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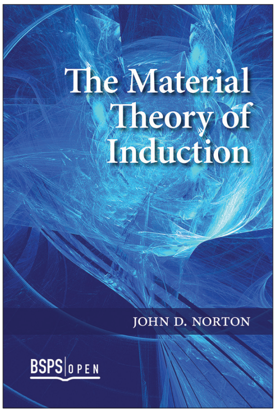

THE MATERIAL THEORY OF INDUCTION

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Epistemic Virtues and Epistemic Values: A Skeptical Critique[1]

5.1. Introduction

Epistemic virtues or epistemic values, we are told, play a major role in our assessments of evidence in science. There is something quite right about this notion; and there is something quite wrong about it. My goal in this chapter is to explain each.

In brief, what is right about the notion of epistemic virtue or value is that criteria such as simplicity and explanatory power do indeed figure overtly in the evidential assessments made by scientists. Any comprehensive account of inductive inference must have a place for them. A material theory of induction accommodates them by treating them as surrogates for further background facts that ultimately do the epistemic work.

What is wrong about the notion is the words used to express it. The problem is simple enough to be described here fully at the outset. The terms “virtue” and “value” have prior meanings and rich connotations. These prior meanings conflict with the idea that the criteria they label are successful epistemically—that is, that they do guide us closer to the truth. Unless we erase these prior meanings and connotations, we tacitly adopt a form of skeptical relativism about inductive inference. More specifically, when we use the terms in this context, we place the criteria on the wrong side of two distinctions—that is, on the side that indicates that the criteria do not serve their epistemic purpose.

1 I thank Heather Douglas for helpful discussion that informed this chapter.

The first distinction is between means and ends. In a non-skeptical view, the goal of inductive inference in science is to get closer to the truth. The criteria that guide us are *means* to this end. Values and virtues are commonly understood to be things that we esteem in their own right. They are *ends*. If we now label the criteria as ends, we are tacitly discounting their function as means. We are, in effect, indicating that scientists prize simplicity for simplicity's sake, thereby overlooking that simplicity is sought in an epistemic context as an intermediate that, we hope, brings us closer to the truth.

The second distinction is between things that are *imposed* by outside conditions on a community versus those that a community freely *chooses* for itself. Criteria that guide a community toward true theories cannot be freely chosen, or at least they cannot be freely chosen if they are to be successful guides. The world constrains powerfully which criteria succeed. If we choose guides that breach these constraints, we will be guided poorly. We should not rely on the reading of entrails or astrological signs as guides to the truth, for our world is not such that they succeed. If we choose guides that are better adapted to the world, we will enjoy the success of modern science. If one holds that such criteria can be freely chosen, one forfeits the difficult and delicate adjustment of the criteria to the world that is needed if they are to be successful guides to truth. This is the view of a skeptic, much as skeptics about astrology believe that astrologers can freely choose the predictive significance of each star sign, for these skeptics hold that no choice leads to a successful prediction.

Facts are traditionally distinguished from values. We may not know what the facts of the matter are in a particular case, but a factual claim is either true or false; it cannot be both. And if two people disagree on a factual claim, then at least one of them is wrong. It is not so with values. (And the same may be said of values that underwrite our judgment of what is virtuous). Two people can legitimately hold contradicting values. There is no corresponding necessity that at least one of them is wrong. They choose their values as they please and, while each may try to argue for the superiority of his or her values, ultimately they can legitimately agree to disagree.

When we label criteria for theory choice as "values" or "virtues," the choice of language connotes that they are freely chosen. This is incompatible with the idea that the criteria are successful, for whether a criterion is successful is not a matter of our choice. It is imposed by the world, and the successful criteria are to be discovered or inferred from suitable analysis, not stipulated as conventional choices. In this second way, the terms "value" and "virtue" for the criteria conveys the skeptical view.

In the following, Section 5.2 reviews a standard and celebrated instance of the use of epistemic values: the supplanting of geocentric by heliocentric astronomy. Section 5.3 describes how the material theory of induction can accommodate inductive inferences in which epistemic values or virtues are invoked. These values, the theory asserts, are convenient surrogates for more complicated background facts that provide the warrant for the inferences. A common way that epistemic virtues enter into scientific discourse is reviewed in Section 5.4. Bare hypothetico-deductive confirmation is too permissive in how it accords evidential support. Demanding in addition the presence of certain epistemic virtues provides a way of restricting its permissive scope.

Section 5.5 looks at an early instance of the present confusion over values in philosophy of science. In 1953, Richard Rudner advanced an influential argument, summed up in the title of his paper "The Scientist *Qua* Scientist Makes Value Judgments." I respond that Rudner's paper establishes no such thing. It shows only something few doubt: that scientists *qua* members of society make ethical value judgments. Finally, Section 5.6 turns to Thomas Kuhn's highly influential 1973 Matchette Lecture, "Objectivity, Value Judgment, and Theory Choice." In it, Kuhn laments that his critics have misread his writings as espousing a radical skepticism about the rational grounding of science. While he promises to set the record straight, Kuhn proceeds with an account that invites the same criticism. Kuhn's paper introduces characteristics used in theory choice and soon redescribes them misleadingly as values. The narrative focuses on such questions as how different scientists may assign different weights to different values when those values compete. Whether and how these values might be truth conducive in theory choice, however, is never addressed.

5.2. The Classic Example: Ptolemy versus Copernicus

A celebrated example has long figured prominently in the epistemic virtues literature. In the sixteenth and early seventeenth century, astronomers were weighing competing celestial systems. Should they follow the traditional geocentric system of Ptolemy? In this system, the sun, moon, and planets were held to orbit the earth in motions that were compounds of circular motions. Or should they follow the heliocentric system of Copernicus? In this view, the earth with its orbiting moon was like the planets. All orbit the sun.

Both systems were quite successful at the routine task of astronomy of predicting just when each celestial body would appear in each place in the sky. This purely descriptive task is known as "saving the appearances" or "saving the phenomena." Since the Copernican account was constructed from more recent observations, it fared somewhat better at this task. However, it was well within the reach of Ptolemaic methods to equal it, if only some Ptolemaic astronomer was willing to put the effort into tinkering with the system.

The decision between the systems was made on other grounds. There were competing considerations. The difficulty with the Copernican hypothesis was making physical sense of an earth that was supposed to be careening through the heavens. The great appeal of the Copernican system was that it qualitatively simplified Ptolemy's system. The Copernican system acknowledged that our view of the planets came from a moving platform that takes one year to return to the same spot. Our moving vantage point gives the illusion of further circular motions by the planets. Since these illusory motions resulted from a single origin, the motion of our vantage point, the illusory motions are highly correlated. Crudely put, the planets appear to wobble in synchrony because we view them from a wobbling platform. With this insight, Copernicans could then identify certain correlated motions within the Ptolemaic system as being just these projections. The projections could be separated from the true motions of the planets themselves. This gave the Copernicans a powerful advantage, for they could explain the coordination among these motions as necessities of a heliocentric system, whereas Ptolemaic astronomers could only ascribe them to arbitrary coincidences within the geocentric system.

The greater simplicity and harmony of the Copernican system carried the day. The victory depended on a strong appeal to aesthetic sensibilities. This is reflected in Copernicus' own dim assessment of the geocentric system in his Preface to *On the Revolutions of the Heavenly Spheres*:

> [The geocentric astronomers'] experience was just like some one taking from various places hands, feet, a head, and other pieces, very well depicted, it may be, but not for the representation of a single person; since these fragments would not belong to one another at all, a monster rather than a man would be put together from them. ([1543] 1992, p. 4)

Later in the Preface, Copernicus exults over the harmony of his system, listing how coincidences of the Ptolemaic system are explained by his system:[2]

> In this arrangement, therefore, we discover a marvelous symmetry of the universe, and an established harmonious linkage between the motion of the spheres and their size, such as can be found in no other way. For this permits a not inattentive student to perceive why the forward and backward arcs appear greater in Jupiter than in Saturn and smaller than in Mars, and on the other hand greater in Venus than in Mercury. This reversal in direction appears more frequently in Saturn than in Jupiter, and also more rarely in Mars and Venus than in Mercury. Moreover, when Saturn, Jupiter, and Mars rise at sunset, they are nearer to the earth than when they set in the evening or appear at a later hour. But Mars in particular, when it shines all night, seems to equal Jupiter in size, being distinguished only by its reddish color. Yet in the other configurations it is found barely among the stars of the second magnitude, being recognized by those who track it with assiduous observations.

2 For an account of how Copernicus understood notions of harmony and symmetry in this context, see Goldstein and Hon (2008, chap. 5).

> All these phenomena proceed from the same cause, which is in the earth's motion. ([1543] 1992, p. 9)

We are to be repulsed by the monstrous Ptolemaic system and captivated by the harmony of its heliocentric competitor. While each can in principle perform equally well at saving the appearances, it is these aesthetic considerations, Copernicus argued, that should lead us to favor his system.

In so far as we characterize these factors as aesthetic, they are vague. Beauty, as the popular saying goes, is in the eye of the beholder. There are many ways that the Copernican system might be said to be aesthetically superior. It may merely be simpler in requiring fewer independent hypotheses. Or we may judge the heliocentric system to be more harmonious in locating the centers of more of the gross motions in the sun. Here, we understand harmony as appealing to some sense of beauty, perhaps captured in some aesthetic of parsimony or perfection of balancing parts. Or we may judge the superiority to lie in the way the systems relate to the evidence supplied by the celestial appearances. While both systems save the appearances, the Copernican system does a better job of this. It attributes certain coordinated motions in the appearances of all planetary motions to the singular cause of our earth's motion. Or we may judge the Copernican system to be better tested by the appearances. For the apparent motion of one planet will enable us to fix our earth's motion. We must then find this motion reflected in the apparent motions of the other planets on pain of refuting the Copernican hypothesis.

Whichever account of the superiority of the Copernican system we choose, this superiority is expressed in the same general way. The Copernican system in its relation to the evidence of the appearances is more virtuous than the Ptolemaic. The virtue is of a special type. It is epistemically potent. The system that possesses the epistemic virtue is better supported by the evidence.

5.3. Epistemic Virtues and the Material Theory of Induction

How can the possession of these virtues be epistemically potent and strengthen the inductive support provided by evidence? This is the principal question to be addressed here. Are we to seek some general principle of inductive logic that affirms greater inductive support to simpler, more harmonious hypotheses that explain better or enter into relations of overdetermination?

The material theory takes a quite different approach. It allows that some such principles work more-or-less well in some domains. But any such principle will always have a limited scope, and eventually we shall pass beyond its domain of applicability to examples where it fails. The material theory dictates that there can be one answer to the question of the origin of its epistemic power. Ultimately, the properties that are commonly called epistemic virtues must be surrogates for background facts or assumptions. They provide the warrant for the inductive inference.

Below, I will try to locate a little more precisely how these properties can enter into accounts of inductive inference. In the next chapter, I will give a more detailed analysis of one of the best-known properties, simplicity, and I will show how its inductive power—in so far as it has any—derives from its role as a surrogate for background facts or assumptions.

5.4. Repairing Hypothetico-Deductive Confirmation

There are no universal rules for inductive inference. Correspondingly, there are no universal rules governing the nature of the properties often called epistemic virtues and how they enter into evidential relations. But there are broad and common circumstances in which these properties play a reasonably well-defined role. They arise as part of efforts to repair an excessively permissive account of inductive inference, namely hypothetico-deductive confirmation.

In this account of confirmation, we have cases of hypotheses, hypotheses with auxiliary assumptions or theories that deductively entail certain evidential statements. The truth of these evidential statements is then taken to support the hypotheses that entailed them. The idea is familiar and examples abound. Big Bang cosmology predicts a three-degree-kelvin cosmic background radiation as a residual of the inferno of the early universe. Starting with celebrated measurements by Arno Penzias and Robert Wilson in 1965, the existence of this thermal background radiation was confirmed and eventually judged to provide strong evidence for Big Bang cosmology.

This bare account has had a troubled history. Both geocentric and heliocentric systems can do a good job of entailing the observed motions of celestial objects. This means that they save the phenomena. Whether this provided evidence of their respective systems' truth was the divisive issue of the sixteenth and early seventeenth century. In the most famous case of forgery known to science, Copernicus' publisher Osiander introduced a spurious preface to Copernicus' celebrated work in 1543. There, Osiander argued that Copernicus' hypotheses "need not be true nor even probable"; they "merely provide a reliable basis for computation," which means that they should be regarded as nothing more than a reliable means for astronomers to predict the observable motions of celestial objects. He provided a quite powerful argument against reading truth into the hypotheses that saved the phenomena. It was an elementary fact of the astronomy of his time that two different constructions could yield the same observable motions. He gave the widely known example of the equivalence of an eccentric circle and a suitably designed deferent-epicycle. Successfully saving the phenomena would favor each equally so that pragmatic considerations directed the choice of construction: "the astronomer will take as his first choice that hypothesis which is the easiest to grasp" ([1543], 1992, p. xvi).

The difficulties for this bare notion of hypothetico-deductive confirmation remain today. We see them most easily through the following consideration. Let *A* and *B* represent two propositions whose truths are quite independent of one another. One gets a good approximation of this condition by drawing the propositions from widely different domains. Proposition *A* may be drawn from astronomy, for example, and *B* may be some proposition in economics. We can form the following deductive inference:

Hypothesis: *A* and *B*

Evidence: *A*

The hypothesis deductively entails the evidence. But does the truth of the evidence now supply inductive support to the hypothesis as the hypothetico-deductive scheme indicates? Clearly the hypothesis (*A* and *B*) gets no inductive support from the evidence *A* beyond the simple fact that *A* is itself a logical part of the hypothesis. For the hypothesis to gain inductive support from the truth of the evidence in the sense intended by the hypothetico-deductive scheme, the support of the evidence *A* for itself as a logical part of the hypothesis would somehow have to carry over to the other logical part of the hypothesis *B*. There is no connection that carries the support from *A* to *B* since the two are, by supposition, independent.

In cases of this type, the hypothetic-deductive scheme fails completely. But what about the cases in which it does work? They will be distinguished by the obtaining of further conditions that provide a bridge between *A* and *B* over which the inductive support can pass. The display of properties often called epistemic virtues provides a way of showing that these further conditions hold. Merely saving the phenomena—merely entailing true observations—is not enough. It must be done the right way. We have already seen in the example of Copernican astronomy that there are many ways of characterizing just what the right way may be. We may look to special properties of the hypotheses themselves, which may be simple or harmonious. More realistically, we may compare properties. Of two hypotheses equally able to save the phenomena, we accord more support to the simpler or more harmonious one. Alternatively, we may identify a property of the relation between the hypothesis and the evidence. An explanatory relation is highly prized, and the better the evidence is explained the more support accrues to the explainer.

Conversely, we may find some relations defective. Such is the case with ad hoc hypotheses specifically contrived to conform to the evidence. This means that they get no inductive support from it. In early 1916, Einstein had completed his general theory of relativity, and, in a review article on his new theory, Einstein accused his predecessor, Newton, of just such ad hocery. Newton's theory distinguishes inertial motions from non-inertial motions. In Einstein 's view, it provides no causal account of the difference. Rather, the distinction is simply posited by declaring a preferred "Galilean space" in which an inertially moving body is at rest. As he put it, "The preferred Galilean space … is however a merely ad hoc cause and not an observable thing" (1916, p. 771). Einstein promised that his new theory would provide the observable cause. The distribution of observable masses would determine which were the inertial, Galilean spaces.

5.5. Non-Epistemic Values

So far, I have identified how the properties often called epistemic values and virtues can have a role in inductive inference. This is the part that the epistemic values literature gets right. I now pass to the part it gets wrong. I have already outlined the troubles in the opening of this chapter: the terms "virtue" and "value" introduce a covert skepticism about inductive inference through their prior meanings and connotations. Here, I identify the work of Thomas Kuhn as most responsible for the present misidentification of epistemic criteria. He was aided in establishing the misidentification, I believe, by an earlier tradition in philosophy of science. That earlier tradition challenged the standard notion that scientific practice was free of value judgments, where the values at issue were of the more familiar ethical type, such as the valuing of human life.

In 1953, Richard Rudner published an article in the journal *Philosophy of Science*, of which he would later become editor-in-chief, whose title and main claim were that "The Scientist *Qua* Scientist Makes Value Judgments." Rudner's argument maintained a distinction between the strength of evidential support for some hypothesis and the decision by some scientist to accept it. Values did not enter into the determination of the strength of support; they entered into the decision to accept the hypothesis. He wrote:

> In accepting a hypothesis the scientist must make the decision that the evidence is *sufficiently* strong, or 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 typical 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.* (1953, p. 2; emphasis in original)

While Rudner did not explicitly delineate the sort of values he had in mind, he introduced two examples that clarified them. In his first example, he suggested that our values could slow our acceptance of a hypothesis that a drug was free of a lethal contaminant, since an error would have fatal consequences. In the second example, he wondered correspondingly how high a probability the scientists of the Manhattan project would need to accept that the detonation of the first atom bomb would not trigger a planet-destroying chain reaction.

Rudner's analysis is at best exaggerated and at worst dependent on an equivocation.[3] There are two problems. First, and less seriously, the type of ethical value judgments Rudner describes are rarely made in scientific practice. The types of hypotheses assessed by scientists are overwhelmingly mundane and bereft of dire apparent human import. Decisions over lethal contaminants in drugs and, especially, planet-destroying chain reactions are uncommon. In the latter case especially, the hypothesis of a dire chain reaction could only arise after scientists over many decades had accepted a plethora of hypotheses in quantum theory, chemistry, and engineering, all remote from the ethically fraught hypothesis. In these and many other cases, the scientists could not anticipate the long-term consequences of their discoveries. When Niels Bohr presented his 1913 model of the atom, which played a foundational role in the development of modern quantum theory, was he to anticipate that this theory would ground the development of nuclear fission bombs two decades later and, as a result, alter his threshold of acceptance?

To claim that the "scientist *qua* scientist" makes value judgments admits no gradation. It makes no distinction between the scientist, for whom fraught ethical value judgments are rare and challenging moments, and the judge in a court of law whose day-to-day work requires ethical value judgments routinely. At best, Rudner established that, on rarer occasions, scientists make ethical value judgments in their work.

The second problem is more serious. It pertains to this last conclusion. Rudner's argument equivocates on the term "scientist." There is a narrower and a broader sense. In the narrower sense, a scientist is merely someone who investigates nature, reporting what bearing the evidence

3 For a more extensive analysis of the weaknesses of Rudner's argument, see Levi (1960).

has, with indifference to the broader human ramifications. Virtually all the work of scientists proceeds in this mode. Scientists find strong support in the evidence for the hypothesis that electrons are spin-half particles. In agreement with Rudner's supposition, ethical value judgments do not enter into the assessment of how strongly evidence supports the hypothesis. The hypothesis is accepted, and this is done without any consideration of the human import of the hypothesis, for none is apparent. This work is the province of the scientist in this narrower sense. It requires no ethical value judgments to be made.

This narrowness continues when scientists evaluate hypotheses that may have human import, such as Rudner's examples that a particular preparation procedure produces a contaminant-free drug or that an atom bomb will not trigger a planet-destroying chain reaction. Mere acceptance of hypotheses like these will not have any human import. The import only arises when the acceptance of the hypothesis will lead to consequences in the larger society. The scientist may need to decide whether to endorse the procedure in a published manual of procedures for drug preparation. Or the scientist may need to advise the principals of the Manhattan Project on the dangers of their planned Alamogordo atom bomb test.

That is, the human import only arises when the scientist has ceased to act as a scientist in the narrower sense. The scientist is now acting in the broader sense of someone who practices science and monitors the import of his or her work within the wider human society. When operating in this broader sense, scientists should be aware of the human consequences of their actions, and they should moderate their actions accordingly. In this broader sense, scientists make ethical value judgments in many ways that pertain to their engagement with the larger society. Who do they hire to work in their lab? Who do they fire? Are the safety precautions and procedures in the lab adequate to protect the lab staff? Should they purchase cheap, possibly stolen materials? Should the discharge from their lab be allowed to contaminate a nearby stream?

That ethical quandaries arise for scientists is a direct result of the broader role taken by scientists. It is not specifically a result of their doing scientific work. It is a result of their doing something, whether science or not, that impinges on the broader society.

Hence, Rudner simply got it wrong. Scientists *qua* scientists do not make ethical value judgments. Scientists *qua* members of society make ethical value judgments.

5.6. Kuhn's Obfuscation

While Rudner may have equivocated on the term "scientist," he is not responsible for the conflation of epistemic criteria with values. This distinction belongs to Thomas Kuhn. His 1973 Matchette Lecture "Objectivity, Value Judgment, and Theory Choice" launched the present popularity of the broadened scope of values talk in the philosophy of science.

The origins of the lecture lie in Kuhn's earlier, wildly successful book *The Structure of Scientific Revolutions*. This work brought us the notion that revolutions in science are akin to religious conversions and that they carry us between paradigms that are incommensurable, defying rational comparison. The attempts to compare paradigms rationally become circular since the means of rational evaluation, Kuhn assured us, resides within one or other paradigm. As a result, we are told that "paradigm choice can never be unequivocally settled by logic and experiment alone," and that "as in political revolutions, so in paradigm choice—there is no standard higher than the assent of the relevant community" (1970, p. 94).

These are strong claims sure to raise the hackles of anyone who sees science as aspiring to rationally grounded discoveries about the world. The world does not adopt some state merely because some community agrees it has. Yet Kuhn has declared communal assent to be the highest standard, which means it cannot be overruled by logic and experiment. Curiously, Kuhn (1973, p. 321) professed to be dismayed by critics whom he quoted as accusing him of making theory choice "a matter of mob psychology." This last description is at worst a colorful overstatement of the view Kuhn expressed in *The Structure of Scientific Revolutions* in the academically muted "no standard higher than the assent of the relevant community." Kuhn (1973, p. 321) responded in the Matchette Lecture that these assessments of his views "manifest total misunderstanding." He will set the record straight.

This is a reassuring start. His celebrated book, it seems, did not state clearly what Kuhn really thought about theory choice. Since many of its skeptical assertions were unequivocal, we must assume that he did not mean literally what he said. Or perhaps he expressed his views in a misleading way that invited misinterpretation. We can now learn what he really meant. Perhaps he merely meant that communal assent follows when one paradigm is favored over another according to some epistemically sound criteria. The superiority consists in conformity to these rationally grounded criteria and not in communal assent. Rather, we are to suppose that the relevant community is sufficiently astute to recognize this conformity so that we outsiders can use their assent as a reliable indicator of the superior choice. This is one possible clarification that would escape the charge of relativism. We are ready for some such clarification.

What followed in the Matchette Lecture, however, was simply a repeat of what was wrong in *The Structure of Scientific Revolutions*. Someone expecting an account of the rational basis of theory choice in science finds nothing of the sort.

The account begins with a non-exhaustive list of the characteristics that "provide *the* shared basis for theory choice" (p. 322; emphasis in original). The list comprises accuracy, consistency, scope, simplicity, and fruitfulness. It is not hard to give an account of how these characteristics can be rationally grounded. Consistency is the easiest. If a theory fails to have it—that is, if it is an inconsistent theory—then at least some of its propositions must be false. If we seek truth, we should avoid inconsistency. Accuracy refers to agreement between the consequences of the theory and the results of observation and experiment. This characteristic shows conformity of theory with known facts and, clearly, the better that conformity the better the facts weigh in the theory's favor. The remaining characteristics are not so straightforward but are certainly within the compass of further analysis. The following chapter, for example, treats simplicity from the perspective of a material theory of induction.

Simple affirmations of this type would preclude the impending misunderstanding that Kuhn holds these characteristics to be merely the preferences of some particular group of people at some time in history. Yet no such affirmations are made. Rather, the text moves as rapidly as possible to the question of how scientists weigh the force of the different criteria when they conflict and, eventually, how they change over time. We are only five pages into the lecture when we find a lengthy treatment of how individual differences between scientists have to be considered to explain why different scientists may weigh the criteria differently. It is a curious development in an account that is supposed to display that Kuhn does not hold the skeptical relativism of which he is accused. A simple answer to the accusation is to explain why he thinks these criteria are good guides to the truth after all. Instead, the focus shifts to the flaws and weaknesses of the criteria and how other, extra-rational factors are needed.

A charitable reader may still imagine that Kuhn's criteria form the basis of a rationally grounded system and not merely the predilections of some group. Perhaps Kuhn found the point too obvious to mention. This charity is hard to maintain. Some ten pages into the article (p. 330), what were initially labeled "characteristics" or "criteria" are relabeled "values" or "norms." The transformation is not benign. It is justified by the specious claim that "the criteria of choice with which I [Kuhn] began function not as rules, which determine choice, but values, which influence it" (p. 331). The term criteria is quite properly used to label factors that only influence a choice, and it is a better term to use in so far as it is free of the tendentious connotations of "value." As I noted earlier, the connotations of the terms "value" and "norm" contradict the idea that Kuhn's criteria are the basis of a rationally grounded account of theory choice

First, there is the distinction between means and ends. A characteristic can readily be understood as an intermediate in a fuller account. Selecting for it can be a means to some other end, such as getting closer to the truth. The term value has different connotations. It is normally understood to designate something valued in its own right. It is itself an end or a goal. When theory choice is described as a "value judgment," as in the paper's title, the normal understanding is that the choice is made to realize the values in question as an end. In effect, we are told that we seek consistent or simple theories because we value consistency and simplicity as an end and not because we regard them as an intermediate means for getting closer to the truth.

Second, there is the distinction between that which is imposed on the community by the outside world and that which is chosen freely by the community. In calling the criteria "values," Kuhn indicates that they are of the second type. For we are not forced by reason alone to the values we adopt. We choose them and enjoy considerable freedom in the selection.

In foreign policy, we may debate whether to go to war. The debate becomes irresolvable when we find that the debating parties proceed from different values. The pacifists, we find, base their view on the value judgment that killing is wrong in all circumstances. The militarists make a value judgment that some killing is warranted to preserve sovereignty. We can debate the facts and expect agreement from reasonable people. But if we differ in our values, we have arrived at an irresolvable end. Analogously, if our theories are guided by values that we can choose freely, then debates over the correct choice is correspondingly futile. There is no right choice. This contradicts the idea that these criteria are epistemically successful, for the successful criteria must be discovered. They cannot be chosen as communal conventions.

When Kuhn relabels the characteristics or criteria as "values" and, occasionally, "norms," he is inviting the simple confusion that he thinks they are free choices of a community and sought as worthy ends in themselves, much as these communities may choose to value life, liberty, self-sacrifice, compassion, or the ability to play football well. Kuhn's examples of values do nothing to dispel the confusion. He writes, "improving the quality of life is a value," and he adds, "freedom of speech is a value, but so is preservation of life and property" (p. 330).[4] Each of these is readily identifiable as an end that may be freely chosen. A dour religious sect that values deprivation and suffering will not value the improvement of quality of life; and they may also be indifferent to the preservation of both life and property. For they believe better awaits in the world to come. A highly authoritarian society may not value freedom of speech, since they regard it as contravening their values of obedience and respect of authority. Lest Kuhn leave any doubt that others may choose different values, the paragraph ends with the remark that most of us have "an acute consciousness that there are societies with other values and that these value differences result in other ways of life, other decisions about what may and may not be done" (p. 331).

This freedom of choice in our values conforms with the troublesome assertion in *The Structure of Scientific Revolutions*: "As in political revolutions, so in paradigm choice—there is no standard higher than the assent of the relevant community." The language mirrors Rudner's tendentious

4 Kuhn offers these examples as part of a discussion of how values may conflict.

claim of the role of social values in theory acceptance. In both cases, "values" determine what the scientists accept. The supposed misunderstanding of Kuhn's book is invited again.

Is it too much to ask for Kuhn to answer the accusation of skeptical relativism by giving the rational grounding of his criteria? He suggests that it is too great a demand. He dismisses the search for an "algorithm" that could determine theory choice as "a not quite attainable ideal" (p. 326). What of the extraordinary power of science to "repeatedly produc[e] powerful new techniques for prediction and control"? Kuhn replies: "To that question, I have no answer at all, but that is only another way of saying that I make no claim to have solved the problem of induction" (p. 332).[5] Here, Kuhn seeks to escape the burden of displaying an account of the rationality of theory choice that shows how its choices guide us closer to the truth. He seeks to escape it with a dilemma: either give an algorithm for theory choice and solve the problem of induction or give nothing at all. It is a false dilemma. There is a path between its horns. One can seek to show that the criteria he lists are conducive to the truth at least in some cases. This can be done without providing an algorithm for theory choice or without solving the problem of induction. The criterion of consistency, as I remarked above, is easy. Lose consistency and we know we are farther from the truth. I will argue in the next chapter that the criterion of simplicity is really a surrogate for specific facts that do guide us well, locally.

In sum, what are we to make of Kuhn's Matchette Lecture? As far as I can see, it is a muddled paper by a well-meaning but confused scholar. He has failed to see that his notion of rationality is a radically skeptical one, and he is irked and baffled when his critics point it out to him. If that were all there was at issue, the paper would be best left and forgotten. However, that is not all there is. This paper has since become the *locus classicus* of a new literature on values in science. It has legitimated the mislabeling of the criteria for theory choice as "epistemic values" or "epistemic virtues." There is a banal fact that scientists use criteria in choosing among theories. That banality is now redescribed in language whose connotations convey

5 Also Kuhn writes: "Though the experience of scientists provides no philosophical justification for the values they deploy (such justification would solve the problem of induction), those values are in part learned from that experience and they evolve with it" (p. 335).

a skepticism about the rational grounding of those choices. There is no treatment of how these criteria might bring us closer to the truth or even mention that they do so. Rather, theories are chosen because scientists value consistency and simplicity, much as a religious body might value piety.

The effect is to group together the use of these benign criteria with Rudner's tendentious claim that scientists *qua* scientists make ethical value judgments. The blurring of the distinction between criteria and values invites a fallacy. Scientists do use criteria like consistency and simplicity in theory choice. Misdescribe this banality as scientists choosing theories by value judgments, and we appear to have established that values permeate the apparently value-neutral content of scientific theories. This rhetorical subterfuge, whether intentional or not, is avoided simply by reverting to the neutral language of "criterion" and "characteristic."

The confusions and conflations of Kuhn's Matchette Lecture have exercised considerable influence. They were endorsed by the otherwise astute President of the Philosophy of Science Association Ernan McMullin in his Presidential Address.[6] McMullin argued that the epistemic criteria at issue really were values. He based this extraordinary conclusion on the same fragile grounds as Kuhn: they influence but do not determine the outcome. McMullin wrote,

> These criteria clearly operate as *values* do, so that the theory choice is basically a matter of value-judgment. Kuhn puts it this way:
>
> > The criteria of [theory] choice function not as rules, which determine choice, but as values which influence. Two men deeply committed to the same values may nevertheless, in particular situations, make different choices, as in fact they do. (1982, p. 16)

While criteria may be like rules in so far as they influence but do not determine outcomes, they are unlike values in the two senses I have outlined:

6 McMullin was President in 1981–82. Kuhn was himself later President in 1989–90.

criteria are means, not ends; criteria are imposed, not chosen. Their relabeling as values is unsupportable.

McMullin persisted, designating "epistemic values" as those "which are presumed to promote the truth-like character of science" (p. 18). They are distinguished from non-epistemic values, such as the political, moral, social, and religious. It is encouraging that the distinction appears to be maintained cleanly. Epistemic values are distinguished as those whose choice is "likely to improve the *epistemic* status of the theory, that is, the conformity between theory and world" (p. 19; emphasis in original). This is a serviceable standard for delineating epistemic criteria, however they are named. Yet such caution is ineffective when the distinction is ridden over, rough shod, by such claims as "Value judgment permeates the work of science as a whole" (p. 18).[7]

Finally, one may object that the issue is merely one of connotation and that, after Kuhn, the terms "value" and "virtue" have lost the connotations that trouble me. If that is so, why not revert to the neutral language? This reversion would, no doubt, be resisted. For it would break the connection between the provocative but mistaken role for values in science supposed by Rudner and the benign but common role for criteria like consistency in theory choice. The literature in "science and values" would become the heterogeneous literature in "science, criteria for theory choice and ethical values" and Kuhn's paper, "Objectivity, Value Judgment, and Theory Choice," would become "Objectivity, Criteria-Based Judgment, and Theory Choice." The misleading connotations do persist and do matter.

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7 For completeness, I note that the concluding Section 6 of McMullin's paper is devoted to arguing that the objectivity of science can be defended from the relativism suggested by its permeation with values. Would the section have been needed had he merely retained the neutral term "epistemic criteria" thereby erasing the epistemically deleterious connotations of the term "value"?

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