http://www.jstor.org

Values In Science

Ernan McMullin

University of Notre Dame

Thirty years ago, Richard Rudner argued in a brief essay in Philoso-phy of Science that the making of value-judgements is an essential part of the work of science. He fully realized how repugnant such a claim would be to the positivist orthodoxy of the day, so repugnant indeed that its acceptance (he prophesied) would bring about "a first-order crisis in science and methodology" (1953, p. 6). Carnap, in particular, had been emphatic in excluding values from any role in science proper. His theory of meaning had led him to conclude that "the objective valid-ity of a value...cannot be asserted in a meaningful statement at all" (1932/1959, p. 77). The contrast between science, the paradigm of mean-ing, and all forms of value-judgement could scarcely have been more sharply drawn: "it is altogether impossible to make a statement that expresses a value-judgement." No wonder, then, that Rudner's thesis seemed so shocking.

Thirty years later, the claim that science is value-laden might no longer even seem controversial, among philosophers of science, at least, who have become accustomed to seeing the pillars of positivism fall, one by one. One might even characterize the recent deep shifts in theory of science as consequences (many of them, at least) of the growing realiza-tion of the part played by value-judgement in scientific work. If this way of describing the Kuhnian "revolution" seems unfamiliar, it is no doubt due in part to the uneasiness that the ambiguity of the terms 'value' and 'value-judgement' still engenders. There are other ways of describing what has happened since the 1950's in philosophy of science that do not require so much preliminary ground-clearing.

Nevertheless, I shall try to show that the watershed between "classic" philosophy of science (by this meaning, not just logical posi-tivism but the logicist tradition in theory of science stretching back through Kant and Descartes to Aristotle) and the "new" philosophy of science can best be understood by analyzing the change in our perception of the role played by values in science. I shall begin with some

---

general remarks about the nature of value, go on to explore some of the historical sources for the claim that judgement in science is value-laden, and conclude by reflecting on the implications of this claim for traditional views of the objectivity of scientific knowledge-claims. I will not address the problem of the social sciences, where these issues take on an added complexity. They are, as we shall soon see, already complicated enough in the context of the natural sciences.

1. The Anatomy of Value

'Value' is one of those weasel words that slip in and out of the nets of the philosopher. We shall have to try to catch it first, or else what we have to say about the role of values in science may be of small use. It is not much over a hundred years since the German philosopher, Lotze, tried to construct a single theory of value which would unite the varied value-aspects of human experience under a single discipline. The venture was, of course, not really new since Plato had attempted a similar project long before, using the cognate term 'good' instead of 'value'. Aristotle's response to Plato's positing of the Good as a common element answering to one idea was to point to the great diversity of ways in which the term 'good' might be used. In effect, our response to Lotze's project of a general axiology would likewise be to question the usefulness of trying to find a single notion of value that would apply to all contexts equally well.

Let us begin with the sense of 'value' that the founders of value-theory seem to have preferred. They took it to correspond to such features of human experience as attraction, emotion and feeling. They wanted to secure an experiential basis for value in order to give the realm of value an empirical status just as valid as that of the (scientific) realm of fact. The reality of emotive value (as it may be called) lies in the feelings of the subject, not primarily in a characteristic of the object. Value-differences amount, then, to differences of attitude or of emotional response in specific subjects.

If one takes 'value' in this sense, value-decision becomes a matter of clarifying emotional responses. To speak of value-judgement here (as indeed is often done) is on the whole misleading, since 'judgement' could suggest a cognitive act, a weighing-up. When the value of something is determined by one's attitude to it, the declaration of this value is a matter of value-clarification rather than of judgement, strictly speaking. It was primarily from this sense of value that the popular positivist distinction between differences of belief and differences of attitude took its origin, though Stevenson (who, when specifying his own notion of attitude, recalls R. B. Perry's definition of "interest" as a psychological disposition to be for or against something) allows that value-differences may have components both of attitude and belief (1949, p. 591).

It seems plausible to hold that emotive values are alien to the work of natural science. There is no reason to think that human emotionality

is a trustworthy guide to the structures of the natural world. Indeed, there is every reason, historically speaking, to view emotive values, as Bacon did, as potentially distortive "Idols", projecting in anthropomorphic fashion the pattern of human wants, desires and emotions on a world where they have no place. When "ideology" is understood as a systematization of such values, it automatically becomes a threat to the integrity of science. The notion of value which is implicit in much recent social history of science, as well as in many analyses of the science-ideology relationship, is clearly that of emotive value.

A second kind of "value" is more important for our quest. A property or set of properties may count as a value in an entity of a particular kind because it is desirable for an entity of that kind. (The same property in a different entity might not count as a value.) The property can be a desirable one for various sorts of reasons. Speed is a desirable trait in wild antelope because it aids survival. Sound heart action is desirable in an organism with a circulatory system because of the functional needs of the organism. A retentive memory is desirable for a lawyer because of the nature of the lawyer's task. Sharpness is desirable in a knife because of the way in which it functions as a utensil. Efficiency is desirable in a business firm if the firm is to accomplish the ordinary ends of business... .

Let us focus on what these examples have in common. (In another context, we might be more concerned about their differences.) In each case, the desirable property is an objective characteristic of the entity. We can thus call it a characteristic value. In some cases, it is relative to a pattern of human ends; in others, it is not. In some cases, a characteristic value is a means to an end served by the entity possessing it; in others, it is not. In all cases, it serves to make its possessor function better as an entity of that kind.

Assessment of characteristic values can take on two quite different forms. One can judge the extent to which a particular entity realizes the value. We may be said to evaluate when we judge the speediness of a particular antelope or the heart-beat of a particular patient. On the other hand, we may be asked to judge whether or not (or to what extent) this characteristic really is a value for this kind of entity. How much do we value the characteristic? Here we are dealing, not with particulars, but with the more abstract relation of characteristic and entity under a particular description. Why ought one value speed in an antelope, rather than strength, say? How important is a retentive memory to a lawyer?

The logical positivists stressed the distinction between these two types of value-judgement, what I have called evaluation and valuing.[1] Valuing they took to be subjective and thus foreign to science. Evaluation, however, may be permissible because it "expresses an estimate of the degree to which some commonly recognized (and more or less clearly defined) type of action, object, or institution is embodied in a given instance" (Nagel 1961, p. 492).[2] Notice the presupposition here: clear definition of the characteristic is required in order that

there be a standard against which an estimate may be made. It was already a large concession to allow a role for mere estimation (as against measurement proper) in science; no further concession would be allowed.

Value-judgement in the sense of evaluation could thus fall on the side of the factual, and the old dichotomy between fact and value could still be maintained. Value-judgement in the sense either of valuing or of evaluating, where the characteristic value is not sharply defined, was still to be rigorously excluded from science. Such value-judgement (so the argument went) is necessarily subjective; it involves a decision which is not rule-guided, and therefore has an element of the arbitrary. It intrudes individual human norms into what should ideally (if it were to be properly scientific) be an impersonal mapping of propositions onto the world.

What was offensive about value-judgement, then, was not its concern with characteristic values. Indeed, when such values are measured (when, for example, human blood-pressure is measured as a means to determining any departure from "normality"), the results are obviously "scientific" in the most conservative sense. Not every judgement in regard to characteristic value counts therefore as a "value-judgement", as this term has come to be used. Such a judgement must not only be concerned with value, but must function, not as measurement does, but in a non-mechanical, individual, way. Since it is a matter of experience and skill, individual differences in judgement can thus in the normal course be expected.

It is clear, therefore, where the tension arises between value-judgement and not only the positivist view of science but the entire classical theory of science back to Aristotle. Max Weber spoke for that long tradition when, in his effort to eliminate value-judgement from social science, he opposed any form of assessment which could not immediately be enforced on all. The objectivity of science (he insisted) requires public norms accessible to all, and interpreted by all in the same way (Weber 1917).

What I want to argue here is that value-judgement, in just the sense that Weber deplored, does play a central role in science. Both evaluation and valuing are involved. The attempt to construe all forms of scientific reasoning as forms of deductive or inductive inference fails. The sense of my claim that science is value-laden is that there are certain characteristic epistemic values which are integral to the entire process of assessment in science. Since my topic is "values in science", there are, however, some other construals of this title that ought to be briefly addressed first, in order to be laid aside.

2. Other Construals

One value, namely truth itself, has always been recognized as permeating science. In the classic account, it was in fact the goal of the entire enterprise. Unlike other values, it was deemed to have nothing

of the personal about it. On the contrary, it connoted an objective re-
lation of proposition and world and thus was constitutive of the very
category of fact itself. But this was not thought to weaken the maxim
that science should be value-free, because the values that were thus be-
ing enjoined from intrusion into the work of science were the particular
ones that would tend to compromise the objectivity of the effort and not
the transcendental one which defined the tradition of science itself.

There has been much debate in recent philosophy of science about the
sense in which truth can still be taken to be constitutive of science.
The correspondence view of truth as a matching of language and mind-
independent reality has been assailed by Wittgenstein and many other
more recent critics like Putnam and Rorty. More to the point here, it
seems clear that when a scientist "accepts" a theory, even a long-held
theory, he is not claiming that it is true. The predicate in terms of
which theory is valued is not truth, as the earler account held it to
be. We speak of a theory as being "well-supported", "rationally accept-
able", or the like. To speak of it as true would suggest that a later
anomaly that would force a revision or even abandonment of the theory
can in principle be excluded. The recent history of science would make
both scientists and philosophers wary of any such presumption, except
perhaps in cases of very limited theories or ones which are vaguely
stated.

It can, however, be argued that truth is still a sort of horizon-
concept or ideal of the scientific enterprise, even though we may not be
able to assert truth in a definitive manner of any component of science
along the way. There are many variations of this view, one which was
clearly articulated a century ago by Peirce. I do not intend to discuss
this issue further here (though I will return to it obliquely in my con-
clusion), because to argue that truth is at least in some sense a char-
acteristic value admissible in science is hardly novel, and does not
constitute the point of division with classic logicist theories of sci-
ence that I am seeking to identify.

Nor am I concerned here with ethical values. Weber and the positiv-
ists of the last century and this one recognized that the work of
science makes ethical demands on its practitioners, demands of honesty,
openness, integrity. Science is a communal work. It cannot succeed un-
less results are honestly reported, unless every reasonable precaution
be taken to avoid experimental error, unless evidence running counter to
one's own view is fairly handled, and so on. These are severe demands,
and scientists do not always live up to them. Outright fraud, as we
have been made uncomfortably aware in recent years, does occur. But so
far as we can tell, it is rare and does not threaten the integrity of
the research enterprise generally. In any event, there never has been
any disagreement about the value-ladeness of science where moral values
of this kind are concerned. If I am to make a claim about a change in
regard to the recognition of the proper presence in science of value-
judgement, it cannot be in regard to those moral values which have al-
ways been seen as essential to the success of communal inquiry.[3]

In support of his claim that "value-judgements are essentially in-
volved in the procedures of science", Rudner argued that the acceptance
of a scientific hypothesis:

> is going to be a function of the importance, in the typically
> ethical sense, of making a mistake in accepting or rejecting
> the hypothesis. Thus, to take a crude but easily manageable
> example, if the hypothesis under consideration were to the
> effect that a toxic ingredient of a drug was not present in
> lethal quantity, we would require a relatively high degree of
> confirmation or confidence before accepting the hypothesis, for
> the consequences of making a mistake here are exceedingly grave
> by our moral standards. (1953, p. 2).

This notion of hypothesis "acceptance" is dangerously ambiguous.
Rudner takes it to mean: "approve as a basis for a specific kind of ac-
tion." But acceptance in this sense is not part of theoretical science,
strictly speaking. When a physicist "accepts" a particular theory, this
can mean that he believes it to be the best-supported of the alterna-
tives available or that he sees it as offering the most fruitful
research-program for the immediate future. These are epistemic assess-
ments; they attach no values to the theoretical alternatives other than
those of likelihood or probable fertility. On the other hand, 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-judgements in their regard as part of his sci-
entific work. The values of life and death involved in a decision to
use or not to use a possibly toxic drug in a case where it alone seems
to offer a chance of recovery are not relevant to the much more limited
question as to whether or ñot the drug would be toxic for this patient.

The utilities typically associated with the application of science to
human ends in medicine, engineering and the like, cannot, therefore, be
cited as a reason for holding natural science itself to be value-laden.
The conclusion that Rudner draws from his analysis of hypothesis-
"acceptance" is that "a science of ethics is a necessary requirement if
science's progress toward objectivity is to be continuous." But scien-
tists are (happily!) not called on to "accept" hypotheses in the sense
he is presupposing,[4] and so his conclusion does not go through.[5] If we
are to hold that the work of science is value-laden, it ought to be for
another reason.

My argument for the effective presence of "values in science" does
not, then, refer to the constitutive role in science of the value,
truth, nor to the ethical values required for the success of science as
a communal activity, nor to the values implicit in decision-making in
applied science. Rather, it is directed to showing that the appraisal
of theory is in important respects closer in structure to value-

judgement than it is to the rule-governed inference that the classic tradition in philosophy of science took for granted.

Not surprisingly, the recognition of this crucial epistemological shift has been slow and painful. Already there are intimations of it among the more perceptive nineteenth-century philosophers of science. Whewell, for example, describes a process very like value-judgement in his influential account of the "consilience of inductions", though he draws back from the threatening subjectivism of this line of thought, asserting that consilience will amount to "demonstration" in the long run (Laudan 1981). The logical positivists, as already noted, reso-lutely turned the theory of science back into the older logicist chan-nels once more. Yet as they (and their critics) tried to characterize the strategies of science in closer detail, doubts began to grow. To these earlier anticipations of our theme, I now briefly turn.

3. Anticipations

The prevailing inductivism of the nineteenth century made it seem as though science ultimately consisted of <u>laws</u>, that is, statements of em-pirical regularities. These laws were arrived at by generalization from the facts of observation; the facts themselves were regarded as an un-problematic starting-point for the process of induction. It was, of course, realized that the laws were open to revision as measuring appa-ratus was improved, as the ranges of the variables were extended, as new relevant factors were discovered. There was no <u>logic</u>, strictly speak-ing, which would lead from a finite set of observation-statements to a universally valid law of nature.

Human decision had to enter in, therefore, by way of curve-fitting, extrapolation, estimates of relevance. Such decision was not arbitrary; there were skills and techniques to be learnt which would aid the scien-tist in drawing the best generalizations from the data available. Was this not a matter of value-judgement rather than of a common logic of formal rules? We would say so today. But the point was not so evident then, or perhaps it would be more accurate to say that it seemed of lit-tle importance.

The reason was that the laws were taken to be true to a degree of ap-proximation that could be improved indefinitely. Thus the influence of these decisional aspects, where the individual skills of curve-fitting, extrapolation, estimation of relevance, entered into the process of formulating a law, would be progressively lessened, as the law came closer and closer to being an exact description of the real, that is, as the law gradually attained the status of fact. Thus, even though value-judgement did enter, in a number of ways, into the process of inductive generalization, its presence could in practice be ignored. It was, after all, no more than an accessory activity, of little significance to the ultimate deliverances of science, namely, the exact statements of the laws of nature.

The logical positivists still adhered to this nomothetic ideal. But from the beginning, they encountered difficulties as soon as they tried to spell out how an inductive method might work. The story is a familiar one. I am going to focus on only two episodes in it, one involving Popper and the other Carnap, in order to show how "value-uneasiness" was already in evidence among philosophers of science fifty years ago, though in neither of these episodes was it altogether satisfactorily characterized.

As we all know, Popper rejected the nomothetic ideal of science that the logical positivists took over from the nineteenth century. For him, science is a set of conjectures rather than a set of laws. The testing of conjectures is thus the central element in scientific method and it can work only by falsification, when a basic statement conflicts with a conjectured explanation, leading to the rejection of the conjecture. The entire logical weight of this operation is carried by the "basic statements", that is, reports of observable events at particular locations in space and time.

But now, a difficulty arises. Could not the basic statements themselves be falsified? They could not consistently be held immune to the test-challenge that Popper saw as the criterion of demarcation between science and non-science. But if the basic statements themselves are open to challenge, how is the whole procedure of falsification of conjecture to work? It sounds as if a destructive regress cannot be avoided.

Popper's answer is to say that:

> Every test of a theory, whether resulting in its corroboration or falsification, must stop at some basic statement or other which we decide to accept... . Considered from a logical point of view, the situation is never such that it compels us to stop at this particular basis statement rather than at that... . For any basic statement can again in its turn be subjected to tests, using as a touchstone any of the basic statements which can be deduced from it, with the help of some theory, either the one under test or another. This process has no natural end. Thus if the test is to lead anywhere, nothing remains but to stop at some point or other and say we are satisfied for the time being (1934/1959, p. 104).

Thus the designation of a statement as a "basic" one is not definitive, and hence falsification is not quite the decisive logical step Popper would have liked it to be. He continues: "Basic statements are accepted as the result of a decision or convention; and to that extent they are conventions." (p. 106). His choice of the term, 'convention', here is a surprising one since it carries the overtone of arbitrariness, of arbitary choice and not just choice. But Popper is explicit in excluding this suggestion. He criticizes Neurath, in fact, who (he says) made a "notable advance" by recognizing that protocol statements are not irrevocable, but then failed to specify a method by which they might be evaluated. Such a move, he goes on,

leads nowhere if it is not followed by another step; we need a set of rules to limit the arbitrariness of 'deleting' (or else 'accepting') a protocol sentence. Neurath fails to give any such rules and thus unwittingly throws empiricism overboard. For without such rules, empirical statements are no longer distinguished from any other sort of statements. (p. 97).

For Popper, the need for such a line of demarcation takes precedence over any other demand. So if there are to be decisions regarding the basic statements, these must (he says) be "reached in accordance with a procedure governed by rules." (p. 106). If there are rules, however, to guide the decision, it sounds as though a definite answer might be obtained by the application of these rules in any given case. And so the properly decisional element would be minimal, and value-judgement (as we have defined it) would not enter in.

But, in fact, we discover that the word, 'rule', here (like the word, 'convention') is not to be taken literally. When Popper specifies how these "rules" would operate, all he has to say is that we can arrive at:

a procedure according to which we stop only at a kind of statement that is especially easy to test. For it means that we are stopping at statements about whose acceptance or rejection the various investigators are likely to reach agreement. (p. 104).

So that ease in testing is to guide the investigator in deciding which statements to designate as basic. But this clearly operates here as a value rather than as a rule. There could be differences in judgement as to the extent to which the value was realized in a given case. Popper himself says of his "rules" that though they are "based on certain fundamental principles" which aim at the discovery of objective truth, "they sometimes leave room not only for subjective convictions but even for subjective bias." (p. 110).

Thus what we have here is value-judgement, not the application of rule, strictly speaking. There is no rule as to where to stop the testing process. If some investigators prolong it further than others do, we would not be inclined to describe this as either "following" or "breaking" a rule. But we would call it the pursuing of a particular goal or value.

Popper's use of the term 'convention' to describe the element of value-judgement in the designation of basic statements has proved misleading to later commentators, even though he explicitly rejected classical conventionalism, mainly because it was unable, in his view, to generate a proper criterion of demarcation between science and non-science (McMullin 1978a, section 7). Lakatos, for example, described Popper's view as a form of "revolutionary conventionalism" because of its explicit admission of the role of decisional elements in the scientific process. This led him to characterize his own MSRP as a way of

"rationalizing classical conventionalism", rational because the criteria
for distinguishing between "hard core" and "protective belt" can be par-
tially specified, as can the criteria of theory-choice, but "convention-
al" because the process is not one of a mechanical application of rule,
involving, as it does, individual judgement (1970, p. 134). Agassi
likewise proposed that the most accurate label for Popper's theory of
science is 'modified conventionalism' (Agassi 1974, p. 693) to which
suggestion Popper rather testily responded "I am not a conventionalist,
whether modified or not." (Popper 1974, p. 1117).

Much of the confusion prompted by Popper's use of the term 'conven-
tion' might have been avoided if he had used the notion of value-
judgement instead. It has precisely the flexibility that he needed in
order to distance himself, as he wished to do, from both positivism and
conventionalism, from positivism because of his insistence upon the
decisional elements in the selection of basic statements and from con-
ventionalism because he believed that the values guiding judgement in
this case are grounded in the "autonomous aim" of science, which is the
pursuit of objective knowledge (Popper 1974, p. 1117).

Though the admission of value-judgement into science had moved Popper
away from his rationalist moorings, it is significant that he never ex-
tended the range of value-judgement to theory-choice, which today to us
would seem the much more likely locus. Even though he allows that "the
choice of any theory is an act, a practical matter" (1935/1959, p. 109),
his opposition to verification made him wary of allowing that theories
might ever be "accepted". To the extent that they are, it is a provi-
sional affair, he reminds us. But this sort of provisional acceptance
is still, in his view, decisively influenced by the success of the
theory in avoiding falsification (McMullin 1978a, p. 224). Rationalism
is thus preserved at this level by the assumption of a more or less de-
cisive method of choosing between theories at any given stage of devel-
opment.

This is the assumption that Carnap helped, somewhat unwittingly per-
haps, to undermine. In 1950, he drew his famous distinction between
"internal" questions, which can be answered within a given linguistic
framework and "external" questions, which bear on the acceptability of
the framework itself (1950, p. 214). The point of the distinction was
to clarify the debate about the existence of such abstract entities as
classes or numbers to which Carnap assimilated the question of theoreti-
cal entities like electrons. To ask about the existence of such enti-
ties within a given linguistic framework is perfectly legitimate, he
said, and an answer can be given along logical or empirical lines. But
to ask about the reality of such a system of entities taken as a whole
is to pose a metaphysical question to which only a pseudo-answer can be
given. The question can, however, be framed in a different way and then
it becomes perfectly legitimate. We can ask whether the linguistic
framework itself is an appropriate one for our purposes, whatever they
may be. This is the form in which external questions should be put in
order to avoid idle philosoper's questions about the existence of num-
bers or electrons.

Once the question is put in this way, he goes on, it is seen to be a practical, not a theoretical matter. The decision to accept a particular framework:

> although itself not of a cognitive nature, will nevertheless usually be influenced by theoretical knowledge, just like any other deliberate decision concerning the acceptance of linguistic or other rules. The purposes for which the language is intended to be used, for instance, the purpose of communicating factual knowledge, will determine which factors are relevant for the decision.(1950, p. 208).

And he goes on to enumerate some of the factors that might influence a pragmatic decision of this sort: he mentions the "efficiency, fruitfulness and simplicity" of the language, relative to the purposes for which it is intended. These are values, of course, and so what he is talking about here (though he does not explicitly say so) is value-judgement.

In this essay, Carnap is worrying mainly about the challenge of the nominalists to such entities as classes, properties and numbers. He wants to answer this challenge, not by asserting the existence of these entities directly--this would violate his deepest empiricist convictions--but by appealing to the practical utility of everyday language where terms corresponding to these entities play an indispensable role. And so he counters Occam's razor with a plea for "tolerance in permitting linguistic forms" (1950, p. 220). As long as the language is efficient as an instrument, he says, it would be foolish, indeed harmful, to impoverish it on abstract nominalist grounds.

But Carnap conceded much more than he may have realized by this manoeuvre. By equating the general semantical problem of abstract entities with the problem of theoretical entities in science, he implied that pragmatic "external" criteria are the appropriate ones for deciding on the acceptability of the linguistic frameworks of science, that is, of scientific theories. For the first time, he is implicitly admitting that the tight "internal" logicist criteria which he had labored so long to impose on the problems of confirmation and explanation are inappropriate when it is the very language of science itself, that is, the theory, that is in question.

It is the theory that leads us to speak of electrons; to assess this usage, we have to evaluate as a single unit the theory in which this concept occurs and by means of which it is defined. If more than one "linguistic framework" or theory is being defended in some domain, the decision as to which is the better one has to be resolved, not by inductive logic, but by these so-called "external" criteria.

The term, 'external', was obviously an unhappy choice, as things would turn out. The questions Carnap dubs "external" would be external to science only if theory-decision is external to science. They were external to his logicist conception of how science ought to be carried on, of course. Only if science can be regarded as a "given" formal

system can the enterprise of the logician get under way. The question of whether a particular theory should be "given" or whether another might not accomplish the theoretical ends of science better, cannot be properly (i.e.,"internally") posed in the original positivist scheme of things.

Once Carnap allowed it to be posed, however "externally", it would not be long until theory-evaluation would be clearly recognized as the most "internal" of all scientific issues, defining as it does scientific rationality and scientific progress. After we have discarded his term 'external', we still retain his insight that the structure of decision in regard to the acceptability of a theoretical language is not one of logical rule but of value-judgement.

4. Theory-choice as value-judgement

This gets us up only to 1950, which seems like very long ago in phi-losophy of science. Yet the shape of things to come is already clear to us, even though it was by no means clear then. The watershed between classic theory of science and our as-yet unnamed post-logicist age has been variously defined since then. But for our purposes here, it can best be laid out in four propositions, three of them familiar, the other (P3) a little less so.

P1: The goal of science is theoretical knowledge.

P2: The theories of science are underdetermined by the empirical evidence.

P3: The assessment of theories involves value-judgement in an essential way.

P4: Observation in science is theory-dependent.

P1 tells us that the basic explanatory form in science is theory, not law, and thus that retroduction, not induction, is the main form of scientific validation. Theories by their very nature are hypothetical, tentative; they remain open to revision or even to rejection. P2 reminds us that there is no direct logical link, of the sort that clas-sical theories of science expected, between evidence and theory. Since one is not compelled, as one would be in a logical or mathematical demonstration, one has to rely on oblique modes of assessment. And P3 tells us that these take the form of value-judgements.

P4 serves to emphasize that a thesis in regard to theory-appraisal has broader scope. To the extent that scientific observation is theory-dependent, it is also indirectly value-impregnated. This last point is not stressed any further in what follows, but it is well that it should be kept in mind lest it be thought that only one element in science, theory-choice, is affected by the shift described here, and that the traditional logicist/empiricist picture might be sustained at all other points.

So much for the schema. It can be found, more or less in the form in which I have sketched it here, in the work of Kuhn, specifically in his 1973 essay "Objectivity, value-judgement, and theory choice" (1977). He asks there what are the characteristic values of a good scientific theory and lists, as a start, five that would be pretty well agreed upon. I will rework his list just a little, and add some comments.

Predictive accuracy is the desideratum that scientists would usually list first. But one has to be wary about the emphasis given it. As Lakatos and Feyerabend in particular have emphasized, scientists must often tolerate a certain degree of inaccuracy, especially in the early stages of theory-development. Nearly every theory is "born refuted"; there will inevitably be anomalies it cannot handle. There will be idealizations that have to be worked out in order to test the theory in complex concrete contexts. Were this demand to be enforced in a mechanical manner, the results for science could be disastrous. Nevertheless, a high degree of predictive accuracy is in the long run something a theory must have if it is to be acceptable.

A second criterion is internal coherence. The theory should hang together properly; there should be no logical inconsistencies, no unexplained coincidences. One recalls the primary motivating factor for many astronomers in abandoning Ptolemy in favor of Copernicus. There were too many features of the Ptolemaic orbits, particularly the incorporation in each of a one-year cycle and the handling of retrograde motions, that seemed to leave coincidence unexplained and thus, though predictively accurate, to appear as ad hoc.

A third is external consistency: consistency with other theories and with the general background of expectation. When steady-state cosmology was proposed as an alternative to the Big Bang hypothesis in the late 1940's, the criticism it first had to face was that it flatly violated the principle of conservation of energy, which long ago attained the status almost of an a priori in mechanics. Even if Hoyle had managed to make his model satisfy the other demands laid on it, such as the demand that it yield testable predictions in advance and not just after the fact, it would always have had a negative rating on the score of external consistency.

A fourth feature that scientists value is unifying power, the ability to bring together hitherto disparate areas of inquiry. The standard illustration is Maxwell's electromagnetic theory. A more limited, but quite striking, example would be the plate-tectonic model in geology. Over the past twenty years, it has successfully explained virtually all major features of the earth's surface. What has impressed geologists sufficiently to persuade most (not all) of them to overcome the scruples that derive, for example, from the lack of a mechanism to account for the plate-movements themselves, is not just its predictive accuracy but the way in which it has brought together previously unrelated domains of geology under a single explanatory roof.

A further, and quite crucial, criterion is <u>fertility</u>. This is rather a complex affair (McMullin 1976). The theory proves able to make novel predictions that were not part of the set of original explananda. More important, the theory proves to have the imaginative resources, functioning here rather as a metaphor might in literature, to enable anomalies to be overcome and new and powerful extensions to be made. Here it is the <u>long-term</u> proven ability of the theory or research program to generate <u>fruitful</u> additions and modifications that has to be taken into account.

One other, and more problematic, candidate as a theory-criterion is <u>simplicity</u>. It was a favorite among the logical positivists because it could be construed pragmatically as a matter of convenience or of aesthetic taste, and seemed like an optional extra which the scientist could decide to set aside, without affecting the properly epistemic character of the theory under evaluation (Hempel 1966, pp. 40-45). Efforts to express a criterion of "simplicity" in purely formal terms continue to be made, but have not been especially successful.

One could easily find other desiderata. And it would be important to supply some detailed case-histories in order to illustrate the operation of the ones I have just listed. But my concern here is rather to underline that these criteria clearly operate as <u>values</u> do, so that theory choice is basically a matter of value-judgement. Kuhn puts it this way:

> The criteria of [theory] choice function not as rules, which
> determine choice, but as values which influence it. Two men
> deeply committed to the same values may nevertheless, in par-
> ticular situations, make different choices, as in fact they do.
> (1977, p. 331).

They correspond to the two types of value-judgement discussed above in section 1. First, different scientists may <u>evaluate</u> the fertility, say, of a particular theory differently. Since <u>there is</u> no algorithm for an assessment of this sort, it will depend on the individual scientist's training and experience. Though there is likely to be a very large measure of agreement, nonetheless the skills of evaluation here are in part personal ones, relating to the community consensus in complex ways.

Second, scientists may not attach the same relative weights to different characteristic values of theory, that is they may not <u>value</u> the characteristics in the same way, when, for example, consistency <u>is to</u> be weighed over against predictive accuracy. It is above all because theory has more than one criterion to satisfy, and because the "valuings" given these criteria by different scientists may greatly differ, that disagreement in regard to the merits of rival theories can on occasion be so intractable.

It would be easy to illustrate this by calling on the recent history of science. A single example will have to suffice. The notorious disagreement between Bohr and Einstein in regard to the acceptability of

the quantum theory of matter did not bear on matters of predictive accuracy. Einstein regarded the new theory as lacking both in coherence and in consistency with the rest of physics. He also thought it failing in simplicity, the value that he tended to put first. Bohr admitted the lack of consistency with classical physics, but played down its importance. The predictive successes of the new theory obviously counted much more heavily with him than they did with Einstein. The differences between their assessments were not solely due to differences in the values they employed in theory-appraisal. Disagreement in substantive metaphysical belief about the nature of the world also played a part. But there can be no doubt from the abundant testimony of the two physicists themselves that they had very different views as to what constituted a "good" theory.

The fact that theory-appraisal is a sophisticated form of value-judgement explains one of the most obvious features of science, a feature that could only appear as a mystery in the positivist scheme of things. Controversy, far from being rare and wrong-headed, is a persistent and pervasive presence in science at all levels. Yet if the classical logicist view of science had been right, controversy would be easily resolvable. One would simply employ an algorithm, a "method", to decide which of the contending theories is best confirmed by the evidence available. At any given moment, there would then be a "best" theory, to which scientists properly versed in their craft ought to adhere.

But, of course, not only is this not the case, but it would be a disaster if it were to be the case (McMullin 1983). The clash of theories, Popper has convinced us, is needed in order that weak spots may be probed and potentialities fully developed. Popper's own theory of science made it difficult to see how such a pluralism of theories could be maintained. But once theory-appraisal is recognized to be a complex form of value-judgement, the persistence of competing theories immediately follows as a consequence.

Kuhn characteristically sees the importance of value-difference not so much in the clash of theories--such controversy is presumably not typical of his "normal science"--as in the period of incipient revolution when a new paradigm is struggling to be born:

> Before the group accepts it, a new theory has been tested over time by the research of a number of men, some working within it, others within its traditional rival. Such a mode of development, however, requires a decision process which permits rational men to disagree, and such disagreement would be barred by the shared algorithm which philosophers generally have sought. If it were at hand, all conforming scientists would make the same decision at the same time. ... I doubt that science would survive the change. What from one point of view may seem the looseness and imperfection of choice criteria conceived as rules may, when the same criteria be seen as values, appear an indispensable means of spreading the

risk which the introduction or support of novelty always
entails (1961, p. 220).

It almost seems as though the value-ladenness of theory-decision is
specially designed to ensure the continuance of controversy and to pro-
tect endangered but potentially important new theoretical departures. A
Hegelian might see in this, perhaps, the cunning of Reason in bringing
about a desirable result in a humanly unpremeditated way. But, of
course, these are just the fortunate consequences of the nature of
theory-decision itself. It is not as though theories could be appraised
in a different more rule-guided way. One.is forced to recognize that
the value-ladenness described above derives from the problematic and
epistemologically complex way in which theory relates to the world. It
is only through theory that the world is scientifically understood.
There is no alternative mode of access which would allow the degree of
"fit" between theory and world to be independently assessed, and the
values appropriate to a good theory to be definitively established. And
so there is no way to exchange the frustrating demands of value-
judgement for the satisfying simplicities of logical rule.

5. Epistemic values

Even though we cannot definitively establish the values appropriate
to the assessment of theory, we saw just a moment ago that we can pro-
vide a tentative list of criteria that have gradually been shaped over
the experience of many centuries, the values that are implicit in con-
temporary scientific practice. Such characteristic values I will call
epistemic, because they are presumed to promote the truth-like character
of science, its character as the most secure knowledge available to us
of the world we seek to understand. An epistemic value is one we have
reason to believe will, if pursued, help toward the attainment of such
knowledge. I have concentrated here on the values that one expects a
good theory to embody. But there are, of course, many other epistemic
values, like that of reproducibility in an experiment or accuracy in a
measurement.

When I say that science is value-laden, I would not want it to be
thought that these values derive from theory-appraisal only. Value-
judgement permeates the work of science as a whole, from the decision to
allow a particular experimental result to count as "basic" or "accepted"
(the decisional element that Popper stressed), to the decision not to
seek an alternative to a theory which so far has proved satisfactory.
Such values as these may be pragmatic rather than epistemic; they may
derive from the finiteness of the time or resources available to the
experimenter, for example. And sometimes the borderline between the
epistemic and the pragmatic may be hard to draw, since (as Duhem and
Popper among others have made clear) it is essential to the process of
science that pragmatic decisions be made, on the temporary suspension of
further testing for example.

Of course, it is not pragmatic values that pose the main challenge to
the epistemic integrity of the appraisal process. If values are needed

in order to close the gap between underdetermined theory and the evidence brought in its support, presumably all sorts of values can slip in: political, moral, social, religious. The list is as long as the list of possible human goals. I shall lump these values together under the single blanket term, 'non-epistemic'. The decision as to whether a value is epistemic or non-epistemic in a particular context can sometimes be a difficult one. But the grounds on which it should be made are easy to specify in the abstract. When no sufficient case can be made for saying that the imposition of a particular value on the process of theory choice is likely to improve the epistemic status of the theory, that is, the comformity between theory and world, this value is held to be non-epistemic in the context in question. This decision is itself, of course, a value-judgement and there is an obvious danger of a vicious regress at this point. I hope it can be headed off, and will return to this task in a moment.

But first, one sort of factor that plays a role in theory-assessment can be hard to situate. Externalist historians of science have been accustomed to grouping under the elastic term, 'value' not only social and personal goals but also various elements of world-view, metaphysical, theological and the like. Thus, for example, when Newton's theology or Bohr's metaphysics affected the choice each made of "best" theory in mechanics, such historians have commonly described this as an influence of "values" upon science. (See, for example, Graham 1981).

Since I have been arguing so strongly here for the value-ladenness of science, it might seem that I should welcome this practice. But it is rooted, I think, in a sort of residual positivism that is often quite alien to the deepest convictions of the historians themselves who indulge in it (McMullin 1982). They would be the first to object to the label 'externalist', but here they are assuming that a philosophical world-view is of its nature so "external" to science that it must be flagged as a "value", and consequently dealt with quite differently from the point of view of explanation.

Let me try to clarify the source of my opposition to this practice. A philosophical system can in certain contexts serve as a value, as a touchstone of decision. So for that matter can a scientific theory. But this does not convert it into a "value" in the sense in which social historians sometimes interpret this term, namely as something for which socio-psychological explanation is all-sufficient. The effect of calling metaphysics a "value" can be to shift it from the category of belief to be explained in terms of reasons adduced, in the way that science is ordinarily taken, to the category of goal to be explained, in terms of character, upbringing, community pressures, and the rest.

What I am arguing for is the potentially epistemic status that philosophical or theological world-view can have in science. From the standpoint of today, it would be inadmissible to use theological argumentation in mechanics. Yet Newton in effect did so on occasion. In describing this, it is important to note that theology functioned for him as an epistemic factor, as a set of reasons that Newton thought were

truth-bearing (McMullin 1978b, p. 55). It did not primarily operate as a value if by 'value' one were to mean a socio-psychological causal factor, superimposed upon scientific argument from the outside, to be understood basically as a reflection of underlying social or psychological structures.

Now, of course, the historian may find that someone's use of theological or philosophical considerations did, on a given occasion, reflect such structures. But this has to be historically proven. The question must not be begged by using the term 'value' as externalist historians have too often done. Incidentally, the pervasive presence of non-standard epistemic factors in the history of science is the main reason, to my mind, why the one-time popular internal-external dichotomy fails. Sociologists of science in the "strong program" tradition are more consistent in this respect. They do take metaphysics and theology to be a reflection of socio-psychological structure, but of course, they regard science itself in the same epistemically unsympathetic light. My point here has simply been that it is objectionable to single out non-standard forms of argument in science by an epistemically pejorative use of the term, 'value' (McMullin 1983).

6. The place of fact in a world of values

That being said, let me return to the question that must by now be uppermost in the reader's mind. What is left of the vaunted objectivity of science, the element of the factual, in all this welter of value-judgement? Once the camel's nose is inside, the tent rapidly becomes uncomfortable. Is there any reasoned way to stop short of a relativism that would see in science no more than the product of a contingent social consensus, bearing testimony to the historical particularity of culture and personality much more than to an objective truth about the world? I think there is, but I can at this stage only provide an outline of the argument needed. It requires two separate steps.

Step one is to examine the epistemic values employed in theory-appraisal, the values that lie at the heart of the claim that theory-assessment in science is essentially value-laden, and to ask how they in turn are to be validated, and how in particular, circularity is to be avoided in doing so. First, let me recall how the skills of epistemic value-judgement are learnt. Apprentice scientists learn them not from a method book but from watching others exercise them. They learn what to expect in a "good" theory. They note what kinds of considerations carry weight, and why they do so. They get a feel for the relative weight given the different kinds of considerations, and may quickly come to realize that there are divergences here in practice. Their own value-judgements will gradually become more assured, and will be tested against the practices of their colleagues as well as against historical precedent (Polanyi 1958; Kuhn 1962).[6]

What is the epistemic worth of the consensus from which these skills derive? Kuhn is worried about the validity of invoking history as warrant in this case:

> 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 (1977, p.
> 335).

This is to take the Hume-Popper challenge to induction far too seriously
(unless, of course, 'justification' were to be taken to mean definitive
proof). The characteristic values guiding theory-choice are firmly
rooted in the complex learning experience which is the history of
science; this is their primary justification, and it is an adequate one.

We have gradually learnt from this experience that human beings have
the ability to create those constructs we call "theories" which can pro-
vide a high degree of accuracy in predicting what will happen, as well
as accounting for what has happened, in the world around us. It has
been discovered, further, that these theories can embody other values
too, such values as coherence and fertility, and that an insistence on
these other values is likely to enhance the chances over the long run of
the attainment of the first goal, that of empirical accuracy.

It was not always clear that these basic values could be pursued si-
multaneously.[7] In medieval astronomy, it seemed as though one had to
choose between predictive accuracy and explanatory coherence, the
Ptolemaic epicycles exemplifying one and Aristotelian cosmology the
other. Since the two systems were clearly incompatible, philosophers
like Aquinas reluctantly concluded that there were two sorts of astro-
nomical science, one (the "mathematical") which simply "saved the ap-
pearances", and the other (the "physical") whose goal it was to explain
the truth of things (Duhem, 1908/1969, Chapter 3). Galileo's greatest
accomplishment, perhaps, was to demonstrate the possibility of a single
science in which the values of both the physical and the mathematico-
predictive traditions could be simultaneously realized (Machamer 1978).

There was nothing necessary about this historical outcome. The world
might well have turned out to be one in which our mental constructions
would not have been able to combine these two ideals. What became clear
in the course of the 17th century was that they can be very successfully
combined, and that other plausible values can be worked in as well.
When I say "plausible" here, I am suggesting that there is a second con-
vergent mode of validation for these values of theory-appraisal (for
"valuings" in the sense defined in section 1).

We can endeavor to account for their desirability in terms of a
higher-order epistemological account of scientific knowing. This is to
carry retroduction to the next level upwards. It is asking the philoso-
pher to provide a theory in terms of which such values as fertility
would be shown to be appropriate demands to lay on scientific theory.
The philosopher's ability to provide just such a theory (and it is not
difficult to do this) in turn then testifies to the reliability of tak-
ing these criteria to be proper values for theory-appraisal in the

first place. This is only the outline of an argument, and much more remains to be filled in. But perhaps I have said enough to indicate how one could go about showing that the characteristic values scientists have come to expect a theory to embody are a testimony to the <u>objectivity</u> of the theory, as well as of the involvement of the subjec<u>tivity</u> <u>of the</u> scientist in the effort to attain that objectivity.

There is a further argument I would use in support of this conclusion, but it is based on a premiss that is not shared by all. That is the thesis of scientific realism. I think that there are good reaons to accept a cautious and carefully-restricted form of scientific realism, prior to posing the further question of the objective basis of the values we use in theory-appraisal (McMullin 1983). The version of realism I have in mind would suggest that in many parts of science, like geology and cell-biology, we have good reason to believe that the models postulated by our current theories gives us a reliable, though still incomplete, insight into the structures of the physical world.

Thus, for example, we would suppose that the success of certain sorts of theoretical model would give us strong reason to believe that the core of the earth <u>is</u> composed of iron, or that stars <u>are</u> glowing masses of gas. We have <u>no</u> direct testimony regarding either of these beliefs, of course. To claim that the world <u>does</u> resemble our theoretical models in these cases, is to claim that <u>the</u> method of retroduction on which they are based, and which rests finally on the values of theory-appraisal I have already discussed, is in fact (at least in certain sorts of case) reliable in what it claims.^{8} Obviously, the realist thesis will not hold, or will hold only in attenuated form, where theory is still extremely underdetermined (as in current elementary-particle theory) or where the ontological implications of the theory are themselves by no means clear (as in classical mechanics).

And so, to conclude step one, there is reason to trust in the values used commonly in current science for theory-appraisal as something much more than the contingent consensus of a peculiar social sub-group.

But a further step is needed, because these values do not of themselves <u>determine</u> theory-choice, a point I have stressed from the beginning. And so other values <u>can</u> and <u>do</u> enter in, the sorts of value that sociologists of science have <u>so</u> successfully been drawing to our attention of late, as they scrutinize particular episodes in the history of science. I am thinking of such values as the personal ambition of the scientist, the welfare of the social class to which he or she belongs, and so on. Has the camel not, then, poked its wet nose in beside us once again?

It has, of course, but perhaps we can find a way to push it out--or almost out--one final time. The process of science is one long series of tests and tentative imaginative extensions. When a particular theory seems to have triumphed, when Pasteur has overcome Pouchet, to cite one nineteenth-century illustration that has recently come in for a lot of attention from social historians of science (Farley and Geison 1976),

it is not as though the view that has prevailed is allowed to reign in peace. Other scientists attempt to duplicate experimental claims; theoreticians try to extend the theories involved in new and untried ways; various tests are devised for the more vulnerable theoretical moves involved, and so on. This is not just part of the mythology of science. It really does happen, and is easy to document.

To the extent that non-epistemic values and other non-epistemic factors have been instrumental in the original theory-decision (and sociologists of science have rendered a great service by revealing how much more pervasive these factors are than one might have expected), they are gradually sifted by the continued application of the sort of value-judgement we have been describing here. The non-epistemic, by very definition, will not in the long run survive this process. The process is designed to limit the effects not only of fraud and careless-ness, but also of ideology, understood in its pejorative sense as dis-tortive intrusion into the slow process of shaping our thought to the world.

Once again, this is only an outline of an argument, a sketch of work remaining to be done. I have assumed that this is what a Presidential Address is supposed to provide: an overview of the terrain we have been crossing and a glimpse of the country that lies ahead. If I am right, that country will look very different from anything we have so far traversed.

Notes

[1]The terminology of "evaluation" and "valuing" is used by Kovesi (1967) in a somewhat different way. He supposes value-judgement to ap-ply to things via their descriptions. Thus, we "evaluate" particulars insofar as they "fall under a certain description" (p. 151). Whereas we "value" things "insofar as they are such and such". We would "evalu-ate" a particular lawyer as a lawyer (being given a description of the qualities that make up a lawyer), whereas we would "value" lawyers for what they are, as indispensable to the conduct of complex communities or however we might wish to describe their "value" in some broader context. (His aim is to contrast "evaluation" with moral judgement.) My focus is on specific characteristic values, on the Y-ness of X's, where his is on entity-descriptions, on X-ness itself as a subject for evaluation or valuing. The advantage of the former is that it makes the basis of the value-judgement specific. It focusses evaluation on the characteristic which can be present to a greater or lesser degree. And it provides a context for valuing which Kovesi's notion appears to lack, thus risking confusion with emotive value. Finally, Kovesi's emphasis on description could mislead, since the characteristic value need not be described, strictly speaking. Indeed, as we shall see below, the frequent inabil-ity to give explicit descriptions of characteristic values is an essen-tial feature of evaluation as it occurs in science. The emphasis on X-ness (which does need describing) rather than on the Y-ness of X's

(where the Y may be only summarily indicated) is the root of the difference. I am indebted to Carl Hempel and David Solomon for discussions of the topics of this section.

[2]Nagel used the terms 'characterize' and 'appraise' instead of our 'evaluate' and 'value'. The example he gives of "characterizing" is the evaluation of the degree of anemia a particular animal suffers from against a standard of "normality" in the red blood-corpuscle count. (See "The value-oriented bias of social inquiry", Nagel 1961, pp. 485-502.)

[3]Some philosophers assimilate epistemic values to moral values, so that for them the values implicit in theory-appraisal are broadly moral ones. Putnam, for example, takes adherence to these values on the part of scientists to be "part of our idea of human cognitive flourishing, and in hence part of our idea of total human flourishing, of Eudaimonia" (1981, p. 134). The analysis of characteristic value given in section 1, and even more the discussion of the warrant for epistemic value in section 6 below, would lead me to question this assimilation of the epistemic to the moral under the very vague notion of "flourishing". To pursue this further would, however, require further analysis of the nature of moral knowledge.

[4]A point already made by Richard Jeffrey (1956) in a response to the Rudner article.

[5]In fairness, it should be added that Rudner drew attention in the same paper to the value-implications of the new directions that Carnap and Quine were just beginning to chart. But these consequences were obscured by his emphasis on the ethical aspects of theory-acceptance. He evidently supposed that all of these considerations would converge, but in fact, they did not, and could not.

[6]Polanyi and Kuhn relate such skills as that of theory-assessment (and pattern-recognition, which is ultimately theory-dependent) in rather different ways to the learning experience of the apprentice scientist. I would lean more to Kuhn's analysis in this case, but in the context of my argument here, it is sufficient to note the affinity between these two authors rather than to press their differences.

[7]Kuhn attaches a higher degree of fixity to the epistemic values of theory-choice than I would. He takes the five he describes to be "permanent attributes of science", provided the specification be left vague (1977, p. 335).

[8]This is where I would diverge from Putnam (1981), who otherwise defends a view of the role of value-judgement in science similar to the one outlined here. In the spirit of Kant, he wants to find a middle way between objectivism and subjectivism, between what he regards as the extremes of "metaphysical realism" and "cultural relativism". The former he defines as being based on "the notion of a transcendental match between our representation and the world" which he briskly characterizes as "nonsense" (1981, p. 134). Blocked from taking the epistemic values

to be the means of gradually achieving such a correspondence, he is thus forced to make them in some sense ultimates. "Truth is not the bottom line; truth itself gets its life from our criteria of rational accepta-bility" (p. 130). What he wants to stress, he says, is "the dependence of the empirical world on our criteria of rational acceptability" (p. 134). Instead of merely holding that "our knowledge of the world pre-supposes values" (the thesis that I am arguing for in this essay), he is led then to "the more radical claim that what counts as the real world depends upon our values" (p. 137).

But such a position leaves him (in my view) with no vantage-point from which it would be possible to correct, or gradually adjust, the epistemic values themselves. They constitute for him "part of our con-ception of human flourishing" (p. xi). But there can be many such con-ceptions; against Aristotle (whom he takes to defend a single ideal of human flourishing), he argues for a "diversity" of ways in which such flourishing might properly be construed (p. 148). But how then can he also reject some such ways as "wrong, as infantile, as sick, as one-sided" (p. 148)? What grounds are available in his system for such a rejection? He says that "we revise our very criteria of rational ac-ceptability" in the light of our "theoretical picture of the empirical world" (p. 134), but gives no hint as how this is to be done in prac-tice. He cites "coherence" as a sort of super-criterion which appears to be necessary to any ideal of human flourishing (p. 132). But what if someone were to reject such a criterion? Putnam says such a person is "sick". But are there arguments he could use to warrant this diagnosis?

I do not think that in the end this "middle way" works. The tilt to idealism is obvious. But it would take a more elaborate analysis to show this. (This footnote and footnote 3 were added in proof. Had I seen Putnam's book before I wrote this text, I would have attempted a fuller discussion of it.)

References

Agassi, Joseph. (1974). "Modified Conventionalism is More Comprehensive than Modified Essentialism." In Schilpp (1974). Pages 693-696.

Carnap, Rudolf. (1932). "Überwindung der Metaphysik durch logische Analyse der Sprache." Erkenntnis 2: 219-241. (As reprinted as "The Elimination of Metaphysics through Logical Analysis of Language." (trans.) Arthur Pap. In Logical Positivism. Edited by A.J. Ayer. Glencoe, Ill.: Free Press, 1959. Pages 60-81.)

--------------. (1950). "Empiricism, Semantics and Ontology." Revue internationale de Philosophie 4: 20-40. (As reprinted in Meaning and Necessity. 2nd ed. Chicago: University of Chicago Press, 1956. Pages 205-221.)

Duhem, Pierre. (1908). Σώζειν τὰ φαινόμενα: Essai sur la notion de théorie physique de Platon a Galilée. Paris: A. Hermann. (As reprinted as To Save the Phenomena: An Essay on the Idea of Physical Theory from Plato to Galileo. (trans.) E. Doland and C. Maschler. Chicago: University of Chicago Press, 1969.)

Farley, John and Geison, Gerald. (1976). "Science, Politics, and Spontaneous Generation in 19th Century France: The Pasteur-Pouchet Debate." Bulletin of the History of Medicine 48: 161-198.

Graham, Loren. (1981). Between Science and Values. New York: Columbia.

Hempel, Carl G. (1966). Philosophy of Natural Science. Englewood Cliffs, N.J.: Prentice-Hall.

Jeffrey, Richard. (1956). "Valuation and Acceptance of Scientific Hypotheses." Philosophy of Science 23: 237-246.

Kovesi, Julius. (1967). Moral Notions. London: Routledge and Kegan Paul.

Kuhn, Thomas. (1961). "The Function of Measurement in Modern Physical Science." Isis 52: 161-190. (As reprinted in Kuhn, Thomas. The Essential Tension. Chicago: University of Chicago Press, 1977. Pages 178-224.)

------------. (1962). The Structure of Scientific Revolutions. Chicago: University of Chicago Press.

------------. (1977). "Objectivity, Value Judgment, and Theory Choice." In The Essential Tension. Chicago: University of Chicago Press. Pages 320-339.

Lakatos, Imre. (1970). "Falsification and the Methodology of Scientific Research Programmes." In _Criticism and the Growth of Knowledge._ Edited by I. Lakatos and A. Musgrave. Cambridge: Cambridge University Press. Pages 91-195.

Laudan, Larry. (1981). "William Whewell on the Consilience of Inductions." In _Science and Hypothesis. (University of Western Ontario Series in Philosophy of Science._ Volume 19.) Dordrecht: Reidel. Pages 163-180.

Machamer, Peter. (1978). "Galileo and the Causes." In _New Perspectives on Galileo. (University of Western Ontario Series in Philosophy of Science._ Volume 14.) Edited by R. Butts and J. Pitt. Dordrecht: Reidel. Pages 161-180.

McMullin, Ernan. (1976). "The Fertility of Theory and the Unit for Appraisal in Science." In _Essays in Honor of Imre Lakatos. (Boston Studies in the Philosophy of Science._ Volume 39.) Edited by P. Feyerabend _et al._ Dordrecht: Reidel. Pages 395-432.

---------------. (1978a). "Philosophy of Science and its Rational Reconstructions." In _Progress and Rationality in Science. (Boston Studies in the Philosophy of Science._ Volume 58.) Edited by G. Radnitzky and G. Andersson. Dordrecht: Reidel. Pages 221-252.

---------------. (1978b). _Newton on Matter and Activity._ Notre Dame, IN: University of Notre Dame Press.

---------------. (1982). "The Role of 'Values' in Understanding Science." _Hastings Center Report_ 12(6): 38-40.

---------------. (1983). "Scientific Controversy and its Termination." In _Scientific Controversies._ Edited by A. Kaplan and H.T. Engelhardt. Cambridge: Cambridge University Press.

---------------. (forthcoming). "The Case for Scientific Realism." In _Scientific Realism._ Edited by J. Leplin. Los Angeles: University of California Press.

Nagel, Ernest. (1961). _The Structure of Science._ New York: Harcourt, Brace.

Polanyi, Michael. (1958). _Personal Knowledge._ London: Routledge and Kegan Paul.

Popper, Karl. (1934). _Logik der Forschung._ Vienna: Springer. (As reprinted as _The Logic of Scientific Discovery._ London: Hutchinson, 1959.)

------------. (1974). "Replies to My Critics." In Schilpp (1974). Pages 961-1197.

Putnam, Hilary. (1981). _Reason, Truth and History._ Cambridge: Cambridge University Press.

Rudner, Richard. (1953). "The Scientist _qua_ Scientist Makes Value-judgements." _Philosophy of Science_ 20: 1-6.

Schilpp, Paul A. (ed.). (1974). _The Philosophy of Karl Popper. (The Library of Living Philosophers._ Volume 14, Book II.) LaSalle, Ill.: Open Court.

Stevenson, Charles. (1949). "The Nature of Ethical Disagreement." In _Readings in Philosophical Analysis._ Edited by H. Feigl and W. Sellars. New York: Appleton-Century-Crofts. Pages 587-593.

Weber, Max. (1917). "Der Sinn der 'Wertfreiheit' der soziologischen und ökonomischen Wissenschaften." _Logos_ 7: 40-88. (As reprinted as "The Meaning of 'Ethical Neutrality' in Sociology and Economics." In _The Methodology of the Social Sciences._ (trans.) E.A. Shils and H.A. Finch. Glencoe, IL: The Free Press, 1949. Pages 1-47.)