect a comprehensive historical record of research into the treatment of depression. So, the historian must be more conservative and selective, but how well can even this more modest aim be accomplished? We shall see that a philosopher of science Paul Feyerabend has stated that one can prove any claim in the philosophy of science simply by selectively searching through the vast historical record of science and bringing forth the episodes that are consistent with one's argument.

A third problem in historiography is fairly constructing the historical record. The historian does not want biases to distort the record. But how can this be accomplished and shown to others that it was indeed accomplished? Consider problems in constructing the historical record when political biases are taken into account. If one were to ask the question, during what periods has the country prospered the most? Democrats would construct the historical record in one way and

Republicans in another. Thus, a key question is what biases might be at play in constructing the historical record as it is being presented?

A third problem in historiography is known as a Whiggish interpretation of history. In this problematic view, the past is seen as an inevitable progression toward an ever greater present. Thus, history is sorted out as having winners and losers based on current biases and notions. Thus, because we live in a more secular society, religious folks in prior centuries are viewed as wrongheaded and pernicious influences, while secular individuals are seen as heroes. But part of the problem is if the zeitgeist were to change, then the historical story would need to be flipped. In the history of science, histories are seen as Whiggish when they focus on the successful chain of theories and experiments that lead to present-day science while ignoring or being quite critical of failed theories, of dead ends, or of past critiques of present theories.

It is beyond the scope of this introductory book to cover all these controversies, but I wanted to point these out to enhance the reader's ability to critique arguments from the history of science. Most claim that there is a reflective equilibrium between the history of science and the philosophy of science. As Hanson (1962) put it, "History of science without philosophy of science is blind … philosophy of science without history of science is empty."

Therefore, it is reasonable to ask critical questions regarding arguments that cite the history of science:

1. How good is your history of science? Does the evidence really show that this is how things happened? Is the historical record complete and fairly constructed?
2. Why are you arguing from these particular historical events—if we were to look at a wider sample—would all these historical events also conform with your argument and conclusions? Just because your claim may be true about, say, the history of physics, why does this mean that it also has to be true about say the history of psychology?
3. Is your analysis Whiggish—does it show a bias toward present views?
4. On the other hand, are your normative claims about how science actually operates consistent with the historical record—if, for example, you claim that theories ought to be appraised thus and so, have in the past successful scientists followed your prescriptions? If they have not, is not this a problem for your account?

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Chapter 3
Epistemology and Logical Positivism

Logic positivism is dead, or as dead as a philosophical movement ever becomes—John Passamore (1967).

Epistemology

I start this chapter with a brief introduction to contemporary epistemology. *Epistemology* is the philosophical study of knowledge—more specifically, the questions of "What is knowledge?"; "Is knowledge even possible?"; (also known as the *skeptical* question)—and "How does knowledge grow?" being some of the central problems of epistemology. In most contemporary views, epistemology and the philosophy of science are highly interrelated because science attempts to produce knowledge—and for many, it has shown an unique ability to produce knowledge. Thus, some have said (e.g., Quine 1969) that the epistemologist ought to focus their study on the sciences in order to make optimal progress on their questions about knowledge. Interestingly, for our purposes, Quine (1969) said something more specific:

> Epistemology … simply falls into place as a chapter of psychology and hence of natural science. It studies a natural phenomenon, viz., a physical human subject. This human subject is accorded a certain experimentally controlled input—certain patterns of irradiation in assorted frequencies, for instance—and in the fullness of time the subject delivers as output a description of the three-dimensional external world and its history (pp. 82, 83).

Quine thought ultimately that psychology (specifically the psychology of perception and learning) would answer the questions of epistemology! Quine recommended this "naturalized" epistemology as a replacement for the more traditional position of epistemology as a "first philosophy" in terms of which all our knowledge of the world must ultimately be grounded. The traditional view is that the first task of the philosopher is to resolve epistemological questions—because questions about knowing are basic to any other philosophical questions. The philosopher must have an account of knowledge before they move on to knowledge claims about ethics or political philosophy, for example. For Quine, on the other hand, there is nothing more fundamental than the knowledge-generating processes of the natural sciences themselves; accordingly, abandoning the foundationalist philosophical project ought to allow us to replace traditional philosophical

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questions about justification with purely empirical questions about the causal route from stimulus to belief and its expression.

But this is bit of a preview—*naturalized, evolutionary epistemology* will be discussed in detail in the next chapter on Popper as Popper and his students did much to develop this account. First, since we mentioned the traditional epistemological view, let us describe it a bit.

Epistemology 101

Since Plato, philosophers have generally accepted that knowledge is *justified, true,* and a *belief.* Roughly, the argument is that asserting "Sally knows *p*" (where *p* can be replaced by any proposition) forces one, in order to be consistent, to also assert that:

1. That Sally believes *p* (It is logically contradictory to say Sally *knows* that $2 + 2 = 4$, but Sally does not *believe* it);
2. That *p* is true (One cannot *know* a false statement); and
3. That Sally's belief in *p* is *justified* (Sally may correctly believe that 5,893 will be the winning lottery number, that is, she may hold this true belief and therefore meet the first two conditions, but this lucky guess is not a case of knowledge because there were no grounds, good reasons, or arguments for this true belief).

Thus, knowledge has been traditionally regarded as a type of belief that differs from other kinds of belief in that it is true, and it is justified. Traditional epistemology is concerned with the evaluation and construction of methods by which we may arrive at clear cases of knowledge, for example absolutely certain knowledge (Descartes 2010). Implicit in this account is the suspicion that our normal, everyday conception of knowledge is too vague and unrefined—a view that often precedes a philosopher's more careful and detailed conceptual analysis. Our everyday notions of knowledge are generally too lenient—these let in too much belief that is not true and/or not justified.

Further, according to Plato, opinion changes, while knowledge remains constant. Another essential feature of traditional theories of epistemology is their *normativity*: To know is to meet certain epistemic *norms* (e.g., proper justification and proper standards regarding truth), and mere belief always fails to do so. Traditional epistemology was—and for some thinkers continues to be—a discussion of how one *ought* to reason in order to arrive at knowledge, rather than how one in fact does reason.

Plato's (1997) original theory (knowledge = justified true belief), outlined in his dialogues the *Theatetus* and the *Meno,* has been modified by subsequent philosophers (see e.g., Ayer 1952) in an attempt to address the problems associated with articulating adequate theories of justification, truth, and belief. In order for the traditional approach to epistemology to accurately account for knowledge, these problems must be solved.

Justification

What precisely does it mean for a claim to be "justified?" Justification is a matter of the degree to which one can support a knowledge claim with some sort of warrant or evidence, such as citing sensory evidence or a deductive argument. A mere belief, even if it happens to be true, will not count as knowledge unless it has been properly justified. For example, the belief that "Client X suffers from depression" without any supporting evidence (e.g., scores on the BDI-II, observed feelings of helplessness and worthlessness, etc…) amounts to a mere belief, hunch, guess, or opinion—but not knowledge. Supporting this claim with further observational claims such as valid assessment results creates (at least partial) justification for the claim. According to this formulation, a true belief without justification is just that a true belief that falls short of knowledge.

Truth

What exactly is "truth?" Two major theories of truth have been articulated within the framework of traditional epistemology: *the correspondence theory of truth* and *the coherence theory of truth*. Before discussing these opposing theories, it is important to understand how philosophers have treated the topic of truth in general. Truth is a property of propositions (e.g., statements, sentences, beliefs), **not** of objects in the world. As such, it makes sense to say that the statement, "The cat is on the mat" is true (if the cat is, in fact, on the mat), but the cat and the mat and their relation to one another *qua* objects in the world are just a brute fact (neither true nor false).

Correspondence Theory of Truth

In this view, truth is a property of propositions that accurately reflect (correspond to) reality. When a belief accurately depicts reality (e.g., "The cat is on the mat" when, in fact, the cat is on the mat), then the statement is said to be true. The correspondence theory of truth further assumes that insofar as the natural sciences rest on sensory evidence, we should be able to develop a language that accurately captures and faithfully transmits the observed structure of reality (e.g., the cat on the mat). The more exacting our language is with respect to reporting our observations, the deeper and more accurate our explanation of the natural world is Precision, clarity, and rigor, regarding both observation and language, according to these thinkers, will eventually generate epistemic certainty. Many, namely the sense-data theorists such as Russell (1985) and, as we shall see, the logical positivists such as Carnap (2003), attempted to construct such fine-grained ideal sensory languages that translated perceptual experiences into language without losing objectivity or accuracy.

Coherence Theory of Truth

The coherence theory of truth suggests that the truth of any given proposition is generated by its logical "agreement" (or fit) with a set of other relevant beliefs. The belief "Zinc dissolves in acid" is true according to this account because this belief agrees or "fits" with the set of beliefs having to do with elements, their properties, chemical interactions, statements that describe observations, and so on. Likewise, each member of the set of beliefs having to do with the elements (to which the original belief was compared) is also true (or false) by virtue of their logical agreement (or lack thereof) with other statements. Coherence theorists view knowledge as a belief network that logically supports itself and, in doing so, generates truth. There is no requirement for such belief networks to correspond to any objective reality because such a reality, in this view, is ultimately unknowable.

Correspondence theories are criticized due to difficulties in understanding the relationship between raw percepts and language. However, individuals do generate observational sentences, and it is critical in this account that these observational sentences cohere with—but not correspond to—all other relevant claims. For example, the observational claim "I just saw zinc fail to dissolve in acid" does not logically cohere with the universal claims "All zinc dissolves in acid."

Problems with the Traditional Account of Knowledge

Problems with Justification

Justification can be an ambiguous and slippery requirement. Some may treat justification as identical to that which is reasonable, acceptable, or personally believable (e.g., "That is a justified belief"). This is a problem because that which is reasonable, acceptable, or personally believable varies from individual to individual—probably because individuals use a wide variety of standards of evidence. The most difficult feature of the justification requirement has been referred to as the "regress problem" (BonJour 1985). The regress problem is the result of foundationalist attempts to justify beliefs. Foundationalism, as Alston (1976) characterizes it, is the thesis that:

> Our justified beliefs form a structure, in that some beliefs (the foundations) are justified by something other than their relation to other justified beliefs; beliefs that are justified by their relation to other beliefs all depend for their justification on the foundations (p. 165).

Stated more simply, a person might argue that, "My belief that my car is parked in the driveway is (partially) justified by my belief that I left my car there last night. The latter belief is justified, in turn, by my belief in the general veracity of my memory. I can further justify my belief by taking the steps necessary to empirically verify the location of my car (e.g., I can look and thus create observational statements). Having seen my car in the driveway, I can further justify my belief that it is there by appealing to certain basic assumptions regarding the veracity of sense perception. In the end, my justificatory trail will lead to an infallible belief (i.e., its foundation)."

Unfortunately, the foundationalist assumption leads to an infinite regress (or what Bartley (1999) called "a retreat to commitment"—faith). According to the foundationalist, each foundational belief is of the sort that it is self-evident and in need of no justification. Foundational, or basic, beliefs are considered self-justifying. Are there really any such beliefs? What kinds of beliefs would count? Descartes (2010) proposed his "cogito ergo sum" as an example of a self-justifying belief. The "cogito" ("I think, therefore I am") was thought to be self-justifying because, as Descartes believed, one could not possibly think the contrary and any attempt to do so inevitably reaffirmed the very statement. That is, a person's thinking simply guarantees the truth of the claim. Descartes' foundation, however, has not proved to be as sturdy as he had hoped.

Problems with Truth Criteria

Both the correspondence and coherence theories of truth fail. The correspondence theory of truth presupposes our ability to (1) engage in "raw" unbiased observation of the world and (2) translate these raw perceptions into a meaningful scientific language. Empirical literature regarding perception clearly indicates that we have no such privileged access to raw sense data (Pashler and Yantis 2004). The logical positivist's goal of constructing an ideal sensory language for the sciences failed as well (Smith 1986). Furthermore, Munz (1985) argued that evolution entails that our perceptions never "represent" the environment but rather are "tolerated" by the environment because of their "truth likeness." (However, interestingly for Munz, their existence ensures that something in fact is beyond our perceptions.) He stated:

> Nobody it seems stopped to think about the biological basis of perception and the phenomenon of adaptation. Everybody seemed content with the idea that mankind had evolved to the point of Enlightenment at which one simply knew that observation was a good guideline and infinitely better than any other source of information. 'We should consider ourselves lucky to have eyes to see light' everybody was saying, 'and not frivolously throw such a gift to the winds and give credence to intuition, authority, tradition or reason.' It never occurred to anybody that there was a very good reason, given the existence of light, why we had eyes and an equally good reason why we should prefer the testimony of our eyes to that of authority or revelation. Instead all people worried about was whether what they saw was what there was or whether it was an appearance and if an appearance, whether there was a reality behind it and if so, whether that reality was likely to be significantly different, etc., etc. The thought that the presence of the eye was guarantee of the presence of light, that light had selected organisms with eyes for survival, and that may be reason why we should go by our eyes rather than by revelation never seems to have crossed anybody's mind! (p. 10).

The correspondence theory of truth has been abandoned by most epistemologists for these, and other, reasons.

A simple, yet definitive, argument against the coherence theory of truth was formulated by the logical positivist, Schlick (1973):

> If one is to take coherence seriously as a general criterion of truth, then one must consider arbitrary fairy stories to be as true as a historical report, or as statements in a

> textbook of chemistry, provided the story is constructed in such a way that no contradiction ever arises. I can depict by help of fantasy a grotesque world full of bizarre adventures: the coherence philosopher must believe in the truth of my account provided only I take care of the mutual compatibility of my statements, and also take the precaution of avoiding any collision with the usual description of the world, by placing the scene of my story on a distant star, where no observation is possible. Indeed, strictly speaking, I don't even require this precaution; I can just as well demand that the others have to adapt themselves to my description; and not the other way around. They cannot then object that, say, this happening runs counter to the observations, for according to the coherence theory there is no question of observations, but only of the compatibility of statements (p. 419).

Problems with Belief and Skepticism

The *skeptical* question "Is knowledge even possible?" has been answered in many ways. Given the potentially strict interpretation of the doctrine of justified true belief, some philosophers have maintained a skeptical stance regarding the very possibility of knowledge. Such philosophers have claimed that knowledge is not possible (e.g., the skeptics of ancient Greece). Descartes (2010) is famous for claiming that one should doubt everything that cannot be known with absolute certainty. The scientific method was initially considered a corrective to the skeptical argument because it promised to reduce typical human errors of reasoning and, most importantly, provide a pathway to rigorous knowledge. However, the promise of science to overcome the skeptic's argument has not come to pass as science is generally seen as fallible and even subject to revolutionary revisions of claims previously taken to be justified and true.

A problem with skeptical arguments is that the skeptic refuses to acknowledge the fact that humans do possess knowledge. A survival advantage is possessed by the creatures with cognitive faculties that are lacking in creatures without them. Kitcher (1992) puts it this way: "If our initial cognitive equipment were as unfortunate as the skeptic portrays it as being, then, the suggestion runs, our ancestors would have been eliminated by natural selection. They were not, so it was not" (p. 91).

The skeptic also takes issue with assuming knowledge (e.g., scientific findings) in order to explain the possibility of knowledge. Kitcher (1992) responds to this skeptical concern as well:

> One complaint against the appeal to Darwin is rightly dismissed. If skeptics protest that a part of contemporary science is being taken for granted in evaluating aspects of the historical process out of which science emerged, the appropriate naturalist reply is, "Of course. What else?" As I hope to have made clear, a central naturalist thesis is that some parts of our current scientific beliefs must be assumed in criticizing or endorsing others (p. 91).

The skeptic has overstated our shortcomings as cognitive agents and created pseudo-problems regarding knowledge that are easily resolved. The evolutionary epistemologist resolves the skeptical question "Is knowledge even possible?" by refusing to entertain it as a legitimate problem. We have concerned ourselves with the mere possibility of knowledge for too long, thus ignoring the obvious fact that we do, in fact, possess knowledge. The demand for certainty in knowledge inevitably results in skepticism because certainty is beyond our reach.

Thus, the characterization of knowledge as justified true belief runs into problems typical of any analytical definition, namely the question is shifted from "What is knowledge?" to questions regarding the alleged constituent properties. What is meant by "justified"—*apodicticity* (self-evidently true—e.g., "x = x"?); consistency with *all* possible tests?; consistency with a certain subset (how many?) of all possible tests, that is, all conducted tests?; more consistent with these tests than not?; more supporting evidence than known rivals?; or something else? What exactly is supporting evidence? What is "truth"—correspondence with the facts?; coherence with other beliefs?; pragmatically useful beliefs?; or something else? And finally, there are questions about the third alleged property—what is "belief?"—any percept such as "red, now"?; a proposition that I am immediately conscious of, as in "I am now typing"?; something that may be more dispositional, such as the background belief, "Antarctica is cold"?; or something else? For the past 2,000 years, epistemologists have attempted to provide a satisfactory account of knowledge by supplying acceptable accounts of "justification," "truth," and "belief."

Gettier Cases

Moreover, Gettier (1963) in an important and revolutionary paper "Is justified true belief knowledge?" suggested that there are cases in which justified true belief is not knowledge, and therefore, the traditional analysis needs to be revised. These examples have come to be known as "Gettier counterexamples." The following is an example. Suppose one night you see a man leave a bar, then he is staggering, weaving when he walks, and singing in a slurred manner. You see him take a long drink out of what appears to be a bottle in a brown paper bag. You see him weave to his car and drop his keys several times. Finally, he enters his car, and he drives away in an erratic manner. You conclude: "There is at least one drunk driver on the road tonight." Let us call this as proposition *p*. Moreover, let us further suppose the following: (1) that the man you saw was not in fact drunk (he was drinking water from the bottle in the bag). Rather, he suffers from a neurological problem that affected his coordination and speech; and (2) that although you are ignorant of this fact that at approximately the same time an intoxicated individual left another bar and began driving while drunk.

Now the question becomes, do you actually know *p*? Two conditions for knowledge are clearly met: (1) *P* is true (due to the second individual) and (2) you believe *p*. It also seems that you are justified in believing *p*, because normally witnessing such evidence (someone exiting from a bar, slurred speech, abnormal gait, coordination problems, drinking from a brown paper bag, erratic driving) conjointly are excellent grounds for believing *p*. However, the Gettier counterexamples are designed to show that justified true belief are not sufficient conditions for knowledge. For example, the claim would be that you do not know *p* because your justification for knowing *p* is based on a false premise, namely that the man you saw is the drunk driver.

Responding to the Gettier counterexamples has consumed a lot of time and energy in contemporary epistemology. One response that has a fair number of adherents is that the Gettier counterexamples demonstrate a need for a fourth condition for knowledge. One such proposed condition is the following:

> There is no true proposition *Q* such that if *Q* were added to the individual's beliefs then he would no longer be justified in believing *p*.

*Q*s are known as "defeaters," and such analyses have come to be called *defeasibility analyses*. The defeater in our example is, "The man I saw is not a drunk driver."

We end our brief exposition of some of the major moves in epistemology and turn now to an explication of the first major account of science: logical positivism. These epistemological problems though serve as an important context and background for this discussion.

Logical Positivism

Historical Sketch

Logical positivism began as a philosophical movement in the 1920s as a strong reaction to idealist *metaphysics*. Its geographical origins were both in Berlin (the Berlin circle) and in Vienna (the Vienna circle). Some of the major names in this movement were Rudolf Carnap, Moritz Schlick, Carl Hempel, Hans Reichenbach, and A.J. Ayer. Logical positivists were also heavily influenced by the physicist/philosopher Ernst Mach and the philosopher Ludwig Wittgenstein. Many of the logical positivists were Jewish, and with the rise of Hitler in Germany and the beginning of World War II, many of these individuals emigrated to the English-speaking world—particularly to the United States and England.

Philosophical Background

Idealistic Metaphysics

In the nineteenth century, there were a number of philosophers who wrote on what is commonly called metaphysics. *Metaphysics* is sometimes defined as the branch of philosophy attempting to study the fundamental nature of being and the world. It attempts to ask, what *kinds* of things exist—for example do abstractions such as "red" exist or do only concrete red things exist? What kind of thing is "redness" or "threeness" and how do these sorts of things differ from a particular instantiation—say as specific red wagons and red apples? Do Gods exist? How about other minds? If we were to take an inventory of all that exists—what *kinds* of things would be found?

However, some of the writings of these philosophers were a bit obtuse. Heidegger (1959) infamously claimed, "the nothing nothings." Other metaphysicians stated things like "Spirit is the principle of the world" or "God is tripartite" (note that a large part of religion is, on this definition, metaphysics. Part of the controversy regarding logical positivism is its dismissal of religious claims as meaningless).

A question can arise—"Is the claim that the Nothing nothings, true or false?"; "Is God tripartite or not?"; the logical positivists claimed, however, that there is a prior question to questions of truth and falsity. Truth and falsity are properties only of *meaningful* indicative sentences. Meaningless sentences are neither true nor false—they are simply *meaningless.* If I claim "Green ideas sleep furiously," it is nonsense for you to say that this claim is true and equally nonsensical for you to say that this claim is false. The only proper reaction is for you to say: "Your utterance is meaningless!" The positivists thought many philosophical problems were due to "language gone on holiday." Or to use (Wittgenstein's 1967) colorful phrase, "Philosophy is a battle against the bewitchment of our intelligence by means of language." The positivists claimed that we do not have to spend a lot of time trying to figure out if the nothing nothings—because such metaphysical statements are meaningless. Their project, to use the title of one of Carnap's essays, was, "the elimination of metaphysics through the logical analysis of language."

This view that metaphysical claims are nonsense was not entirely new to them. The British empiricist David Hume (1797) had stated previously:

> When we run over libraries, persuaded of these principles, what havoc must we make? If we take in our hand any volume; of divinity or school metaphysics, for instance; let us ask, *Does it contain any abstract reasoning concerning quantity or number?* No. *Does it contain any experimental reasoning concerning matter of fact and existence?* No. Commit it then to the flames: for it can contain nothing but sophistry and illusion.

A somewhat more technical version of this notion is provided by the logical positivist Schlick (1932):

> A proposition which is such that the world remains the same whether it be true or false simply says nothing about the world; it is empty and communicates nothing; I can give it no meaning (p. 88).

Let us examine a specific application of Carnap's principle. If I assert "God is tripartite" and I want to test it for its meaningfulness, the logical positivist would have to be ask the question—"How would this make a difference to my observations of the world?" "What observations could I make if God were one versus if God were not one?" If one cannot identify any such observations, then the sentence is actually meaningless. Metaphysicians were for the positivists, "musicians without musical ability."

The Verifiability Principle

Going for even a bit more precision, according to Schlick (1932):

> A statement is meaningful if and only if it can be proved true or false, at least in principle, by means of the experience—this assertion is called the verifiability principle [aka the "verifiability criterion of meaning"]. The meaning of a statement is its method of verification;

that is we know the meaning of a statement if we know the conditions under which the statement is true or false. When are we sure that the meaning of a question is clear? Obviously if and only if we are able to exactly describe the conditions in which it is possible to answer yes, or, respectively, the conditions in which it is necessary to answer with a no… a statement has a meaning if and only if the fact that it is true makes a verifiable difference.

He concluded:

Metaphysical statements are not empirically verifiable and are thus forbidden: they are meaningless. The only role of philosophy is the clarification of the meaning of statements and their logical interrelationships. There is no distinct "philosophical knowledge" over and above the analytic knowledge provided by the formal disciplines of logic and mathematics and the empirical knowledge provided by the sciences. Philosophy is the activity by means of which the meaning of statements is clarified and defined Schlick (1932).

Wittgenstein

As mentioned previously, the Austrian philosopher Ludwig Wittgenstein also influenced the logical positivists. Two of Wittgenstein's most influential books are the *Tractatus Logico Philosophicus* and *Philosophical Investigations*. Interestingly, Wittgenstein's views changed dramatically, and scholars often talk about the earlier Wittgenstein of the *Tractatus* and the later Wittgenstein of *Philosophical Investigations*. The logical positivists were influenced by his earlier work. Wittgenstein saw his later work as refuting his earlier work, and in fact, many regard *Philosophical Investigations* as critical in the demise of logical positivism.

Wittgenstein wrote in aphorisms—brief (often very pregnant phrases) and part of the puzzle in Wittgenstein exegesis is both to understand each of these aphorisms and to understand the relations of these to one another. Here are key examples of his statements:

What can be said at all can be said clearly, and what we cannot talk about we must pass over in silence.
The limits of my language mean the limits of my world.—Wittgenstein (TLP, 5.6)
We feel that even when all possible scientific questions have been answered, the problems of life remain completely untouched. Of course there are then no questions left, and this itself is the answer.—Wittgenstein (TLP, 6.52)
(Whereof one cannot speak, thereon one must remain silent.)—Wittgenstein (TLP, 7)

The Analytic/Synthetic Distinction

The philosophical context of the rise of logical positivism also had to do with debates regarding legitimate sources of knowledge: between the *empiricists* (such as Locke, Berkeley, and Hume) who thought that observation was necessary and the *rationalists* who thought that reason alone could produce knowledge (such as Descartes and Leibniz). The German philosopher Immanuel Kant thought that there was a third way. Kant was impressed with science but thought that science provided some knowledge that was so certain that it could not simply be based on observation. As examples of this knowledge, he thought Newton's laws of motion, the principle of causality (every effect has a cause), and Euclidean geometry were based on observations but had a special status in which they were certain to be true. He claimed that there was a third kind of knowledge that he called *the synthetic a priori.*

To understand this distinction, we first have to review a few other distinctions he made.

Analytic statements are true by virtue of their meaning (e.g., "All bachelors are unmarried"; "The brown dog is brown"; and "Tomorrow it will snow or it will not snow").

Synthetic statements: These are not analytic but predicate something about the world (e.g., "Tomorrow it will snow" or "President Obama is 24 years old").

The second class of distinctions is between *a priori* and *a posteriori* statements:

A priori statements: The truth of these statements can be established *without observation*. Examples include "All brothers are male" and "All squares have four sides."

A posteriori statements: The truth or falsity of these statements can only be established *with* observations. Examples include "My foot has 5 toes" and "It is snowing."

Kant's table

	A priori	*A posteriori*
Analytic	All bachelors are unmarried.	
Synthetic	Every event has a cause. 7 + 5 = 12 Euclidean geometry The law of conservation of matter Newton's laws of motion	Tomorrow it will rain.

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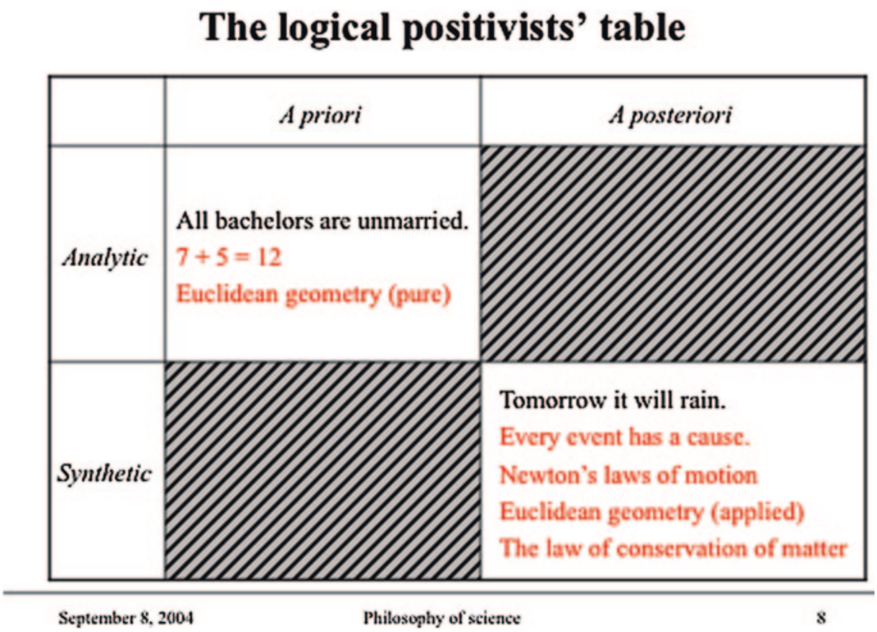
Metaphysicians were most interested in a particular combination of these categories—the *synthetic a priori*—they wanted to discover profound truths about the world without making observations. The logical positivists essentially claimed that *there were no such things as synthetic a priori claims*—when one try to make this kind of claim, one is uttering something meaningless.


The logical positivists' table

	A priori	*A posteriori*
Analytic	All bachelors are unmarried. 7 + 5 = 12 Euclidean geometry (pure)	
Synthetic		Tomorrow it will rain. Every event has a cause. Newton's laws of motion Euclidean geometry (applied) The law of conservation of matter

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www.ln.edu.hk/philoso/staff/.../ps/1%20Logical%20positivism.ppt

Thus, the logical positivists claim that there are no meaningful synthetic a priori statements and hence no metaphysics. Thus, the logical positivists verifiability criterion recognized only two kinds of statements as meaningful: analytic, in which the predicate simply "unpacks" the subject ("Bachelors are unmarried"; "unmarried" is already contained in the subject"), and synthetic statements, that is, observations about the world.

Problems with the Verifiability Criterion

To the credit of logical positivists, they would criticize their own views—particularly the adequacy of the verifiability criterion and attempt to respond to these criticisms by improving it. Here are some of the major criticisms:

1. The verifiability criterion judges some canonical scientific statements to be meaningless. For example, scientific laws seem to become meaningless according to the verifiability criterion. Take the scientific law, "All copper conducts electricity." However, one can never observe *all* copper; hence, one cannot build this statement from observational reports. Hence, according to the verifiability criterion, it is meaningless.

2. The verifiability criterion when applied to itself is judged meaningless. The verifiability criterion itself is neither analytic nor a product of observations. Thus, the core regulative statement relied upon by the logical positivists is actually meaningless! And if the logical positivists allow themselves some meaningless statements, why cannot others—such as the religious?
3. There seems to be no valid inductive logic—no set of valid inference rules which allow observations to entail a larger (*ampliative*) statement such as a scientific law. No number of observed white swans allows one to validly deduce that "All swans are white." (More about this in the next chapter.)
4. The logical positivists needed an account of how perception—taken to be non-propositional—for example the experience of the raw percept of red—can be translated into linguistic terms without error, to form the observations sentences they needed to "support" theoretical sentences and scientific laws.
5. Quine (1951) in his classic, "The Two Dogmas of Empiricism" argued the former point but also argued that the analytic and synthetic distinction was not as clear as the logical positivists needed. Quine argued that the analytic/synthetic distinction was circular. Part of the problem was it relies on an unclear notion of "synonymy" because it relies upon replacing terms like bachelor with terms like "unmarried."
6. Other statements which we take to be meaningful, such as "Genocide is morally wrong," also become meaningless. Let us examine this a bit.

Ethics and Moore's Is/Ought Distinction

What about ethical statements such as "Lying is morally wrong." Are these meaningful according to the logical positivist? Are these analytic statements or verifiable by empirical observations? First, the logical positivists were influenced by Hume's *Is/Ought Distinction* and G.E. Moore's *the naturalistic fallacy*.

The Is/Ought Distinction

Hume's stated (1737):

> In every system of morality, which I have hitherto met with, I have always remarked, that the author proceeds for some time in the ordinary ways of reasoning, and establishes the being of a God, or makes observations concerning human affairs; when all of a sudden I am surprised to find, that instead of the usual copulations of propositions, *is*, and *is not*, I meet with no proposition that is not connected with an *ought*, or an *ought not*. This change is imperceptible; but is however, of the last consequence. For as this *ought*, or *ought not*, expresses some new relation or affirmation, 'tis necessary that it should be observed and explained; and at the same time that a reason should be given; for what seems altogether inconceivable, how this new relation can be a deduction from others, which are entirely different from it. But as authors do not commonly use this precaution, I shall presume to recommend it to the readers; and am persuaded, that this small attention would subvert all the vulgar systems of morality, and let us see, that the distinction of vice and virtue is not founded merely on the relations of objects, nor is perceived by reason.

In some, "ought cannot be derived from is." That is, Hume concludes that the moral "ought" can never be logically derived from any set of descriptive "is" statements. Thus far, it would seem, then, that the logical positivists would view ethical statements as meaningless.

The Naturalistic Fallacy

Moore (1903) also claimed that a philosopher commits a formal logical fallacy when he or she attempts to prove a conclusion about ethics by appealing solely to empirical observable terms. Defining the concept "good," Moore argued, is as impossible as defining the concept "yellow"; yellow is just a simple concept. It is simple in that it cannot be further defined in terms of any other concept (for instance, blue). Yellow is just yellow, and this is as far as one can get when trying to define it. Just so with good. Good cannot be defined or analyzed, particularly with any other natural terms. Again, however, it would seem that the logical positivists' verifiability criterion is not satisfied and ethical statements, although, apparently syntactically correct, are, by the application of the verifiability criterion, found to be meaningless. And just so. The logical positivist, Ayer (1952) stated:

> For we have seen that, as ethical judgments are mere expressions of feeling, there can be no way of determining the validity of any ethical system, and, indeed, no sense in asking whether any such system is true. All that one may legitimately enquire in this connection is, what are the moral habits of a given person or group of people, and what causes them to have precisely those habits and feelings? (Ayer 1952, p. 112).

And further:

> Such aesthetic words as "beautiful" and "hideous" are employed, as ethical words are employed, not to make statements of fact, but simply to express certain feelings and evoke a certain response. It follows, as in ethics, that there is no sense in attributing objective validity to aesthetic judgments, and no possibility of arguing about questions of value in aesthetics, but only about questions of fact. A scientific treatment of aesthetics would show us what in general were the causes of aesthetic feeling, why various societies produced and admired the works of art they did, why taste varies as it does within a given society, and so forth (Ayer 1952, p. 113).

This has been called *the emotive theory of ethics* that ethical statements do not have normal cognitive meaning—there meaning is solely emotional. These are like other emotional utterances"—"Yuck" and "Wow."

Forms of Scientific Explanation: Hempel

The logical positivist Hempel (1970) engaged in a logical explication of the concept of *scientific explanation*. What does it mean to explain something? What does it mean to provide a scientific explanation? Hempel argued that to explain something was to subsume that phenomenon under scientific laws. Hempel thought that there were two types of scientific laws and hence two types of scientific explanation.

1. *Deductive nomological explanation*. These explanations have the form of a deductive argument in which the statement-to-be-explained is the conclusion and the premises contain at least one universal scientific law (also called a nomological). All the premises also have to be true in order to be an explanation. Here is an example: "Why did this oxygen expand when heated?"

   DN Explanation
   (a) Oxygen is a form of gas.
   (b) *All gases expand when heated under constant pressure (the scientific law).*
   (c) Therefore, this oxygen expanded when heated.

   Note that this has the form of a deductive argument, its premises are all true, and the premises contain at least one scientific law: Boyle's law. Thus, for Hempel, this is a successful example of a scientific explanation.

2. *Inductive/statistical explanation*. The second type of explanation is called inductive statistical explanation because the law in the premises is not a universal scientific law but rather a probabilistic law. Again, scientific explanation occurs, and individual events are subsumed under laws—but this time, the laws state probabilities instead of certainties. Also as in DN explanation, all the premises need to be true and the statement-to-be-explained is the conclusion which is deduced from the premises. Here is an example of an attempt to explain why John recovered from pneumonia after taking penicillin:

   Inductive Statistical Scientific Explanation
   (a) John had pneumonia.
   (b) There is a high probability that after taking penicillin, pneumonia will be cured.
   (c) *John took penicillin*.
   (d) Therefore (it was probable that), John was cured.

   This last form of scientific explanation is much more controversial. How probable do the premises have to make the conclusion—more probable than not? Reduce prior uncertainty?

Unity of Science

The logical positivists also thought that all science was one large interrelated edifice. They argued that some sciences were more basic than others—and these less basic sciences could be "reduced" to the more basic ones. The logical positivists thought, for example, that all the laws of biology ought to be reduced to the laws of chemistry and all the laws of chemistry ought to be further reduced to the laws of physics. Psychology for them should be able to be reduced to biology, which can in turn be reduced to chemistry… etc. However, when they attempted such theoretical reductions, they usually failed; they could not reduce, say, Boyle's law to physics. However, it did raise interesting questions: what are the relationships between laws of two different sciences, are they just *sui generis*, or is one more basic than another?

The Demise of Logical Positivism

Because the logical positivists could not come up with a verifiability criterion that was internally consistent, and because of what many regarded as the negative implications of their criterion such as rendering ethical claims as meaningless, the logical positivists eventually faded away. However, some of their legacy is worthwhile. Contemporary analytic philosophy still is focused on an analysis of language in that it typically engages in conceptual analysis, and there is a heavy reliance on symbolic logic. Philosophers and those influenced by philosophers have taken "the linguistic turn" and paid attention to language, meaning, and logic.

Special Topic I: Logical Positivism and Radical Behaviorism

Some quite reputable scholars have argued that B.F. Skinner was a logical positivist or at least that he was so influenced by logical positivism that when logical positivism was falsified, Skinnerian psychology was also falsified. The historian of behaviorism Smith (1986) has suggested that historians such as Koch (1964) have advanced three distinct theses about the affiliation between logical positivisms and radical behaviorism: (a) *the importation thesis*, which states that Skinner imported his philosophy and methodology from logical positivism; (b) *the subordination thesis*, which states that Skinner regarded his psychological views as subordinate to these prior philosophical views; and (c) *the thesis of linked fates*, in which the fate of Skinner's behaviorism was therefore linked to the fate of logical positivism.

Smith argued that these three are all false. Although for the complete case, I would recommend reading Smith's excellent book (1986), and I will give one piece of Smith's refuting evidence for each thesis. Regarding the importation thesis, a review of the historical record reveals that Skinner never spoke positively about the verifiability criterion, never cared to develop a demarcation between meaningful and meaningless statements, never carried out a logical analysis of constructs, and, in short, never extrapolated the central tenets of logical positivism into his psychology. Instead, he developed an indigenous, psychological analysis of epistemology and psychology, where knowledge was the result of conditioning processes producing effective behavior. Skinner did talk about the operational definitions of psychological terms, but he cashed this out in terms quite different than those of the logical positivists; that is, he did not want to define psychological terms intersubjectively. Rather, he called for an analysis of the scientists' verbal behavior to discover environmental variables that govern its emission and effectiveness.

Regarding the subordination thesis, Skinner never viewed his work as subordinate to philosophical concerns. An anecdote is very revealing of his priorities here: When the young Skinner was told by the philosopher Alfred North Whitehead that a psychologist should closely follow developments in philosophy, Skinner replied, "it is quite the other way around—we need a psychological epistemology." And Skinner eventually produced a psychological epistemology. Thus, because the alleged links between logical positivism and Skinner's do not exist, they do not share linked fates.

Finally, the logical positivists took physics as the most important science and the one that should serve as the exemplar for others. They thought that all other scientists should mimic the way physicists were doing science and that all other sciences should be reduced to physics. Skinner, in contrast, thought that biology was the most important science for psychology. Thus, although Skinner was influenced by Mach's biological positivism, he was not influenced and his theory was not derived from logical positivism.

Special Topic II: Epistemic and Philosophical Problems of the APA's Ethical Code

The American Psychological Association (2002), like many professional organizations, has generated an Ethical Code, called "The Ethical Principles of Psychologists and Code of Conduct" (one can review it at http://www.apa.org/ethics/code/index.aspx). Psychologists must adhere to the letter of this code or at least in principle face penalties—including the loss of one's professional license to practice as states have adopted adherence to this code in their state laws. The APA's Ethical Code makes a series of claims such as

> **5.05 Testimonials** Psychologists do not solicit testimonials from current therapy clients/patients or other persons who because of their particular circumstances are vulnerable to undue influence.
> **6.07 Referrals and Fees** When psychologists pay, receive payment from, or divide fees with another professional, other than in an employer–employee relationship, the payment to each is based on the services provided (clinical, consultative, administrative, or other) and is not based on the referral itself.
> **10.06 Sexual Intimacies with Relatives or Significant Others of Current Therapy Clients/Patients** Psychologists do not engage in sexual intimacies with individuals they know to be close relatives, guardians, or significant others

of current clients/patients. Psychologists do not terminate therapy to circumvent this standard.

Your initial reaction might be that these specific claims seem reasonable, and in fact, you might agree with these. However, for the philosopher, initial plausibility is not sufficient; philosophers want to know answers to basic and important questions such as (1) What are the arguments for these ethical conclusions?; (2) How sound are these arguments?; and (3) Are the terms used in these claims clear?

Here are some more specific and, in my humble opinion, tough and problematic questions regarding the APA's Ethical Code:

1. *What does the APA mean by "ethical?"* Are the logical positivists right in that these kinds of statements are not empirically meaningful—that these are just psychologist's emotional utterances concerning certain things? If the logical positivists are wrong, what observations can an empirically inclined psychologist use to understand the meaning of these ethical claims?
2. *What is the case—the arguments—for each of these claims*? Interestingly, the APA simply does not offer **any** arguments for these claims. For example, there is no companion publication that lies out the case for each of these ethical claims. Instead, these claims are presented *ex cathedra*—as proclamations whose truth seems only to be warranted only by an appeal to authority—the authority of the APA (such as it is). The APA seems to be saying, "Do this or don't do this, BECAUSE WE SAID SO!" But psychologists have typically been unwilling to accept the truth of a claim simply based on an appeal to an authority. Why does APA force them into this position with these ethical pronouncements—pronouncements that carry serious punitive consequences for them? Why not publish the arguments for these ethical pronouncements so that all can evaluate the quality of these? This seems particularly important because the APA across versions of its Ethical Code has sometimes dramatically changed its ethical pronouncements (e.g., sometimes claiming that bartering is wrong—sometimes not; sometimes precluding all sexual contact—sometimes not).
3. *What is mean by these terms, for example, "undue influence?"* Are all the terms used throughout the code sufficiently clear or do they hide prejudgments that are none to clear—such as "undue"?
4. *How are deterministic assumptions that often underlie science consistent with ethical assumptions in human free will and choice*? Kant stated that "ought implies can"—that to claim that someone morally *ought* to do something implies that they have a choice and at least is *able* to do this. For example, the ethical claim "You morally ought to learn four foreign languages in one day" is regarded as false simply because this cannot be accomplished. But science often assumes determinism (see for example Skinner 2002). Determinism assumes that there is no "choice"—including moral choices—but rather causes operate and necessitate certain events. Scientific laws also assume this—copper must conduct electricity—it has no choice in the matter. If humans have choice, how can we conduct a scientific study of human behavior and discover scientific laws?

5. *What is the normative ethical account that underlies the ethical reasoning of the APA's ethical pronouncements*? Is the APA relying upon a *utilitarian* moral theory in which the positive and negative consequences of acts are being calculated, and thus, what becomes ethical is the behavior with the best set of positive outcomes and fewest negative outcomes? Are they utilizing a *deontic* ethical theory in which the duties of a psychologist are being explicated, conflicts between duties are resolved, and specific acts are either proscribed or necessitated? Or, are they utilizing a *virtue* ethical theory such as Aristotle's? Unfortunately, the APA is mute on this critical question.
6. *How does the APA's ethical account relate to psychologists' own empirical and theoretical work*? Gilligan (1982) in her famous *In a Different Voice* has suggested that men and women reason in quite different moral ways? Yet the APA's Ethical Code shows no recognition of the work of psychologists on this important issue. Is this a problem? Her mentor Kohlberg (1971) suggested that individuals develop morally and transition from stage to stage. Level II stages represent "conventional morality" in which the individual obeys the standard moral norms in a particular context—in this example, the APA's Ethical Code. But Kohlberg suggested that there were higher "post-conventional" moral stages in which one criticized conventional morality, transcending it and conforms to higher principles of morality relating to universal rights and democratic principles that often require civil disobedience to conventional morality. Thus, do individuals who have developed more morally transcend the APA Ethical Code and behave inconsistent with it—and would not then this, if the psychologist Kohlberg, is correct, actually be a good thing? How is the APA to address this indigenous work? Right now, it seems simply ignore these views.
7. *Is the Ethical Code a problematic attempt to avoid external policing—which actually might do a better job on actually enforcing reasonable standards for the behavior of psychologists*? For example, psychologists often fail to administer evidence-based interventions; they administer assessment devices with problematic psychometrics such as the TAT? (Lilienfeld et al. 2000). Yet in the current system, they usually "get away" with this quite problematic behavior? And their clients suffer. Ought one to be cynical and wonder is this just the function of the APA's Ethical Code: to give the *appearance* of a genuine concern about ethics—while all the while providing a smoke screen to allow the guild to actually get away with a lot of problematic behavior?

These are just a few of the kind of deep and provocative questions a philosopher can ask about the Ethical Code. Philosophers starting with Socrates have sometimes been thought of as *gadflies*—folks that upset the *status quo*—by raising problems that other folks simply do not notice or prefer not to notice. But the philosopher wants her intellectual house in order and wants not to be hypocritical—for example, to claim rationality but then to have serious gaps in this by, for example, irrationally and dogmatically adhering to an Ethical Code that is presented to them purely by authoritarian appeals by their professional organization and state boards.

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Chapter 4
Popper: Conjectures and Refutations

Popper's Falsificationism

Popper has arguably been the most influential philosopher of science in the 20th century. His influence is partly shown by the fact he was knighted—actually Popper in the latter part of his life was known as Sir Karl Popper. To capture the richness and complexity of Popper's thought, I will divide his work into three sections. In the first, section I will discuss his early work on the demarcation question (i.e., how to distinguish science from nonscience), the problems of induction and falsifiability. In the second section, I will present his theory of rationality and the role of his rationality principle in explaining human behavior. In the third section, I will discuss his views on objective knowledge and evolutionary epistemology. Rather than seeing these as inconsistent, some have taken his account as providing successively deeper accounts of knowledge as it proceeds for the first to the third stages (Bartley 1987). Finally, I will examine some of the interrelationships between the work of Popper and the work of psychologists Donald Campbell and Paul Meehl, as well as the work of the prominent philosopher W.V.O. Quine.

We have seen that the traditional account of knowledge was the Platonic conception that it is justified, true, belief. We have seen that the logical positivists and others tried to develop an inductive logic of justification—they tried to develop a truth preserving, *ampliative logic* that "builds" propositions of wider scope ("All copper…") from observations of small scope ("This sample of copper…") Popper disagreed with the entire analysis for reasons different than we have so far discussed. *Popper claims that truth, justification, and belief are not the distinguishing characteristics of knowledge*. In fact, Popper has a radically different idea concerning what kind of thing knowledge is.

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Knowledge is Not Justified

According to Popper, we can never justify propositions or theories for several reasons. One such reason is that we can never subject our theories to *all* possible tests, and therefore, it is always possible that one of the unconducted tests might falsify our theory. In fact, Popper claimed that all the tests that will be conducted will be a very small fraction of all possible tests—so small that what is unknown—the outcomes of all these unconducted tests—overwhelms what is actually observed. For Popper, this does not lead to epistemological nihilism or skepticism because he maintains that we can rationally *criticize* our theories and tentatively hold those that have best survived our criticism. Confirmation is not the best method for arriving at truth but criticism is the best method of *error elimination*. Knowledge, according to Popper, grows only through the correcting of our mistakes. The best way to correct mistakes is to attempt to falsify our beliefs and theories. Theories that survive our attempts to falsify them should not be regarded as "confirmed" because subsequent tests may show them to be false. Passing attempts to falsify a theory is said to *corroborate* the theory.

Knowledge is Not True

Whatever knowledge is, it is not something that is certainly true or even probably true. At best, we can say it has *verisimilitude* (literally, "truth-likeness") because it has survived our attempts to refute it. The survival of our beliefs to these attempts at falsification allows us to say that these "look like the truth"—but are not truth in any absolute, final sense. The eventual falsification of Newton's theory by experiments derived from Einstein's theory is a case that illustrates that even though a theory has survived many tests it should not be regarded as the final truth.

Knowledge is Not Belief

Knowledge is not a matter of subjective belief but rather is *objective* in two ways. First, knowledge claims when evaluated, become objects—the objects of criticism. Second, problems, theories, and arguments exist independently of whether anyone believes these, asserts these, or acts on these. Third, Popper as an evolutionary epistemologist—he took biology as the most informative science for philosophy of science—suggested that evolutionary theory implies that we exist as (real, objective) evolved organisms facing problems of survival and reproduction in a real, objective environment. We can never know "things in themselves"—things independent of our observations, but we can know that the environment tolerates our perceptions, theories, and movements through it, because we are surviving. And, of course, false theories, perceptions, and movements can get us killed. Thus, error elimination is a critical epistemic process according to Popper.

How Did Popper Arrive at These Conclusions?

Popper (1963) stated that in 1919, he became interested in what he called the *demarcation question*, that is, when should a theory be characterized as scientific? He wanted to know what distinguished genuine science (examples for him were Newtonian and Einsteinian physics) from *pseudoscience* (he took Freudian and Adlerian psychology and Marx's theory of history to be pseudosciences). He found that the accepted answer of the time was that science relied upon an inductive, empirical method while pseudoscience did not. Popper, however, rejected this answer because he thought that obvious pseudosciences such as astrology often made appeals to observation and experiment, but still were not properly scientific. Popper stated that his demarcation question became, "What characterizes a genuinely empirical method from a pseudoempirical method?"

His answer is somewhat surprising. He came to believe that the problem with pseudoscientific theories is that *the world was full of verifications of these theories. In fact, these theories were confirmed no matter what happened.* For example, Popper recounted the following:

> As for Adler, I was much impressed by a personal experience. Once, in 1919, I reported to him a case which to me did not seem particularly Adlerian, but which he found no difficulty in analyzing in terms of his theory of inferiority feelings, although he had not even seen the child. Slightly shocked, I asked him how he could be so sure. 'Because of my thousandfold experience,' he replied; whereupon I could not help saying: 'And with this new case, I suppose, your experience has become thousand and onefold' (p. 35).

He took these "confirmations" as confirming only that cases could be interpreted in light of the theory. But he took this to be rather trivial because he thought that every conceivable case could be interpreted in light of these theories. Popper thought Freudian theory was pseudoscientific because it ruled out no observable states of affairs. A neurotic individual with unresolved Oedipidinal conflicts may fear his father (because of castration anxiety), or he may love his father (due to reaction formation), or he may hate his father (due to problems in identifying with him). Thus, any sort of reaction is possible and is "explainable" by the theory. Popper stated that he came to take this apparent strength as in fact exactly the theory's fatal weakness (although see Grunbaum (1985) for an extended and refined account of the falsifiability of Freudian theory).

According to Popper, the impressive aspect of Einstein's theory is that it is inconsistent with certain possible results of observation. It makes *risky predictions*: it states that certain states of affairs cannot happen. An important example for Popper was the Michelson and Morley experiment in which Einstein's theory predicted something different from Newton's theory. Einstein's theory made a risky prediction that ruled out many possible states of affairs. According to

Popper, a theory is scientific if and only if it rules out some observable states of affairs. Otherwise, why look? and why "test" it? The "confirmation" is a foregone conclusion because all possible outcomes are compatible with the theory. Thus, a test of a theory is a legitimate test only if it is an attempt to falsify that theory by seeing if states of affairs it rules out occur or not. If a theory passes such an attempt to falsify it, this is something in its favor.

Thus, Popper recognized that a widespread and important problem with rational belief formation in general, but also unfortunately in wrongheaded views of the scientific method, is what we now call confirmation bias (Kahneman et al. 1982). Admittedly, this problem has been recognized for quite a while; one of the first philosophers to recognize it was the 17th century philosopher Sir Francis Bacon. Bacon once told a story about a church in which sailors, before their journeys, would pray for their safe return. When they did indeed return safely, they would hang in the church a picture of themselves in gratitude for the efficacy of their prayers. Some took all these paintings as indications that prayers do in fact work. However, Bacon (as quoted in Urbach (1982) stated:

> And therefore it was a good answer that was made by one who when they showed him hanging in a temple a picture of those who had paid their vows as having escaped shipwreck, and would have him ay whether he did not now acknowledge to the power of the gods,—"Aye," asked he again. "But where are they painted that were drowned after their vows?" (p. 88).

Thus, every good theory should divide the set of all statements derivable from it into two subsets. One set contains observation statements that are consistent with the theory. This set is uninteresting from an epistemological, and thus research, point of view. However, the complementary set that for every scientific theory should be nonempty is the set of potential falsifiers. Scientific testing consists of efficiently and ardently attempting to see whether one of these potential states of affairs actually obtains.

Let me give a quick and admittedly somewhat quirky example of some of these points. Let's say someone theorizes the following: "New York City is the only place where humans live." Popper would want us to ask, is this theory scientific? And to answer this question by applying this decision rule—is it falsifiable, that is, does it rule out certain possible states of affairs? When we apply this criterion, we find this theory is indeed scientific. What does it rule out?—it rules out people living in any place other than NYC. Thus, if we look and find folks living in Boston, Chicago, or Montreal, the theory would rule out these states of affairs, and thus, when found, would be shown to be false.

One other quick point. Note the striking differences in research strategies between a confirmationist and a falsificationist. The confirmationist (foolishly according to Popper) in attempting to test the theory in question would do so by only looking at NYC. When the outcome of their tests is that indeed humans are living in NYC, they would conclude that these observations "confirm" the theory. Popper said this strategy is entirely wrongheaded, wasteful, and simply fails to be a real substantive test of the theory. Popper's Falsificationism advises a quite different strategy. He suggested that the researcher ask, "What does the theory rule out—what observable states of affairs are inconsistent with the theory?" Thinking about this for a moment, we would find that this theory rules out a human residing in Boston, Chicago, San Francisco, or indeed any place that is not NYC. So what would the research strategy be to properly test this? According to Popper, the researcher ought not to look in NYC at all—instead the researcher ought to direct her energies in observing these other places and if she finds humans there, she has done a great job as a researcher because in falsifying her initial theory, she has eliminated error from her beliefs. And that is the real function of science—error elimination.

Popper pointed out that theories can differ on the degree to which they are potentially falsifiable. Theories that make point predictions (e.g., the average IQ of females is 106.4) are extremely falsifiable because their sets of falsifiers include as elements all points except the particular point predicted by the theory in this instance. In general, the more precise the statement is, the more falsifiable the statement is. Furthermore, the statement, "All humans are aggressive" is more falsifiable than the statement, "All women are aggressive" because it excludes states of affairs (unaggressive men) that the second statement does not. In general, the more universal the statement, the more falsifiable the statement is. (Existential statements—"There is a Santa Claus," unfortunately are not falsifiable, as one cannot observe all possible space-time points.) Some theories, while technically scientific—because they do rule out some observable states of affairs—are problematic because they don't rule out many. For example, "Some people will show some improvement with therapy *x*" really only rules out the proposition "No one will get better with therapy *x*." This outcome seems to be a success for Popper's theory: Initially we would think that a paradigmatic scientific statement would be precise and would be very general or even universal, and this is just what Popper's falsifiability criterion also entails.

Popper also valued *severe testing* in which we attempt to deduce the most improbable consequences of our theory and check on whether these obtain. The general notion is that if one wants to falsify the claim that "Priests don't swear," it's better to observe them at a golf course than in a pulpit. As another example, if a clinician is testing her theory that "Treatment *x* always cures depression," it is a more severe test to treat severe depression and complex cases, than easy cases; it also is a more severe test to have more stringent criteria of "cure" than less stringent ones.

Popper noted that there is an inverse relationship between what he calls *the logical probability of a statement* and its *degree of falsifiability*. That is, tautologies (aka, analytic statements) such as "All brown dogs are brown" have a logical probability of 1 (they are necessarily true), but these tautologies have a zero degree of falsifiability because they exclude no observable states of affairs. Conversely highly falsifiable claims have a low logical probability: because they exclude many possible states of affairs and thus it is not logically probable that they are will not be refuted. The statement "All objects near the earth accelerate at 9.8 m/s^2" is both initially highly improbable and highly falsifiable. Another way of saying this is that tautologies have no *empirical content* and highly falsifiable statements have high empirical content. As Magee (1973a) asserts:

> It is not truisms which science unveils. Rather, it is part of the greatness and the beauty of science that we can learn, through our own critical investigations that the world is utterly different from what we ever imagined—until our imagination was fired by the refutations of our earlier theories (p. 37).

Science as Problem Solving

Popper (1963) claimed that "the history of science should be treated not as a history of theories, but as a history of Problem-Situations" (p. 177). According to Popper, problems are the originating source of all scientific inquiry and the products of science—theories—an only be understood in relation to their *Problem-Situations*. Good inquiry influences these problems to evolve into different and perhaps deeper problems. Popper schematically represents the growth of knowledge as follows:

$$P_1 \rightarrow \mathrm{TS}_1 \rightarrow \mathrm{EE} \rightarrow P_2.$$

An initial problem (P_1) gives rise to a tentative solution (TS) which gives rise to error-eliminating tests (EE) which give rise of a new problem (P_2). Inquiry, for Popper, begins and ends with problems.

Popper's view is in sharp contrast to the views of the logical positivists and other inductivists in which the good scientist is thought to start with no point of view or interest. The good scientist is supposed to be an unbiased receptor of all experience, and the story goes, if the scientist is a bit lucky as well as unbiased relationships will reveal themselves in these data. Popper rejects the view of the scientist as a passive recipient of sense perception and what he sometimes calls "the bucket view of the mind." He believes that a search light is a more apt metaphor in that he claims the scientist always has an interest, a point of view, a problem—and indeed must in order to know when, where, and how to observe. Popper stated:

> The belief that we can start with pure observations alone, without anything in the nature of a theory is absurd… Twenty five years ago I tried to bring home the same point to a group of physics students in Vienna by beginning a lecture with the following instructions: 'Take pencil and paper; carefully observe, and write down what you have observed. They asked of course, *what* I wanted them to observe… Observation is always selection. It needs a chosen object, a definite task, an interest, a point of view, a problem' (Popper 1963, p. 46).

Popper argued that problems must come before observation and data collection otherwise we have no way to decide what, when, and how to observe among the myriad possibilities.

There are many different kinds of problems in which the scientist may be interested. These can range from questions concerning particular matters of fact (e.g., "What is the surface temperature of Venus?"), to more general questions of fact (e.g., "What is the incidence of child sexual abuse in the United States?"), to questions of cause ("What causes an individual to sexually abuse a child?") to deeper questions of cause ("Why do males abuse much more frequently than females?"). Koertge (1980) suggested that problems arise for a variety of reasons,

> Scientific problems arise when our expectations are violated, when what we consider to be regularities call for a deeper explanation, when two previously disparate fields look as if they could be unified, or when a good scientific theory clashes with our familiar metaphysical framework (p. 347).

The last part of this quote is particularly interesting, given the logical positivist's claim that metaphysics is meaningless. Metaphysics is meaningful (although not testable) according to Popper, but for Popper, metaphysics can be important in that problems can arise when our metaphysically conditioned expectations are violated. I have argued elsewhere (O'Donohue 1989) that psychological research and clinical practice are influenced by metaphysics not only in problem formation, but also in determining what is a "plausible" hypothesis, in determining what "plausible" hypotheses need to be ruled out in one's research design, in the assumptions of our measurement procedures, and in revising our theories and hypotheses.

Popper believed that philosophy and science are intimately interrelated. He (1963) claimed,

> There is at least one philosophical problem in which all thinking men are interested: the problem of understanding the world in which we live; and thus ourselves (who are part of that world) and our knowledge of it. *All science is cosmology*, I believe, and for me the interest of philosophy, no less than of science, lies solely in its bold attempt to add to our knowledge of the world, and to the theory of our knowledge of the world (p. 136, italics added).

According to Popper, scientific problems are often descended from philosophical problems. Moreover, Popper claims that philosophy can also provide some tentative solutions, although often these are quite "hazy" (Popper 1963, p. 38). For example, Democritian atomism preceded modern atomic theory. Other precursors to falsifiable scientific theories include theories of terrestrial motion, the corpuscular theory of light, and the fluid theory of electricity (Popper 1968, p. 278).

While scientific problems and conjectures may derive from philosophical problems and conjectures, they may also be primarily based on other scientific considerations. However, according to Popper (1963), significant philosophical problems "are always rooted in urgent problems outside philosophy, and they die if these roots decay" (p. 72). Popper engaged in revisionist history and claims that Plato's theory of forms was a response to the mathematical discovery of irrational numbers, while Kant (2011) wrote his *Critique of Pure Reason* in an attempt to answer the question, "How is pure natural science possible?" because of Newton's unprecedented attainment of such knowledge.

Philosophy differs from science, however, because its tentative solutions are only criticizable and not falsifiable. That is, because philosophical positions do not rule out any observable states of affairs these cannot be falsified, but these can be criticized based on arguments that appeal to such things as logical fitness, plausibility, and the relationship to contemporary science. Popper suggested that a good *philosophical method* proceeds as follows. First, the problem is defined as clearly as possible and why it is interesting and important is elucidated. Deficiencies in previous definitions of the problem are also addressed. Second, previous attempted solutions are clarified and criticized. Third, one's own proposed solution is clearly described and it is shown how this account solves the original problem. One should also state conditions under which one would abandon this account. Finally, one attempts to criticize one's own solution, although Popper admitted that critics often can do a better job of this.

We will have a bit more to say on Popper's view of problems in the sections on situational logic and evolutionary epistemology. We will see that for Popper, Problem-Situations have a central role in explaining human behavior and ultimately in explaining life itself.

The Problems of Induction

Popper rejected the received answer of this time that the distinguishing feature of science is that it relies on an empirical, *inductive method*. Later, we will examine issues relating to the empirical character of science. In this section, we will attempt to understand what induction is and what its problems seem to be.

Induction is usually taken to be a valid form of reasoning distinct from deduction. Deductive reasoning is taken to be demonstrative. That is, the conclusions of sound deductive arguments are necessarily true. Valid deductive arguments are always *truth preserving*. That is, starting from true premises valid deductive inference rules always generate only true conclusions. However, deductive reasoning is *nonampliative*, that is, it is not content increasing as deductions simply unpack what is already contained in the premises. For example, in the following deductive argument:

1. All men are mortal.
2. *Socrates is a man*.
3. Therefore, Socrates is mortal.

The conclusion is implicitly contained in the premises because to establish that all men are mortal one must have established that a member of this set, Socrates, is also mortal.

Many have taken deduction's nonampliative character as a sure sign that science does not—cannot—rely on deduction. The argument is that because science seeks new information, new knowledge, it must rely on *ampliative* reasoning. Science attempts to produce laws or theories that hold for all parts of space and time without restriction—even to parts that we have not yet observed. Moreover, if science attempts to determine whether the consequences of a theory are true, deduction seems to be inadequate. That is, the logic of research cannot be as follows:

1. If my theory is true, then I ought to observe such and such.
2. *I observe such and such.*
3. Therefore, my theory is true.

Because it is a well-known logical fallacy known as *affirming the consequent*, to be able to better see that it is a fallacy let us take a simpler but analogous example:

1. If it is raining, then the streets are wet.
2. *The streets are wet.*
3. Therefore, it is raining.

This is logically fallacious because the streets might be wet for other reasons than due to rain, for example, they might have been recently washed. Because we have found an example in which the premises are true but the conclusion is false, we have shown that this is not truth—preserving and therefore not a valid deductive argument.

Moreover, as Russell (1985, 1998) pointed out one cannot argue "backward" from the truth of the conclusion to the truth of the premises. For example, in the following argument, the true conclusion does not establish the (obviously) false premises.

1. Males are not mortal but females are.
2. *Sigmund Freud was a female*.
3. Therefore, Sigmund Freud was mortal.

Induction is taken to be an ampliative and *nondemonstrative* form of reasoning. That is, conclusions of inductive arguments contain more information than their premises contain, but these conclusions are nondemonstrative because at best these are only *probably* true, that is, they may still be false. The key problems becomes rationally determining the exact probability, especially how probability chances with new evidence.

For example, notice the following about the conclusion of the following inductive argument: (1) its scope includes more than the scope of the premises (the conclusion refers to a previously unexamined individual); and (2) even if we take the premises to be true and the logical form of the inductive argument to be valid, the conclusion still might be false:

1. 90 % of depressed individuals are on thin reinforcement schedules.
2. *Dave is a depressed individual (Dave was not examined to establish 1)*.
3. Therefore, Dave is probably on a thin reinforcement schedule.

The "problem of induction" began to concern philosophers in the 19th century. Induction raises the following questions: "Are inferences from the observed to the unobserved logically justifiable?" "Do observed facts give us sound evidence for conclusions about situations that we have not observed?" Or more to the point, "How do you know that the existence of present regularities provides any evidence at all that the future will be similar to the past?"

The 19th century philosopher David Hume argued that *there are no demonstrative inferences that are also truth preserving*. He noted an interesting paradox: We cannot justify the inference deductively, because then it would be nonampliative.

And if we try to justify it nondemonstratively (for example because in the past it has worked, or because the probability of it working is high), we are begging the question—we are making appeal to the very principle we want to justify! This is known as *the problem of induction*. It is a dilemma—if we want to justify inductive inferences, we cannot do it inductively because this leads to an infinite regress; we cannot do it deductively because then it would not be an ampliative (an inductive!) inference—and there appears to be no third way.

Hume attempted to save induction by suggesting that although it had no logical justification that it was essentially a psychological law—given consistent association or constant conjunction, humans tend to expect that what they are consistently observing is a regularity and that this regularity will continue to apply in the future. (After all, I expect this because I have seen that other regularities have held in what was then the future).

However, Popper rejected this move to psychological induction. Popper argued that the kind of repetition envisaged by Hume can never be perfect: the cases he has in mind cannot be cases of perfect sameness; they can only be cases of similarity. (For example, each time we turn on a switch, our movements are slightly different). Thus, these are repetitions only from a certain point of view. (What has the effect upon me of a repetition may not have this effect upon a spider). But this means that, for logical reasons, there must always be a point of view—such as a system of expectation, anticipations, assumptions, or interests—before there can be any repetition, which point of view, consequently, cannot be merely the result of repetition. Popper stated:

> We must replace, for the purposes of a psychological theory of the origin of our beliefs, the naive idea of events that are similar by the idea of events to which we react by interpreting them as being similar…For even the first repetition-for-us must be based upon similarity-for-us, and therefore upon expectations—precisely the kind of thing we wished to explain (pp. 444, 445).

Hume also pointed out that any number of singular observations does not entail a universal statement. That is, observation of a thousand, a million, or even several million white swans does not entail the truth of the statement, "All swans are white" because it is logically possible that some as yet to be observed swan will turn out not to be white.

A response to this problem has been that although no number of observations conclusively confirms a universal statement, these observations allow an assignment of *some degree of (increased) probability* to the statement. In this view, the degree of probability is raised upon each confirming instance. Moreover, with many confirming instances, the statement becomes probable to a degree that is indistinguishable from certainty. However, Popper argued that universal laws have a very large or even an infinite number of consequences. Therefore, assessing the probability of a universal statement by comparing the number of tested and confirmed instances to the number of possible tests will always result in a probability of 0 or near 0. Therefore, according to Popper, false theories and well-confirmed theories will have equal probabilities.

Three Key Paradoxes of Induction

Other philosophers pointed to still more problems with induction. Kyburg (1961) raised a further problem with the probabilistic interpretation of induction known as the “lottery paradox.” Suppose there are 100 lottery tickets numbered consecutively from one to one hundred and that in a fair drawing one is chosen. Now let us consider the ticket numbered “1.” The probability that it is the winner is 1/100. Moreover, this entails that the probability that another ticket was drawn is 99/100. Assuming that .99 is a sufficiently high probability to confirm the conclusion that some other ticket is drawn; let us infer from this that some other ticket was drawn. (Notice that the probability of .99 is not essential to our argument. If one insists upon a higher probability for confirmation, all we need to do is to construct an example with more lottery tickets.) Now let us consider the ticket numbered “2,” by the same reasoning we conclude that some other ticket was drawn. We can use this same reasoning for tickets numbered 3, 4, 5… 99. In each case, the conclusion that some other ticket was drawn seems to be confirmed by its high probability. However, this set of conclusions is inconsistent with our knowledge that one ticket was drawn. Therefore, we cannot argue that something is the case simply because it has a high probability of being so.

Hempel’s (1965) *paradox of the ravens* points out a further problem with induction. “All ravens are black” is logically equivalent to the proposition, “All nonblack things are nonravens.” The second proposition can be deduced from the first using the logical law known as the law of contraposition. The law of contraposition states that “All A’s are B’s” is logically equivalent to “All nonB’s are nonA’s.” Because these two propositions are logically equivalent, evidence that confirms one must confirm the other. Therefore, the observation of a yellow pencil—a nonblack thing that is a nonraven—would appear to confirm the hypothesis that all ravens are black.

Similarly, “All ravens are black” is logically equivalent to “Every object is either black or not a raven.” Thus, “All ravens are black” seems to be confirmed by any black object (whether a raven or not) as well as by any nonraven (whether black or not). Critics of induction have taken these examples to show that certain logically proper “confirmations” seem to be substantively irrelevant.

Finally, Goodman’s (1975) “grue-bleen paradox” suggests that another problem of induction is that any finite number of observation statements is consistent with an indefinitely large number of different explanatory theories. Goodman defines two colors “grue” and “bleen” as follows. An object is “grue” if it is green and the time is before the end of the 21st century, and if it is blue thereafter. An object is “bleen” if is blue prior to the end of the 21st century and green thereafter. Now, the observation of green emeralds is taken by many to confirm the hypothesis that all emeralds are green. However, since it is before the end of the 21st century, the observation of a green emerald is also consistent with the hypothesis, “All emeralds are grue.” (The logician Henry Kyburg has suggested that this is one of the most urgent problems in epistemology because we have only a few decades to solve it.)

Some have responded that Goodman's example is artificial because it contains an arbitrary reference to a particular point in time. However, Goodman has pointed out that if we first adopt the grue-bleen terminology, then we would define "green" as "grue before the end of the 21st century and bleen thereafter" and "blue" as "bleen prior to the end of the 21st century and grue thereafter." Goodman asks whether there is any reason beyond historical accident to prefer our usual color words over his grue-bleen terminology?

Modus Tollens and the Duhem-Quine Thesis

Because of what Popper took to be the insurmountable problems associated with induction, Popper boldly conjectured that *there is no such thing as induction*. For Popper, this presented no real problem for science because Popper claimed that science relies on a deductive method. That is, in order to test theories, Popper claimed that scientists *deduce* observational consequences from theories and initial conditions. The logic of research may be represented as follows:

1. Theory or law
2. *Statement of initial conditions*
3. Therefore, observation statement.

Remember that it is a logical fallacy to reason from the *truth* of the observation statement consequence to the *truth* of the theory or law. However, Popper pointed out that reasoning from the *falseness* of the observation consequence to the f*alseness* is logically valid. This inference is known as *modus tollenss* and may be schematized as follows:

Modus Tollens

1. If A, then B.
2. *Not B*
3. Therefore, not A.

Here's a simple example:

1. If it is raining, then the streets are wet.
2. *It is not the case that the streets are wet.*
3. Therefore, it is not raining.

Moreover, Popper noticed a further logical asymmetry between verification and falsification. Although no collection of particular observation statements entails a universal statement, a single observation statement is sufficient to falsify a universal statement. That is, the following is a valid logical inference (sometimes known as quantifier negation). "All x are y" is logically equivalent to "There does not exist an x that is not y." Therefore, evidence which falsifies one must falsify the other. An example:

1. All swans are white $\leftrightarrow$ (is logically equivalent to) There does not exist a swan that is not white.
2. *There is a black swan.*
3. Therefore, it is not the case that "All swans are white."

Popper relied on these valid inference rules to provide the basis upon which science may be rescued from the logical impasse brought about by induction and provided with a sound logical foundation. However, a further logical difficulty arises and it is debated to what extent Popper had recognized this difficulty before it was stressed by his critics and to what extent this difficulty damages his theory of knowledge.

The Duhem-Quine Thesis

This difficulty is known as the *Duhem-Quine thesis*, named after the French physicist, Pierre Duhem, and the prominent American philosopher Wilfred Van Orman Quine (more about him later). Both of these individuals stressed that due to the number of what may be regarded as auxiliary propositions (Aux) that are involved in research, the actual logic of research is as follows:

1. If Theory and Aux_1 and Aux_2 and Aux_3… and Aux_n, then Observation.
2. *Not Observation.*
3. Therefore, Not (Theory and Aux_1 and Aux_2 and Aux_3…Aux_n)
4. Therefore, Not Theory or not Aux_1 or not Aux_2 or not Aux_3 or not $Aux_{n.}$

This is a valid logical argument. But notice the ultimate conclusion is rather indecisive. Instead of having the arrows of *modus ponens* decisively falsifying our theory under test as Popper would wish, the conclusion simply states that some proposition involved in the deduction is false. But it does not tell us which. Logic can no longer be the guide. We are logically free to attribute blame to any one or any set of propositions. (This is also why some prominent philosophers of science such as Brown (1988) and Quine are *pragmatists* and believe that practical judgment is a key to the actual practice of science—the scientists must wisely choose how to distribute the arrows of *modus tollens* among these possibilities. The scientist must not prematurely rule out a theory, but also must not be too stubborn and resist pointing the arrows of modus tollens to favored theories).

This is an extremely dangerous state of affairs to the whole notion of "testing" because it can defeat the very purpose of testing. We can always "save" our theory by simply attributing blame for a prediction failure to one of these auxiliary statements. Although we will discuss Quine's views in a later chapter, Quine argued that this logical ambiguity suggests that the statements of science cannot be tested one by one. Rather Quine (1969) asserted that "The unit of empirical significance is the whole of science" (p. 42). Further:

> The totality of our so-called knowledge or beliefs, from most casual matters of geography and history to the profoundest laws of atomic physics or even of pure mathematics and logic, is a man-made fabric which impinges on experience only along the edges. Or, to change the figure, total science is like a field of force whose boundary conditions are experience. A conflict with experience at the periphery occasions readjustments in the interior of the field. Truth values have to be redistributed over some of our statements. Reevaluation of some statements entails reevaluation of others, because of their logical interconnections—the logical laws being in turn simply certain further statements of the system, certain further elements of the field. Having reevaluated one statement we must reevaluate some others, which may be statements logically connected with he first or may be the statements of logical connections themselves. But the total field is so underdetermined by its boundary conditions, experience, that there is much latitude of choice as to what statements to reevaluate in the light of any single contrary experience. (pp. 42–43).

This has become to be known as *Quine's holism thesis,* and we will have more to say about this in a latter chapter.

Moreover, Quine suggested that there are six virtues of hypotheses and these should be kept in mind when deciding how to revise a web of belief when an anomaly presents itself:

1. *conservatism*—preservation of prior beliefs
2. *modesty*—use of familiar terminology
3. *simplicity*—lack of unnecessary information
4. *generality*—applicable to a wide range of events
5. *refutability*—capable of being disproved
6. *precision*—statement of clear, distinct boundaries.

These desirable properties sometimes must be traded off; however, they are the key in making wise, practical decisions about how to modify the web of belief when it is faced with anomalous experience that shows that somewhere in the web, there is at least one false belief.

Popper's Three-World Metaphysics

Popper was an objectivist and realist—there is an outside world (partly because this is what evolutionary theory claims). He thought there were three broad kinds of things: Popper divided the kinds of things that exist into three "worlds." "World 1" is the ordinary world of concrete things such as chairs and human bodies—the world that materialists say is all that there is. Popper's "World 2" is the world of consciousness and minds—the world that idealists say is the only real one. Popper offered this second world because the thought of a chair is a different kind of thing than the actual chair—the thought for example does not have a specific location—one cannot point to it, in the way that a chair does. Dualists, of course, say that both of Popper's first two worlds exist. Popper added a third world, the world of objective problems and objective knowledge. In this third world, Popper does not include writings on paper that are stored in libraries (because these are World 1 objects) nor does he mean the subjective consciousness of the meanings of these marks in the minds of scholars thinking about them (this is World 2) but the problem itself and the knowledge itself inhabits this third world, which is said to "exist" independently of being known by a conscious subject. Thus, Popper thought, for example, that problems such as, "How do I calculate the hypotenuse of a triangle?"—have an objective existence. They are objects that can be responded to, modified, etc. And Pythagoras's answer to this problem also has an objective existence—and it too can be criticized and modified.

The Rationality Principle and Objective Knowledge

Popper threw a bit of a monkey wrench into his account of science when he thought the social sciences ought to proceed quite differently than the natural sciences. Popper argued that when attempting to explain human behavior, one should analyze people's Problem-Situations, their aims, their theories about what they view as open possibilities for them, and their evaluations of each of these options. For Popper, it is the ability of individuals to react reasonably to their Problem-Situations and to respond to criticisms that make them rational and autonomous. The rationality principles state: "Agents always act appropriately to their situations." Explanations of human behavior again have the form of a deductive argument (Koertge 1980):

1. *Description of the Problem-Situation*: Agent A thought he was in Problem-Situation of Type C.
2. *Analysis of the Problem-Situation:* The result of appraising C the appropriate thing to do is X.
3. *Rationality Principle: Agents always act appropriately to their situations.*
4. Therefore, A did X.

Let us examine an example. Suppose we want to explain why Bob went to the store to buy food. Popper would have us use the following premises:

1. *Description of the Problem-Situation*: Bob's Problem-Situation is that he is out of food.
2. *Analysis of the Problem-Situation*: Bob's analysis is that a reasonable way to resolve the problem is to buy groceries at the supermarket.
3. *Rationality Principle: Agents always act appropriately to their situations.*
4. Therefore, Bob went to the supermarket to buy groceries.

Popper thought that the social scientists ought never to abandon the rationality principle, instead they should revise the other premises. For example, even in explaining odd or crazy behavior, one can still use the rationality principle. For example, in explaining why Ted has lined his house with aluminum foil, the psychologist can formulate the following deductive argument:

1. *Description of the Problem-Situation*: Ted's problem is that he believes that the FBI is trying to read his thoughts.
2. *Analysis of the Problem-Situation*: Ted's analysis is that aluminum foil can block the FBI's ability to detect his thoughts.
3. *Rationality Principle*: *Agents always act appropriately to their situations.*
4. Therefore, Ted lines the walls of his house with aluminum foil.

Thus, there is a kind of rationality in Ted's actions: whether you accept his definition of his Problem-Situation and his formulation of remedies. Popper thought it was a strength of his rationality principle that could be used to describe even odd and unexpected behavior. However, it does place the social scientists in a bit of an odd situation: other scientists ought to behave in a way in which they generate initial theories, conduct severe tests to falsify these, and revise these—but scientists such as psychologists ought to do no such thing. Psychologists construct deductive arguments using the rationality principle.

Criticisms of Popper's Views

We will see more criticisms of Popper in the following chapters as Kuhn, Lakatos, Feyerabend, and Laudan all reacted critically to his views. However, as a brief overview here are some of the major criticisms:

1. Popper's account is just not consistent with the historical record of science—this is not the way actual scientists behave—even in successful episodes of science, scientists are not attempting to falsify their theories. Darwin, for example, in his voyages was trying to confirm and deepen this evolutionary theory, not falsify it.
2. The Quine-Duhem problem shows that falsifications are actually not logically possible. It does show some belief is wrong, but it provides no logic on how to distribute truth values.
3. For Kuhn, Popper's account of science does not capture the revolutionary episodes in it but rather is a more cumulative in which later theories are in some ways natural extensions of prior theories. Kuhn also claimed that in conducting "normal science" anomalies to theories are observed but scientists do not view their theories as falsified; they live with these (by ignoring them; or revising the theory a bit); until a new theory comes along (in a scientific revolution) that is generally more consistent with these anomalies.
4. Philosophers of science such as Lakatos stated that Popper fails to capture the theoretical competition within science—that theories are competing against other theories and are appraised on a number of comparative dimensions. Also Lakatos thought that all propositions in a theory are not created equal—some are protected from the arrows of modus tollens (the hard core); while others are not. Popper failed to capture this complexity.

5. A fifth criticism can be leveled regarding Popper's two accounts for science: one for the human sciences and another quite different one for the natural sciences. The rationale for such as distinction and the inapplicability of his falsificatory evolutionary epistemology for psychology is not well developed by him.

Popper's Evolutionary Epistemology

Evolutionary epistemology (EE) is a naturalistic account of human knowledge that developed within the last half of the twentieth century. The goal of the evolutionary epistemologist is to explain the emergence of some cognitive or perceptual feature (e.g., stereoscopic vision) and examine its impact on, and implications for, human knowledge. Knowledge, and the growth of knowledge, is seen as the instantiation of a selectionist procedure in which proposals (e.g., thoughts, theories, ideas, hypotheses, etc.) are presented to the environment and only some continue to survive. EE further seeks to address, and potentially overcome, the many problems historically associated with more traditional theories of epistemology (e.g., Plato's justified true belief formulation). EE is the thesis that cognition, behavior, and the physical structures implicated in knowledge develop out of natural selection processes, articulated in evolutionary biology, and thus, epistemology becomes *naturalized*; its traditional questions are answered by science.

Evolutionary epistemology differs from traditional epistemology in that it takes into account empirical findings regarding human cognitive capacities, as well as the natural selection processes that led to these capacities. There are other important differences:

- Traditional epistemology was a prescriptive endeavor—it attempts to tell the would-be knower what to do–while evolutionary epistemology is largely descriptive—it just describes what knowledge is and how it actually comes about.
- Traditional epistemology relied on self-evident certainty as a starting point for knowledge (e.g., Descartes) while evolutionary epistemology settles more pragmatically on reliable beliefs as the product of trial and error.
- In traditional epistemology, knowledge is entirely verbal (e.g., in order to satisfy Plato's "justified true belief" account) while evolutionary epistemology allows for the potential of nonverbal, and even *physically embodied, knowledge*—teeth, for example, are regarded as embodying certain kinds of knowledge of the environment.

Naturalizing Epistemology

Naturalistic Epistemology

Naturalistic epistemology is an approach to the study of knowledge that is broadly compatible with, and informed by, our scientific knowledge of how the perceptual and information processing systems of humans and other animals have developed over the course of evolution and currently function. Naturalized epistemology assumes that human knowers and human knowledge are part of the natural world and therefore should be studied with the same scientific techniques that are used to study the other parts of nature. As such, naturalistic epistemology is seen as a descriptive epistemology. The eminent analytic philosopher Quine (1994) characterized the recent changes in epistemology nicely when he said: "The old epistemology aspired to contain, in a sense, natural science; it would construct it somehow from sense data. Epistemology in its new setting, conversely is contained in natural science, as a chapter of psychology" (p. 25). The naturalistic turn in epistemology has been developing since the early 20th century.

Evolutionary Theory

Evolutionary theory is commonly misconstrued of as either goal-directed (purposeful) change or absolutely random change. A more accurate characterization is somewhere in between these two extremes. Evolution is cumulative change (Simon 1996). Evolutionary processes produce physical and biological structures from which other structures are more likely to follow.

Herbert Simon's (1996) example of the two watchmakers, Hora and Tempus, is an excellent illustration of cumulative advantage in evolutionary theory. Both watchmakers create beautiful watches that are relatively indistinguishable, but Hora prospers in his vocation while Tempus fails. Why? Due to their fine craftsmanship, each watchmaker gets numerous phone calls in their workshops while they are working on their products. These interruptions cause Tempus to lose everything that he is working on at the time because he has no way to keep the small watch parts together when he lets go of the pieces. Tempus builds his watches one component at a time in an elaborate and painstaking way until all 1,000 pieces are together. When interrupted, he loses all he has accomplished on a particular watch. Whether he has completed only 15 of the 1,000 steps or 987 of the 1,000 steps, all are lost when the watch is let go. Hora, on the other hand, builds his 1,000-part watches in 10-part component stages. When Hora is interrupted, he only loses whatever work is in progress on a 10-part component, not the entire watch. Because Hora works in a hierarchically based mode, he can produce more watches than Tempus.

While Popper clearly shows an early influence of an evolutionary perspective in his first book, *Logik der Forschung* (1934; first published as an English translation 1959)—by likening the rational process of theory selection in science to Darwin's concept of survival of the fittest—he did not work on evolutionary epistemology for several decades, until 1961 (Simkin 1993). He acknowledges these early influences in his treatise on the subject, *Objective Knowledge* (1972, p. 67); namely, Darwin and the Post-Darwinists, Lloyd Morgan and H.S. Jennings. Regarding later influences, although Popper (1972) acknowledges the influence of his friend, Donald T. Campbell—whom he credits for coining the term "evolutionary epistemology”—he asserts that his “own (epistemic) approach has been somewhat independent of these influences” (p. 67, parenthesis, authors).

Existence is Problem Solving

All organisms are faced with the perpetual threat of extinction. According to Popper (1976), “this threat takes the form of concrete problems which it has to solve” (p. 177). In other words, “problem-solving is the primal activity: and the primal problem is survival” (Magee 1973b, p. 56). An obvious example is maintaining a state of caloric equilibrium, such that energy expenditure does not exceed energy consumed; otherwise, organisms eventually die of starvation. Of course, many day-to-day problems are not survival problems per se, for a given organism or organisms. Perhaps, certain problems have survival value only when taken at an aggregate level, when one considers overall species-specific patterns. Or, while not immediately relevant to the behaving organism, its activities might be crucial for the survival of its offspring. It follows, therefore, that existence is best characterized as “problem-solving” rather than “end-pursuing” (Popper 1976, p. 178).

Problems occur when an organism’s “expectations” about its environment (i.e., innate or acquired experientially) turn out to be wrong (Popper 1999, p. 4). An example of a concrete problem is a bird that builds a nest in a tree, where neighborhood cats have ready access. Nest building is an innate “expectation”—a phylogenetically determined behavior. It is also ontogenetically determined, given the organism’s idiosyncratic history (e.g., perhaps it successfully laid eggs and reared offspring in the same nest). Accordingly, this incongruence between its expectancies and the contingencies of the natural world forces the organism to formulate new expectancies and test these. Over time, after several or many attempts (i.e., building nests elsewhere), the organism may happen upon the correct solution for the exigencies at hand (i.e., rearing young, in a temporary haven, away from potential predators). It will continue implementing the provisional solution should that particular external press remain unaltered. Inevitably, however, it will face other problems (e.g., the town cuts down the tree). Again, the organism will have to formulate new expectancies and employ these as new tentative solutions; until these, too, become obsolete, as the environment is always unstable, and in a constant state of flux.

Error Elimination

This iterative process of formulating and testing expectancies is the method of trial and error or error elimination (cf. Campbell 1974). Nature eliminates errors in two ways: (1) by extinguishing unsuccessful forms (i.e., genetic mutations) via natural selection and (2) modifying or suppressing unsuccessful organs or behaviors (Popper 1972). Nature is ripe with examples of (1). We cannot lose sight of the fact that 98 % of every species, since the earth's origin, has become extinct (Ehrlich and Ehrlich 1981). Presumably, these (erroneous) species were selected out or eliminated by nature. An illustration of Popper's second point is the human appendix. Presumably, at one point, the appendix served a useful function; perhaps, it aided in digestion. However, with time, the environment had no use for this organ—the appendix now resides in our bodies as a vestigial appendage. Accordingly, organs and their functions are tentative adaptations of the world, comparable to the nesting practices in the example above (Magee 1973a). Eiseley (1958) put this nicely:

> [the] evolutionary past of every species of organism—the ghostly world of time in which animals are forever slipping from one environment to another and changing their forms and features as they go. But he marks for the passage linger, and so we come down to the present bearing the traces of all the curious tables at which our forerunners have sat and played the game of life. Our world, in short, is a marred world, an imperfect world, a never totally adjusted world, for the simple reason that it is not static. The games are still in progress and all of us, in the words of Sir Arthur Keith, bear the wounds of evolution. Our backs hurt, we have muscles which no longer move, we have hair that is not functional. All of this bespeaks another world, another game played far behind us in the past. We are indeed products of "descent with modification" (p. 197).

Problem-Solving Schema

At its most fundamental level, Popper's evolutionary epistemology is represented by the following problem-solving schema: $P_1 \rightarrow \mathrm{TT} \rightarrow \mathrm{EE} \rightarrow P_2$.

> …we start from some problem P_1, proceed to a tentative solution or tentative theory TT, which may be (partly or wholly) mistaken; in any case it will be subject to error-elimination, EE, which may consist of critical discussion or experimental tests; at any rate, new problems are not in general intentionally created by us, they emerge autonomously from the field of new relationships which we cannot help bringing into existence with every action, however little we intend to do so (Popper 1972, p. 119).

"Problems emerge autonomously from the field of new relationships" as unintended effects. That is to say, solutions to old problems in and of themselves create new environmental problems (e.g., global warming with the burning of fossil fuels). According to Popper (1972), we exist in a universe that is by and large "highly irregular, disorderly, and more or less unpredictable" (p. 207). Likewise, our provisional solutions have "highly irregular, disorderly, and more or less unpredictable" unintended effects.

Problem solving takes place at three levels (the third level being unique to our species): genetic; behavioral; and scientific discovery, in the form of bold conjectures and theories, which he considers a special case of behavioral adaptation (Popper 1985, p. 78). These "knowledge" structures are transmitted via gene replication on the genetic and behavioral levels; and by way of social tradition and imitation on behavioral and scientific levels (Popper 1985, p. 79).

"The gene structure of the organism" corresponds to the first level (Popper 1985, p. 79). At the genetic level, as alluded to earlier, the environment selects out or eliminates certain physical characteristics of species that are disadvantageous. For example, poorly camouflaged organisms, cohabiting with newly introduced natural predators, are easy prey and thus eliminated. Nature continues to eliminate this "error" until the characteristics related to poor camouflage (i.e., genes) are selected out of the species. Eventually, what remains in the species are organisms that do not possess this structural attribute (i.e., error). In a manner of speaking, remaining members of the species become exemplifications of this "knowledge"; that is, incarnated knowledge vessels (Bartley 1987). In our example, the genetic makeup of surviving organism exemplifies the knowledge of visible contrast in relation to surrounding objects. Otherwise, those members are eliminated by degrees, until all remnants are extinguished. "The innate repertoire," or manners of responding available in the organism's repertoire, corresponds to the second level (Popper 1985, p. 79). This level concerns species-specific behavior (e.g., mating behavior).

The method of trial and error learning is "fundamentally the same whether it is practiced by lower or higher animals, by chimpanzees or by men of science" (Popper 1972, p. 216). Popper (1999) thus, considered science a "biological phenomena" (p. 5); a means by which the human species adapts itself to the environment (Popper 1985, p. 78). Scientific knowledge only differs with other knowledge in the methods by which errors are systematically criticized and rectified (Popper 1962, p. 216). Accordingly, the "difference between the amoeba and Einstein" is that "the amoeba dislikes to err while (Einstein) consciously searches for his errors in the hope of learning by their discovery and elimination" (Popper 1972, p. 70, parenthesis, authors).

The growth of scientific knowledge is thus characterized by "the repeated overthrow of scientific theories and their replacements by better and more satisfactory ones" (Popper 1962, p. 215). The function of science is "not to save the lives of untenable systems but, on the contrary, to select the one which is by comparison the fittest, by exposing them all to the fiercest struggle for survival" (p. 42). The "fittest" theories help the human species adapt best to its current environment. They may contain "the greater amount of empirical information"; they may be "logically stronger"; or have "greater explanatory" or "predictive power" (Popper 1962, p. 217). Conversely, insofar as unsuccessful species become extinct, so too do untenable scientific theories (e.g., Aristotelian-Ptolemaic formulations of nature and the universe).

Accordingly, science is constantly in a state of flux, in light of the fact that nature is "highly irregular, disorderly, and more or less unpredictable." Scientists are thus continuously formulating new theories as tentative solutions to threats to our existence; until these theories, in turn, become obsolete as the contingencies change.

Munz (1985), a student of Popper's, provided an interesting conjecture regarding the noncognitive, affiliative function of dogmatically held beliefs:

> With the emergence of consciousness, we get a further change in the nature of change. Conscious organisms can create falsehoods; they can lie and delude and deceive both themselves and others… In this way, cultures are created. The most elementary strategy

> used in the development of cultures is the artificial protection of knowledge from criticism. Certain pieces of knowledge, though obviously not all knowledge, are set aside and protected from critical appraisal. The thunder is identified with a god, the shadow of a man with his soul, and twins with cucumbers. Rational doubts are nipped in the bud by the mere absence of competing alternative proposals. Such protected knowledge can be used as a social bond. People who subscribe to it are members of a society; people who don't are outside that society. In this way, a lot of knowledge is siphoned off and used for non-cognitive purposes—that is, as catechism. But such siphoning-off though initially obviously counter-adaptive, is an oblique advantage. A society o so constituted is larger than a group of people bonded by nothing but the web of kinship and is therefore capable of effective division of labour and cooperation (p. 282).

Special Topic I: Three Other Key Evolutionary Epistemologists: Donald Campbell, W. V. O. Quine, and B. F. Skinner

Three Key Thinkers of the 20th Century

Donald Campbell

Donald Campbell, prominent psychologist and methodologist, first articulated and popularized the naturalistic approach to epistemology known as evolutionary epistemology (Heyes 2001). Campbell criticized traditional epistemology on two grounds: (1) it demands the impossible of us (e.g., Descartes' radical skepticism) and (2) it makes knowledge impossible to attain. "Given up is the effort to hold all knowledge in abeyance until the possibility of knowledge is logically established, until indubitable first principles or incorrigible sense data are established upon which to build" (Campbell 1987, p. 53).

Evolutionary epistemologists view biological features of humans as the expression of embodied knowledge: "evolution is a process in which information regarding the environment is literally incorporated, incarnated, in surviving organisms through the process of adaptation" (Bartley 1987, p. 23). In short, the phenotype "knows" an acceptable solution to various environmental problems related to survival and reproduction. An interesting example of evolved abilities is vision. Despite the vastness of the electromagnetic spectrum, humans respond only to a very short range (roughly between 400–700 nm). The question is, "Why only this range?" Campbell (1974) and Wächterhäuser (1987) suggested that this particular range might be visible to us for two reasons. First, in response to food shortages, primitive organisms developed the capability to photosynthesize, a process that provided a new source of food. Thus, what we call visible light was originally important because it was edible radiation. The second reason that this particular range of light is visible to us is because in this range things that are not transparent also usually cannot be moved through: human movement is blocked by solid bodies, but not by air or water (things that are to some degree transparent). Thus, an organism that is able to use vision as a substitute for movement would have an advantage over organisms that cannot.

BV + SR Model

According to Campbell (1974), an evolutionary process has three essential features: (1) a mechanism of variation, (2) a mechanism of selection, and (3) a mechanism of transmission. Campbell extended this iterative evolutionary process to human knowledge when he outlined his BV+SR model: "Blind Variation Plus Selective Retention." When considered in an epistemic context (rather than biological) this model describes the process by which beliefs (e.g., knowledge) are selected based upon their fit with the natural world. The environment produces challenges to be overcome (e.g., finding food, shelter, etc.), and potential problem solutions are generated (in the form of beliefs and behaviors) and then applied to the environment. The idea that actually leads to a solution (even partial solutions) is maintained, while the others are discarded.

Vicarious Selection

Following a process of natural selection, an organism adapts by developing an internal selection process that Campbell refers to as "Vicarious Selection." In the case of humans, this process is best understood as thinking, imagining, and problem solving. This internal selection process can anticipate (to some extent) natural selection, thereby reducing the necessity to encounter potentially threatening environmental situations. That is, humans can "think about" possibilities and consequences, criticize them, and discard those that fail to have apparent survival value, without having to directly encounter them in the environment.

Epistemic "fit"

The better equipped we are with respect to our vicarious selectors (e.g., cognitive abilities), the more accurate we are with regard to predicting consequences and successfully negotiating environmental challenges. This is what Campbell refers to as "fit." Our eyes have developed to "fit" the environment better by developing sensitivity to a particular bandwidth of the electromagnetic spectrum (e.g., the bandwidth that allows us to sense dangers and food). Moreover, we have evolved in such a way that our senses attend to salient environmental features while ignoring or "tuning out" nonessential features (e.g., we attend to sensations of pain and ignore others while pain is present). Furthermore, our cognitive abilities have evolved to better fit the environment by doing the same. The cognitive biases and heuristics literature (see e.g., Kahneman et al. 1982) is full of examples of such survival oriented, but imperfect, fits. The more fit between humans and their environment, the more knowledge is present. For Campbell, and most other evolutionary epistemologists, fit equals knowledge.

W. V. O. Quine

The Harvard philosopher Wilfred Van Orman Quine is arguably the most influential analytic philosopher of the 20th century. He made important contributions to mathematical logic, ontology, semantics, and his paper "The Two Dogmas of Empiricism" is generally credited as being the most influential in disposing of logical positivism, arguing against both the existence of analytic statements as well as the verification principle, viz. that sentence acquire their meaning and truth value from the sense perceptions upon which they are based. Roger Gibson (1988) categorized Quine philosophy as behavioristic and provides a nice summary of Quine's major theses:

> From within this behavioristic framework Quine can argue, for example, that meaning is indeterminate, that reference is inscrutable, that ontology is relative, that theories are underdetermined by experience in principle, that the truth value of any sentence or statement can be revised, that there are no meanings, no propositions, no attributes, no relations, no numbers, no synonymity, no facts, no analytic truths, and so forth (Gibson 1988, p. xx, italics in the original.)

There is no first philosophy. Quine points out that a fundamental question for an investigator is whether one begins with philosophical considerations regarding knowledge and how it can be gained, or with scientific considerations. For example, does the inquirer begin with a putative philosophical conclusion regarding what knowledge is and how one can discover new information, for example, knowledge can only be generated by inductive extrapolation from sense impressions and then conduct science accordingly; or does one begin with a scientific consideration, for example, humans are products of natural selection and then address philosophical and scientific questions from this point of view?

Quine denied the existence of a "first philosophy" from which a scientific theory ought to be constructed, and instead places epistemology (i.e., the study of knowledge) within science itself. Quine argued that no philosophy is firmer than science, and hence, the typical concerns of the philosopher can be addressed from within science. More specifically, he stated the fundamental goal of a theory of knowledge is to give a factual natural account of the relationship between observation and theory (in his words "between the meager input and the torrential output" Quine 1974, p. 83).

Quine's Evolutionary Epistemology

Quine argued that it is behavioral psychology—particularly the psychology of the learning laboratory—that is best suited to give us a picture of knowing. He stated, "The stimulation of his sensory receptors is all the evidence anybody has had to go on, ultimately, in arriving at his picture of the world. Why not just see how this construction actually proceeds? Why not settle for psychology?" (p. 75). Thus, Quine believed the key issue in epistemology is scientifically developing a factual account of the link between observation and theory. He further stated,

> Epistemology or something like it, simply falls into place as a chapter of psychology and hence of natural science. It studies a natural phenomenon, viz., a physical human subject. This human subject is accorded a certain experimentally controlled input—certain patterns of irradiation in assorted frequencies, for instance—and in the fullness of time, the subject delivers as output a description of the three-dimensional external world and its history. The relation between the meager input and the torrential output is a relation that we are prompted to study for somewhat the same reasons that always prompted epistemology; namely, in order to see how evidence relates to theory, and in what ways one's theory of nature transcends any available evidence… (Quine 1994, p. 82).

Thus, Quine saw the naturalistic study of knowing as the study of learning in the psychological laboratory. He stated, "Learning, thus viewed, is a matter of learning to warp the trend of episodes, by intervention of one's own muscles, in such a way as to stimulate a pleasant earlier episode" (Quine 1974, p. 28). Quine saw the ability to learn as "itself a product of natural selection, with evident survival value. Moreover, Quine asserted that one result of naturalizing epistemology in that it becomes an iterative, reflexive process: science is used to understand knowing and our knowledge of the growth of knowledge is used to further develop science. Because evolution is such a critical research program in contemporary science it is used to further develop our naturalized account of knowing."

B. F. Skinner

Skinner espoused a selectionist view in both his analysis of behavior and the survival of species. Skinner's most fundamental argument is that all human behavior results from a confluence of three levels of variation and selection: natural selection, selection by consequences, and cultural selection (Skinner 1990a, b). These will be taken up shortly. "Knowledge" of the world, according to Skinner (1953), is "our behavior with respect to the world" (p. 140)—nothing more. Therefore, Skinner's experimental analysis of the behavior of organisms translates directly in a theory of knowledge (O'Donohue and Smith 1992).

Knowledge in general and scientific knowledge in particular, therefore, culminates out of these processes. From this perspective, there is no "first philosophy" or meta-level from which the veracity of knowledge can be ascertained (O'Donohue and Ferguson 2001). Skinner rejected normative questions dealt with in traditional epistemology (e.g., What is truth?). One simply cannot "step out of the causal stream and observe behavior from some special vantage point… In the very act of analyzing human behavior (humans are) behaving" (Skinner 1974, p. 234, parenthesis mine). Environmental contingencies in the natural world account for the behavior of the scientific community in its production of verbal behavior (e.g., metrological forecasts), as they account for the behavior of other organisms (e.g., responding to seasonal climatic fluctuations, such as hibernation). Consistent with this view, Skinner arranged his work environment in ways that maximized these variable-ratio schedules governing the growth of knowledge (O'Donohue and Ferguson 2001). Skinner's epistemic view is illustrated in the following passage, selected from his paper, "A Case History in Scientific Method":

> If we are interested in perpetuating the practices responsible for the present corpus of scientific knowledge, we must keep in mind that some very important parts of the scientific process do not lend themselves to mathematical, logical, or any other formal treatment. We do not know enough about human behavior to know how scientists do what they do (p. 75)…science does not progress by carefully designed steps called "experiments," each of which has a well-defined beginning and end. Science is continuous and often a disorderly and accidental process…The subjects we study reinforce our behavior much more effectively than we reinforce theirs (p. 94)….I believe that my behavior is as orderly as that of the organisms I study and that my rats and pigeons have taught me far more than I have taught them (p. 97) (Skinner 1953; reprinted 1982).

Skinner's perspective on epistemology is thus naturalized—a "psychological epistemology" of sorts (Skinner 1979, p. 29). In this vein, Skinner regarded his experimental analysis of behavior a natural science, more akin to biology than the mentalistic psychology at the time. Let us turn next to Skinner's psychological epistemology, the first level of which concerns natural selection.

Natural Selection

Natural selection gives us the organism (Skinner 1990a, b). In a sense, it provides the physiological equipment to emit certain response topographies. An opposable thumb, for example, enables us to manipulate objects with fine motor control. Binocular vision enables us to perceive the relative distance between objects. Most importantly for human organisms, "when our vocal musculature came under operant control in the production of speech sounds," our species proceeded to soar with all its "distinctive achievements" (e.g., art, science, literature; Skinner 1986, p. 117). At this point, humans were capable of emitting verbal behavior via vocal apparatus. Eventually, and most importantly as it concerns the scientific enterprise, verbal behavior also took written form (Skinner 1957).

Through natural selection, the environment selects those physical characteristics and behaviors that promote the survival of species. For example, those individuals with highly sensitive autonomic nervous systems (ANS) were presumably selected by the environment because they were able to react more quickly in response to danger. The fight-or-flight mechanism, the sympathetic branch of the ANS, enabled earlier humans to step out of harms way when a predator was about to attack. Those individuals with a poorly developed ANS simply became some creature's meal. From a natural selection perspective, it should come as no surprise that fears such as ophidiophobia (fear of snakes), arachnophobia (fear of spiders), nyctophobia (fear of the dark), and necrophobia (fear of corpses) are easily acquired and culturally universal. Our ancestors who learned quickly to avoid these, by way of conditioned reflexes, lived to procreate (Skinner 1974, p. 38ff.).

Selection by Consequences (Operant Conditioning)

According to Skinner (1990a);

> All types of variation and selection have certain faults, and one of them is especially critical for natural selection: Classical conditioning prepares a species only for a future that resembles the selecting past. Species behavior is only effective in a world that fairly closely resembles the world in which the species evolved. If we were to wait for natural selection to fashion a relatively simple behavioral repertoire, this would take millions of years spanning countless generations, as selection is contingent on genetic variation. That fault was corrected by the evolution of a second type of variation and selection, operant conditioning, through which variations in the behavior of the individuals are selected by features of the environment that are not stable enough to play any part in evolution (p. 1206).

While natural selection concerns the species, selection by consequences concerns the individual—more specifically what the individual is likely to do. An operant is behavior that "operates" on the individual's immediate environment to produce certain consequences (Skinner 1953, p. 65). It is through this mechanism of selection that an organism readily adjusts its behavior to rapidly changing environing circumstances. So-called reinforcers (e.g., food, sexual contact) increase the likelihood of the behavior that preceded them.

While operant conditioning better coordinated human behavior with a capricious environment, a single repertoire is extremely limited outside of social influence. Within a single individual's lifetime, he or she would not have learned that cooking food destroys harmful bacteria, storing food is advantageous in the event of a drought, and hunting big game as a group is more energy efficient than hunting rodents alone. The fault of operant conditioning was therefore corrected by cultural selection, when humans began sharing each other's repertoires by way of imitation (Skinner 1990a, b, p. 1206).

Cultural selection (social contingencies). Human beings are inherently social animals—as this tendency itself has survival and reproductive advantages. For millennia, humans have coexisted under mutual protection, reared young collaboratively, and so on. Within these collectivities, cultural practices could be transmitted via imitation. Imitation, of course, is not distinctly human. Japanese macaque monkeys, for instance, have been shown to imitate unorthodox behaviors demonstrated by other members of the collective (e.g., sweet potato washing and wheat-washing). The direct benefit of imitation is that it brings individuals into contact with reinforcers that are relatively remote; after which, contingencies of reinforcement take over the control of the behavior (O'Donohue and Ferguson 2001).

Comparable to the preceding levels of selection, the environment also selects out cultural practices that have a higher probability that its members and their offspring will survive. "A culture which raises the question of collateral or deferred effects is most likely to discover and adopt practices which will survive or, as conditions change, will lead to modifications which in turn will survive" (Skinner 1972, p. 45). For example, cultures that promote the practice "safe sex" are in a better position to control the spread of lethal sexually transmitted diseases. Cultures that do not adopt these practices are more likely to contract such diseases and ultimately spread these to offspring. The offspring, of course, usually die before they are able to reproduce.

Comparison of the Four Thinkers

All four epistemological perspectives in this paper endorse a naturalistic account of human knowledge, the emphasis of which is on natural selection. Namely, the environment selects those characteristics of the human species, both structurally (e.g., eyes, ears, opposable thumb) and behaviorally (e.g., shelter building, producing fire, reproductive practices), because these have overcome survival problems (e.g., ensure successful genetic transmission to viable offspring). By contrast, organisms whose bodies or behaviors thwart the "correct" solution to survival problems eventually die off. For example, the genes of orthodox shakers who adopted a policy of total sexual abstinence in the 19th century have been selected out of the current gene pool.

Given that human knowers and human knowledge are considered part of the natural world, Quine, Popper, Campbell, and Skinner argue that knowledge is not only best amplified, through the methods of science, but also best understood. Most importantly, science, particularly through biology and psychology, enables our species to systematically identify errors, and in due time, correct these (Popper 1962, p. 216). Although agreeing fundamentally on this point as well as others, each author focuses on different aspects of a similar puzzle. Campbell devoted much of his work to explicating the mechanisms by which knowledge is obtained, his elaborate Blind Variation Plus Selection Retention model (e.g., nonmnemonic problem solving, vicarious locomotor devices, visually supported thought). Although Quine also eschewed traditional epistemology—opting for the investigation into the psychological mechanisms that take us from sensory stimulation to beliefs and theories about the world—he dedicated little time to detailing these processes. Rather, he left the elaboration to those areas of psychology that investigate these phenomena (i.e., the learning laboratory) will achieve this end. Therefore, for Quine psychology serves as a placeholder of sorts. In between these opposite poles, Skinner in an important sense fills in some of Quine's details through his analysis of the selection of behavior by consequences. Popper, on the other hand, presented a much more cognitively based evolutionary epistemology than Skinner. Popper's ontology (1972) posits three kinds of existents: (1) world 1 of physical objects; (2) world 2 of mental events; and (3) world 3 of the objective contents of books, arguments, scientific laws and theories, etc. His heuristic of $P1 \rightarrow \mathrm{TT} \rightarrow \mathrm{EE} \rightarrow P2$, for example, although instantiated in a particular scientist in world 2 also according to Popper, has an independent objective existence as propositional content.

Evolutionary epistemology allows what Popper calls bold conjectures (and then criticism attempts). This is a liberating epistemology that allows scientists to be opportunistic and creative to come up with theories of high empirical content, generality (and of course falsifiability). It is in direct opposition to the view that theories need to be carefully built up through some inductive process from indubitable empirical evidence. This view is most commonly associated with logical positivism. In contradistinction, the view from evolutionary epistemology allows scientists (or entrepreneurs for that matter) to examine the Problem-Situation and conjecture boldly. Quine and other philosophers of science such as Brown (1988), however, have then pointed out that judgment becomes a critical issue in the progress of science.

Although Quine's naturalized epistemology has implications for understanding how natural selection bears on social behavior, he did not extend his analysis to cultural phenomena to the extent that Popper, Skinner, and Campbell do. Popper (1957, 1962), for example, insisted on an "open society" where all aspects of a society, especially governmental institutions, ideally open themselves to criticism. He suggested having safeguards in place that efficiently expel ineffective or harmful governments (i.e., errors). Skinner (1948, 1971, 1977, 1990b) advocated society adopting an experimental community approach (especially in his Walden Two), whereby cultural practices are tested on a small scale before put into practice. That is to say, should the environment select a given practice? (e.g., harvesting technique that optimizes production), it would be thus adopted by the society on the whole. Campbell (1975, 1979, 1982, 1983, 1991), while not providing a "utopian vision" as to what a society ought to look like, provides extensive sociobiological analyses of cultural practices, advocates both piecemeal social engineering, and "reforms" as experiments to maximize selective criticisms.

Conclusions Regarding Evolutionary Epistemology

1. There is a remarkable degree of convergence of the views of four very influential 20th century scholars who in general had little direct influence upon each other. Rather, they were lead to evolutionary epistemology due largely to the development of their own systems of thought.
2. Knowledge, and the growth of knowledge, is seen as the instantiation of a selectionist procedure in which proposals are presented to the environment and only some continue to survive. This selection process can come in a wide variety of forms, from bodily structures or processes contributing to an organism's survival, to scientific assertions surviving the peer review process. Criticism is essential to the growth of knowledge.
3. Cognitive therapies which attempt to capture and implement more rational and functional belief formation can profit from evolutionary epistemology.

O'Donohue et al. (in preparation) have criticized the extant cognitive therapies of Beck and Ellis as relying upon a problematic epistemology and are developing a cognitive therapy based on evolutionary epistemology.

4. Psychologists need to think about selectionist models of causality instead of "push models" of causality more usually associated with physics.
5. Psychologists would do well to study more of evolutionary psychology and sociobiology to understand more of the implications of evolutionary psychology for their particular problems. It is very surprising, for example, the small role of these to fields have had on Skinnerian psychology, given that his concept of "contingencies of survival" is his place holder for exactly this type of work
6. Psychologists ought to also study more of the noncognitive functions of beliefs. Munz (1985) had the interesting speculation that beliefs held immune from the criticism process may go a long way to help define and cement groups. If so, it would be interesting to study this phenomenon empirically and to both other functions of such beliefs.
7. According to these evolutionary epistemic perspectives, there is an objective reality—contrary to social constructivists. Although our perceptual systems may not perfectly mirror this reality, they capture enough verisimilitude to allow us to survive.

Special Topic II: Popper's Political Philosophy

During World War II, Popper developed a political philosophy—which he called his "war work" in two key books, *The Open Society and its Enemies* and *The Poverty of Historicism*. In these two books, he examined the problems with Marxism (which influenced the statism and the horrors of Marxism–Stalinism in the former Soviet Union and Eastern Europe and the horrors of maoism in China). And statism which was responsible for the horrors of Nazism in Europe. He suggested that these movements were based on a wrongheaded metaphysical view of history that he called *historicism.* Historicism claimed that there were inevitable historical laws and the future is determined by these. For example, Marx claimed that there was an inevitable progression from capitalism, to socialism, to communism—and there were certain economic and historical dynamics that made this inevitable. Popper disagreed. Popper argued that because the growth of human knowledge is a causal factor in the evolution of human history, and since "no society can predict, scientifically, its own future states of knowledge it follows" (p. 27), he argued, that there can be no predictive science of human history. For Popper, the future is also "open"—we just can't predict what it will be like. No one in the 1940s, in principle, could have predicted the role of the internet and personal computing on human behavior—because to do so would one have to be able to foresee novel scientific and technological developments in the 1940s, which would not make them novel in the latter half of the century.

Two other concepts in Popper's political philosophy are *the open society* and *piecemeal social engineering*. Marxists and Nazis are also utopians—that is, they believe that their political proposals are the best solutions to questions of governance and society—and that they in fact have the "answers,"and thus those that oppose them or criticize them are so misguided that they are enemies of the state. Popper's fallibilist epistemology suggests such confidence in political proposals is misplaced. Political beliefs, like scientific beliefs, can be wrong and must be criticized and put to the test. He suggested that the rational society is an open one—in which citizens are free to propose all sorts of ideas—and all citizens are free to criticize these ideas to see which best survive this critical process. This "open society" is in direct contrast to "closed societies" in which criticism is banned or severely limited, and some ideas are censured. Popper proposed a criterion for democracy as that political system which permits the citizens to rid themselves of an unwanted government without the need to resort to violence.

Finally, Popper thought that because human beliefs contain error that "*piecemeal social engineering*" is the best process for proposing and testing claims to improve political or social problems. This again is in direct contrast to statist utopian solutions in which the entire society adopts a new proposal. Popper argued for smaller-scale reforms that could be tested for their effectiveness as well as unintended negative effects, before these are implemented on a wider scale. Popper again, argued for learning by trial and error—and the society needs to be sufficiently open to admit to its errors. In this way, he was much more a Jeffersonian, who influenced the US Constitution by arguing for a small less powerful Federal Government and argued that most of the powers should reside with the states so they are free to try a variety of different approaches to problems.

Also Popper argued that statist solutions often are epistemologically problematic. In the old Soviet economy, central planners would attempt to predict needs and consumer demands. But the knowledge they would need to do this with adequate accuracy is simply not available to them. Popper thought as others (e.g., the Austrian economist Fredrick Hayek) that the free market was epistemically superior—it could transmit and react to much more information more accurately.

Another key concept in Popper's political philosophy is the notion of *unintended consequences* to any act—particularly political reforms. Popper stated:

> Owing to our slowly increasing knowledge of society, i.e., owing to the study of the unintended repercussions of our plans and actions… one day, men may even become the conscious creators of an open society, and thereby of a greater part of their own fate. But… although we may learn to foresee many of the unintended consequences of our actions (the main aim of social technology), there will always be many which we did not foresee.

What are examples of these "unintended consequences" of reform? For example, the intended consequences of welfare are to make sure that the poor have adequate food and clothing—however, the unintended consequences may be that it discourages work. Popper used the concept of unintended consequences to critique what he called "utopian social engineering." The notion is that the reformer advocates for her reform by listing all the positive consequences that the reform will produce. However, for Popper, there is also a set of unintended consequences that the reform will also produce, and a key critical question becomes: will these unintended consequences outweigh the purported benefits? One more example, the intended consequence of a minimum wage law is to assure a decent living wage. The unintended consequence is that it increases unemployment—as it forbids employers and potential workers to form employment contracts at lower rates, which may be necessary for a certain business to be viable.

Finally, and I think quite astutely, Popper suggested that utopian social engineering also can lead to the growth of epistemically challenged bureaucracies. Popper writes:

> My views on piecemeal engineering have constantly developed since about 1922, when I first realized the problem of bureaucracy, and the fact that none of my socialist friends… was interested in this awful problem, but were, on the contrary, for further bureaucratization of our life (Popper 1976, p. 21).

And, in an article published in 1988, Popper referred to bureaucrats as "our civil servants or uncivil masters… whom it is difficult, if not impossible, to make accountable for their actions." I think this a ripe but heretofore neglected research question for clinical psychologists: what effects, if any, do large problematic bureaucracies such as the IRS, DMV, Social Security Administration, Centers for Medicaid and Medicare, and even those in the private sector such as the United Airlines reservation system play in human psychopathology?

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Chapter 5
The Spell of Kuhn on Psychology

Introduction

In their meta-scientific studies of psychology, psychologists often use what they take to be the views of the historian of science, Thomas Kuhn. Although a critical examination of psychology or aspects of psychology is laudatory, psychologists also need to accurately understand and to assume a critical stance toward the meta-scientific views that they employ. In this chapter, the views of the historian of science, Thomas Kuhn, are described and examined. The following major questions are addressed: What were Kuhn's investigative methods? What are his views of science? What exactly do Kuhn's conclusions about science mean? How does Kuhn rely on psychology? and What does Kuhn have to say about psychology? The extent to which psychologists find Kuhn so attractive is puzzling given the significant ambiguities and inconsistencies in Kuhn's views, his informal and unsystematic use of psychology, and his disparaging comments about psychology. It is recommended that psychologists adopt a more critical stance toward Kuhn and that they consider other meta-scientific theories in their studies of psychology.

Kuhn's Normal and Revolutionary Science

The views of Thomas Kuhn, especially those expressed in the first edition of his *The Structure of Scientific Revolutions (SSR)* (Kuhn 1962, 1970a 2nd ed), have had a rather interesting influence upon psychologists. When psychologists talk about their scientific and clinical pursuits, they usually employ Kuhnian concepts and claims. Coleman and Salomon (1988) reviewed psychological journals for approximately a 15-year period and found that Kuhn was the most frequently cited historian/philosopher of science. They found 652 articles that cited Kuhn's SSR. Two of their other findings are noteworthy: (1) Psychologists tended to be highly favorable toward Kuhn, in that 95 % of articles casually mentioning Kuhn and 83 % of those intensively citing Kuhn were rated as favorable toward his

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views and (2) The majority of the citations were rated as superficial uses of Kuhn. Coleman and Salmon stated:

> The majority (75 %) of articles merely mentioned Kuhn; abundant references solely to the earlier edition (Kuhn 1962) persisted after the second edition (Kuhn 1970a) had appeared, and more than 90 % of the articles cited just one of Kuhn's publications; less than 3 % of the articles were strictly about Kuhn or about the application of Kuhnian ideas. Only half of the Kuhn-citing articles cited any other works in the philosophy of science, and about 75 % of these articles cited three or fewer philosophers of science (p. 435). These authors conclude that "Citing Kuhn (Kuhn 1962, 1970a) minimally showed that the writer was *au courant* and, therefore, may have served more as a rhetorical strategy than as a substantive assertion in most of the citations" (p. 435).

Moreover, Peterson (1981) found a few studies in the psychological literature in which Kuhnian ideas were used not at the meta-scientific level, but rather were used internally in psychological research. One study used the notion of paradigms to understand aspects of social cognition (Rychlak et al. 1974), while another used this notion to study cognitive styles (Kirton 1976).

Given psychologists' use (and possible misuse) of Kuhn's account of science, it is important to understand more clearly the relationship between Kuhn's views of science and psychology. This is especially important given the evidence in Coleman and Salmon (1988) of psychologists' superficial use of Kuhn, since this superficiality may lead to misconstruals of Kuhn or misunderstandings of the basic problems in meta-science. To this end, this chapter will examine the following questions: (1) What were Kuhn's investigatory methods? (2) What are Kuhn's views of science? (3) What exactly do Kuhn's claims mean? (4) How does Kuhn rely upon psychology? and (5) What does Kuhn say about psychology? In pursuing these aims, an attempt will be made both to understand why psychologists find Kuhn so attractive and to determine whether these views are of sufficient quality that psychologists should, indeed, find these views so appealing.

Therefore, the main task of this chapter is exegetical. Kuhn's writings will be closely examined in an attempt to explicate accurately his views. Exegesis is not only important for gaining a faithful understanding of a writer, but also an important method of criticism. A clear exposition of a writer's set of views carries both the potential of explicating strengths and the possibility of revealing ambiguities, inconsistencies, equivocations, confusions, logical leaps, question-begging inferences, and manifestly false claims.

What were Kuhn's Methods?

Kuhn considered himself to be a historian of science. He stated that he proceeded by "examining closely the facts of scientific life" (Kuhn 1970c, p. 236). He reported that he originally became interested in understanding science because he was puzzled by differences between the social sciences and the natural sciences:

> Particularly, I was struck by the number and extent of the overt disagreements between social scientists about the nature of legitimate scientific problems and methods… Yet,

> somehow, the practice of astronomy, physics, chemistry, or biology normally fails to evoke the controversies over fundamentals that today often seem endemic among, say, psychologists or sociologists. Attempting to discover the source of that difference led me to recognize the role in scientific research of what I have since called "paradigms"… Once that piece of my puzzle fell into place, a draft of this essay (SSR) emerged rapidly. (Kuhn 1970a, p. 8)

Kuhn stated that he examined *part* of the historical record in the biological and physical sciences. Kuhn reported, however, that to increase the coherence of SSR, he only cited evidence pertaining to the physical sciences in it. One puzzling aspect of Kuhn's methodology needs to be pointed out immediately. If Kuhn wanted to discover the source of a difference between the social sciences and the physical sciences, then why did he study *only* the physical (and biological) sciences? To discover differences between two entities, one needs to make a comparison which, of course, involves a detailed study of both entities. It is an elementary methodological point that to account for differences between two entities, it is insufficient to simply show that one entity has some property: at a minimum, it is also necessary to provide evidence indicating the absence of that property in the other entity. Thus, to address his original puzzle and to complete his case for the important role of paradigms in reducing the degree of controversy within a field, Kuhn would need to show the presence of paradigms in the physical sciences and the absence of paradigms in the social sciences. This second concern would of course involve a study of the social sciences. Moreover, given that Kuhn was formally trained as a physicist, it would be plausible to assume that a detailed study of the social sciences would be essential for him to complete the expected lacuna in his knowledge. Given that he stated that he did not study the social sciences, one wonders what exactly his beliefs about the social sciences were and how he acquired these.

He also stated, however, that he examined the historical record of the physical and biological sciences because he wanted to discover the essentials of science and he wanted to discover the reasons for the special efficacy of science. Perhaps in pursuing these ends, he thought that he could properly neglect a study of the less successful social sciences.

It is important for psychologists to have an accurate understanding of Kuhn's methods and what subjects he examined with these. If psychologists read Kuhn for his historiography, then there still is an important question regarding the generalizability of Kuhn's views: that is, to what extent are conclusions drawn from the history of the physical sciences applicable to the history and development of psychology? It is certainly possible that these will develop differently such that no object lessons for psychology can be drawn from a study of the history of the physical sciences. On the other hand, if psychologists want advice from Kuhn regarding how to do good science, then there are two questions. (1) Can a study of history–a study of contingent facts–be used to derive normative conclusions? Isn't this attempting to derive "ought" be from "is" (or more precisely, "was")? (2) Even if we assume that history can provide us with normative information, the question remains to what extent can lessons drawn from the history of the physical sciences be applicable to contemporary psychology? Thus, there are some important preliminary questions regarding the relevance of Kuhn's conclusions to psychology raised by his methods of enquiry.

In other parts of SSR, he acknowledges that in his study of the historical record, communities of investigators became his basic unit of analysis. He stated that he attempts a "social psychology of science":

> Already it should be clear that the explanation must, in the final analysis, be psychological or sociological. It must, that is, be a description of a value system an ideology, together with an analysis of the institutions through which that system is transmitted and enforced. (Kuhn 1970b, p. 21)

Again, given his formal training as a physicist and his apparently informal training as an historian, one immediately wonders to what extent a physicist/historian is adequately prepared to construct a meaningful social psychology of science. However, I will now attempt an explication of the conclusions Kuhn arrived at using his historical methodology and these preconceptions.

What are Kuhn's Views of Science?

There are at least three immediate problems in answering this question: (1) Kuhn has been criticized for the lack of clarity in expressing his views (Masterman 1970; Watkins 1970); (2) Kuhn‘s views have changed somewhat from those expressed in the first edition of SSR (Kuhn 1974); and (3) the scope of these views is unclear. That is, it is unclear whether these views hold for the physical sciences, the physical and biological sciences, or these and the social sciences. I will first attempt to provide a quick and I hope accurate summary of Kuhn's early views of science, and in the subsequent section, I will briefly review some of his more important clarifications and emendations.

Pre-Paradigm or Immature Period

According to Kuhn, antedating the emergence of scientific study of a set of phenomena, there is a field of study in which there is no single view about the phenomena of interest that is generally accepted by the community of investigators. Rather, there is a series of competing schools. According to Kuhn, the pre-paradigm period is characterized by "frequent and deep debates over legitimate methods, problems, and standards of solution, though these serve rather to define schools than to produce agreement" (Kuhn 1970a, pp. 47–48). Beyond debating about fundamentals, investigators are "casual fact gatherer[s]" (Kuhn 1970a, p. 16), as opposed to "puzzle solvers." A field in a pre-paradigm period fails to demonstrate progress, especially progress in puzzle solving.

Paradigmatic or Mature Science

Normal science or paradigmatic science emerges when there is agreement concerning what are to be the legitimate methods, problems, and standards of solution. This transition usually occurs "in the aftermath of some notable scientific achievement" (Kuhn 1974, p. 460). During this period, the number of schools is reduced, normally to only one. The most essential aspect of normal science is puzzle solving. During this period, the field demonstrates cumulative progress. Exemplars of successful puzzle solving are provided in textbooks. Scientists solve other puzzles by using these exemplars as models. The agreement over foundations and concerning exemplars helps produce the "communities of scientists" that are important components of the paradigm. These communities of like-minded scientists are important because they allow scientists to assume that their audience shares their values and beliefs so that they can take a set of norms for granted. Normal science involves a "dogmatic attitude" in which there tends to be "one ruling theory." During periods of normal science, scientists do not seek novel facts or theories. Rival paradigms usually are not taught.

Kuhn suggested that the paradigm tells the scientist what entities nature does and does not contain (i.e., an ontology) and the ways in which those entities behave. These prescriptions provide a map whose details are elucidated by mature scientific research "… In learning a paradigm the scientist acquires theory, methods, and standards together, usually in an inextricable mixture" (Kuhn 1970a, p. 109).

Kuhn (1970a) pointed out the omnipresence of anomalies during normal science. Paradigms create a set of expectations that throw into relief whatever fails to confirm them. However, in normal science, these failures to solve puzzles are attributed to problems with the puzzle-solving ability of individual scientists rather than to problems with the adequacy of the paradigm.

Revolutionary or Extraordinary Science

These anomalies have an important role in the development of scientific revolutions or "extraordinary science." Anomalies at times can result in a "crisis" in which there is a "blurring of a paradigm and the consequent loosening of the rules for normal research" (Kuhn 1970a, p. 84). The field is now similar to the pre-paradigm period, except the differences are "smaller and more clearly defined" (Kuhn 1970a, p. 84). Sometimes a crisis may end with the proposal of a new paradigm. The change of one paradigm to another is not cumulative, due to the often radically different conceptual framework of the new paradigm. Scientists experience what Kuhn calls a "gestalt switch" as the new paradigm alters perception. Kuhn stated "when paradigms change, the world itself changes with them" (Kuhn 1970a, p. 111). This new paradigm results in new ways of seeing old things, in new puzzles, in new exemplars, and in innovations in instrumentation and method.

The old and the new paradigm now compete for the allegiance of the scientific community. Proponents of the new paradigm often claim that it can solve the crisis-provoking problems. However, such claims are often not compelling as the new paradigm also has anomalies and the competing paradigms are to an important degree, incommensurable because neither side will grant all the nonempirical assumptions that the other needs in order to make its case (Kuhn 1970a, p. 148). For example, there is often disagreement about the list of problems that need to be solved, and standards of science are not the same across paradigms. Kuhn stated:

> The man who embraces a new paradigm at an early stage must often do so in defiance of the evidence provided by problem-solving. He must, that is, have faith that the new paradigm will succeed with the many large problems that confront it, knowing only that the older paradigm has failed with a few. A decision of that kind can only be made on faith. (Kuhn 1970a, p. 158)

This process of normal science and revolution repeats itself but in a somewhat progressive way in that this developmental process is a process "whose successive stages are characterized by an increasingly detailed and refined understanding of nature" (Kuhn 1970a, p. 170).

What Exactly Do These Conclusions Mean?

In this section, I will examine some critical points that require clarification. Kuhn has been criticized for being "essentially vague" (Watkins 1970, p. 30), and his writing style has even been described even by an admirer as "quasi-poetic" (Masterman 1970, p. 61). Kuhn himself makes an oblique reference to the ambiguity of his writings when he stated "Part of the reason for its (SSR) success is… that it can be too nearly all things to all people" (Kuhn 1974, p. 59).

What is a Paradigm?

Masterman (1970) in an analysis of SSR found that Kuhn uses at least 21 different meanings of "paradigm." She stated that his uses fall into three major classes: metaphysical paradigms, sociological paradigms, and artifact paradigms. Kuhn (1970c) agreed that his use of this term was quite ambiguous. Partly to rectify this, he has attempted to clarify this term by further characterizing "paradigm." He stated that he has two main uses for this term. Firstly, a paradigm refers to the shared elements that account for the relatively unproblematic character of professional communication and for the relative unanimity of professional judgment (p. 462). Secondly, he stated that the term paradigm is equivalent in meaning to the phrase, "disciplinary matrix." The disciplinary matrix consists of symbolic generalizations, models, values, and exemplars. Symbolic generalizations are formal or quasi-formal expressions such as "$f = ma$" or "action equals reaction" that are employed without controversy by members of a scientific community. Models supply the members of the scientific communities with permissible explanation schemata, analogies, and metaphors. Thus, "Gas molecules behave like elastic billiard balls in random motion" is an example of such a model. Values are common normative commitments of the scientific group such as "Predictions should be accurate" and "Theories should be simple, logically consistent, and plausible." Finally, exemplars are "concrete problem-solutions" (Kuhn 1970a, p. 187). It is interesting to note that Kuhn in 1974 seemed to drop values as part of the disciplinary matrix without providing any rationale for this move (see Kuhn 1974, p. 463).

It is not clear whether Kuhn's attempts to rehabilitate his central concept of paradigm have been successful. Firstly, "values" is a notoriously vague term. What exactly are values? Kuhn failed to specify how these are to be identified. Is a commitment to clear concise writing a value that is part of the disciplinary matrix (cf. Feyerabend 1975)? Secondly, regarding symbolic generalizations, what degree of generality is required? Universality? Finally, if a field of study fails to utilize metaphor or analogy, does it then fail to have a paradigm?

Kuhn also stated that "pre-paradigmatic science" is a misnomer because even in its primitive beginnings, a field often has a paradigm or a set of paradigms. He now states that the reason for the transition to maturity of a field is the emergence of a special kind of paradigm—"a paradigm able to support a puzzle-solving tradition" (Kuhn, April 1989, "personal communication").

Moreover, in publications subsequent to SSR (Kuhn 1970a), Kuhn emphasized what might be called the "micro-community" structure of science. He stated that a paradigm is usually held by a small group of scientists, often no more than 25 individuals. Thus, one cannot simply ask whether there was a paradigm change in a particular episode. According to Kuhn, one must ask, for whom? Kuhn stated that "Many episodes will then be revolutionary for no communities, many others for only a single small group, still others for several communities together, a few for all of science" (Kuhn 1970c, p. 253).

How Do Scientific Revolutions Take Place?

Since Kuhn emphasized the omnipresence of anomalies, the question becomes why do revolutions take place at one time rather than another? Is it because there are more serious anomalies or simply because a critical number of anomalies had been exceeded, or for some other reason? This ambiguity stems from a fundamental tension between two claims that Kuhn made. Kuhn claimed that in the puzzle-solving activity that is characteristic of normal science, the scientist rather than the paradigm is blamed for a puzzle-solving failure. However, Kuhn also stated that paradigms are continually brought into question by anomalies. So unless Kuhn is inconsistent, it appears that not all failures in puzzle solving are to be regarded as anomalies.

Kuhn admitted that the onset of revolutions is necessarily vague because, although there is a critical level of anomaly, this level is not the same for everyone "nor need any individual specify his own tolerance level in advance" (1970c, p. 248). To further complicate the issue, Kuhn stated that crises due to anomalies do not always precede scientific revolutions (Kuhn 1970c, p. 181). Revolutions may occur due to other factors such as developments in instrumentation in another field.

According to Kuhn, scientists can react to anomalies in a variety of ways. Kuhn stated that most anomalies are simply set aside. Other failures at problem solving are blamed on some personal inadequacy of the scientist. Others, perhaps because of their importance, become research problems within the paradigm. Finally, some anomalies can prepare the way for the perception of novelty by loosening some of the ties of the paradigm.

Kuhn stated that "fundamental discrepancies" tend to have an increased potential to bring about revolutions. According to Kuhn, the fundamentality of an anomaly is related to the following factors: (1) whether the anomaly is involved with the solution of an important practical problem, (2) the length of time the puzzle has been refractory to solution, (3) its resistance to the efforts of the ablest practitioners of the paradigm, and (4) whether other developments in science increase the importance of what was previously a minor anomaly.

Thus, according to Kuhn, it is not at all clear what counts as an anomaly. Furthermore, anomalies are not necessary for the occurrence of a scientific revolution, and finally, there is no level of anomaly that is sufficient to produce a revolution.

Are Paradigms Incommensurable?

According to the Oxford English Dictionary (1971) "incommensurable" means "having no common standard of measurement; not comparable in respect of magnitude or value." Kuhn repeatedly used this term when describing the relationship between competing paradigms (e.g., Kuhn 1970a, p. 150). He gave a partial description of the reasons for this incommensurability:

> If there were but one set of scientific problems, one world within which to work on them, and one set of standards for their solution, paradigm competition might be settled more or less routinely by some process like counting the number of problems solved by each. But, in fact, these conditions are never met completely. The proponents of competing paradigms are always at least slightly at cross-purposes. Neither side will grant all the non-empirical assumptions that the other needs in order to make its case… Though each may hope to convert the other to his way of seeing his science and its problems, neither may hope to prove his case. (Kuhn 1970a, pp. 147–148)

According to Kuhn, the incommensurability of two competing paradigms is a result of the following factors: (1) their lists of problems are not the same, (2) their definition and standards of science are not the same, and (3) communication between them, at best, is only partial because shared terms have different meanings or applications between the two paradigms. Thus, according to Kuhn, adherents to two different paradigms belong to different worlds since they speak different languages, see different things, and even intuit in different ways.

Kuhn‘s study of the history of physics has led him to claim that after a scientific revolution, a new paradigm can adopt a radically different ontology than its predecessor. For example, in physics, Aristotle’s ontology involving elements and natural places was eliminated by the Copernican revolution, and Newtonian absolute space and time was eliminated by the Einsteinian revolution. Kuhn pointed out that these differences in ontological commitments make direct comparison of the two paradigms difficult or impossible since they are literally talking about different things.

However, in other passages Kuhn seems to claim that two different paradigms are comparable. For example, he stated that at times, paradigms can be compared in their problem-solving ability:

> Probably the single most prevalent claim advanced by the proponents of a new paradigm is that they can solve the problems that have led the old one to a crisis. When it can legitimately be made, this claim is often the most effective one possible. (Kuhn 1970a, p. 153)

And in another passage:

> The decision to reject one paradigm is always simultaneously the decision to accept another, and the judgment leading to that decision involves the comparison of both paradigms with nature and with each other. (Kuhn 1970a, p. 77)

Kuhn also believed that science demonstrates progress. This implies that later paradigms would, in general, have to be “better” in some sense than earlier paradigms. However, if this is the case, then paradigms are obviously comparable.

Kuhn also claimed that adherents to different paradigms can agree on common values upon which paradigms can be properly criticized:

> … [It is possible to provide] a preliminary codification of good reasons for theory choice. These are, furthermore, reasons of exactly the kind standard in philosophy of science: accuracy, scope, simplicity, fruitfulness, and the like. It is vitally important that scientists be taught to value these characteristics and that they be provided with examples that illustrate them in practice. If they did not hold values like these, their disciplines would develop very differently. (Kuhn 1970c, pp. 261–262)

However, Kuhn stated that these values will not prove decisive in evaluating two competing theories because some of these values are incompatible with others and therefore dictate different conclusions. Thus, there is often a further question of priority among these values. Moreover, disagreements may also arise because there can be differences in the application of these common values. For example, although simplicity may be regarded as a proper value, a judgment of which paradigm is more simple may differ.

Therefore, there is a lack of clarity regarding whether Kuhn believed that paradigms are indeed incommensurable, or whether they are comparable but these comparisons are quite difficult because there is no mechanical decision procedure for choosing between two competing paradigms. Kuhn in the last chapter of SSR appears clearly to argue for incommensurability and against any clear sense of scientific progress but, as seen above, in other passages seems to make claims that at the minimum create a certain amount of tension, (and tension that appears to be inessential).

What is Special About Science?

Kuhn believed that normal science distinguishes science from all other enterprises. What makes normal science distinctive for Kuhn, however, is not at all clear. Is it because during episodes of normal science, science makes progress? At times Kuhn stated that "… the sciences, at least after a certain point in their development, progress in a way that other fields do not…" (Kuhn 1970a, p. 209). However, in another passage Kuhn claimed:

> Scientific progress is not different in kind from progress in other fields, but the absence at most times of competing schools that question each other's aims and standards makes the progress of a normal-scientific community far easier to see. (Kuhn 1970a, p. 163)

Moreover, the supposedly special status of normal science is unclear because Kuhn, in keeping with his sociological orientation, stated that the distinctiveness of normal science occurs because its proffered solutions to puzzles will be accepted as proper solutions by many professional colleagues (due to the sharing of a common paradigm). However, at other times, he claims that science progresses partly because over time theories are increasingly "matched to nature at an increasing number of points and with increasing precision" (Kuhn 1970b, p. 20). This latter statement is especially surprising given Kuhn's generally anti-positivistic stance (as well as his views on the incommensurability of paradigms).

Thus, it is not clear what Kuhn believes is special about science. Is science special because it progressively represents nature more accurately? Is it because it solves puzzles about nature? Is it because it becomes increasingly better at solving puzzles about nature? Because its paradigms simply allow a consensus about fundamentals and about what constitutes a solution to a puzzle? Or, is it because of some combination of these factors?

The issue of whether science has a special status and the exact nature of the distinctiveness of science is important to psychologists because psychologists have typically distinguished their particular approach to the study of human behavior and human problems from other approaches (e.g., religious, humanistic, folk) on assertions that psychology is scientific (e.g., McFall 1991). The scientific study of human behavior is thought to be special and qua science thought to have clear advantages over other approaches (e.g., a more efficient method of generating relevant knowledge). Thus, when the issue of what is special about science becomes unclear, psychologists' assertions about the merits of a scientific approach to the study of human behavior (e.g., the scientist-practitioner model) over all the diverse nonscientific approaches become problematic.

Are Kuhn's Views Descriptive or Normative?

Kuhn has been criticized (by Feyerabend 1970, among others) for being ambiguous regarding whether his account is merely a description of science devoid of any evaluative import or whether his account is prescriptive in that it attempts to point out what is good science or what is good about science. If the first is correct, Kuhn might be read as an historian who simply recounts contingent facts regarding how science has developed so far and as making no claims regarding what are the essential properties of good science. In the second reading, Kuhn's account would describe the essence of science, as well as telling of its regulative properties. Thus, Kuhn's account could tell what scientists ought to do qua good scientists.

Kuhn (1970a) clearly stated that in his account of science, the descriptive and the normative are inextricably wed:

> Are Kuhn's remarks about scientific development, he [Feyerabend] asks, to be read as descriptions or prescriptions? The answer, of course is that they should be read in both ways at once. If I have a theory of how and why science works, it must necessarily have implications for the way in which scientists should behave if their enterprise is to flourish. (Kuhn 1970c, p. 237)

Kuhn believed that on this score, his views are no different than other accounts of science: that is, he stated that the views of Popper, Lakatos, Feyerabend, Toulmin, and Watkins are also both descriptive and normative. It should be noted, however, that it is not at all clear what normative claims Kuhn's analysis actually supports. Would these claims be something like the following? If your field has yet to mature then you ought to find a paradigm that will support puzzle solving and that will allow a consensus to form regarding fundamentals of your field. If your field is in an episode of normal science, then you ought to continue solving puzzles within the framework of your paradigm. However, if there is a crisis in your field, due perhaps to an unacceptable level of anomaly, then look for an alternative paradigm that better handles these anomalies that will allow another consensus to emerge and then solve puzzles using this. It is not at all clear whether these prescriptions are anything more than slightly jargonized platitudes.

The frequent use of Kuhn by psychologists is puzzling given the lack of clarity of the normative implications of Kuhn's account. Presumably, at least part of the reason for psychologists' interest in meta-scientific enquiry is to attempt to understand how they might do better science. Philosophers of science such as Popper (1963) are very clear on this—specify your problem dearly, make a bold conjecture, and attempt to falsify this! Mulkay and Gilbert (1981) supply evidence that Popper's prescriptions were so variously interpreted by a group of biochemists in a research network that these prescriptions were unable to provide clear constraints on the behavior of the scientists. Mulkay and Gilbert interpreted this problem as stemming from Popper's neglect of the issue of how acts are related to rules. However, for Kuhn, the problem appears to be more basic: it is unclear what rules or advice (if any) Kuhn is giving.

The Special Status of Psychology

In appraising the adequacy of a descriptive account of science, psychologists have a particularly perspicacious vantage point. The behavior of scientists (including perceptual processes; cognitive processes, heuristics and biases; mechanisms of attitude change, group influences, and social cognition) would all appear to be subsumed under psychological laws or described by some model, if such laws are universal, generalizations or the models' scope includes all human behavior. (Of course, the physicist also has a vantage point—the scientist's physical behavior will follow all physical laws. However, physics does not provide the kind of information typically sought in attempts to understand the scientist's behavior.) Thus, a psychologist interested in appraising a descriptive account of science can always ask: Is this account of the behavior of the scientist in line with our best understanding of human behavior? Admittedly, this is complicated by the scarcity of a clear consensus in some areas on what comprises the best understanding of human behavior, and by the fact that the behavior of the scientist may be used as evidence that received claims concerning psychological regularities need to be modified. However, for our purposes, the question becomes this: Is Kuhn's account of scientific behavior in line with what psychology tells us about human behavior? Interestingly enough, psychologists who would seem to be disposed to disagree with the kind of psychology Kuhn utilizes still have an overwhelmingly positive reaction to him. O'Donohue (1990) pointed out the inconsistency of behaviorists' approbation of the Kuhnian account. I will examine the question of Kuhn's use of psychology in the next section.

How Does Kuhn Use Psychology?

Although Kuhn claimed that he proceeded as an historian of science, he does not appear to have included a study of the history of psychology in his original investigation despite the fact that much of the work was done while he was a Fellow in the Center for Advanced Studies in the Behavioral Sciences at Stanford. Thus, the first point to be made is that the history of psychology was not a direct part of his historical database.

It is not clear why this is the case. One possible explanation is that Kuhn regarded psychology as a science but due to pragmatic factors—he was trained as a physicist and therefore less familiar with social science, and a study of all science would have been extremely time consuming–he choose not to examine the history of psychology. However, another possibility is that he did not regard psychology as a science–in his terms, as possessing a paradigm capable of supporting puzzle solving and therefore psychology did not have the proper characteristic to be included as part of the database. It is difficult to decide between these two alternatives. The first seems to be supported by Kuhn's use of psychological research in his analytic framework. The second seems to be supported by his negative statements about psychology that will be examined in the next section of this paper.

Although the history of psychology was not examined by Kuhn and therefore was not part of his object language, psychology apparently had some influence on his method of enquiry and upon his meta-language. Kuhn's writings give evidence of the following influences from psychology: (1) the work of Piaget, (2) work concerning the psychology of perception, especially the gestalt psychologists, (3) work in experimental cognitive psychology, and (4) work in social psychology. The diversity and extent of the influence is all the more remarkable given the apparent lack of explicit influence by other disciplines such as logic, philosophy (with the exception of Wittgenstein), and even sociology. We will turn now to a more detailed examination of these psychological influences.

Piaget and the Gestalt psychologists are mentioned by Kuhn rather than given any considered prominence. In the preface of SSR, Kuhn mentions being influenced by Piaget but Piaget's work is not examined or used in any detail. Kuhn (1976) stated that he was led to the work of Piaget by a footnote in R. K. Merton's Science, Society and Technology in Seventeenth Century England. It seems that Kuhn would like Piaget's views concerning the perceiver's contributions to perception and the often sudden transition from one set of perceptual abilities to another. The Gestalt psychologists are not discussed in any detail either, although Kuhn often relies upon a notion of a "gestalt switch" in his account of perception.

Kuhn did discuss in slightly more detail a few studies concerning the psychology of perception that seem to be a part of experimental cognitive psychology. Before discussing Kuhn's use of these studies, two preliminary points should be made. Firstly, Kuhn did not appear to systematically review the experimental cognitive literature on the psychology of perception. He described only two of these studies and mentions in a footnote two more (Hastorf 1950; Bruner et al. 1951). Secondly, the two studies he does describe in some detail, although they may be properly regarded as classics, were quite old even by the time of the first edition of SSR. Bruner and Postman's experiment was reported in 1949 and Stratton's in 1897.

The Bruner and Postman (1949) experiment entitled "On the Perception of Incongruity: A Paradigm" received the most attention from Kuhn. (One also wonders if Kuhn acquired the term "paradigm" from this study.) In this experiment, subjects were briefly presented with normal and anomalous playing cards (e.g., a black four of hearts). Subjects tended to misperceive the anomalous cards as being normal, and even with long exposure times, many subjects had difficulty specifying what was anomalous about the cards. Kuhn takes this experiment as demonstrating that "In science, as in the playing card experiment, novelty emerges only with difficulty, manifested by resistance, against a background provided by expectation" (Kuhn 1970a, p. 64).

In the well-known Stratton (1897) study, a subject wore goggles that caused the retinal image to be the reverse of what it is normally. As a result, the subject initially saw everything upside down. Soon, however, the visual system underwent some sort of adaptation and the subject no longer saw the objects as upside down, but rather in their normal orientation. Kuhn took this study as demonstrating that "The assimilation of a previously anomalous visual field has reacted upon and changed the field itself" (Kuhn 1970a, p. 112).

Although Kuhn seems to have been influenced by at least a few experiments in constructing his theory of perception, he made puzzling references to his account being a "social psychology" of science. For example, he stated, "My recourse has been exclusively to social psychology (I prefer 'sociology') a field quite different from individual psychology reiterated in times" (Kuhn 1970c, p. 240). In another passage he stated, "The type of question I ask has therefore been: how will a particular constellation of beliefs, values, and imperatives affect group behavior?" (Kuhn 1970c, p. 240).

One encounters several difficulties in attempting to explicate Kuhn's use of social psychology. Firstly, the research reviewed above is not usually classified as social psychological research. It seems that his explicit use of psychological research is not the use of social psychological research. Secondly, his method of enquiry is not that of contemporary experimental social psychology. That is, he does not conduct experiments concerning how groups affect the behavior of the individual scientist (Baron and Byrne 1987). Williams (1970) has claimed that a proper analysis of the social structure of science has yet to be conducted. What is clear is that Kuhn did not use the methods of experimental social psychology or the methods of contemporary sociology. He did not empirically demarcate groups of scientists, measure the extent of their consensus, identify the exact mechanisms of social influence, and determine empirically the exact dynamics of change. (For beginnings of this, see the journal, Social Studies of Science.) Thirdly, even his historical method tends to highlight individual behavior, and the behavior of others in the scientific community is usually in the background and then rarely even vaguely characterized. Thus, his use of the phrase "social psychology" does not seem to refer to the principles and practices of academic social psychology. Rather he seems to rely on a much more informal definition of this phrase in which it is semantically correct to say one is interested in social psychology (or even sociology) simply because one is interested in the behavior of groups.

If Kuhn were to have reviewed the social psychology literature, one would expect that he would have found relevant research that pertains to his account of science. Asch's (1951) conformity experiments directly pertain to the influence of a group on perception (or reported perception). In these experiments, Asch constructed groups that consisted of several experimental confederates and one subject. Everyone was asked to examine a series of lines and to state which was the longest. Asch found that when the confederates in the group first reported what would normally be an obviously wrong judgment, a significant number of subjects conformed to this wrong judgment. This sort of research might be used by Kuhn to support his notions of group influence on basic judgments, but it also might present worries about this group influence in decreasing accuracy.

Another source of relevant social psychological research that Kuhn might have cited are the experiments of Sherif (1935, 1936) on social judgment, social norms, and group behavior. For example, in the autokinetic effect, Sherif (1935) found that subjects' judgments about the apparent movement of a (actually stationary) point of light were highly influenced by the judgments of others. In this experiment, half of the subjects first made judgments about the extent of movement while alone, and subsequently made judgments in small groups. Another group made judgments in the opposite sequence. Subjects who first made their judgments alone tended to develop a personally fairly stable estimate—a personal norm. However, this estimate varied considerably across individuals. Subsequently when subjects, each with highly different personal norms, were asked to make judgments in small groups, subjects' judgments converged toward greater homogeneity—a group norm. The experimental group ran in the opposite order, subjects first developed a group norm, and this norm persisted when asked to make judgments alone. This experiment may shed light on how the judgments of scientists may adjust to the judgments of other scientists and thereby move to consensus. This may be an important process in the development and maintenance of a scientific community or micro-community.

Other more recent research in social cognition (Nisbett and Ross 1980) has direct implications for Kuhn's more recent views, sometimes corroborating some of his views, and sometimes calling other views into question. Research into confirmation bias (Snyder and Swann 1978) in which humans display a tendency to attend to information that supports their preconceptions and to ignore contradictory evidence should support and provide additional detail for the Kuhnian account. Studies of illusory correlation (Hamilton and Gifford 1976) in which individuals find an expected relationship to be confirmed despite objective disconfirming evidence also seems to be relevant for a further explication of mechanisms involved in Kuhn's account. Research into the representativeness heuristic (Tversky and Kahneman 1982) which suggests that humans' process information based upon its resemblances to typical cases might be used to gain an additional understanding of the role of paradigms, and especially exemplars, in perception and information processing. Theory perseverance (Ross et al. 1977), which is the tendency of people to maintain beliefs even when they have learned that the belief is false, has direct implications for understanding the mechanisms of paradigm maintenance and change.

Finally, other principles in social cognition would seem to require some modifications in Kuhn's account of science. The false consensus effect (Ross et al. 1977), which suggests that individuals will tend to overestimate the degree to which other individuals will make the same judgments, choices and hold the same opinions, might supply interesting insights regarding the individual's perceptions and misperceptions of the scientific community. In his more recent writings, Kuhn could have called upon the actor-observer effect (Jones and Nisbett 1971). This effect suggests that humans tend to attribute their own behavior to situational factors and the behavior of others to internal or dispositional factors. This principle would seem to predict that scientists will attribute their own failures at puzzle solving to some external factor, perhaps to the paradigm, but that they will tend to attribute blame to the personal inadequacies of other scientists when their experiments fail to solve problems. The self-serving bias (Miller and Ross 1975), in which individuals take credit for positive outcomes but place blame on external causes for negative outcomes, would also predict that individual scientists would not attribute blame to themselves when their puzzle-solving attempts have failed.

It is beyond the scope of this chapter to review exhaustively and critically the research in social psychology that is relevant to the development of a social psychology of science. The aim of this chapter is mainly exegetical. It does seem reasonable to conclude that Kuhn's use of psychology was pedestrian. Psychology was not included as a part of his original database in his studies of "science." His use of psychology in his analytic framework also seems to be quite informal. He drew superficially from Gestalt psychology and the psychology of perception. Moreover, Kuhn has not proceeded as a contemporary social psychologist would in studying science, nor has he used results from social psychology that seem relevant to his general positions.

What does Kuhn Say About Psychology?

I will not attempt to draw out all the implications of the Kuhnian account of science for psychology as this has been done elsewhere (Palermo 1971; Warren 1971; Burgess 1972; Weimer and Palermo 1973; Buss 1978; Peterson 1981; Gholson and Barker 1985). However, it is noteworthy that there is little consensus about whether psychology or subfields of psychology possess a paradigm or paradigms capable of supporting puzzle solving.

We will attempt to determine Kuhn's own view about the status of psychology by examining his comments about psychology. Two preliminary points need to be made. Firstly, it would be reasonable to take Kuhn's use of psychology in his analytic framework as prima-facie evidence that Kuhn regarded psychology as a science. Secondly, in Kuhn's more recent emphasis on the micro-community structure of science, the question of whether psychology has a paradigm is wrongly put because it is too broad. Kuhn ("personal communication," April 1989) stated:

> … [P]sychology is probably too much of a catchall field to generalize about. I've no reason to suppose that the same answers would be forthcoming if the same questions were addressed, say, to learning theory, clinical psychology, perceptual psychology, and intelligence testing. What the answers would be if the field were appropriately subdivided, I'm not the one to say. You have to know the field(s) from the inside to do that.

Kuhn made a great many comments that suggest that he does not view psychology as a mature science. Against his use of psychology as *prima-facie* evidence of a positive evaluation, Kuhn (1970a) stated:

> If he [Popper] means that the generalizations which constitute received theories in sociology and psychology (and history?) are weak reeds from which to weave a philosophy of science, I could not agree more heartily. (Kuhn 1970c, p. 235)

Kuhn often spoke more broadly about "social science," although due to the lack of his explicit discussion of any social science other than psychology, it seems plausible to assume that these remarks can be taken as relevant to psychology. Kuhn seems to equivocate in his remarks regarding the status of the social sciences. At times, he stated it is an "open question" whether subfields of the social sciences have paradigms. In other places, he suggested that in some parts of the social sciences, the emergence of paradigm governed normal science "may well be occurring today" (Kuhn 1970a, p. 21). In another passage, he stated that the social sciences will "surely" experience the transition to maturity in the future (Kuhn 1970b, p. 245).

However, it seems fair to say that in general Kuhn takes the social sciences to be "proto-sciences" (Kuhn 1970c, p. 244) because although they generate testable hypotheses they do not result in "clear cut progress" (Kuhn 1970c, p. 244). The social sciences are still characterized as a "tradition of claims, counterclaims, and debates over fundamentals" (Kuhn 1970b, p. 6).

> In the physical sciences disagreement about fundamentals is, like the search for basic innovations, reserved for periods of crisis. It is, however, by no means equally clear that a consensus of anything like similar strength and scope ordinarily characterizes the social sciences. Experience with my university colleagues and a fortunate year spent at the Center for Advanced Study in the Behavioral Sciences suggest that the fundamental agreement which physicists, say, can normally take for granted has only recently begun to emerge in a few areas of social science research. Most other areas are still characterized by fundamental disagreements about the definition of the field, its paradigm achievements, and its problems. While that situation obtains (as it did also in earlier periods of the development of the various physical sciences), either there can be no crises or there can never be anything else (Kuhn 1977, p. 222).

Interestingly, Kuhn made what can plausibly be taken as disparaging remarks about psychology even when he is citing psychological research that forms an important part of his account. In the introduction of his extensive discussion of the Bruner and Postman (1949) experiment, he stated that it "deserves to be far better known outside the trade" (Kuhn 1970a, p. 62, italics added). He even seemed to be extremely cautious concerning the results of this experiment. He stated, "Either as a metaphor [sic!] or because it reflects the nature of the mind, that psychological experiment provides a wonderfully simple and cogent schema for the process of scientific discovery" (Kuhn 1970a, p. 64). It appears that for Kuhn, conclusions from psychological research must not be taken too literally.

How does Kuhn account for the developmental delay of psychology and the other social sciences? Because of the large relevance of these disciplines for urgent practical problems, Kuhn argued that they lack isolation from the demands of everyday concerns. Kuhn stated:

> … [T]he insulation of the scientific community from society permits the individual scientist to concentrate his attention upon problems that he has good reason to believe he will be able to solve. Unlike the engineer, and many doctors, and most theologians, the scientist need not choose problems because they urgently need solution and without regard for the tools available to solve them. In this respect, also, the contrast between natural scientists and many social scientists proves instructive. The latter often tend, as the former almost never do, to defend their choice of a research problem—e.g., the effects of racial discrimination or the causes of the business cycle–chiefly in terms of the social importance of achieving a solution. Which group would one then expect to solve problems at a more rapid rate? (Kuhn 1970a, p. 164)

Thus, social scientists are forced to address problems that due to their social importance demand solution, rather than having the ability to choose problems that are judged by their paradigms to have a good potential for solution, as their colleagues in the natural sciences can do.

Another reason for the developmental delay of psychology, according to Kuhn, is that failures can be explained by the enormous complexity of these problems, and therefore, failures do not give rise to research problems. Because social scientists often choose to investigate important social problems, they tend to work on problems that are extremely complex. A general consequence of problem complexity for Kuhn is that failures do not give rise to research problems. Kuhn believed this is due to the abundance of possible sources of difficulty, most of which are beyond the investigator's "knowledge, control, or responsibility" (Kuhn 1970b, p. 9).

Conclusions

In a close and I hope accurate examination of Kuhn's writings on science, we have found that his views are unclear at several critical points: his central concept of a paradigm is neither clearly nor consistently defined; The conditions for scientific revolutions are only imprecisely characterized; Kuhn seems to equivocate on the question of whether paradigms are incommensurable; it is not clear what Kuhn regards as special about science or even whether he regards science as special; and, Kuhn's normative prescriptions about what a good scientist ought to do are not clearly delineated.

Kuhn's use of psychological research in his account is also puzzling. Although apparently of some influence, Kuhn's use of psychology does not seem to be the result of anything approaching a systematic and critical review of the relevant research. His stated goal of developing a social psychology of science is an interesting and potentially even an important one. It is beyond the scope of this paper to review exhaustively relevant social psychological research and draw out all the potential implications for a social psychology of science or for Kuhn's account. However, the point was made that, given Kuhn's lack of consideration of obviously relevant studies, his use of psychological research was unsystematic and pedestrian. Moreover, the studies he does report are not social psychological research. That he relies on psychological research at all is puzzling given his disparaging remarks about the scientific status of psychology. If psychology and more specifically the psychology of perception are not mature sciences, then why draw upon this research?

Philosophers should be mindful of Kuhn's unsystematic use of psychology. A nonpsychologist might receive the impression in reading Kuhn that some of his views are soundly derived from research in the psychology of perception and social psychology and they are not. Kuhn's use of psychology is informal and his methodology is not an investigatory method utilized by experimental psychologists. He perhaps qualifies as a social historian of science, but not as an experimental social psychologist.

Kuhn‘s view of science seems to have a questionable value for psychologists. He did not examine the history of psychology or any of its subfields in his original studies, nor has he examined it subsequently in an attempt to test his model. There has not been a clear consensus among historians of psychology concerning the accuracy of his account when applied to psychology. Of course, the ambiguities and inconsistencies in Kuhn’s account also limit its usefulness for psychologists.

As pointed out by Coleman and Salamon (1988), the emphasis that psychologists have placed on Kuhn is worrisome. Gholson and Barker (1985) have argued that other philosophers have given a more satisfying account of science and thus are deserving of more attention from psychologists. Gholson and Barker specifically argue that Lakatos (1970) and Laudan (1977)—who we will cover in the next chapter—provide a more accurate and useful account of science for psychologists. Furthermore, philosophers such as Quine and Popper provide important views of science and related matters and have advanced positions that have been much more influential in the development of philosophy than Kuhn’s.

The question of why psychologists have found the views of Kuhn so appealing remains. Here, I can only offer some conjectures. Perhaps psychologists have found Kuhn attractive because at times, he sounds like a psychologist. However, if we look closely, he does not use investigatory methods that psychologists use nor are his views based on a systematic use of psychology. He also at times makes some very disparaging remarks concerning psychology. Kuhn can be properly regarded as an historian of the physical sciences. The relevance of this kind of information for problems in psychology is an open question.

Perhaps psychologists have found Kuhn attractive because he might describe and explain the frustrations of psychologists and offer consolations. Although he does offer some interesting possibilities concerning the special problems of applied research concerning humans, his model of science is based upon the physical sciences, and as such it offers little, interesting analysis of the history or current status of psychology beyond some quick and negative generalizations. Given Kuhn’s comments about psychology and his description about the deep debates concerning fundamentals that characterize immature science, it seems that much of psychology would likely deserve the status of immature science. It is unclear how much consolation psychologists would take from this categorization.

Psychologists have particularly emphasized the notion of “paradigm.” Perhaps psychologists like the relativism that is sometimes associated with this notion. Psychologists might believe that Kuhn offers them a “feel good” account of science in which everybody wins and all the diverse schools within psychology can claim their own truth and legitimacy. Psychologists might also hope that Kuhn will offer some sound advice as to how to improve psychology. But, it would seem that due to the lack of informative normative implications of Kuhn’s account, Kuhn again disappoints.

Finally, the popularity of Kuhn‘s views among psychologists might be based on the perceived promise in his views for collaboration between psychologists and philosophers of science in attempts to understand science. This certainly is an interesting and promising avenue of enquiry. However, it is important for psychologists to understand that other collaborative efforts already exist. Psychologists are beginning to develop the psychology of science (Houts 1988) and philosophers of science have attempted to develop cognitive accounts of psychology which rely heavily and more faithfully on experimental cognitive psychology (Giere 1988). Thus, to use what should now be familiar phrasing, if normal meta-science in psychology has been dominated by the Kuhnian paradigm, I suggest a revolution in which other philosophical paradigms are considered.

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Chapter 6
Four Other Major Philosophers of Science

In this chapter, we will provide a brief overview of four other major philosophers of science: Paul Feyerabend, Imre Lakatos, Larry Laudan, and Alan Gross. Each of these philosophers' work has been impactful and the reader must have some acquaintance with this literature if they are to be conversant in the contemporary meta-scientific literature.

Paul Feyerabend

The only principle that does not inhibit progress is: anything goes.
Feyerabend (1975), p. 23.

Paul Feyerabend was a student of Karl Popper's, although as we shall see he had very different views of a number of key issues. He fought in the German Army in World War II was seriously injured by a bullet which lodged into his spin, and walked for the rest of his life with a cane. Feyerabend has come to be known as a *methodological anarchist*. His central claim is that if one carefully reviews the history of science, there is no one scientific method; and even stronger, whatever methodological prescriptive rules philosophers of science might tell scientists to follow, the history of science also reveals that in successful episodes of science, scientists have violated these rules. For example, successful scientists ignored anomalous—potentially falsifying–observations (instead of using these to falsifying their favored theories); they let external forces such as politics and metaphysics influence their interpretation of data (instead of just relying upon the raw observations); and they chose to favor a theory that had much less empirical support than a rival, etc.

Feyerabend also called himself a *methodological Dadaist*—because he thought his view involved questioning widely accepted and time-honored conventions and promulgating radical new views. In the art world, Dadaists, such as Marcel Duchamp, engaged in radical acts such as sneaking shovels purchased from hardware stores and placing them in art museums—thus radically calling into question, "What is art?" and "What is the role of museums in defining art?"

W. O'Donohue, *Clinical Psychology and the Philosophy of Science*,
DOI: 10.1007/978-3-319-00185-2_6,

Here are a few key quotes to get a better idea of Feyerabend's views. I also want you to appreciate Feyerabend's rhetorical style—he is more than a bit of a provocateur:

> Everywhere science is enriched by unscientific methods and unscientific results, … the separation of science and non-science is not only artificial but also detrimental to the advancement of knowledge. If we want to understand nature, if we want to master our physical surroundings, then we must use all ideas, all methods, and not just a small selection of them.
> Feyerabend (1975), 305–306.

> Given any rule, however 'fundamental' or 'necessary' for science, there are always circumstances when it is advisable not only to ignore the rule, but to adopt its opposite. For example, there are circumstances when it is advisable to introduce, elaborate and defend *ad hoc* hypotheses, or hypotheses which contradict well-established and generally accepted experimental results, or hypotheses whose content is smaller than the content of the existing and empirically adequate alternative, or self-inconsistent hypotheses, and soon.
> Feyerabend (1975), 23–24.

> It is clear, then, that the idea of a fixed method, or of a fixed theory of rationality, rests on too naive a view of man and his social surroundings. To those who look at the rich material provided by history, and who are not intent on impoverishing it in order to please their lower instincts, their craving for intellectual security in the form of clarity, precision, 'objectivity', 'truth', it will become clear that there is only one principle that can be defended under all circumstances and in all stages of human development. It is the principle: anything goes.
> Feyerabend (1975), 27–28.

> Science is an essentially anarchic enterprise: theoretical anarchism is more humanitarian and more likely to encourage progress than its law-and-order alternatives.
> Feyerabend (1975), 9.

> Unanimity of opinion may be fitting for a church, for the frightened or greedy victims of some (ancient, or modern) myth, or for the weak and willing followers of some tyrant. Variety of opinion is necessary for objective knowledge. And a method that encourages variety is also the only method that is comparable with a humanitarian outlook.
> Feyerabend (1975, p. 8) (emphasis in original).

Early Feyerabend: Methodological and Theoretical Pluralism

Feyerabend's radical anarchism had some interesting early roots. In his early writings (before his classic *Against Method*), he advocated *methodological and theoretical pluralism*. He thought that if scientists take seriously Popper's admonition to *maximize criticism*, then in order to accomplish this end scientists would need to develop and examine other theories because these will provide novel perspectives where new criticisms can be generated. For example, if the field of psychotherapy only contained cognitive behavioral theories, a certain amount of criticism certainly would be generated internally, but this criticism would be constrained by shared assumptions and theoretical commitments. It would generate only a small subset of all possible criticism. However, when radically different theories are generated, then new and potentially important criticisms are also generated. For example, a psychoanalyst could provide criticisms regarding the possibility of symptom substitution or problems outside the client's awareness—constructs and problems not typically on the cognitive behavior therapists radar screen due to their assumptive framework.

One might think, "Well and good—if a field is lucky enough to have multiple theories that all enjoy roughly equivalent empirical support, then this strategy seems reasonable—let these theories generate criticisms regarding one another." However, it is important to understand that Feyerabend is recommending a quite different and more radical strategy. Feyerabend insisted that because any theory, no matter how weak, may eventually become empirically adequate that *any* theory may contribute to this critical process. To maximize criticism, he therefore suggested, allowing a place at the table for any theory, including implausible conjectures having no empirical support, as well as conjectures which are inconsistent with data and even inconsistent with well-confirmed laws. Furthermore, the scientist should retain theories that are in trouble, and invent and develop theories that contradict the observed phenomena, just because in doing so we will be respecting the rational ideal of maximizing criticism and thus maximizing testability. Thus, good scientists need to *manufacture dissent*. However, this runs counter to the notion that science produces consensus—and that this consensus is rational because what is believed has been so well tested and supported. Feyerabend is concerned that this is in fact dogmatic—treating that which is really not settled, as settled, and beyond the pale of doubt and criticism.

Let me examine an example to make this a bit clearer. Suppose we are attempting to critically assess the value of some treatment procedure. In the usual case, let us assume that there are physicians who tend to be more intervention oriented and will treat in the hopes of having a positive impact. Let us also assume for the purposes of this example that this interventionist approach has serious iatrogenic effects. If there are only physicians who aggressively intervene, this interventionism itself might never be critiqued—and thus iatrogenic effects of intervention might not be seen. The physicians may disagree on which particular medication in the formulary, or dosages, but they might all share the view that some intervention is called for. What value might a radically different view have—say that of a Christian Scientist who would insist that no medical intervention ever be given but instead prayer should be used? Feyerabend might point out that the benefit of the theory of the Christian Scientist would be shown when such natural no-treatment control conditions would help reveal that the aggressive medical treatment is actually iatrogenic—all these interventions were actually causing harm. This would never be seen if the ruling theory always called for some sort of intervention. Feyerabend may also point out that the prayer of the Christian Scientist—done with a loving family, friends, and church members—might also be more comforting to the individual—the ill individual, for example, find more connectedness, comfort, and even meaning in their illness than the more sterile, technological medical approach. Thus, Feyerabend wants us not to be immediately dismissive of such radically different views but to take deeper looks and see what they offer—even if what they do offer does not share the assumptive framework of science and is not among the effects typically measured or deemed important in science.

The Democratization of Science

Feyerabend should not be read as "anti-science." Feyerabend seemed to think that there are other values that are at least as important as those associated with science—and perhaps even more important. Feyerabend considered the question of what is the relationship between values such as freedom and human dignity and the values typically associated with science. Is it the case the concepts such as "truth" and "empirical support" are more important than "kindness" and "sympathy?" Are the regulative concepts of science just rhetorical devices used by those in power to oppress other people? Are the scientist's "objectivity" and "research methods" weapons that eventually undermine the values of the less empowered, for example, the cultures of First Peoples? Feyerabend, however, can become anti-science to the extent that he sees that in its practice, it becomes a dogma that is used politically to impinge on human freedom. Science for him can become tyrannical. Government now uses science advisors in a similar way that medieval monarchs used religious advisors. Feyerabend did not claim that science is necessarily a dogma, but rather that science has become dogmatic, as it has become an ideology that has gained an effective monopoly—especially among the ruling elites. Feyerabend (1975, p. 295) has stated:

> The separation of state and church must be complemented by the separation of state and science, that most recent, most aggressive, and most dogmatic religious institution.

Feyerabend supports an openness to a wide variety of thought and liberty of thought, and this for him means a plurality of different kinds of thought. This view of course puts him in conflict with those who insist that scientific reasoning is *the* superior mode of thought. Scientists can falsely see themselves as open minded and liberal in their thought—however Feyerabend points out that it is often these very people who advocate intellectual oppression.

Watson (2003) saw Feyerabend as saying:

> Science is not the only worthwhile human goal, and within science as a goal there is no one proper method. Perhaps, if the task of science is ever completed, we will be able to look back over its history and discuss whether any particular method would have been sufficient to the entire task. In the meantime, we are better off with many methods than with dogmatic adherence to any single method. So let us have many methods, and many spirited debates as to why one method is better than another. Let the practitioners of each method boast with their results, the progress that they make, the technologies they develop, the discoveries they bring to light, their explanatory or predictive power; and let them adopt all the best techniques of their opponents as they recognize them.

Feyerabend pointed out that science and scientism (an ideology involving dogmatic and authoritarian view of science) has resulted in a number of social ills. From Hiroshima to the destruction of the cultures of indigenous peoples to totalitarian regimes allegedly based on science—such as the racist eugenics of the Nazis. For Feyerabend, it may well be that science deserves a somewhat special seat in the pantheon of knowledge, but on the other hand, maybe it is no more or less important than other kinds of knowing. If Feyerabend is right, all branches of knowledge should adopt an attitude of humility, and encourage diversity of opinion rather than engaging in a process of elimination. They should do this for the sake of their own progress, as well as for intellectual liberty in general.

Houts (2009), for example, contrasted two very different kinds of knowing: the detached, "objective" logical reasoning valued by the Athenian philosophers versus the subjective, particular, involved, and personal knowing described in religious teachings; Houts (2009) stated:

> At the other end of the universal-versus-particular-pole lies the individual instance. In focusing on this way of thinking, the individual person and what goes on in the emotionally laden and embodied musing of the heart are the focus on concern and the model for how to think. The Hebraic and early Christian texts of the bible are filled with examples of stories about individuals. Teaching is conducted by concrete example rather than abstract principle. Truth is obtained not from detachment, but from engagement and commitment to some particular tradition encountered in powerful experiences (pp. 262–263).

Thus, for Feyerabend, there are very important aspects of the world—a mother's love; a cancer stricken individuals' experience of dying, a child's trust—that cannot be fully understood by the way's of knowing advocated by science, but can best be known when other modes of knowing are countenanced and valued. This is potentially important for the psychologist as these kinds of human experiences need to be captured if psychology is going to be complete.

We will see some of these kinds of thoughts also in the postmodern critiques that we will cover in the final chapter. Feyerabend is certainly the intellectual forefather of criticisms of science that came later from feminists and Marxists, among others.

Lakatos Research Programs and "Sophisticated Methodological Falsification"

> It is not that we propose a theory and Nature may shout NO; rather, we propose a maze of theories, and nature may shout INCONSISTENT. Imre Lakatos.

Imre Lakatos was also a student of Popper's. Lakatos' account of science can be viewed as taking the Duhem-Quine problem quite seriously and attempting to resolve it. He eventually came to call Popper's view *naïve falsificationism*—because he claimed scientists are correct if they do not abandon a theory when research reveals an anomalous observation—that, when confronted with the Duhem-Quine problem, they do not direct the arrows of *modus tollens* (falsification) to the theory being tested. For Lakatos, it is both descriptively accurate and can be prescriptively proper (provided certain conditions are met) for a good scientist to direct the arrows of *modus tollens* to an auxiliary hypothesis. According to Lakatos, Popper's account "ignores the remarkable tenacity of some theories." The decisions to blame the auxiliary hypotheses for a prediction failure are not always *ad hoc*, contrary to Popper. Some also have seen Lakatos' analysis as an attempt to present a compromise between Popper's prescription that an observed anomaly should always falsify a theory, and Kuhn's observation that in normal science, a paradigm continues to be adhered to even when there are numerous anomalies associated with it.

However, one can ask if all scientists are deflecting the arrows of *modus tollens* away from the favored theory how do we distinguish between good science and bad science? Lakatos advocated what he took to be a more *sophisticated methodological falsificationism* in which individual experiments are not judged but rather science is judged by several criteria by how it changes over time—especially how it changes when faced with anomalies. Lakatos offered some novel technical terms that we will have to know in order to understand his account of science:

- *A research program* consists of a *hard core* (theory), *protective belt* (auxiliary hypotheses), and a *positive* and *negative heuristic*.
- The *hard core* of the research program is a set of theoretical assumptions that are never abandoned even when anomalies are observed.
- *Auxiliary hypotheses*: we have seen these before when we described the Duhem-Quine problem. These are statements and theories that are smaller in scope that are used in combination with the hard core to make experimental predictions. In addition, the auxiliary hypotheses are used to explain the evidence that threatens the hard core. The arrows of *modus tollens* are always directed toward these auxiliary hypotheses. These are seen as expendable. They are altered or abandoned when inconsistent evidence threatens the hard core.
- *A heuristic,* according to Lakatos, is "powerful problem solving machinery, which digests anomalies and turns them into positive evidence. For instance, if a planet does not move exactly as it should the Newtonian scientist checks his conjectures concerning atmospheric refraction, concerning propagation of light in magnetic storms and hundreds of other conjectures that are all part of the program. He may even invent a hitherto unknown planet and calculate its position, mass and velocity in order to explain the anomaly" (Lakatos 1970, p. 5). The negative heuristic forbids scientists to question or criticize the hard core of the research program. The positive heuristic "consists of practically articulated set of suggestions or hints on how to change, develop the 'refutable' variants of the research program, how to modify, sophisticate, the 'refutable' protective belt" (Lakatos 1970, p. 135).

Lakatos' unit of analysis for evaluating science was how a research program develops over time. Thus, science according to Lakatos may be pictured as

$$\text{HardCore} + \text{AuxiliaryHyp1} + \text{Heuristics} \rightarrow \text{Predictions (Anomalies)} \rightarrow$$
$$\text{HardCore1} + \text{AuxilaryHyp2} + \text{Heuristics} \rightarrow \text{Predictions} \ \ldots \text{ and so on}$$

Now in appraising this progression, Lakatos said the progression of these changes to the auxiliary hypotheses can be problematic, which he called *degenerating* (he agrees with Popper at least at times these changes can be *ad hoc*) or this progression can be good—his term was *progressive*. In order to be progressive this, series of research programs has to meet two criteria:

1. The modifications to the auxiliary hypotheses have to make novel predictions (i.e., predict empirical states of affairs that the earlier research program did not). It can then be said to be *theoretical progressive*.
2. At least one of these new predictions has to be observed to be true. The research program can then be said to be *empirically progressive.*

If these two conditions are met then the research program is seen as progressive. If one is not met then it is seen as degenerating. Another way of saying this is that progressive research programs have increased explanatory and predictive power; while degenerating research programs have decreased explanatory and predictive power. Therefore, accurate anticipation is the key for Lakatos; not falsification.

For example, Lakatos would suggest that astrology is not a progressive scientific research program. Astrology makes false predictions; but then the question becomes for Lakatos how does it accommodate these anomalies? Astrology has no theoretically progressive shifts, and therefore no empirically progressive problem shifts. That is, it makes no *novel* predictions, despite that fact that it makes many predictions. If it makes no novel prediction, in principle it cannot be empirically progressive (since it is impossible for a novel prediction to be corroborated). Therefore, astrology was not progressive science.

Lakatos' other examples of degenerating research programs included Freudian psychoanalysis, Ptolemaic astronomy, Soviet Marxism, and somewhat surprisingly, Darwin's evolutionary theory. Others also have been concerned with some evolutionary accounts which they have derisively called "just so stories." For example, some evolutionary theorists may be tempted to devise auxiliary assumptions in an *ad hoc* way by working backwards from what is to be explained. For example, if we see from the fossil record that horses teeth have become elongated, the evolutionist may be tempted to use evolutionary theory to imply that there was some sort of change in the environment that made shorter teeth less fit and less adaptive, and then explain the change by appealing to the law of natural selection that "only the fittest survive."

One implication of Lakatos' analysis is that the methodology of scientific research programs does not offer instant rationality. In Popper's analysis, we can see in a discrete episode of research whether the scientist's theory is falsifiable and whether the scientist is attempting to falsify it. However, for Lakatos, it is not irrational for a scientist to work on a young research program if the scientists think it has potential in the future to be progressive. And it is irrational for a scientist to stick with an old research program that has been degenerating provided the scientist can eventually make it progressive. Thus, Lakatos appears to agree with Kuhn that the rational appraisal of theory change is a rather fuzzy undertaking. But Lakatos does insist that it depends on an assessment of *objective* facts—whether revisions make new predictions and these predictions are corroborated. The decision of scientists relies on their subjective appraisals of the future course of science.

Let us consider an example from clinical psychology. How would the research and theory of behavior analysis fit Lakatos' analysis? Let us suppose the following:

Hard core: Operant conditioning and other laws of learning such as classical conditioning and habituation.
Auxiliary hypotheses: Effective reinforcers can be identified through paper and pencil measures of pleasurable events.
Heuristics. Positive heuristic: Preference assessment is a good way of identifying reinforcers. *Negative heuristic*: If we observe an instance in which a reinforcer is given contingently after a certain behavior, and the frequency of that certain behavior does not increase, we ought **not** to blame operant conditioning, but rather we should direct the arrows of *modus tollens* to our method of identifying reinforcers.

Let us say, the behavior analyst applies a reinforcer chosen from a positive event checklist and applies it to a behavior that she would like to increase, say studying, but studying does not increase. According to Lakatos (and this seems fairly accurate here), the behavior analyst would not take this as a falsification of operant conditioning, but rather direct the arrows of *modus tollens* to one of their auxiliary hypothesis. But what next? If the behavior analyst simply stops here, their research program can be said to be degenerating as no new theory is contained in their protective belt and no new predictions are made or corroborated.

But what if the revision in the research program becomes as follows:

Hard core: Operant conditioning and other laws of learning such as classical conditioning and habituation. (note this does not change).
Auxiliary hypothesis: The Premack principle (roughly that any high-frequency behavior will serve as a reinforcer for any lower-frequency behavior) will be used to identify reinforcers.
Heuristics. Positive heuristic: Preference assessment is a good way of identifying reinforcers but actually observing preferences by looking at frequency of behavior is better. *Negative heuristic*: If we observe an instance in which a reinforcer is given contingently after a certain behavior, and the frequency of that certain behavior does not increase, we ought not to blame operant conditioning, but rather we should direct the arrows of modus tollens to our method of identifying reinforcers.

Let us further suppose that the Premack principle makes some new predictions, namely that behavior such as going to school (not checked on the pleasurable activity list) will reinforce studying (a lower-frequency behavior). Now further suppose that some of these novel empirical predictions are tested and found to hold. Lakatos would then say this is a progressive research program. (And when anomalies are found using Premack, and the researcher replaces this with a Allison and Timberlake's (1975) response deprivation analysis, and novel predictions are made and seen, again, the research program can be seen to be progressive).

Criticisms of Lakatos

Some of the major criticisms are whether negative heuristics and positive heuristics are specified in advanced or only opportunistically manufactured when needed. Some of the criticisms are fairly predictable: Popper criticized it as not placing a sufficient emphasis on the importance of falsification: Feyerabend critiqued it because he does not want anything eliminated; even degenerating research programs. However, probably the most serious criticism is that Lakatos' analysis does not include the appraisal of rival theories. Why does it matter if one theory is making new predictions and thus is progressive if a rival theory is just much better at explaining and predicting the phenomena under study? This problem is dealt with in the next philosopher we will consider: Larry Laudan.

Laudan

> The rationale for accepting or rejecting any theory is thus fundamentally based on the idea of problem solving progress. If one research tradition has solved more important problems than its rivals, then accepting that tradition is rational precisely to the degree that we are aiming to "progress", i.e., to maximize the scope of solved problems. In other words, the choice of one tradition over its rivals is a progressive (and thus a rational) choice precisely to the extent that the chosen tradition is a better problem solver than its rivals.

Larry Laudan in his classic *Progress and its Problems* made a few central claims. First he uses the term *research traditions* to describe what Kuhn called paradigms and Lakatos called research programs. Laudan stated that research traditions have both metaphysical and methodology content. Research traditions are characterized by the following:

- "A set of beliefs about what sorts of entities and processes make up the domain of inquiry." (151)
- "A set of epistemic and methodological norms about how the domain is to be investigated, how theories are to be tested, how data are to be collected, etc." (151)

Research traditions may have different theories associated with them over time (e.g., behavior therapy in its early days had theories such as Wolpe's reciprocal inhibition; and more recently, theories such as Barlow's alarm theory—despite these different theories—it is still the behavior therapy research tradition). Second, Laudan said that science is essentially a *problem-solving activity* and episodes of science should be judged on their problem-solving effectiveness. Science makes progress because successive theories solve more problems. Third, he claimed that science has two kinds of problems: *empirical problems* and *conceptual problems*. There are three types of empirical problems as follows:

1. Potential Problems: "What we take to be the case about the world, but for which there is as yet no explanation." (146)

2. Solved Problems: “Class of germane claims about the world which have been solved by some viable theory or other.” (146)
3. Anomalous Problems: “Actual problems which rival theories solve but which are not solved by the theory in question. A problem is only anomalous for some theory if that problem has been solved by a viable rival.” (146)

Conceptual problems include the following:

1. Internal inconsistency problems.
2. A theory that “makes assumptions about the world that run counter to other theories or to prevailing metaphysical assumptions.”
3. When a theory “violates principles of the research tradition of which it is a part.” (146)
4. When a theory “fails to utilize concepts from other, more general theories to which it should be logically subordinate.” (146)

A theory has to be appraised by evaluating its effectiveness in solving both kinds of problems. Laudan’s problem-solving model “argues that the elimination of conceptual difficulties is as much constitutive of progress as increasing empirical support” (147). Laudan even stated: “… it is possible that a change from an empirically well-supported theory to a less well-supported one could be progressive, provided that the latter resolved significant conceptual difficulties confronting the former.” The better theory solves more conceptual problems while minimizing empirical anomalies. Implicit in this account his another novel element of Laudan’s approach; instead of examining earlier and later versions of a research tradition (as Lakatos would have us do); Laudan explicitly stated competing research traditions should be compared; and the rate at which empirical and conceptual problems are solved should be the criteria used to rationally chose among these competitors.

Some of the criticisms leveled against Laudan are that it is not clear how to enumerate rate at which two competing theories are solving and creating empirical and conceptual problems. Laudan said in appraising competing theories by “… assessing the number and the weight of the empirical problems (a theory) is known to solve; similarly, assess the number and centrality of its conceptual difficulties or problems…. Prefer that theory which comes closest to solving the largest number of important empirical problems while generating the smallest number of significant anomalies and conceptual problems.” (149)

Laudan also wrote that our cognitive stance toward theories should not be exhausted by either “belief” or “unbelief.” There are further options as follows:

- A theory may deserve further investigation.
- A theory warrants further elaboration.

For example, what is the rate at which psychoanalysis has solved problems? What are its conceptual problems? Is the rate at which behavior therapy solved conceptual and empirical problems faster than psychoanalysis? Is there a weighting of problems—does “solving” the problem of the treatment of enuresis of less value than solving the problem of say depression? Moreover, Laudan stated that it can be rational for a scientist to abandon a research tradition that has had the highest rate of problem-solving effectiveness when a large conceptual or problem has recently emerged? Does behavior therapy has some of these large conceptual problems—such as what exactly is reinforcement? A final problem is that similar to Kuhn is that his concept of "research tradition" is somewhat fuzzy.

Gross: The Rhetoric of Science

They say 65 *% of all statistics are made up right there on the spot.* Todd Snider Statistician's Blues.

Alan Gross in his *Rhetoric of Science* claimed that the practice of science is fundamentally a rhetorical activity—an activity meant to persuade. The rhetorical analysis of science becomes "the application of the machinery of rhetoric to the texts of sciences" (p. ix). Traditionally, one of the purposes of language—texts or talks—is persuasion (Quine and Ullian 1978). However, in this strong view, rhetoric does not just influence believes, rhetoric becomes constitutive of knowledge. Bazerman (1988, p. 321) stated:

> Persuasion is at the heart of science, not at the unrespectable fringe. An intelligent rhetoric practiced within a serious knowledgeable committed research community is a serious method of truth seeking. The most serious scientific communication is not that which disowns persuasion, but which persuades in the deepest, most compelling manner, thereby sweeping aside more superficial arguments. Science has developed tools and tricks that make nature the strongest ally of persuasive argument, even while casting aside some of the more familiar and ancient tools and tricks of rhetoric as being only superficially and temporarily persuasive.

For over two millennia, originating with the Sophists in ancient Greece, "the art of persuasion" has been formally studied under the rubric "rhetoric" (Luks 1999). In science as is the case with other fields that rely heavily on persuasion (e.g., law, education, philosophy, politics, literature—perhaps even psychotherapy), the central goal of the practitioners is to persuade their audience. "Rhetorically, the creation of knowledge is a task beginning with self-persuasion and ending with the persuasion of others" (Gross 1990, p. 3).

Gross pointed out that scientists have several fundamental and concrete rhetorical problems. First, the scientist must persuade himself of some point ("The meter reading is correct"; "The experiment is well-designed." "I am working in the most progressive paradigm"; "My data actually do (or do not) refute my theory" (remembering Quine-Duhem and so on). Second, the scientist must persuade *others*—journal editors, grant review committees, IRBs (of the ethics of the research), journal readers, policy makers—in short, others in the community of science. That is, the scientists has a *persuasive burden* and must meet this persuasive burden using *tropes*—rhetorical moves. And good rhetorical moves can be claims to "objective" reading of "evidence" "validly" collected, or even moves that clearly point out limitations to the research design used—to influence by the display of the virtue of epistemic humbleness.

Many philosophers of science have argued for key "underdetermination theses" (e.g., Kuhn 1962; Popper 1972; Quine and Ullian 1978). An underdetermination thesis states that the move from some claim to another claim (for example, from claims regarding data to conclusions) is not entirely a matter of logic and therefore not necessarily truth preserving. Let us examine a few of these. Quine and Ullian 1978 and Popper (1972) have argued for *semantic underdetermination*. That is, the move from some raw perception to some words that are used to "capture" or refer or describe the raw perception (e.g., from percepts to the sentence "The cat is on the mat") is underdetermined. Another way of saying this is that the raw perception does not logically entail the semantic "reference." Instead, from a purely logical point of view, there is "a jump"—a disconnect. As another example, all laws and theories are underdetermined by empirical evidence—*theoretical or nomological underdetermination*. Every piece of copper has not been observed to conduct electricity and thus the claim that "All copper conducts electricity" is not entailed by actual empirical evidence. Again, another jump–whose legitimacy, again becomes a matter of persuasion.

Thus, these under determination theses imply that these linguistic moves are not matters of logical necessity but rather are matters of persuasion. The scientist must first "persuade" herself that what she sees is a correctly functioning thermometer that is actually displaying the value of 98.6° F. Further, the scientist must persuade herself and others that given the alternatives the evidence best supports the statement that "All copper conducts electricity." These are matters of judgment and persuasion, not necessity.

Therefore, persuasion is necessary in science because in science, there are no certain truths—no apodictic givens, no indubitable foundations, no ampliative demonstrative deductions, and no truth preserving inductive inferences. One is rarely *logically compelled* to agree with any scientific claim or any scientific choice, thus persuasion becomes the issue. Therefore, in this view, *knowledge*—perhaps because it has the problems that have already been discussed in this and other chapters–is underdetermined by data, perhaps because it is theoretically and assumptively laden, perhaps because it is perennially open to revision is contrasted to *persuasion*. The problems with knowledge, and its ultimate limitations, leave the field to be dominated by persuasion–rhetoric.

Rhetoric is also used in gaining an understanding of key "external" matters. External matters, for the philosopher of science, occur not in the logic of research, but in the sociology of science—how scientists interact with one another and the community of science. For example, through rhetoric, scientists prescribe what empirical and conceptual problems are worthy of funding, worthy of publication and even "news worthy." "Importance" and "significance" are key issues in science and are, again, matters of argument, judgment, and persuasion. The view that some research methods or procedures are "legitimate" ways to discover knowledge—while others are not—is a key to a science and this issue too is not a logical affair but an issue of rhetoric and persuasion. In psychology, debates about single-subject designs, proper control conditions, ways to interpret conflicting results across experiments, occur and listeners are variously persuaded about which are legitimate methodologies to be used to best produce knowledge. Important consensuses emerge which allow the field to move beyond certain debates to other more circumscribed issues (note the similarity with the move Kuhn described when a discipline moves from pre-paradigmatic status to paradigmatic status. Kuhn would also agree that there is not a logic of these sort of decisions but rather a sociology—members of the scientific community have, for some reason or another, become persuaded).

The use of rhetoric can also be seen most clearly at the cutting edges of science—where the important controversies lie. These controversies may be concerning the interpretation of experiments, the importance of experiments, the ways multiple experiments can be summarized, the ethics of certain research, the writing of scientific history, the exegesis of some scientific texts (poor exegesis—creating straw men—may be an intentional rhetorical strategy), the implications of science for social policy, etc.—but here scientists are bringing to bear their best rhetorical machinery to persuade listeners—whether these are new students searching for their allegiances, to administrators of professional organizations or grant reviewers. Ridicule is a rhetorical trope and notice how it is used in these scientific controversies—the scientists on the other side of the issue are so beyond the pale that all one can do is to make fun of these poor lost souls. Do the clothes worn by a speaker at a scientific convention have a rhetorical function—do Birkenstocks and a Jerry Garcia tie persuade the audience of speaker's ability to think independently and confront the conventional? On the other hand, does the obesity of the weight loss researcher undermine the research they are presenting? Does the speaker's attractiveness persuade? On these, cutting-edge controversies are again underdetermined and thus, judgments must be made.

Moreover, there is no "logic" of a particular experiment but rather each individual experiment also is an attempt at persuasion. Take, for example, the issue of the scientific study of whether therapy X is effective. The investigator when designing their research program needs to be mindful of reasons why he or others might be legitimately *unpersuaded* that this therapy is effective. The good experimental design allows for these concerns to be handled in a convincing fashion—that is the good experiment knows its *persuasive burden*. Random sampling is a move designed to persuade those concerned with the claim that "The sample was biased and so therefore the results are unpersuasive due to their unrepresentativeness." Random assignment is a move to persuade those legitimately concerned with the claim "The groups might have been different from the start." The no-treatment control condition is a scientific move designed to persuade those concerned with the claim "The problem would have spontaneously remitted." (Note that all control conditions are designed to rule out "plausible" rival hypotheses. But "plausibility" again is not a matter of logic, but again a matter of judgment and persuasion (O'Donohue 1989). The "importance" of the results is also a matter of persuasion—is the magnitude of the effect clinically significant? Was the procedure "cost-effective?" Were possible iatrogenic effects (complications caused by diagnosis and/or treatment) appropriately measured and found to be insignificant? Did patients find the treatment to be acceptable? These, again, are all matters of persuasion. Finally, if the author *persuades* the peer reviewers that these and other matters have been handled adequately, the paper is published. And perhaps published in a "high profile" journal whose contents are more often read and may, due to this status, be more persuasive.

The word "rhetoric" often has a negative connotation—it is often taken to mean attempting to persuade through trickery or other empty or invalid means. Gross (1990) clearly does not use this phrase in this way. Clearly, in science, the use of what are seen as "valid" and "rational" research methods are usually warranted as these are usually the most persuasive. However, that is not to say that style and other presentation aspects are irrelevant. Feyerabend's (1975) analysis of Galileo's arguments for the Copernican system used "propaganda, emotion, *ad hoc* hypotheses and appeal to prejudices of all kinds" (p. 153). Feyerabend stated that it is usually the case that early in a theory's development, at a time when the theory is drastically underdetermined by evidence, matters of "style, elegance of expression, simplicity of presentation, tension of plot and narrative, and seductiveness of content become important features of our knowledge" (1975, p. 157).

Each of these persuasive tasks is not isolated and independent. Scientists work in a community, and important consensuses emerge due to arguments. This is the scientist's aim. Gross (1990) stated:

> To rhetoricians, science is a coherent network of utterances that has also achieved consensus among practitioners… But to say that scientific knowledge represents a consensus concerning the coherence and empirical adequacy of scientific utterances, that the various methods of science are essentially consensus-producing, is not to denigrate science; it is rather to pay tribute to the supreme human achievement that consensus on complex issues represents… The truths of science, then, are achievements of argument (p. 203).

A Brief Introduction to Rhetoric

Aristotle called rhetoric "the faculty of observing in any given case the available means of persuasion." Aristotle further divided the three major rhetorical appeals into *logos*, *pathos*, and *ethos*. *Ethos* relates how the character and credibility of a speaker can influence audiences. Is this part of the function of being called "Doctor?" Do the institutional bylines of publications or of a speaker involve this rhetorical dimension—does "Harvard" have more persuasive impact than "Western Nevada Community College?" Isn't someone counting as an "expert" fundamentally a rhetorically move? Does a psychologist telling how they used to suffer from problem X but used their therapy method to cure themselves a use of ethos to persuade? *Pathos*, on the other hand, makes an emotional appeal to alter the listener's judgments. This can be done through the use of a narrative (e.g., a clinical case study that shows a suffering client's pain being eased), through direct emotional appeals—for example, compelling descriptions of the tragedy of child suicide influencing grant decisions or hiring decisions. *Logos*, finally, appeals to the use of reasoning and arguments, statistics, math, logic. Philosophers of science, according to Gross, have concentrated too much on this final dimension. Gross would agree that all these rhetorical strategies ought not to be used as a means of deceit but instead as a means to reveal truth.

Thus the object of rhetorical analysis is some sort of discourse (a speech, a poem, a joke, a newspaper article), the aim of rhetorical analysis is not simply to describe the claims and arguments advanced within the discourse, but, most importantly, to identify the specific persuasive strategies employed by the speaker to accomplish specific persuasive goals. Therefore, after a rhetorical analyst discovers a use of language that is particularly important to achieving persuasion, she typically moves onto the question of "How does it work?" That is, what effects does this particular use of rhetoric have on an audience, and how does that effect provide more clues as to the speaker's (or writer's) objectives?

According to Wikipedia, the major forms of rhetorical analysis are as follows:

- *Ideological criticism*: critics engage rhetoric as it suggests the beliefs, values, assumptions, and interpretations held by the rhetor or the larger culture. Ideological criticism also treats ideology as an artifact of discourse, one that is embedded in key terms (called "ideographs") as well as material resources and discursive embodiment.
- *Feminist criticism*: rooted in the feminist movement, which seeks to improve conditions for women and change existing power relations between men and women. It critiques rhetorical forms and processes that allow oppression to be maintained and seeks to transform them.
- *Cluster criticism*: a method developed by Kenneth Burke that seeks to help the critic understand the rhetor's worldview. This means identifying terms that are "clustered" around key symbols in the rhetorical artifact and the patterns in which they appear.
- *Generic criticism*: a method that assumes certain situations call for similar needs and expectations within the audience, therefore calling for certain types of rhetoric. It studies rhetoric in different times and locations, looking at similarities in the rhetorical situation and the rhetoric that responds to them. Examples include eulogies, inaugural addresses, and declarations of war.
- *Narrative criticism*: narratives help to organize experiences in order to endow meaning to historical events and transformations. Narrative criticism focuses on the story itself and how the construction of the narrative directs the interpretation of the situation.

Rhetorical analysis of science even looks at unique uses of particular styles of language in science: Is there a technical use of language that persuades more than "ordinary language?" Does a phrase like, "Her Full Scale Score on the WAIS-III exceeded one standard deviation above the mean" persuade more than the ordinary nontechnical claim "She is smart?" Moreover:

> In the twentieth century, we find the scientific article [a rhetorical genre] growing considerably more uniform across national boundaries and scientific disciplines. Most striking of all, 'scientific English' has become the international discourse of science, which involves

> not only a specific language but also a suite of stylistic features: relatively short, syntactically simple sentences containing complex noun phrases with multiple modification, verbs in the passive voice, noun strings, technical abbreviations, quantitative expressions and equations, and citational traces… Whatever the language or disciple, the style is streamlined to refocus the reader's attention on the things of the laboratory and the natural world beyond the printed page, rather than to draw attention to the text itself or its author (p. 230).

Conclusions: Regarding Rhetoric and Science

Note that the object of our concern—the philosophy of science itself can also be seen through the rhetorical lens: what rhetorical moves have been made (e.g., selective appeals to the history of science) and how successful have these been on the part of the logical positivists, Popper, Kuhn, and even Gross? And psychotherapy can also be seen to have a number of rhetorical tasks—persuading oneself as the therapist, the client (and even the managed care utilization and review worker) of the correctness of the diagnosis, the reasonableness of the case formulation, the sufficient fidelity of treatment implementation, the accuracy of any measurement, the point at which therapy should be terminated, the need for any relapse prevention, etc.).

The importance of rhetoric in these kinds of practical concerns can be seen in the following joke. Two men like to smoke and want to convince their ministers that it ought to be OK for them to smoke while they pray. The first (who is rhetorically naïve) asks his minister if it is ok that if while he is praying that he smoke. The minister somewhat predictably says, "Of course not, prayer is sacred and if you are going to pray you need to focus and just pray." The second man, who is more rhetorically sophisticated asks his minister, "Reverend, prayer is important to me, and you know that unfortunately I smoke, but because prayer is so important to me, is it OK that while I am smoking that I pray?" The reverend is impressed and says "Yes of course" as, put this way, he sees it as more prayer. Sometimes, so much rides on rhetorically apt phrasing.

Special Topic: A Fifth Account of Science: B. F. Skinner's Indigenous, Behavioral Account of Science

> We do not know enough about human behavior to know how the scientist does what he does. (Skinner 1955, p. 221)

We saw in the first case study that the historian of psychology Smith (1986) has argued persuasively that Skinner's behaviorism is not historically or logically tied to logical positivism, as the received view of history often asserts. In this book, Smith also argued that Skinner developed an indigenous—that is, psychological account of science. It is worthwhile to take a look at what such as behavioral account of science might look like as scientific behavior is human behavior, and thus, there is at least a *prima facie* case that a psychological account of scientific behavior might be a valuable perspective to examine meta-scientific questions. It is also worthwhile for us to examine Skinner's behavioral account of scientific behavior for two reasons: (1) He has explicitly stated what he thought the successful psychological scientist does, while many other prominent psychologist have not done this. Thus, Skinner rose to this meta-perspective and thus provides grist for our mill, while, many other successful researchers have not provided us with such fodder. Rather most of these have followed what they took to be the received view of science in their discipline. (2) Skinner was extraordinarily successful; his work on operant conditioning, schedules of reinforcement, shaping, generalization, etc. are extraordinarily robust, general and have had enormous practical implications.

Skinner (1955) laid out his views in an article that appeared in the *American Psychologist* called *A Case History in Scientific Method*. In this Skinner stated: "Scientific thinking is the most complex and probably the most subtle of all human activities. Do we actually know how to shape up such behavior?" (p. 221). Immediately, note that Skinner is seeing scientific behavior as learned behavior and this learning is operant—behavior is selected by its consequences. Thus, for Skinner, the same methods of analysis are used to study scientific behavior as studying feeding behavior, or the behavior of someone going to the movies. However, he also suggested:

> But it is a mistake to identify scientific practice with the formalized constructions of statistics and scientific method. These disciplines have their place, but it does not coincide with the place of scientific research. They offer *a* method of science but not, as is so often implied, *the* method. As formal disciplines they arose very late in the history of science, and most of the facts of science have been discovered without their aid. It takes a great deal of skill to fit Faraday with his wires and magnets into the picture which statistics gives us of scientific thinking. And most current scientific practice would be equally refractory, especially in the important initial stages. It is no wonder that the laboratory scientist is puzzled and often dismayed when he discovers how his behavior has been reconstructed in the formal analyses of scientific method. He is likely to protest that this is not at all a fair representation of what he does. (p. 221).

Skinner, here, was in agreement with many of the modern critiques of philosophical accounts of science, for example, those of Feyerabend—these formal philosophical analysis are not in agreement with the facts of science as uncovered by historians of science.

Skinner also noted that there was much "amateurish" discussion of meta-scientific issues that he soon found he should avoid. He also judged that was too much of what he came to call "premature physiologizing"—that the Zeitgeist of psychology of his time thought it was imperative in any discussion of perception and learning must be cased out in terms of the physiology of the nervous system. He reported that at Harvard, he found a mentor W. J. Crozier who was a student of Jacques Loeb who "resented the nervous system" and talked of behavior–behavior or environment-behavior relations without going inside the skin to explain behavior. His mission became to find order in "the organism as a whole" and found important clues from Pavlov's study of classical conditioning—namely, "control your conditions and you will see order" (Skinner 1955, p. 223). He began to tinkerer (he was a first rate tinkerer) to develop new experimental apparati that could be used to study environment-behavior relationships in the hopes of uncovering such order. He reported that his early efforts were all failures (note here that even the scientist may make proposals to the environment to see what the environment selects—evolutionary epistemology!).

After a series of failures he moved away from mazes, and averages, and began to study the behavior of a single organism in a controlled environment in which the organism can operate on the environment (by pressing a lever) and which this behavior is measured on a cumulative recorder—which importantly measures *rate* of behavior (instead of just time, or provides some sort of average). A key for Skinner was finding a different dependent variable—one that would reveal order.

He then provided his principles of successful scientific behavior:

- When you run onto something interesting, drop everything else and study it.
- Some ways of doing research are easier than others.
- Some people are lucky.
- Apparatuses sometimes break down.
- *Serendipity* happens—the art of finding one thing while looking for something else.

There are a few features of this account that ought to be emphasized. First, we can see that Skinner's framework is psychological: it attempts to understand scientific behavior in the same way a behavioral psychologist would attempt to study any behavior—as a function of environmental variables. Admittedly, in this approach valuational, normative constructs also need to be used—for example, "rational," "successful," "effective," but this sort of problem faces psychologists in many of their problem situations—for example, the study of marital communication, or of problem solving, or of rational versus irrational approaches to germs or the risk appraisal of a psychotic individual. Second, there is no formal analysis; this is not a look at the logical relationship between sentences, for example, observation sentences and theoretical claims. That is, this is decidedly not a philosophical account. It is quite distinct, although it could share some commonalities, with the more empirically oriented account of Kuhn—but gain, it is clearly not sociological Skinner makes little mention of other scientists and norms or consensus but rather focuses on the individual scientist. Third, it might be said it is a founding of a new paradigm—Skinner thought quite originally and independently rejecting many of the received views of what is proper science and research of his day. Fourth, we can see the beginnings of evolutionary epistemology—the environment is selecting from the scientists trial and error attempts. Finally, we can also see a rough pragmatism—Skinner is giving rules of thumb of the craftsman. He is not providing algorithms nor is he providing laws of scientific behavior—but rather practical tips that he believes will increase the likelihood of the scientist's success. Thus, again, we see a quite different meta-science here—a case study of one successful scientist's behavior that leads to some possible useful information about how to do more successful research. An analysis that Skinner would suggest is much more practically important than some of the concerns of philosophers of science.

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Chapter 7
Post-Modernism, Social Constructionism, and the Science Wars

Any overview of the philosophy of science would not be complete without covering these recent views as they have received a lot of attention in the past few decades. Many of these views are quite controversial on a number of dimensions—sometimes the criticism is that these views are meaningless or at a minimum obscurantist (see the so-called Sokal affair that will be discussed below); sometimes the claim is that they are unfairly and unduly incendiary—for example, a prominent feminist critic of science Harding (1986) has called Newton's classic *Principia* "a rape manual" because "science is the male rape of female nature"; and of course sometimes the criticism is simply that they are wrong.

However, if the reader is to be conversant in twenty-first century meta-science, he or she must have a basic understanding of the major dimensions of these views and the controversies that they have spawned. Unfortunately, any expositor of these views has a difficult task for two major reasons: (1) many of these views are not clearly or simply presented, perhaps again due to their inherent complexity or perhaps due to the rather obscure linguistic style (in their own terms, is this a rhetorical move or just an unfortunate but perhaps necessary complexity due, for example, to the reflexive nature of their language use—language is being used to criticize language?) of many writers belonging to this movement; (2) there is some significant variance among these views—not all postmoderns agree with one another—if only because there are different points of emphasis, and perhaps it is most fair to say that these views form somewhat of a family—a family tree of postmodernists, social constructionists, post-structuralists, hermeneutists, radical feminists, and contemporary sociologists of science and technology. However, my goal is to provide the broad outlines and a description of some of the key views of this general movement. There are other more extended treatments that are quite excellent; see for example, Cahoone (2003); Brown (2001); Gross and Levitt (1997) and Koertge (1998).

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Roots of Post-Modernism and the Philosophy of Science

First, we must recognize that these views are to some extent anticipated by some of the philosophers of science that we have previously discussed. We can see some of the roots of these meta-scientific claims in several of the conclusions of several of the previous philosophers of science. For example, the logical positivists examined language and thought that they "exposed" that the claims of religion were meaningless. The focus of these postmodern philosophers also is often on language, and these philosophers also believe through their *deconstructions* or *interpretations* that they also are exposing previously unnoticed but very problematic aspects of language, including paradoxes, false assumptions, and other serious problems. Kuhn emphasized the sociology of science—for example, how social factors contrive to create consensus in the scientific community during periods of normal science, and thus, these social factors have a large role in explaining science. Kuhn suggested that a scientist's acceptance of a theory was not due just to *internal factors* such as its correspondence with evidence but also to some extent to *external factors* such as beliefs and norms of the scientist's group. These postmodernist philosophers also emphasize the social relationships involved in science—whether it is social decisions in the laboratory about what constitutes "data" to how certain dichotomies such as gender are created and disadvantage certain groups. Popper, and other philosophers such as Lakatos, allowed metaphysical beliefs—beliefs about politics, religion, and ethics, among other topics—to enter into science as long as these produced falsifiable theories. These postmodern meta-scientists would agree that there are these metaphysical influences on what is taken to be science although they elevate their importance considerably. Quine argued that logic did not direct the arrows of *modus tollens* to any particular statement in the web of belief, but rather pragmatic judgment was necessary to square the web of belief with anomalies. These postmodern philosophers would agree with this *underdetermination thesis* as well as Quine's *holism* thesis—that the scientist's entire belief system is involved in his or her scientific pursuits. Popper, Kuhn, and Feyerabend all argued for the *theory-ladened nature of observation*. Again, the philosophers examined in this chapter would agree. Gross suggested that rhetoric has a large role in defining knowledge. Moreover, these postmodern philosophers would agree that science is a rhetorical process although they often see political and sometimes quite pernicious and sinister motives to these rhetorical activities.

However, the most influential philosopher of science setting the stage for this movement was clearly Paul Feyerabend. His concerns that scientific ways of knowing not be *privileged*; his interest in epistemic and theoretical *pluralism*; his concern about *democratization* of science; and his concern that science be more humane and not an instrument of *oppression* share the most overlap with the meta-science studies that I will cover in this chapter. Feyerabend, in short, was one of the first philosophers to be concerned about the relationship between power and science, developed a quite political view of science, as do these postmodern meta-scientists.

Moreover, it is also important to note that these postmodern authors all believe, as does Feyerabend, that a fairer assessment of science also would need to include an examination of the technology spawned from it and they generally have a rather negative view of this technology. Many of the previous philosophers focused on what might be called basic science and not on the technology spawned from science. These postmodern meta-scientists examine both. They would see conventional philosophy of science as celebrating technological advances such as inoculations, moon landings, and dishwashers. However, they call for a more complete evaluation of science and technology that includes the atomic destruction of Nagasaki and Hiroshima, Auschwitz's genocidal poisonous showers, Nazi eugenics based on racist science, the politically biased agricultural practices that starved millions in the Stalinist Soviet Union, global warming due to destruction of forests and reliance upon carbon-based fuels as well as other harms to the environment by a consumerist culture, the destruction of First Nations and indigenous cultures by Western hegemony, the alienating labor of an assembly line, the lonely and perhaps dehumanizing deaths in high-tech intensive care units of modern medicine, lobotomies, overmedicated children, and even junk food. Their view is that the received view of traditional Western *scientism*—the worshippers in the church of science—has not given these kinds of scientific applications proper attention in their unfair and overly positive appraisals of science and technology, and they see their views as a necessary corrective.

What is Post-Modernism?

It is difficult to precisely define postmodernism. Let me instead try to list some of its key ideas:

- It is a rejection of modern (post-Enlightenment) views of objective scientific knowledge that claims to describe the essential features of the world, as well as the view that post-Enlightenment science is progress and has produced progress.
- The view that "There is a strict interlinkage between the kind of language called science and the kind called ethics and politics" (Lyotard 1984, p. 8).
- Reality is not mirrored in human "knowledge" but rather is *constructed* by the human mind, and this construction is influenced by ideologies found in culture, tradition, and race. Scientific constructs are *social constructions* in much the same way that balls and strikes in the game of baseball are *social constructions.*
- Knowledge claims are seen as attempts to control others. Knowledge claims or rules about what counts as knowledge, reason, or evidence "privileges" some people and not others.
- *Deconstruction* of texts is key as this can expose ideology, prejudices, assumptions, contradictions, paradoxes, political motivations, and problematic frames of reference.
- Words and sentences do not have a single meaning. Meaning is a very complex problem generally showing multivocality.

- It involves a fairly radical cultural critique—especially a critique of the culture of science and particularly of its relationship to power in the wider culture. There is a deep concern of how texts may implicitly or explicitly be related to cultural hegemony, violence, and exclusion. Michel Foucault, for example, stated "language is oppression."
- Key scientific and everyday concepts—especially dichotomies or "*binaries*" such as male versus female and straight versus gay—are problematic dichotomies and are social constructions influenced by historical contingency, power, and hierarchy that are not "found" or "discovered" in nature and have problematic political uses.
- The assumptions of universality, consensus, generality, essence, and reality are rejected—especially as these relate to "authority"—to be replaced by more local, personal, diverse, and even intuitive and aesthetic *ways* of knowing. They advocate an *epistemic pluralism*—there are multiple ways of knowing, and science should not have a monopoly and should not oppress these other ways of knowing. They would agree with Pascal, "The heart has its reasons which reason know nothing of….We know the truth not only by the reason, but by the heart."
- Interpretation is seen as the key activity; for postmodernists, we never know reality, but rather we have *readings* (plural) of experience that are true for us personally—but not necessarily true for others. *Hermeneutics* is the critical process of interpretation of these readings.
- An important philosopher in this tradition, Jacques Lyotard, stated that the central notion is "Incredulity toward all *meta-narratives*." Lyotard attempts to dissolve *master narratives* like "progress" and "history" by exposing the contingent and constructed *ideologies* involved in these. "Progress" becomes a (among many) failed master narrative. "Reason" is also a failed master narrative. The power of modern medicine would be another failed "big story."
- There is no discovery but rather simply human invention—so-called "discoveries" all have a particular history that will reveal their contingency—they need not have been. The concepts used to engage the world are artificial—literally these discoveries ought to be seen as *human artifacts* not as human discoveries that mirror the world. A scientific discovery is as much a human artifact as a skyscraper.
- It seeks to expose the "*late-capitalistic" ideology* in the consumerist culture. The West's claims of prosperity, freedom are empty promises and a failed meta-narrative that if examined properly (deconstructed), one would find militarism, cultural hegemony, oppression, and a vapid consumerist culture.
- The meta-narratives of the West are oppressive because these denigrate *local tribal ways of knowing and wisdom* that is communicated through myths and legends. Science seeks an oppressive epistemic monopoly by excluding other narratives.
- Truth is a less important norm than aesthetic judgments, for example those of social justice. Scientists have been too concerned with truth and insufficiently attentive toward justice for all humans.

It is fair to say that these folks tend not to vote Republican very often. These views represented are radical—in the sense of "root" in that they are a rejection of some of the foundational beliefs of modern Western civilization. However, it is also important to note that although it can be said that postmodernists are on the Far Left politically, some others such as Noam Chomsky on the Far Left have rejected their views as "obscurantist," "meaningless," or even lacking argument and evidence.

Now it is important to note that there are also deep disagreements among the postmodernists on these issues—from points of emphasis to use of different technical languages, to drawing out different political implications (e.g., radical feminists predictably focus more on gender and sexism than some of the male postmodernists). Thus, there is significant fragmentation (or perhaps what postmodernist would call "locality") in postmodernism.

Another argument against these views is a reflexive one: Is not postmodernism defeated by its own claims as it, like the views it rejects, is making a claim to something objective, general, trans-linguistic, and essential? Postmodernist, of course, has noted this reflexive criticism. Lyotard has responded by saying he is making no claims that his views are true, but rather that they have "strategic value in relation to the questions raised" (Lyotard 1984, p. 7). Interesting move to say the least, but remember "being true" for them is of less importance than for those with more traditional views of regulative epistemic norms. They might see, for example, what they would take as positive political impact as more important "justification" for their views.

What are the Implications of These Views for Science?

Some of this is fairly obvious as the conventional views of science have been a major target for these theorists. However, Koertge (1998) provided a nice summary of the relevance of postmodernist and social constructionist views to science:

- Every aspect of that complex set of enterprises that we call science, including, above all, its content and results, is shaped by and can be understood only in its local historical and cultural context.
- In particular, the products of scientific inquiry, the so-called laws of nature, must always be viewed as social constructions. Their validity depends on the consensus of "experts" in just the same way as the legitimacy of a pope depends on a council of cardinals.
- Although scientists typically succeed in arrogating special epistemic authority to themselves, scientific knowledge is just "one story among many." The more epistemological authority that science has in a given society, the more important it is to unmask its pretensions to be an enterprise dedicated to the pursuit of objective knowledge. Science must be "humbled."
- Since the quest for objective knowledge is a quixotic one, the best way to appraise scientific claims is through a process of political evaluation. Since the "evidence" for a scientific claim is never conclusive, it is always open to

negotiation. The best way to evaluate scientific results is to ask who stands to benefit if the claim is taken to be true. Thus, for the citizen, the key question about a scientific result should not be how well tested the claim is but, rather, *Cui bono?*

- "Science is politics by other means": The results of scientific inquiry are profoundly and importantly shaped by the ideological agendas of powerful elites.
- There is no univocal sense in which the science of one society is better than that of another. In particular, euroscience is not objectively superior to the various ethno sciences and shamanisms described by anthropologists or invented by Afrocentrists.
- Neither is there any clear sense in which we can talk about scientific progress within the European tradition. On the contrary, science is characterized chiefly by its complicity in all the most negative and oppressive aspects of modern history, increasingly destructive warfare, environmental disasters, racism, sexism, eugenics, exploitation, alienation, and imperialism.
- Given the impossibility of scientific objectivity, it is futile to exhort scientists and policymakers to try harder to remove ideological bias from the practice of science. Instead, what we need to do is deliberately introduce "corrective biases" and "progressive political values" into science. There is a call for "emancipatory science" and "advocacy research" (pp. 3–4).

What is Social Constructionism?

We turn now to examine social constructionists because these theorists have garnered a lot of attention in the science studies and share some commonalities with the postmoderns (however, remember the family metaphor used above).

First, let us define a social construct. A *social construct* is an artifact (a human-made concept or practice) of some particular social group. There is a focus on providing explanations of the scientist by examining the *contingent choices* of the individual or group rather than on influences from an independent external reality. Social constructionist aims to discover the way social phenomena are invented, become known, are disseminated, are institutionalized, and finally come to constitute tradition. For example, the construct of gender is seen by social constructionists as having a history, is contingent (not inevitable), and constructed from a series of human choices, invented by humans not "found" in nature, and, when found to be harmful to some individuals or groups, should be reinvented.

In a book that has received much attention, Latour and Woolgar (1979), *Laboratory Life: the Social Construction of Scientific Facts,* conducted what they thought was an anthropological study of a culture: laboratory scientists at work. They thought science should be used to study science—and a reasonable method was that of a participant observer. They studied a neuroendocrinology research laboratory at the Salk Institute. They claimed that in examining actual laboratory practice, Popperian falsificationism, logical positivism, and other objectivist accounts of science simply fail to describe what actually occurs in the laboratory. Their observations suggested that a large issue of laboratory life consisted of making subjective and contingent decisions of what data to keep and what data to throw out. They suggested that the behavior of scientists in the laboratory shows that data are not "given" but are the product of a series of human decisions. The constructs used in the laboratory are creations of scientists as they talk about instruments readings, for example. All scientific facts are really *artifacts*—much like a painting or a beautiful building—these are human creations. Thus, what scientists do is to make decisions in which order is created out of a multitude of possible orders. Scientific facts for them are not uncovered as much as produced or constructed from the decisions and behavior of a variety of different actors, including the scientists, the measurement instruments, the laboratory, the technicians and the subjects being studied and the wider scientific community (this is called Latour's actor–network theory—which he later rejected). For example, in clinical psychology, the human construction of tests such as the Beck Depression Inventory which involves a series of decisions about which items to include (which in turn are based on human decisions about DSM diagnostic criteria), how these should be weighted, what are proper test-taking conditions, etc., all are involved in construction "data" about depression. In order to understand science, one must understand all these actors and the alliances they form.

These views give rise to certain puzzles. What is the status of say microbes, before scientists started to talk about them? For Latour, it appears that they simply did not exist—as humans did not invent them yet. Just as a telephone did not exist prior to Alexander Graham Bell, a microbe did not exist prior to scientists inventing the construct. When French scientists declared in 1976 that tests on the mummy of Pharaoh Ramses II pointed to his death by tuberculosis, Latour claimed this to be impossible because the bacillus virus was not known at the time of the ancient Egyptians!

However, understanding Latour's views gets more complicated because in more recent years, he has rejected his earlier views—although a number of folks in this camp seem to still follow them. He has stated:

> what if explanations resorting to power, society discourse had outlived their usefulness and deteriorated to the point of now feeding the most gullible sort of critique?…threats might have changed so much that we might still be directing all our arsenal east or west while the enemy has moved to a very different place.

and further:

> Sentences such as "the danger would no longer be coming from an excessive confidence in ideological arguments posing as matters of fact …but from an excessive **distrust** of good matters of fact disguised as ideological biases" resonated with many scientists, as did "dangerous extremists are using the very same argument of social construction to destroy hard-won evidence that could save lives."

Many of the critiques of this kind of view came to a head in the late 1990s when a physicist published an article in a key postmodern journal. Let us turn to this now.

What Are the "Science Wars"?

The Sokal Affair

In 1996, the physicist Alan Sokal submitted an article "Transgressing the Boundaries: Towards a Transformative Hermeneutics of Quantum Gravity" to the *Social Text*, an academic journal of postmodern cultural studies, and had the article accepted and published. However, on the date of the publication of the article, Sokal revealed in another journal *Lingua Franca* that the article was actually a hoax and stating that the article was in fact a "pastiche of Left-wing cant, fawning references, grandiose quotations and outright nonsense…structured around the silliest quotations (by postmodern academics) he could find about mathematics and physics." It might be instructive to read a bit of the article. Here are the first few sentences of the concluding paragraph:

> Finally the content of any science is profoundly constrained by the language within which its discourses are formulated; and mainstream western physical science, has since Galileo, been formulated in the language of mathematics. But whose mathematics? The question is a fundamental one, for as Aronwitz has observed, "neither logic nor mathematics escapes the contamination of the social." And as feminist thinkers have repeatedly pointed out, in the present culture this contamination is overwhelmingly capitalist, patriarchal and militaristic: "mathematics is portrayed as a woman whose nature deserves to be the conquered other." Thus, a libratory science cannot be complete with a profound revision of the canon of mathematics. As yet no such emancipatory mathematics exists, and we can only speculate upon its eventual content. We can see hints of it in the multidimensional and nonlinear logic of fuzzy systems theory, but this approach is still heavily marked by its origins in the cirrus of late-capitalist production relations.

What can be learned from this hoax? Sokal suggested the following: (1) his hoax does not show that all of postmodernism is nonsense or that in all of this kind of science studies, standards are lax. That would place too much weight on the publication of one hoax article in one journal. (2) That science is indeed a human endeavor, and it should be subject to a rigorous sociological analysis as historians, sociologists, political scientists, economists, and psychologists have something to say about the ideological, political, and social influences on key scientific questions such as what gets funded, who benefits, and who gets prestige and power. (3) Even so-called internal questions—what types of evidence count, and what types of theories get proposed are also partly influenced by these "external" variables; and (4) there is nothing wrong with research being influenced by political commitment, as long as these political views do not blind the researcher. These critiques can use conventional scholarly and scientific methods and standards to critique problematic science. They ought not to use problematic epistemologies, erroneous readings of science, and faulty logic in conducting these studies, and Sokal suggested that postmodernism is rife with these kinds of problems.

Feminist Critiques of a Gendered Science and "Newton's Rape Manual"

Feminists have had a variety of critiques of science. Their concerns run along the following lines: (1) women have been excluded from science in that the vast majority of scientists have been males; (2) women are denied epistemic authority; (3) women have a unique style of thinking and unique modes of knowledge, but these are denigrated; (4) sometimes science has produced theories in which women are seen as inferior, unimportant, or valued only to the extent that they serve male interests; (5) theories are sometimes produced in which women's interest is made invisible or gendered power relationships are made invisible; and (6) science and technology is produced that reinforces gender hierarchies or does not in any way advantage women (http://plato.stanford.edu/entries/feminism-epistemology/).

Thus, one concern of feminists is that women are underrepresented in many of the sciences, such as chemistry, physics, and mathematics. (Recall the incident when the then President of Harvard, former Clinton economic advisor Larry Summers explored this issue in a brief speech and had to resign as feminists protested that his speech content was insensitive. See http://www.harvard.edu/president/speeches/summers_2005/nber.php for the transcript of his speech.) Feminists have also seen this underrepresentation as mirroring ways in which women are underrepresented and unempowered in wider society—if women do not have power in financial, corporate, military, academic, and political spheres, these inequities might be interrelated to the fact that they are underrepresented in the sciences. Feminists point out that this problem perhaps is even more severe in technology: If historically (and currently) women have failed to have the economic and sometimes the legal power to secure patents, how can they develop and profit from their technological creativity?

An additional concern is that sometimes women are not even sufficiently valued to be the subjects in scientific research—a whole host of medical research was conducted, but it was unclear to what extent the results applied to women because all the subjects were male. For example, in the early studies showing a daily low-dose aspirin regimen decreased heart attacks—all the subjects were men, and thus, it Left of the question of what women ought to do to decrease their risks of heart attacks. The Physicians' Health Study included 22,000 men and zero women (http://feminist.org/research/medicine/ewm_exen.html). Feminists justifiably take credit for a series of reforms of medical research instituted in the 1990s that require the inclusion of female subjects in clinical trials (Schiebinger 1999).

Another concern is that are histories of science written in a way that excludes the contributions and the problems of women? Remember there are legitimate critical questions regarding—historiography, that is, how is history to be validly written? Feminist have suggested that in conventional histories of science, the contributions of women are ignored, the problems of women are ignored, and the problems of men prioritized or that if a fairer history was written, one might even at times see men appropriating the scientific work of women without giving them due credit (e.g., the reliance of Nobel Prize winners Watson and Crick on the work of Rosalind Franklin but received little recognition for her work on the molecular structures of DNA and RNA).

However, probably most radically, some have also suggested that modern science is a male way of knowing and that it privileges this way at the expense of female ways of knowing. For example, feminists assert that males interpret experience differently than females and use different, often more aggressive and violent metaphors. Feminists give examples such as the one we mentioned in the beginning of this chapter, Newton's rape manual:

> One phenomenon feminist historians have focused on is the rape and torture metaphors in the writings of Sir Francis Bacon and others (e.g., Machiavelli) enthusiastic about the new scientific method. Traditional historians and philosophers have said that these metaphors are irrelevant to the *real* meanings and referents of scientific concepts held by those who used them and by the public for whom they wrote. But when it comes to regarding nature as a machine, they have quite a different analysis: here, we are told, the metaphor provides the interpretations of Newton's mathematical laws: it directs inquirers to fruitful ways to apply his theory and suggests the appropriate methods of inquiry and the kind of metaphysics the new theory supports. But if we are to believe that mechanistic metaphors were a fundamental component of the explanations the new science provided, why should we believe that the gender metaphors were not? A consistent analysis would lead to the conclusion that understanding nature as a woman indifferent to or even welcoming rape was equally fundamental to the interpretations of these new conceptions of nature and inquiry. Presumably these metaphors, too, had fruitful pragmatic, methodological, and metaphysical consequences for science. In that case, why is it not as illuminating and honest to refer to Newton's laws as "Newton's rape manual" as it is to call them "Newton's mechanics"?

Feminists have suggested male-centric biases can be found in other areas too: Male reproductive biologists have seen the sperm as active and the egg as passive, but feminists have argued a better case can be made about a more active role of the egg in selecting one sperm during fertilization. They have also suggested that technology based on "controlling" and "dominating" nature is also a male bias and a female-oriented technology would be more ecologically sensitive.

Feminists and the "Situated Knower"

Feminists seek to understand "the situated knower"—that is, the many ways in which the knower's social, psychological, and even physiological characteristics affect what and how one come to know. Their view is that an analysis of the knower's situation will review a patriarchal, sexist, and misogynist context that has an influence on the "known." Feminists seek a libratory science free of misogynistic practices and assumptions so that women's voices, concerns, values, and contributions can be brought forth. Here is their quite interested view of the situated knower—an epistemic account that is quite interesting:

Embodiment. People experience the world by using their bodies, which have different constitutions and are differently located in space and time. In virtue of their different physical locations, observers who stand in front of an object have different information about it than observers who have a distant but bird's eye view of it.

First-person versus third-person knowledge. People have first-personal access to some of their own bodily and mental states, yielding direct knowledge of phenomenological facts about what it is like for them to be in these states. Third parties may know these states only by interpreting external symptoms and imaginative projection or by obtaining their testimony. People also have knowledge *de se* about themselves, expressed in the form "*I* am F *here, now*." This is distinct in character and inferential role from propositional knowledge having the same content, which does not use indexicals.

Emotions, attitudes, interests, and values. People often represent objects in relation to their emotions, attitudes, and interests. A thief represents a lock as a frustrating obstacle, while its owner represents the lock as a comforting source of security.

Personal knowledge of others. People have different knowledge of others, in virtue of their different personal relationships to them. Such knowledge is often tacit, incompletely articulated, and intuitive. Like the knowledge it takes to get a joke, it is more an interpretive skill in making sense of a person than a set of propositions. (The German language usefully marks this as the distinction between *Erkenntnis* and *Wissenschaft*.) Because people behave differently toward others, and others interpret their behavior differently, depending on their personal relationships, what others know of them depends on these relationships.

Know-how. People have different skills, which may also be a source of different propositional knowledge. An expert dog handler knows how to elicit more interesting behavior from a dog than a novice does. Such know-how expresses a more sophisticated understanding of dogs on the part of the expert and also generates new phenomena about dogs for investigation.

Cognitive Styles. People have different styles of investigation and representation. What looks like one phenomenon to a lumper may look like three to a splitter.

Background beliefs and worldviews. People form different beliefs about an object, in virtue of different background beliefs. In virtue of the different background beliefs against which they interpret a patient's symptoms, a patient may think he is having a heart attack, while his doctor believes he just has heartburn. Differences in global metaphysical or political worldviews (naturalism, theism, liberalism, Marxism) may also generate different beliefs about particulars on a more comprehensive scale.

Relations to other inquirers. People may stand in different epistemic relations to other inquirers—for example, as informants, interlocutors, students—which affects their access to relevant information and their ability to convey their beliefs to others (http://plato.stanford.edu/entries/feminism-epistemology/).

In psychology, a very interesting episode occurred in research, regarding moral reasoning. First, a Harvard psychologist Kohlberg (1981) proposed a theory of moral development based on how individuals' reason about what is ethically right and wrong. He suggested that there is a series of stages of moral reasoning in which, for example, an early-stage reasoning is simply based on what one gets punished for and a later-stage moral reasoning is based on universal principles of justice. His former graduate student Gilligan 1977 published a critique of Kohlberg's view in a book called *In a Different Voice*. In this book, Gilligan argued that Kohlberg only used samples of males in his studies and thus failed to see the unique perspectives of women, and in doing so, his theory relegates women to be deviants from the norm. Gilligan's research using samples of females suggested that females use a different type of moral reasoning—women cared more about preserving relationships and nurturing than simply following rules. Gilligan argued that women's moral reasoning should not be seen as inferior but just different—"situated" to use the term discussed above. In all likelihood, there are still further ways that situate a knower who is reasoning morally—race, socio-economic status, etc. But Gilligan's work can be seen as a partial cashing out of this general notion of the situated knower.

Foucault on Psychiatry and Sexuality

> ….modern man no longer communicates with the madman… There is no common language: or rather, it no longer exists; the constitution of madness as mental illness, at the end of the eighteenth century, bears witness to a rupture in a dialogue, gives the separation as already enacted, and expels from the memory all those imperfect words, of no fixed syntax, spoken falteringly, in which the exchange between madness and reason was carried out. The language of psychiatry, which is a monologue by reason about madness, could only have come into existence in such a silence.
>
> Foucault, *Preface to the* 1961 *edition*.

The French philosopher Michel Foucault is an important postmodernist who saw himself as engaging in "archeology," which he sees as a "metaphor for "digging deep" into the underlying rules and assumptions of the human sciences (Windschuttle 2000). He hoped that this digging will show how certain *discourses*—what sometimes Foucault also calls "regimes of truth" come to be accepted (temporarily) as true and how certain practices (again temporarily) come to be accepted as normal. These regimes are always contingent social and historical products, bound up with the economic, cultural, and political realities of their times.

Foucault suggested that modernity adhered to the Enlightenment ideals of scientific rationality, objectivity, dispassionate search for truth, and the universality of knowledge. However, Foucault argued life is full of inequities and oppressive structures of power that need to be exposed and criticized. He often used a historical method and indicates that this method reveals that there was a shift in "discourses"—and these need to be examined by the critical historian because in these linguistic structures can be found part of the mechanisms of power and oppression. In two of his most important books, *Madness and Civilization* and *The History of Sexuality*, Foucault presented a critique of the medicalization of mental illness and sexuality—one discourse that emerged from quite different prior discourses. His critique is based on an examination of the context of post-Enlightenment discourse about madness as a failure of rationality—and how this failure defines the "other" who must be segregated from society.

He saw psychotherapy and psychiatry as critically involving issues of "disciplinary power" and sees the construction of categories used by psychiatry as attempts by the larger society to exert its interests as well as mechanisms by which doctors can exert power over those they deem to be mentally ill. In this contemporary discourse, problems with our beliefs or sexualities show up not as religious, spiritual, or moral issues—as they have in the past—but as technical problems that are open to rational discourse involving examination, classification, analysis, and intervention by suitably trained experts. For Foucault, although this has brought benefits, there are also losses and losers in this process. He also suggested that his historical research does not reveal a linear, progressive trajectory for psychiatry. He sees much therapy as "confessional" in which an individual who has an unsound will and unorthodox passions is "opposed" in a struggle of domination by the "healthy" physician. Foucault stated:

> We must apply a perturbing method, to break the spasm by means of the spasm…. We must subjugate the whole character of some patients, subdue their transports, break their pride, while we must stimulate and encourage the others (Esquirol, J.E.D., 1816 [12].

Foucault also critiqued the increasing internment of psychiatric patients which he thought consisted of involuntary commitment (internment), isolation, interrogations, punishment techniques such as cold showers, moral talks, strict discipline, compulsory work, relations of vassalage, of domesticity, and even of servitude between patient and physician. The physician becomes "the master of madness." Foucault pointed to the history of brutality in which the mentally ill are deprived of freedoms, confined in unsavory institutions, overmedicated, lobotomized, and even periodically shocked.

In some ways, Foucault's critiques are not new. The Soviets used psychiatry and asylums as an arm of the totalitarian state: committing those that would create their perfect state as "insane" in need of rehabilitation. However, it is important to know something of Foucault not only because he is an important postmodernist but also because he turns his attentions to subjects very close to clinical psychology.

Discussion

It is fair to say that the authors we are discussing in this chapter generally cluster on the Left politically. This raises some interesting questions. First, we note that although the postmoderns are writing against what they see as the entrenched orthodoxy and entrenched power, it is actually none too clear that those who hold power are actually on the Right as they seem to assume. What happens to the worth of these postmodern critiques when the Left is in power—or at least has more power—such was clearly the case of the first two years of the Obama administration? Do these critiques become somewhat moot? It does not appear to be so because as Gross and Leavitt (1997) and Brown (2001) have noted, these critiques are usually from the Far Left. Certainly, these postmodernists are much more radically politically than the typical liberal democratic political view—most adherents of this kind of this more mild leftism would not see Newton's *Principia* as a rape manual, would hold more traditional views of science, and would generally be in favor of much more mild political and social reforms. However, if this is the case, then these postmodern critiques are not only relevant to the Right but also relevant to the mild Left often found among scientists, professionals, university faculty and administrators and state bureaucrats. To see these critiques as only applying to the boogey man of the Right is to create a straw man.

The case can be made that the (mild) Left has a fair amount of power in science and particularly in psychology. Rothman et al. (2005) found, for example, a 9:1 ratio of Democrats to Republicans among psychology faculty. This represents a dramatic increase from surveys taken in 1960s of 3.2 (McClintock 1962). (Although this is still not as skewed as other disciplines, the Democrat/Republican ratio in English was 19.3; Philosophy, 24.0; and History, 75.0 as reported in Klein and Stern 2009!) It is hard to make the case that with these highly skewed numbers, the problem with power in academia is that is a handmaiden to the Right.

Thus, through this lens, we actually see then a prong of the postmodern critique as the Far Left criticizing the soft Left. Then ought the force of this criticism be seen not only to apply to capitalists such as Gates and politicians such as Reagan, but the pedestrian management of the average department chair, or the rather general acceptance of the status quo by Left-leaning professional organizations such as the APA (O'Donohue and Dyslin 1996; Redding 2001)? And does the Left have its own oppressive and hegemonic practices such as Campus Speech Codes and other aspects of political correctness?

It is also interesting to note that these postmodern views are advanced with little use of economics, economic history, or political science. In this sense, they can also be quite naïve. What is the actual evidence of the effects of capitalism on poverty? What has brought more wealth and relief of suffering to the poor—capitalism associated with the Right or socialism associated with the Left? Where is the analysis of the evidence relevant to this question for these postmodernists? Are citizen freer in society's leaning to the Left or to the Right? What about historical situations such as East and West Germany? Is a market economy intrinsically a freer economy than a command economy as Popper would suggest? Although for them "progress may be a failed narrative," it seems like the percentage of humans across the world who do not go to bed hungry or sick has moved in the correct direction—and this move is based on increased wealth and the technology wealth can develop and disseminate. Who has done more good for the children of Africa-American white male heterosexual capitalists such as Bill Gates and Warren Buffet or Foucault, Lacan, and Derrida?

Moreover, what if an analysis of the power structure of capitalism shows that certain minority groups that have demonstrated consistent economic success such as Greeks or Jews are overrepresented say on Forbes list of billionaires? Does the sort of critique recommended by the postmodernists become anti-Greek or anti-Semitic? What happens when liberation science discovers internal strains—such as misogynist rap lyrics of the authentic indigenous "local" culture of African-American gangsta culture? What is the quality of the "local knowledge" of Chinese women as they selectively abort females?

There is a salutary movement—because of the concern of money and commercialization of science for researchers—particularly those doing research related to the drug industry to be required to disclose their financial interests before their talks or in a footnote to their journal articles. The basic idea is that such disclosure increases sunshine and allows the reader to see whether the researcher has a stake in the outcome of the research. However, this is only a partial solution. For example, note that not all researchers are required to do this. Psychotherapy outcome researchers are not required, while psychopharmacology outcome researchers are. Psychotherapy researchers are not required to disclose all financial matters—positive results can lead to publications that can lead to merit pay raises at their university. Positive results are also more likely to lead to a higher probability of future grant funding—and researchers pay themselves summer salaries, fund trips, buy nice new computers, and pay for salaries for assistants from these grant funds. Moreover, researchers including psychotherapy researchers can make additional funds by leading workshops, writing books—particularly self-help books. Yet none of these kinds of financial matters are required to be disclosed.

Finally, researchers may not only be "in it for the money." They can also be in it for other kinds of goods such as fame and sex. If we want to disclose all the non-cognitive forces operating on the researcher so that we can assess to what extent the researcher's work represents "good science," do we also need to get some sort of picture of these other kinds of payoffs? How would we even do this—requiring researchers to disclose how many times they got lucky at the last convention? (Remember Kissinger's words, "power is the greatest aphrodisiac.") Or how many graduate students they have been able to marry? Or how many free trips they have earned to speak in nice locations?

I think it is good that the postmodernist have raised questions about the relationships between science and power. I do think it is rather one-sided though and expresses a political bias. It may be useful (and somewhat paradoxically consistent with some of their views) to look at this question from a variety of political lenses and not just that of the Far Left. In addition, it seems that some of the leaders of the postmodernism have given signs that things have gone a bit too far—that the proverbial baby is being thrown out with the bathwater. It is useful then to also seek a more balanced examination of some of the interesting questions raised by the postmodernists.

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Chapter 8
The Complexity of Science Studies: Multiple Perspectives on a Human Endeavor

Although this story might be a bit simplistic, across the chapters in this book, you may have seen something like the following narrative unfold. First, scholars studying science brought the tools of traditional philosophy, namely an examination of arguments (including inductive and deductive arguments) and therefore an examination of the logic and the semantics of arguments. The logical positivists tried to tell the following story: (1) looking at semantics through the lens of the verifiability criterion, the logical positivists argued that science first contains only meaningful sentences and these are either synthetic or analytic as opposed to meaningless metaphysical sentences; (2) In addition, science relies on inductive arguments, and science is progressive and cumulative as more and more observations increase the probability of any scientific law; and (3) at times, these laws get deeper as the laws of chemistry, for example, are reduced to the laws of physics. We also saw the logical positivists using the traditional tools of the philosopher: logic and conceptual explication to deal with what they saw as some conceptual problems in semantics and epistemology. However, the arguments of the logical positivists collapsed as they could not offer a sound version of the verifiability criterion and problems occurred in demonstrating a truth preserving inductive logic.

Popper, too, started off with the traditional tools of philosopher. However, unlike the logical positivists, he argued that science uses *modus tollens* in deductive arguments in an attempt to eliminate error by falsifying favored theories. Popper did not use any criteria of meaningfulness but rather he employed his demarcation criterion which he argued separated scientific theories from nonscientific theories. He stated that a scientific theory had to rule out some observable states of affairs. Popper was also criticized through the use of the traditional tools of the philosopher when Quine and others pointed out that because a number of auxiliary hypotheses are actually involved in deducing any observable consequence of a theory, that therefore, the arrows of *modus tollens* do not uniquely point to the theory under test. Rather, the scientist simply knows that some belief in his or her web of belief is false. Logic can no longer be the guide: Quine proposed a set of pragmatic criteria for distributing the arrows of *modus tollens*.

W. O'Donohue, *Clinical Psychology and the Philosophy of Science*,
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But then a rather interesting thing happens for both Popper and Quine: They start using science to elucidate science. Quine suggested that epistemology should be "naturalized" which for him means studied in the learning laboratory of the psychologist—it is here where we can see the conditions under which knowledge is acquired. And Popper also took a biological turn and suggested that evolution should be used to understand how humans acquire knowledge—including scientific knowledge. He suggested that our bodies are embodied knowledge of our past environments and that science, like evolution, works by trial and error. We see a turn away from the traditional tools of the philosopher to an analytic framework much closer to ways psychologists analyze phenomena (especially behaviorists and evolutionary psychologists).

Kuhn again brought other tools to study science. He emphasized that the history of science should be studied to see what scientists have actually done. Thus, he brought the tools of the historian to bear (although it is debatable how well he did this and how sophisticated is historiography is). Kuhn claimed that when certain key episodes of science are studied, the historian does not see Popperian falsification but something quite different: A series of developmental stages where first there is pre-paradigmatic science in which there is debate about fundamentals and no consensus, then a puzzle-solving solution results in normal science in which a paradigm—a problem-solving exemplar creates consensus and serves as a model for other problem solving; but then puzzle-solving failures result in Revolutionary science in which a new paradigm replaces the old, and so on. Kuhn also suggested that sociological processes are important in science and operate to create consensus about a particular paradigm (although he is a bit vague on exactly how all this happens). We also see with Kuhn a key use of psychology—in understanding perception and the theory ladenness of fact—so-called top-down processing by the cognitive psychologists. Kuhn suggested that with a new paradigm, there is a "gestalt switch" in which old phenomena are seen in radically different ways. Thus, with Kuhn again we see another move away from the traditional perspective and analytic tools of a philosopher when he brings to bear historical, sociological, and psychological tools.

In the next chapter, we see Feyerabend again bringing historical tools to bear on the past record of science, but again arriving at much different conclusions than Kuhn. Feyerabend suggested that a wider study of the history of science would reveal that all rules are broken in order to do successful science and thus "anything goes." However, again like Kuhn, one can question the sophistication of this historiography. Feyerabend also argued that it is important to study science through the lens of politics: Who gains from science? Does science try to be monopolistic? Do scientists have too much power? Does science oppress? So again, we see a move away from the traditional modes of the philosopher to perspectives much more allied with the historian and the political philosopher or political scientist.

We see more conventional philosophical tools at work again with Lakatos and Laudan, although again both are fairly interested in the study of historical episodes. Again, Laudan notes that there are conceptual problems in science and these are important to understand in theory appraisal and in examining the progressivity of science. In this, Laudan can be taken to say that the scientists ought to know some philosophy as philosophers have some tools to deal with these conceptual problems. The next meta-scientists we examined Alan Gross, however, suggested that science relies on rhetoric—persuasion. Although he uses tools from classical rhetoric, social psychologists such as Calpaldi and others have taken these questions regarding persuasion in the social psychology laboratory and have found interesting regularities. Should the effective scientist understand the psychology of persuasion? Gross would tend to think this is an excellent idea. We also see a practicing research psychologist B.F. Skinner also presenting his views of what it takes to produce successful scientific behavior—he analyzed his own past behavior and came up with a series of practical tips, again moving away from traditional philosophical modes of analysis.

We have ended our survey of meta-science with the post-moderns. Again, we can see a move away from the traditional tools of the analytic philosopher (although must closer to the continental philosopher Friedrich Nietzsche) and again analyze science from the perspective of history, including the history of semantics, as well as with the tools of the political scientist, the economist, and the anthropologist. The post-modernists ask the scientists to be self-conscious about political issues in language use as well as asking who is benefiting and who is suffering through these concepts and assumptions. They challenged major meta-narratives of science such as progress and value neutrality. They suggested that science has too often been oppressive and hurt minority cultures such as women and First Nations. The post-moderns and particularly the feminists see the knower not as holding any objective viewpoint but as "situated." When one examines the dimensions of this situatedness; one sees many of the dimensions that are familiar to the psychologist: emotions, top-down processing, culture, gender, etc. One also sees a focus on what psychologists have typically called "individual differences"—a focus on an individual's uniqueness instead of only look at characteristics of the group or "averages."

Thus, we have seen that science is studied as an all-too-human endeavor. It may have all the limitations that evolution has given the human perceiver and knower; it requires complex judgment; it is influenced by the scientists' biases and goals; it involves a messy, complicated use of language; and it has a complicated multivocal history. But even though this picture is quite complicated in it, there is a kind of good news for a psychologist. A psychologist may not be all that familiar with the tools of the analytic philosopher but a psychologist ought to be familiar with the multiple perspectives human behavior can be studied from. And it could be the case that psychology becomes a very important perspective to understand science: It certainly seems to be the case that it plays an important role in many of the meta-sciences we have discussed.

Thus, I will argue that it is best to see meta-science as a collection of problems and analytic tools. This is the case because so many different kinds of questions can be asked about the human endeavor that comprises science. Here are the main types, which you should recognize from our brief survey in the previous chapters.

Logical problems: What is the logic of research? What is the logic of induction, if any? How does one deal with the ambiguities of falsificationism based *on modus tollens?*

Linguistic and semantic problems: (1) How do we come to understand the meaning of a scientific construct or a theoretical or observational sentence? How are meanings of certain kinds of sentences (say observational reports) related to experienced percepts? How do observation sentences relate to the meanings of more abstract sentences such as theoretical sentences? What is the history of the meanings of constructs and are these associated with political assumptions or agendas? What is the meaning of meaning—and what methods can be used to answer this?

Historical problems: (1) What has actually happened in science—or in a particular branch of science? What can we learn from the history of science? If someone is arguing by using the history of science—have they captured this episode fairly? What is the actual historical record? Is their use of this alleged historical episode a rhetorical move that if one looked at other historical episodes, one would find that the historical record is inconsistent with their point? How do the ideologies of the time affect the history of science and the current writing of the history of science? Are the stories of some folks being left out or distorted?

Sociological problems: How does the scientific group—defined either locally—say others in the laboratory or in the department—or more broadly–those who are studying a particular problem—affect the way science is done? How is consensus created (and broken)? What is the role of larger sociological processes such as general cultural beliefs or national norms on science and vice versa?

Philosophical problems: What is unique if anything about scientific ways of knowing? What are a theory's ontological commitments? Is the philosophy of science descriptive or prescriptive? What lessons can be learned from the philosophy of science in order to do science better?

Political problems: Does a certain view of epistemology support a certain view of the proper government (say, Popper's falsificationism support the free market; or the post-modern view support a more radical Leftist socialist view?). How does the political system affect science? How should it affect science? Has science become an elite or entwined with the elite so that it must be in some way tamed and its monopolistic tendencies brought into rein?

Psychological problems: How does perception actually work—even the scientists perceptual activity—is it top down, influenced by theories—or bottom up—influenced by unbiased raw data—or both? Is each human knower—situated—and thus is science partly a reflection of human psychology?

Economic problems: How does money affect science? Certainly wealthier countries produce a lot more of it than poor countries. To what extent has research been corrupted by money (say concerns about the fairness of research and research reporting in pharmaceutical research)? Ought there be some sort of economic democratization of science where the poor are better included—a people's science?

Ethical problems: Is science too important in contemporary society? Is it a tool of oppression? Are all folks properly included? Is its technology all too often ultimately harmful to nature and the Good Life?

Scientific problems: If science is to be studied scientifically, how do we get out of this reflexive loop? Is evolutionary epistemology something with special status as it using the best science to answer a key question about how humans come to know?

Major Lessons from the Survey

Science is complex. We have seen many different kinds of questions can be asked about it. We have seen that there are many different answers given to these questions. The reader can chose (hopefully reasonably) and prioritize certain of these, but ought to be mindful of the criticisms of these and the alternatives. It would seem reasonable to say at least that no simple answer has yet emerged too many of the key questions in meta-science. Psychologists ought to show care regarding any simple pronouncements regarding what science actually is.

There has been in some place a "great dialogue" about meta-scientific questions. I have tried in this book to discuss some of the major aspects of this great conversation. Hopefully, this provides a context and content for any psychologists questioning and conclusions about science. Sometimes, it has been the case that psychologists have become enamored of a certain philosopher (maybe even a very obscure one like Kantor or Pepper) and have acted as if this philosopher is mainstream or if alternatives do not exist to this person's thought, or have not tried at least to place this rather obscure thinker in the context of the great conversation that is occurring in mainstream meta-science.

The philosophy of psychology is not simply related to the philosophy of physics. String theory, chaos theory, etc., may (or may not be) important for the explanation of certain physical phenomena but whether they have any utility regarding psychology or the meta-issues in the philosophy of psychological science is an open question. Although the logical positivists believed that what is going on in physics is vital for understanding all the other sciences, this meta-position has not been born out. In fact, Popper noted a key shift and if any science sheds light on science and human knowledge acquisition it is evolutionary biology not subatomic physics. Evolutionary epistemology has been a central concern of leading scholars such as Popper, Quine, Skinner, and Donald Campbell.

Exegesis is important and it is not sufficient just to borrow unsystematically a few terms from some philosopher of science. Psychologists have been relatively enamored with Kuhn and have imported his concept of paradigm. Although Kuhn is none too clear about its meaning, psychologists have not used it in a canonical fashion: A paradigm has to solve at least one problem and then be used as a heuristic to solve others. Psychologists have missed these two important criteria and speak loosely of psychoanalytic paradigms and humanistic paradigms. What problems have these solved and how have these problem solutions been used as exemplars to try to solve other problems? I think it is fair to say that behavior therapy has problem-solving exemplars (from enuresis, to phobias, to child behavior problems) and these early applications of learning principles were used as exemplars to solve other problems. Thus, behavior therapy constitutes a paradigm in the Kuhnian sense but the other theories need to show how they meet these two criteria before they rightly deserve the honorific "paradigm."

It is important to actually look at the historical record before attributing philosophical commitments or alliances with certain movements in psychology. It has been alleged that logical positivism and behaviorism have connections that after a careful examination, they simply do not have (Smith 1986). This can be a deceitful use of rhetoric—a problematic way of persuading that *x* is bad, because *x* is associated with bad philosophy.

The problems of science are intertwined with problems of language. There are complex questions concerning word meaning and the relations between "observables" and theoretical language. Understanding linguistics and semantics and progress in these fields can be useful. Some philosophers of science have had to give accounts of language as there meta-scientific problems had at least a linguistic dimension.

Part of rationality is criticism and maximizing criticism. One can see in the course of reading the preceding chapters that this is often what these scholars were doing. In addition, some scholars such as Popper and Feyerabend explicitly argue that the essence of rationality is criticism. Popper wants severe testing to most efficiently use criticism to root out error.

How a scientist reacts to prediction failures is complex but key. Does a confirmation bias rear its ugly head and it the failure "explained away"? What auxiliary hypothesis is blamed? Is that same auxiliary hypothesis praised even in the same experiment when it is used to deduce a prediction success (I have seen in dissertation defenses, an auxiliary hypothesis praised when it helps confirm a hypothesis but criticized in the same experiment and blamed for a prediction failure). Is the auxiliary hypothesis strengthen is subsequent research? Are modifications to the theory content increasing?

The history of science, including the history of psychology, is complex. Arguments from history can be simplistic and biased. Arguments from history need to be viewed critically. One has to ask key question such as "How good is this history being argued for here: is this actually what the historical record says?" Why is the scholar pointing to this or this sample of historical incidences when there are many others; is this sample skewed in any way? What does the history of one science, say astronomy, have to do with the history of another, for example, psychology?

The Psychology of Science is an important emerging discipline. It can give important answers regarding several issues: (1) the theory ladenness of perception and top-down versus bottom-up cognitive processing; (2) human knowledge acquisition; (3) how language is learned and how it functions; (4) evolutionary biases in belief formation; (4) the role of psychopathology in certain deviant practices in science, for example, forging data; (5) the role of personality variables in successful and unsuccessful science; (6) the social psychology of science; how others influence a scientists beliefs and behavior, etc.

That the choice of problems in research is critical. Many philosophers of science see science as problem solving. Some have even suggested that there are both empirical and conceptual problems. Psychologists have been slow to clearly recognize the latter and have been slow to develop methods to resolve these and even to value work on these. This needs to change. Psychologists also need to be more self-conscious and critical about their choices of problems (why do contemporary behavior analysts seem almost to fetishize autism but ignore other key problems such as obesity, smoking, exercise, and treatment compliance?). Kuhn also suggested that part of the art of science is to see that certain problems are "ready to be solved" while others are not.

Research programs can be evaluated on their problem-solving effectiveness and even on their rate of problem solving. What problems have been solved by a research tradition? Over the past *x* years has this rate increased, stayed the same, or decreased? What are the competitors to this research program and what has been their rate of problem solving. For example, I would conjecture that the initial rate of problem solving was much higher for behavior analysis and behavior therapy, than the rate has been for the past decade or so. Fortunately though for these folks, their competitors do not have a higher rate.

That there are important tools that can be used in theory appraisal. Is the theory falsifiable? Has it been subjected to severe testing? What are its conceptual problems? What is its rate of problem-solving successes? Does it indeed have any problem-solving successes? How is it handling anomalies? Are its revisions progressive in the Lakatosian sense (content increasing and at least one corroborated?)

That science has an important political dimension and can be examined and criticized along political lines? Who does it advantage? Are its very concepts permeated with political problems? However, I suggested that this political dimension is complex. Critics can certainly look for the problems posed by the Right, but to be fair, they also need to look for problems associated with the soft Left or the far Left. The far Left's concerns with the President of Harvard, Larry Summer's speech is an example of this kind of issue. The near dogmatic status of cultural sensitivity in psychology is a problem presented not by the Right but by the Left (O'Donohue and Benuto 2010). One can be concerned whether minorities it seeks to advantage are actually advantaged or rather the strange distinctions (Asian-American) and the amateur anthropology (cares about families) are an attempt by the majority culture to feel good about themselves regarding concerns about racism and historical wrongs (Steele 2006).

Rhetorical issues are important to examine. How does a scientist persuade? What tropes are being used? What are the strengths and weaknesses of these? What are persuasive burdens and how do these come about (e.g., does folk psychology play a large role?).

Meta-science can be both descriptive and prescriptive, but when prescriptive one needs to ask how fruitful these prescriptions have actually been. For example, much ink is spilled in journals like Philosophy and Behavior regarding how psychologists or indeed any scientist ought to go about doing some aspect of science, for example, constructing a theory, or testing a theory, or choosing theoretical alliances. Typically, the scholar advances some favored framework and often implicitly some evaluative criteria (e.g., parsimony). However, it is also important to examine pragmatically whether in successful episodes of the history of psychology or in the history of science, successful scientists actually adhered to this favored framework. The history and philosophy of science can be very pragmatic: examine what has worked, not what just seems to be elegantly consistent with some proposed criteria.

There are many key unsettled questions that one ought to not take a simplistic approach to, among these are: Does science demonstrate progress? What is the relationship between theoretical terms and observation terms? Should good science rely on falsification? How does induction work? How can one theory be reduced to another? Are there natural kinds? Is there objective knowledge? What are scientific laws? What does it mean to give a scientific explanation? What are the limits of science? How do other kinds of claims such as metaphysical or political play a role in science? Are scientific concepts human artifacts or representations of reality?

And perhaps most importantly hopefully the reader now has some tools to begin to think critically about psychology: For example, how much progress has it shown? What can be done to increase this progress? How good are my theoretical commitments? What are the conceptual problems I face in my professional work and how can progress be made regarding these? How can I think differently about developing a research strategy? What can I learn from the history of science or the history of psychology? What is going on politically with my discipline or my research and how can I better understand this? What are the limits of science and how do I understand "situated knowers"? Does psychology oppress, and if so, who and what can be done about it?

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