ORIGINS OF THE VALUE-FREE IDEAL FOR SCIENCE

WHILE SCIENTISTS TOOK ON an ever more visible, even if more contentious, public role throughout the 1960s and 1970s, philosophers of science came to ignore this public role. One might imagine that philosophers of science would illuminate this role, examining the place of expertise in a democracy and helping to shape public discussion of the proper relationship between science and society. Yet since the 1960s, philosophers of science have been largely silent on these issues. Most philosophers of science consider their work to belong to a subfield of epistemology, the study of knowledge, and as such are solely concerned with epistemological issues in science, such as the relationship between evidence and theory, the status of scientific theories, and the nature of scientific explanation. Issues of how to understand science in society, the role of social values in science, and the responsibilities of scientists have been excluded from the field.[1] Most of this exclusion has been maintained on the basis that science is (or should be) a value-free enterprise, and that scientists should not consider the broader implications of their work when conducting research. Under this view, there is nothing philosophically interesting about the relationship between science and society. Science is our best source for reliable knowledge about the world, and what society sees fit to do with that knowledge is its own affair, outside the purview of both scientists and philosophers of science.

Such a simple-minded understanding of science is not illuminating for

the issues raised by science in policymaking. Nor have other discussions of science in society helped much. For example, while the Science Wars were precipitated by an attempt to directly challenge the epistemic authority of science, this debate was of little interest outside of academia and failed to provide insight on the complex role of science in society. That science should help answer certain questions and guide policy is not at issue. More central are the controversies concerning on which science (or scientists) we should rely, particularly given the protracted disagreements among scientific experts that exist in many cases. Society needs a more nuanced understanding of science in public policy, one that will account for how scientific experts can have lasting and intractable disagreements yet still be honest participants in a debate.[2] And both citizens and policymakers in a democracy must have a way to decide how to interpret scientific findings that are not settled science. A more careful appreciation of the role of values in science is essential to such an understanding. This approach, however, rejects the value-free ideal for science, going against the dominant position of the past forty years in philosophy of science.

In order to understand the value-free ideal in its current form, we need to examine in greater detail the origins of the value-free ideal for science. There are several surprises here. The first is that *in its present form*, the value-free ideal is fairly recent, dating predominantly from the cold war period.[3] In fact, it does not get its stranglehold on philosophy of science until around 1960. The ideal that has held sway since 1960 is a complex one. It does not hold that science is a completely value-free enterprise, acknowledging that social and ethical values help to direct the particular projects scientists undertake, and that scientists as humans cannot completely eliminate other value judgments. However, the value judgments internal to science, involving the evaluation and acceptance of scientific results at the heart of the research process, are to be as free as humanly possible of all social and ethical values. Those scientific judgments are to be driven by values wholly internal to the scientific community. Thus, the value-free ideal is more accurately the "internal scientific values only when performing scientific reasoning" ideal. How this ideal developed and became the dominant view in philosophy of science is chronicled in this chapter. I will also indicate how the philosophers' value-free ideal influenced the scientific community proper, and thus how it has hindered a useful and illuminating understanding of science in the broader scientific and policymaking communities.

The second surprise is that the ideal rests on a problematic presumption, one that is central to this inquiry. What philosophers of science needed to solidify the basis for the value-free ideal was the notion that scientists are not involved in public life, that they provide no crucial advisory functions, and that they provide no assistance for decisionmaking (or at least that scientists should act as if they were not involved with public life in these ways). In other words, philosophers of science assumed that science was fundamentally and acceptably isolated from society. Despite evidence to the contrary (as shown in chapter 2), philosophers continued to insist on this point into the 1980s, so it is not surprising that philosophy of science has been of so little assistance in helping to understand the role of science in public policy.[4] Once we reject the presumption of an isolated scientific community, the ideal of value-free science in its current form crumbles.

Philosophy of Science and the Value-Free Ideal

As Proctor (1991) discusses, the value-free ideal in science has meant different things in different times and places. Proctor locates the beginnings of the ideal in the seventeenth century with Francis Bacon and debates over the relationship between science and the state, but he argues that the ideal shifted in form and content in the nineteenth century, when the rise of the German university, the growing importance of the natural sciences, and the development of the social sciences led to stronger attempts to separate science from values (Proctor 1991, 65–74). As science became more important, the arguments for the value-neutrality of science, particularly for social science, found their strongest advocate in Max Weber. Yet this strong advocacy died with Weber in 1920 (ibid., 134, 157).

Thus, despite these roots in Western culture, the value-free ideal was *not* widely accepted by philosophers of science in the United States as recently as 1940.[5] This can be seen in an influential essay by Robert Merton, first published in 1942, which came to be known as "The Normative Structure of Science." The essay elaborates Merton's famous "ethos of science," with its four components of universalism, organized skepticism, communalism, and disinterestedness (Merton 1942, 270).[6] In the essay, Merton argues that democracies on the whole do a better job of supporting most aspects of the ethos of science than totalitarian states, a heartening argument in the darkest days of World War II. Two aspects of his argument are of particular importance: (1) that being "value-free" is nowhere among the norms, and (2) that the ethos is described as a set of norms internal to science but dependent on the broader society for their functioning. Of the four norms, the one that comes closest to a value-free ideal is disinterestedness. Merton, however, does not eschew a role for values in science in his discussion of disinterestedness, but instead argues that because of the institutional structure of science, scientists exhibit little tendency toward fraud and outright deception of others (ibid., 276–77). Disinterestedness is thus an important ethos for the integrity of science, but it is not equivalent to being value-free. Rather than argue that science is a value-free enterprise, even in its ideal form, Merton suggests that "the ethos of science is that affectively toned complex of values and norms which is held to be binding on the man of science" (ibid., 268–69). This echoes what the value-free ideal would become, a strict adherence to internal norms only, but Merton saw such adherence in more complex terms, pointing out that any ethos of science needed to be in sync with the broader social context, and that scientists had to take responsibility for the social implications of their work, or risk undermining support for scientific research (Merton 1938, 263). It was a rejection of this aspect of Merton's argument that would solidify the modern value-free ideal.

One also cannot find clear or strong endorsements for value-free science among the developing philosophy of science community. At the start of World War II, the predominant philosophical view of science and values was that the two were inextricably intermixed in practice, and that no dichotomy between the two existed. John Dewey, America's most prominent philosopher and leader of pragmatism, viewed science as an essentially value-laden enterprise, dependent upon human needs and concerns for its direction and understanding. The logical empiricists, the European philosophers fleeing an increasingly hostile Central Europe, held similarly complex views on the subject. Although the English philosopher A. J. Ayer dismissed all values as obscurantist metaphysics with no place in a scientific understanding of the world, other prominent logical empiricists disagreed.[7] Many of them emigrated to the United States in the 1930s. Consider, for example, Rudolf Carnap, a central leader of the logical empiricists, who came to the United States in 1937. Carnap argued that one could conceptually separate semantics, syntax, and pragmatics, but he did not think one could pursue a particular project in semantics or syntax (ways of understanding language) without some pragmatic commitments driving the direction of the investigation. Such pragmatics necessarily involved value commitments (Reisch 2005, 47–51). In other words, no *full* account of scientific knowledge could exclude values. Other leaders such as Otto Neurath and Philip Frank were deeply interested in the interrelationships of science, values, and society and did not see them as fundamentally separate domains.[8]

Yet in the 1940s, this understanding of science as embedded in a society, as having a clear and direct impact on society, and of this relationship being of philosophical interest, began to be undermined. Philosophers such as Carl Hempel and Hans Reichenbach pursued a program of logical analysis of scientific reasoning, of attempting to explicate in abstract form the logical relationships between scientific theory and empirical evidence. Reichenbach argued that, philosophically, which theories we are testing is uninteresting and belongs to the purely psychological/social context of discovery, whereas philosophers had their work centered in the context of justification: what evidence would justify accepting any given theory.[9] Hempel (along with Paul Oppenheim) similarly focused on issues such as what the logic of confirmation is (how much confirmation is provided by a piece of evidence) and what logically constitutes a scientific explanation (see Hempel 1965a). These works became the technical core of philosophy of science for decades to come. They aimed at a rational reconstruction of actual scientific practice, of trying to understand the logical relations that ultimately justified the acceptance of scientific knowledge. As such, they ignored not only actual internal scientific practice, but also the relations between science and the society in which it operates.

The implications of this kind of work for the value-free ideal can be seen in Reichenbach's *The Rise of Scientific Philosophy* (1951), where Reichenbach argues that knowledge and ethics are fundamentally distinct enterprises, and that confusions between the two had plagued the history of philosophy, leading much of it astray. In his efforts to keep knowledge and ethics distinct, he declares, "The modern analysis of knowledge makes a cognitive ethics impossible" (277). Cognitive ethics was the idea that ethics are a form of knowledge; thus, according to Reichenbach, ethics cannot be a form of knowledge. He continues, "knowledge does not include any normative parts and therefore does not lend itself to an interpretation of ethics" (277). In Reichenbach's view, knowledge is purely descriptive, ethics is purely normative, and the two have nothing in common. If values are also purely normative, then under this view knowledge has no values involved with it at all. Science, the epitome of knowledge for Reichenbach, would necessarily be value free.

Reichenbach's view did not go unchallenged, as we will see in the next section. But a focus on the logic of science, divorced from scientific practice and social realities, was an increasingly attractive approach for the philosophy of science as the cold war climate intensified. Although debate over whether and how science and values were related continued in the pages of the journal *Philosophy of Science* (see Howard 2003, 67), a progressive narrowing of interest in the field and an exclusion of complicating realities began. The broad range of ideas concerning science and values that held sway in the 1930s and early 1940s was no longer acceptable. The cold war demanded strict dichotomies: either one was with the United States, its capitalism, and its democracy, or one was with the Soviet Union, its communism, and its totalitarianism. Middle paths were unavailable. In this climate, arguing for links between science and values was uncomfortable for several reasons. First, in Marxist philosophy, science and values are closely interrelated. Arguing for any such links put one's work under the scrutiny of anticommunist crusades, whether or not one's work was Marxist.[10] Second, discussions concerning the relationship between science and society tended to be broadly based. There was no canonical literature on which arguments were built; there was no agreed starting point for discussion. The widespread response of academics to the pressures of anticommunist crusades, which were under way by 1948 and lasted into the 1960s, was to professionalize their fields, narrowing their expertise and focusing on a well-defined topic.[11] The topic of values in science made such a narrowing difficult. Yet the desire to professionalize the discipline of philosophy of science was palpable by the late 1950s.[12] Third, even if one could perform such a narrowing concerning science and values, it would be prudent and preferable if philosophy of science looked as apolitical as possible. For philosophers of science, creating a professional domain where science was essentially or ideally apolitical would help philosophers survive the cold war unscathed. Setting up an ideal for science as value free was central to creating an apolitical disciplinary arena for philosophy of science. Yet the value-free ideal was not obviously acceptable in the early 1950s.

The 1950s Debate on Values in Science

Even as the cold war deepened, a debate, the last robust debate, about values in science developed. In the early 1950s, Reichenbach's call for a scientific (and thus value-free) philosophy was not uncritically accepted. In *The Philosophical Review*, Norman Malcolm lambasted Reichenbach's *The Rise of Scientific Philosophy*, arguing that Reichenbach "exaggerates the importance of symbolic logic for philosophy," and that Reichenbach's own arguments show "that 'the logician' and 'scientific philosopher,' just as anyone else, can fall into primitive nonsense" (Malcolm 1951, 585–86). Even the supportive review by R. F. J. Withers (1952) in the *British Journal for the Philosophy of Science* found the book unconvincing in the end. So although the views of philosophers such as Reichenbach and Ayer were widely discussed, and a narrow technical focus for philosophy of science was gaining support, the value-free ideal was not yet in place or fully developed.[13] In reaction to the growing tide of technical, narrowly construed philosophy of science, some philosophers argued for the need for values in science. The responses to their ideas helped to generate the value-free ideal that held sway after 1960.

The most prominent arguments against the view of science as a value-free enterprise were developed by C. West Churchman, Philip Frank, and Richard Rudner. Between 1948 and 1954, both Churchman and Rudner presented widely read arguments that social and ethical values are required components of scientific reasoning, while Frank supported these endeavors. In their view, the scientist as public advisor and decisionmaker was an important role for scientists, and this role necessitated the use of ethical values in scientific reasoning. The Churchman-Rudner arguments forced philosophers of science into a dilemma: either accept the importance of values in science or reject the role of scientist as public decisionmaker. Rather than accept values in science, the field chose the latter option, thus rejecting a careful consideration of the public role of science. This allowed the value-free ideal to take up its position as the widely accepted doctrine it became: that scientists should consider only internal values when doing science.

Pragmatics of Induction: The Last Gasp for Values in Science

In general, the arguments of Churchman and Rudner centered on the issue of how much evidence a scientist should require before accepting (or rejecting) a particular scientific claim or hypothesis. Both Churchman and Rudner suggested that the amount of evidence can and should change, depending on the context in which the accepting or rejecting took place. Thus, depending on the consequences of a wrong choice (which is contingent on the context of choice), scientists could demand more or less evidence before coming to accept or reject a view. The weighing of the consequences required the use of values in the scientist's choice.

This emphasis on context was quite different than the approach of some philosophers (for example, Hempel and Oppenheim) who focused primarily on the nature of confirmation in science in the late 1940s. Confirmation studies dealt with how much a piece of evidence lent support to a particular scientific hypothesis. While Churchman was interested in issues of confirmation, he found such questions insufficient for understanding all of scientific practice, and thus began to ask under what conditions a scientist ought to *accept* a hypothesis, not just consider it *confirmed*. While the degree of confirmation was still important, Churchman (1948a) argued that it was insufficient for deciding whether to accept or reject a hypothesis (see Churchman 1948a and 1948b): "But there would be cases where we would not want to accept an hypothesis even though the evidence gives a high d.c. [degree of confirmation] score, because we are fearful of the consequences of a wrong decision" (Churchman 1948a, 256).

Churchman argued that one must consider the ends to which one will use a hypothesis in order to make a full evaluation of the hypothesis. Because there are a range of ends to which one can use scientific hypotheses, including uses outside of science proper, Churchman believed that "the complete analysis of the methods of scientific inference shows that the theory of inference in science demands the use of ethical judgments" (265). We must have "ethical criteria of adequacy" in order to find that some theory or hypothesis is adequately supported.

An essay by Richard Rudner published five years later in *Philosophy of Science* developed Churchman's argument further. Rudner (1953) argued that social and ethical values were often essential to complete scientific reasoning. He argued for this by making two uncontroversial claims: (1) "that the scientist as scientist accepts or rejects hypotheses," and (2) that "no scientific hypothesis is ever completely verified" (2). With these two claims in hand, Rudner stated,

> In accepting a hypothesis the scientist must make the decision that the evidence is sufficiently strong or that the probability is sufficiently high to warrant the acceptance of the hypothesis. Obviously our decision regarding the evidence and respecting how strong is "strong enough," is going to be a function of the *importance*, in the typically ethical sense, of making a mistake in accepting or rejecting the hypothesis. . . . *How sure we need to be before we accept a hypothesis will depend on how serious a mistake would be.* (2)

According to Rudner, determining the seriousness of a mistake requires a value judgment, and thus value judgments play an essential role in science, in shaping the acceptance or rejection of a hypothesis.

Rudner saw his work as a direct challenge to the eschewal of serious philosophical examination of values advocated by some analytic philosophers:

> If the major point I have here undertaken to establish is correct, then clearly we are confronted with a first order crisis in science and methodology. . . . What seems called for . . . is nothing less than a radical reworking of the ideal of scientific objectivity. . . . Objectivity for science lies at least

> in becoming precise about what value judgments are being and might have been made in a given inquiry—and even, to put it in its most challenging form, what value decisions ought to be made; in short . . . a science of ethics is a necessary requirement if science's progress toward objectivity is to be continuous. (ibid., 6)

Because of the implications of accepting a scientific hypothesis, science is drawn inexorably into ethics, and vice versa. Certainly science should not be value free, according to Rudner. While I do not agree with Rudner that we need a science of ethics in order to procure objective science, Rudner clearly saw that his arguments were a challenge to the growing antipragmatism in philosophy of science, as analytic philosophy rose in prominence. If philosophers and scientists were to take seriously the practical implications of inductive arguments, a conceptualization of objectivity that allowed for values would be needed. Minimally, the values used to make decisions would have to become an explicit part of scientific discussion.

The views of Churchman and Rudner were advanced in several settings and taken quite seriously at the time. At the December 1953 meeting of the American Association for the Advancement of Science (AAAS), a special set of sessions on the "Validation of Scientific Theories" was arranged by the Philosophy of Science Association, the Institute for the Unity of Science, and the History and Philosophy of Science section of AAAS, and sponsored by the newly formed National Science Foundation. The first session, on the "Acceptance of Scientific Theories," focused primarily on the work of Churchman and Rudner, with Churchman and Rudner presenting their main arguments regarding the necessity of values in scientific reasoning. (The papers were reprinted in *Scientific Monthly*, September 1954.) In his opening talk for the session, Philip Frank set the stage for Churchman and Rudner by acknowledging the complex set of goals for science:

> The conviction that science is independent of all moral and political influences arises when we regard science either as a collective of facts or as a picture of objective reality. But today, everyone who has attentively studied the logic of science will know that science actually is an instrument that serves the purpose of connecting present events with future events and deliberately utilizes this knowledge to shape future physical events as they are desired. (Frank 1953, 21–22)

In other words, science is not solely about truth or facts but also about shaping our world in a particular way. And in his essay based on this talk, Frank suggests that there are multiple possible ends that science can serve, multiple ways in which we might want to shape our world. Not only are values a part of science, but science is very much tied to society and our decisions about its future.

Although there were no critiques of the Churchman-Rudner position at the AAAS session, it was not long before other philosophers responded to their arguments. These challenges led directly to the isolated view of science, generating the current ideal of value-free science.

Closing the Ranks: Removing Social Values from Science

The program for philosophy of science that Rudner and others proposed was opposed by those who wanted a narrower focus for philosophy of science. How to stymie the force of these arguments? Two different positions were taken, both of which bolstered the isolated view of science that would become standard for philosophy of science in the ensuing decades. Both lines of critique suggested that scientists should not think beyond their narrow disciplinary boundaries when deciding what to accept or reject as sufficiently warranted science. The first line of argument suggested that scientists do not accept or reject hypotheses at all, but merely assign probabilities. If tenable, this argument would keep scientists from having to think about the possible consequences of error. However, this position had serious flaws that made it unsatisfactory for most. The second line of argument became widely adopted. With this position, scientists are not to think beyond their own scientific communities when evaluating their work. Philosophers accepted the idea that scientists must use values when making decisions in science, but rejected the notion that these values should include broad social or ethical values. Instead, the only legitimate values were narrow, disciplinary ones. By isolating scientists from social concerns and from the social implications of their work, philosophers were able to define and defend the ideal of value-free science.

The first line of argument, that scientists neither accept nor reject hypotheses, was developed by Richard Jeffrey.[14] His 1956 "Valuation and Acceptance of Scientific Hypotheses" argues that scientists should neither accept nor reject hypotheses. Instead, scientists should assign probabilities to hypotheses, and then turn the hypotheses and their assigned likelihoods over to the public. However, Rudner (1953) had already considered this position, arguing that even if this were the case, even if scientists only assigned probabilities to hypotheses and left the outright acceptance or rejection to their audience, the scientist would have to reject or accept the probability attached to the hypothesis, thus simply pushing the problem back one step (4). The scientist must say something, whether outright acceptance/rejection or a probability judgment. Either way, the scientist must decide that there is sufficient warrant for their stand, a decision that requires consideration of values in order to weigh the potential consequences of error. Jeffrey (1956) acknowledges Rudner's argument on this point: "Rudner's objection must be included as one of the weightiest [for the] . . . probabilistic view of science" (246). Despite his acknowledgment of the problem, Jeffrey provided no direct response to Rudner's argument.[15]

Although few have followed Jeffrey in arguing that scientists should not accept or reject hypotheses, several of Jeffrey's arguments were developed to pursue the second line of argument, that scientists should not consider the consequences of error beyond the narrow confines of the scientific community. First, Jeffrey suggests that one could never tell what the seriousness of a mistake in the acceptance or rejection of hypotheses might be. He argues that in general the acceptance or rejection of scientific hypotheses involves such a complex mixture of potential consequences from mistakes that it is unreasonable to expect scientists to consider them all: "It is certainly meaningless to speak of *the* cost of mistaken acceptance or rejection, for by its nature a putative scientific law will be relevant in a great diversity of choice situations among which the cost of a mistake will vary greatly" (Jeffrey 1956, 242). Thus, it is useless to demand that scientists consider these costs. Jeffrey even argues that the acceptance or rejection of hypotheses (and the consideration of values involved) can even be harmful: "If the scientist is to maximize good he should refrain from accepting or rejecting hypotheses, since he cannot possibly do so in such a way as to optimize every decision which may be made on the basis of those hypotheses" (ibid., 245).

Scientific pronouncements can be used in so many contexts, Jeffrey suggests, that the values used to accept a hypothesis in one context could be quite damaging when applied to another. Jeffrey provides no discussion of alternative approaches, such as making the value judgments explicit so that scientific work can be properly applied, or directly involving scientists in particular decisionmaking contexts where their work is used. Jeffrey's response to this complexity was to sever scientists from the context in which their hypotheses are used, and to remove them from considering the potential consequences of error altogether.

This idea, that scientists were too removed (or should be so removed) from the practical effects of their choices for social values to legitimately play a role in their decisionmaking, was developed further in the second line of argument against Rudner and Churchman, and was eventually codified in the value-free ideal. The crucial idea was that one could accept that values are needed for judgments concerning the acceptance or rejection of hypotheses, but limit the scope of those values. Churchman, in his 1956 defense of Rudner's essay from Jeffrey's critique, inadvertently opens this avenue of debate when he suggests that many of the decisions made by scientists on whether to accept or reject hypotheses could be justified in terms of values internal to the scientific process and its aims. Scientists whose work has no clear, practical implications would want to make their decisions considering such things as: "the relative worth of (1) more observations, (2) greater scope of his conceptual model, (3) simplicity, (4) precision of language, (5) accuracy of the probability assignment" (Churchman 1956, 248).

But Churchman did not think such "epistemic values" (as philosophers came to call them) were necessarily the *only* considerations for *all* scientists. This was precisely the position, however, presented by Isaac Levi. Levi argued that scientists should utilize only "epistemic values" in their judgments of whether there is sufficient evidence for accepting a hypothesis. According to Levi, scientists, by being scientists, submit themselves to scientific "canons of inference" which limit the scope of values for consideration:[16]

> When a scientist commits himself to certain "'scientific" standards of inference, he does, in a sense, commit himself to certain normative principles. He is obligated to accept the validity of certain types of inference and to deny the validity of others. . . . In other words, the canons of inference might require of each scientist *qua* scientist that he have the same attitudes, assign the same utilities, or take each mistake with the same degree of seriousness as every other scientist. . . . [It is not the case] that the scientist *qua* scientist makes no value judgment but that given his commitment to the canons of inference he need make no further value judgments in order to decide which hypotheses to accept and which to reject. (Levi 1960, 356)

The canons of inference limit the values scientists can and should use in evaluating whether to accept or reject hypotheses. If the sole goal of science is "to replace doubt by true belief," then "epistemic values" (such as simplicity, explanatory power, and scope) are sufficient for setting decision criteria for scientists, and scientists should not go beyond those values (Levi 1962, 49). Levi was deliberately severing the scientists from the broad social context of decisionmaking that concerned Rudner. Scientists, when using only the canons of inference, should not think about the potential social implications of their work or of potential errors, or consider social or ethical values in the acceptance or rejection of scientific theories. I will argue in chapter 5 that in many of the examples from science relevant to public policy, internal epistemic values are not helpful in weighing competing hypotheses. Nevertheless, in Levi's view, scientists need not and *should not* think beyond their own disciplinary boundaries when making judgments. The Rudner-Churchman arguments were rejected in favor of this value-free ideal. Levi's value-free ideal, that only epistemic values should be considered when doing scientific research, became the standard position for the next several decades.[17]

Embracing the Value-Free Ideal

Levi articulated a way in which philosophers of science could defend a value-free ideal for science. If philosophers of science adopted the ideal, they could more carefully demarcate the boundaries of the newly forming discipline of philosophy of science while insulating their nascent discipline from political pressures.[18] Yet philosophers of science did not immediately embrace the value-free ideal. In the early 1960s, two prominent philosophers, Ernst Nagel and Carl Hempel, discussed the role of values in science and seemed to acknowledge the import of the Rudner-Churchman argument. Their views on the role of values in science expressed some ambivalence toward Levi's value-free ideal. The ultimate acceptance of the value-free ideal rested more on the shifting attention of the young discipline than on a decisive conclusion to the 1950s debate.

Ambivalence over the Value-Free Ideal

In 1961, Ernst Nagel published *The Structure of Science: Problems in the Logic of Scientific Explanation*. As is evident by the title, the book fell squarely into the developing tradition of a more narrowly focused philosophy of science, which was concerned with the logic of explanation, the character of scientific laws, and the status of scientific theories. Toward the end of the book, Nagel addresses concerns arising from the social sciences. One of those concerns centers on the nature of value-based bias in social science, and whether it is an insurmountable problem for the social sciences (Nagel 1961, 485–502). Nagel also compares the issue of values in the social sciences with the issue of values in the natural sciences. In the end, he argues that none of the problems arising in the social sciences are truly peculiar to the social sciences.[19] Thus, his discussion reveals what he thought about values in science generally.

Nagel (1961) divided the potential places for the influence of values on science into four categories: "(1) the selection of problems, (2) the determination of the contents of conclusions, (3) the identification of fact, and (4) the assessment of evidence" (485). For all four possible roles, Nagel argues both that values often do play a role in science and that the issues for social science are no different than for natural science. The first role, values influencing the selection of problems, was a readily acknowledged and unproblematic place for values to influence science. Nagel notes that "the interests of the scientist determine what he selects for investigation," and suggests that "this fact, by itself, represents no obstacle to the successful pursuit of objectively controlled inquiry in any branch of study" (486–87). More problematic would be the second possible role, which Nagel describes as an inevitable infiltration of one's values into one's inquiry, even without one's awareness. To address this potential problem, Nagel presciently suggests, "The difficulties generated by scientific inquiry by unconscious bias and tacit value orientations are rarely overcome by devout resolutions to eliminate bias. They are usually overcome, often only gradually, through the self-corrective mechanisms of science as a social enterprise" (489).

In the 1990s and 2000s, philosophers such as Helen Longino focused on developing a philosophical account of these self-corrective social mechanisms.[20] At the time, Nagel found this issue to be equally problematic for both natural and social sciences. Similarly, for the third possible role, which Nagel explicated as whether values could be kept distinct from facts at all, he argued that this is a problem for both natural and social sciences and needs constant attention. Thus, neither of these two roles are presented as legitimate or laudable avenues for values in science (as the first role is), but as issues for vigilance.

The fourth role for values in science is central to our concerns here. Nagel (1961, 496–97) replayed the Rudner-Churchman arguments about the need for values in the assessment of the adequacy of evidence (although without citing Rudner or Churchman). After discussing the example of which kinds of errors should be more assiduously avoided in the testing of a promising drug for toxicity, Nagel noted that the appraisal of statistical testing could provide one with no fixed rule on such a question because it depends on the potential consequences of error in the particular context: "The main point to be noted in this analysis is that the rule presupposes certain appraising judgments of value. In short, if this result is generalized, statistical theory appears to support the thesis that value commitments enter decisively into the rules for assessing evidence for statistical hypotheses"

(Nagel 1961, 497). Nagel, however, is unsure how far such a result should be generalized. He continues: "However, the theoretical analysis upon which this thesis rests does not entail the conclusion that the rules actually employed in every social inquiry for assessing evidence necessarily involve some *special* commitments, i.e., commitments such as those mentioned in the above example, as distinct from those generally implicit in science as an enterprise aiming to achieve reliable knowledge" (497).

Here we have echoes of Levi's view, that scientists in many cases need not consider values beyond those that make up the "canons of inference" in scientific research, that is, epistemic values. But Nagel does not argue that scientists should only consider such epistemic values. Rather, he suggests that scientists often need not go beyond such considerations, not that they should not. This argument leaves open to the judgment of the scientist in a particular context which values are appropriate for the assessment of evidence. In the end, Nagel provided no strong endorsement of the value-free ideal in science. The scientist is not yet fully severed from the social context.

A similar ambivalence can be found in Carl Hempel's essay, "Science and Human Values" (1965b), which addresses all aspects of the relationship between science and values (first published in 1960, reprinted in 1965).[21] Much of the essay is devoted to the influence science can have on the values that we hold and the decisions we make.[22] Near the end of the essay, Hempel turns to the issue of what influence values should have on science, or "whether scientific knowledge and method presuppose valuation" (90). Hempel first rapidly narrows the scope of the question; he accepts the role of values in both the selection of questions to pursue and the choice of the scientist's career. He denies that values themselves can confirm scientific knowledge. However, the question of whether values play a role in scientific method, or in the doing of science, is a more complex issue.

In questioning whether values have a logical role to play in the doing of science, Hempel answers in the affirmative. In line with Rudner's argument, Hempel recognizes the need for values in the acceptance or rejection of hypotheses.[23] The need for values in science arises from the "inductive risk" inherent in science: "Such acceptance [of a scientific law] carries with it the 'inductive risk' that the presumptive law may not hold in full generality, and that future evidence may lead scientists to modify or abandon it" (Hempel 1965a, 92). Because the chance of error is always present, decision rules for the acceptance of scientific hypotheses are needed. And "the formulation of 'adequate' decision rules requires . . . specification of valuations that can then serve as standards of adequacy" (92). Adequate decision rules would consider the possible outcomes of accepting or rejecting a hypothesis, and these outcomes include the following four main possibilities: (1) the hypothesis is accepted as true and is, (2) the hypothesis is rejected as false and is, (3) the hypothesis is accepted as true but really is false, and (4) the hypothesis is rejected as false but really is true (ibid., 92). The problem of inductive risk lies in the latter two possibilities, where values must be used to decide the seriousness of error in those cases.

The issue then becomes what kind of values are needed to set up adequate rules of acceptance. For Hempel in the 1960s, the values depended on the case. For some situations, nonepistemic values would be required, particularly for the use of science in industrial quality control: "In the cases where the hypothesis under test, if accepted, is to be made the basis of a specific course of action, the possible outcomes may lead to success or failure of the intended practical application; in these cases, the values and disvalues at stake may well be expressible in terms of monetary gains and losses" (Hempel 1965a, 92–93).

Thus, under Hempel's model, social and ethical values, translated into a monetary or utility metric, can and sometimes do play an important role in scientific judgments. However, aside from such cases with clear applications, Hempel suggested that social and ethical values need not be considered. He saw most of scientific practice as being removed from practical implications, that is, as being "pure scientific research, where no practical applications are contemplated" (ibid., 93). Because of this, he argued that epistemic values usually suffice when making judgments about inductive risk in science. Yet social and ethical values do have a rational place in scientific reasoning for some areas of science, particularly those with clear practical import. In sum, Hempel and Nagel appear to have agreed on this view for the role of values in some science in the early 1960s.

In his later writings, Hempel occasionally struggled with the role of values in science. For example, in his "Turns in the Evolution of the Problem of Induction" from 1981, Hempel discusses the Rudner paper and Jeffrey's response explicitly (reprinted in Hempel 2001; see esp. 347–52). He notes that Jeffrey's response to Rudner rested on the view that scientists do not play a role in practical decisionmaking: "But, Jeffrey notes, the scientist *qua* scientist is not concerned with giving advice, or making decisions, on contemplated courses of action" (Hempel 2001, 348). However, Hempel ultimately disagreed with Jeffrey on this point (350). Despite apparently siding with Rudner in theory, in practice Hempel was primarily concerned with science with no practical import, with science solely concerned with "the increase of scientific knowledge" (350–51). Thus, he focused on the nature of epistemic values, trying to nail down the guidance they give (see also Hempel 2001, 372–95). In the practice of his work, Hempel had accepted the value-free ideal for science. But how did the philosophy of science community come to this tacit, and later explicit, endorsement of the value-free ideal from its apparent ambivalence in the early 1960s?

The Acceptance of the Value-Free Ideal

The shift from the acknowledgment of the need for social and ethical values in at least some scientific work, particularly work to be used for decision-making outside of science, to the value-free ideal for all of science required a crucial step: the viewing of the scientific community as demarcated and isolated from the surrounding society. Shortly after the first publication of Hempel's and Nagel's views on values in science, Thomas Kuhn's *The Structure of Scientific Revolutions* was published, the work that famously introduced the concept of scientific paradigms.[24] Although Kuhn historicized science in an exciting way, he also argued for the view that the scientific community was best understood as distinct and isolated from the surrounding society. In Kuhn's eyes such isolation was laudatory, and perhaps essential for the genuine character of science. When discussing how paradigms function in science, Kuhn (1962) notes that a "paradigm can . . . insulate the [scientific] community from those socially important problems not reducible to puzzle form" (37). A focus on solving puzzles—problems assumed to have a well-defined answer—is key to conducting normal science. Thus, insulation from socially important problems is crucial, in Kuhn's view, for the development of science. Paradigms, those defining frameworks for science, are to be local to particular sciences or disciplines, or at most to science itself, rather than representative of broad cultural trends. Paradigm shifts are driven by struggles internal to science. The demarcation between science and society is essential for science to be proper science.

In the closing pages of the book, Kuhn reemphasizes the importance of a science isolated from society. After discussing the nature of progress in science, Kuhn notes:

> Some of these [aspects of progress] are consequences of the unparalleled insulation of mature scientific communities from the demands of the laity and of everyday life. That insulation has never been complete—we are now discussing matters of degree. Nevertheless, there are no other professional communities in which individual creative work is so exclusively addressed to and evaluated by other members of the profession. . . . Just because he is working only for an audience of colleagues, an audience that shares his own values and beliefs, the scientists can take a single set of standards for granted. . . . Even more important, the 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. (164)

Kuhn goes on to argue that because of this insulation from the needs and values of society, natural (not social) scientists are more likely to be successful in solving their puzzles and problems, and thus insulation from society is key to the success of natural science. With such a demarcation, Kuhn's discussion of the workings of science considers the scientific community to be a self-contained community whose problems and debates are discussed and decided *within* that community.

It is hard to underestimate the impact of Kuhn's book on the philosophy of science. Many philosophers developed alternative views of scientific change, the history of science, and scientific progress to counter Kuhn's account. The issue of scientific realism picked up steam in part in reaction to Kuhn's seemingly antirealist view of the history of science. The need for more robust historical accounts in the philosophy of science became a central aspect of the field, leading to programs and departments in the history *and* philosophy of science. Kuhn challenged the prevailing views in philosophy of science in many ways, and his work shaped the field for years to come. As philosophers grappled with some aspects of Kuhn's work, others were left unchallenged, including Kuhn's isolated view of science, which may have been widely accepted in part because philosophers were already leaning toward that idea. The field of philosophy of science was greatly simplified and streamlined if science was thought of as cut off from the society in which it operated.

For the philosophers who wrote in the wake of Kuhn 1962, attention focused on problems conceptualized as wholly internal to science, such as the nature of scientific change, the realism debate, or problems internal to special areas of science. The issue of values in science came to be about the nature of epistemic values, and only epistemic values, in science. This can be seen in the later work of Hempel and the work of Larry Laudan, whose *Science and Values* (1984) is solely concerned with epistemic values.[25] Laudan acknowledges Kuhn's influence in this regard, noting that Kuhn was "ever a believer in a sharp demarcation between science and everything else" (xiii). This aspect of Kuhn was imitated by many of his readers, even as they criticized him for not having an adequately rational account of scientific change, or a rigorous enough definition of a paradigm. And it was this image of science, as demarcated from everything else, that was the final piece needed to cement the value-free ideal. For with Kuhn's isolated view of science in place, Levi's argument (1960) that scientists should not think beyond the canons of scientific inference took on the veneer of the obvious—of course scientists should only think about issues within the realm of science.

Not all philosophers of science accepted Kuhn's understanding of science. Challenges to an isolated view, through challenges to the value-free ideal, did occur occasionally.[26] Leach 1968 and Scriven 1974 are two notable examples.[27] But the arguments in these papers were largely ignored. A similar fate befell James Gaa's "Moral Autonomy and the Rationality of Science" (1977), which critiqued an isolated understanding of science. Gaa notes the importance of presupposing that science is isolated from the rest of society for the value-free ideal and describes moral autonomy as the view that "acceptance (and rejection) decisions ought to be made with regard only to the attainment of the characteristic ('purely epistemic') goals of science" (514). Despite its importance for philosophy of science and the value-free ideal, Gaa notes that "the moral autonomy of science is a largely unexamined dogma" (514). In his examination of this dogma, Gaa suggests that it is not obviously sound: "Even if science does have characteristic goals—goals to distinguish it from other kinds of activities—scientists ought, in their acceptance decisions, to consider more than the utility of a theory for those special goals" (525). In the end, Gaa found the arguments for such a moral autonomy (as much as there were any) unconvincing, particularly in the face of one possible conclusion resulting from moral autonomy, that "scientists can be both *rational* and *irresponsible (immoral)* at the same time" (536). This would occur because scientists would only consider Levi's internal canons of inference, and not the broader implications of their decisions for the society in which science functions. Such a view of scientific rationality was not acceptable to Gaa. Unfortunately, Gaa's detailed examination of this "dogma" went largely unnoticed and undiscussed.[28]

Despite Leach 1968, Scriven 1974, and Gaa 1977, and the rising importance of scientists in public life, philosophers of science continued to hold to the value-free ideal and the isolated view of science on which it rested. In 1982, when policymakers and scientists were struggling to find new ways to better incorporate science advice into the policy process, Ernan McMullin (1983) argued for the value-free ideal, rejecting Rudner's argument and echoing Jeffrey (1956) and Levi (1962):

> If theory is being applied to practical ends, and the theoretical alternatives carry with them outcomes of different value to the agents concerned, we have the typical decision-theoretic grid involving not only likelihood estimates but also "utilities" of one sort or another. Such utilities are irrelevant to theoretical science proper and the scientist is not called upon to make value-judgments in their regard as part of his scientific work. . . . The conclusion that Rudner draws from his analysis of hypothesis-acceptance is that "a science of ethics is a necessary requirement if science's happy progress toward objectivity is to be continuous." But scientists are (happily!) not called upon to "accept" hypotheses in the sense he is presupposing, and so his conclusion does not go through. (McMullin 1983, 8)[29]

Both the apparent obviousness of the scientist's removal from public decisionmaking and the sense of relief inherent in McMullin's comments are striking, particularly given the prevalence of scientists being called upon to give advice or to make decisions regarding which hypotheses are adequately supported in real and crucial contexts. Kuhn's isolated science was well entrenched in mainstream philosophy of science.

Even today, the value-free ideal continues to be the predominant view in the philosophy of science.[30] In Hugh Lacey's *Is Science Value-Free?* (1999), he argues strongly for the value-free ideal that he sees as being at the heart of science.[31] In order to defend the ideal from Rudner's arguments, he defines acceptance of a theory solely in terms of whether or not more research needs to be done on it, ignoring the multiple pragmatic aspects of acceptance, such as whether to use a theory as a basis for decisionmaking (Lacey 1999, 13–14).[32] Lacey sees the ideal as essential to science and raises substantial fears about what would happen if the ideal were rejected. Without the value-free ideal, Lacey warns, we lose "all prospects of gaining significant knowledge" (216). We would be thrown back on merely "wishing" the world to be a particular way or "the back and forth play of biases, with only power to settle the matter" (214–15). As I will argue in chapters 5 and 6, we can reject the value-free ideal without these dire consequences. However, Lacey is correct to worry about science being held under the sway of biases, or allowing wishing to make it so. We need another ideal and a more robust understanding of objectivity to go with it.

In sum, philosophy of science as a discipline has been profoundly shaped by the value-free ideal and still largely adheres to it. The depth of this commitment can be found in the lack of interest in serious critiques of the ideal, in the topics on which the field focuses its energies, and in the relief felt when the ideal can be left intact. Yet the ideal has not received sufficient scrutiny, particularly concerning whether it is an acceptable ideal. Gaa's concerns over the possibility for a rational but immoral science have not been addressed.

While these disputes might seem to be historically interesting only to philosophers of science, they also influenced the way in which scientists and policymakers understood the proper norms for science. This ideal tacitly or explicitly underlies the scientists' approach to their work. Concerns over bias, the role of advocacy in science, and the possibility of junk science all attest to the importance of the ideal for scientists as well as philosophers of science. For example, as noted in chapter 1, Kuhn's isolationist view of science plays a key conceptual role in Peter Huber's understanding of what constitutes a scientific fact, which then distinguishes sound science from junk science in Huber's account. Huber relies on Kuhn's view of the scientific community, concerning itself solely with epistemic values, to determine what is sufficiently reliable to be counted as a fact (Huber 1991, 226); only then does the isolated scientific community turn the "fact" over to society. However, this is not how science advising is structured, nor would it be desirable to have it so structured. Or consider how biologist Lewis Wolpert, in his *Unnatural Nature of Science*, reflects Levi's value-free ideal when he writes that "scientific theories may be judged in terms of their scope, parsimony—the fewer assumptions and laws the better—clarity, logical consistency, precision, testability, empirical support and fruitfulness," thus emphasizing the importance of epistemic values in science within the canons of scientific reasoning (Wolpert 1992, 17–18). Chemist Roald Hoffmann's "Why Buy That Theory?" (2003) also examines the list of classic epistemic values (simplicity, explanatory power, scope, and fruitfulness) as reasons to accept a theory, arguing that it is part of scientists' psychology to prefer theories with these attributes. His account reflects the classic Kuhn-Levi view that these values help scientists decide whether to accept a new theory in the arena of pure science. Admittedly, these pieces of evidence are at best suggestive; scientists rarely discuss the value-free ideal for science with the kind of precision found among philosophers of science, and so the influence of philosophy of science on these ideas is difficult to track. However, we do know that the value-free ideal appears to have influence beyond the philosophy of science.

Conclusion

The value-free ideal was not an obviously acceptable ideal at the beginning of the 1950s. Yet, even as Rudner and Churchman presented clear arguments for the importance of values in science, pressures to professionalize the young discipline of philosophy of science took hold. Arguments that scientists should only consider values internal to science were made. The idealized image of the isolated scientific community gained prominence, reinforced by Kuhn 1962. Although some criticisms of the value-free ideal have persisted, they have been ignored or marginalized by the philosophers of science. And the ideal has been influential among scientists as well as among philosophers of science.

The great irony of this history is that the ideal rests on a faulty presupposition, that science is isolated (or should be isolated) from the rest of society. The account in chapter 2 of scientists gaining ever more importance in the policy process belies the descriptive veracity of this presupposition. But despite the increased importance of science for society, one still might wonder whether science should aim to be value free. In other words, despite the deep ties and interrelationships between science and society, should scientists attempt to follow the value-free ideal, excluding consideration of the consequences of error in their work? Should they rely solely on the canons of inference internal to science? Should science be morally autonomous in this way? These are fundamentally questions concerning the moral responsibilities of scientists. It is to these questions that we will now turn.