Scientific value

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Abstract   *Criteria of scientific value are of different kinds. This paper concerns ultimate criteria, i.e. the axiology of science. Most ultimate criteria are multi-dimensional. This gives rise to an aggregation problem, which cannot be adequately solved with reference to attitudes and behaviour within the scientific community. Therefore, in many cases, there is no fact of the matter as to whether one theory is better than another. This, in turn, creates problems for methodology.*

1. Criteria of scientific value.

I assume that some scientific theories are better than others; they have, as we might say, a higher degree of *scientific value* (or epistemic value). For example, when we talk about scientific "progress", we seem to imply that later theories are better than earlier ones. Moreover, I assume that the scientific value of theories is somehow determined by, or supervenient upon, certain non-evaluative or natural characteristics that different theories may have in different degrees. These characteristics may be referred to as good-making characteristics or as the *criteria* of scientific value.

It is not obvious what these criteria are. A realist may suggest that truth or verisimilitude and explanatory power are the main criteria of scientific value, whereas an instrumentalist would emphasize empirical adequacy or something like problem-solving capacity. Thomas Kuhn says that

five characteristics - accuracy, consistency, scope, simplicity, and fruitfulness - are all standard criteria for evaluating the adequacy of a theory. [. . .] Together with others of much the same sort, they provide *the* shared basis for theory choice. (Kuhn 1977, p. 322)

With reference to Kuhn's account, Carl Hempel writes as follows:

In particular, scientists widely agree in giving preference to theories exhibiting certain characteristics which have often been referred to in the methodological literature as "marks of a good hypothesis"; I will call them *desiderata* for short. Among them are the following: a theory should yield precise, preferably quantitative, predictions; it should be accurate in the sense that testable consequences derivable from it should be in good agreement with the results of experimental tests; it should be consistent both internally and with currently accepted theories in neighboring fields; it should have broad scope; it should predict phenomena that are novel in the sense of not having been known or taken into account when the theory was formulated; it should be simple; it should be fruitful. (Hempel 1983, pp. 87-8)

Presumably, these statements by Kuhn and Hempel would be accepted by many scientists and philosophers of science, but they may be interpreted in different ways by different people. Similarly with criteria such as verisimilitude, explanatory power, and problem-solving capacity.

2. Ultimate, evidential, and strategic criteria.

Criteria of scientific value may be of different kinds. We should distinguish (1) between ultimate, evidential, and strategic criteria, and (2) between complete and partial criteria. Let me start with the first of these distinctions.

Ultimate criteria constitute the goal, or a goal, of science; they define scientific value. For example, verisimilitude or problem-solving capacity may be regarded be many people as ultimate criteria of scientific value. *Evidential* criteria are characteristics which are inductively related to (or more or less reliable signs of) those characteristics which constitute the goal of science. For example, observational success may be regarded by a realist as an evidential criterion, i.e. as something that is not necessarily of any scientific value in itself, but which is generally a sign or a symptom of something else, e.g. truth or verisimilitude, which has scientific value in itself. *Strategic* criteria are still less directly related to scientific value. If T_1 rates higher than T_2 with respect to a strategic criterion, this is no reason to expect T_1 to rate higher than T_2 (other things being equal) with respect to ultimate criteria; but it is a reason to expect that if T_1 is chosen rather than T_2, then the chances are better of finding good theories (i.e. theories which rate high with respect to ultimate criteria) in the future. I suspect that simplicity and fruitfulness, for example, may be regarded - at least by a realist - as strategic rather than evidential criteria. The fact that T_1 is simpler than T_2 may not be a reason for saying that T_1 is, in itself, a better theory than T_2. But it may be a good strategic reason for choosing T_1 over T_2 in a given situation; such a choice may increase our epistemic chances in the long run. Similarly with fruitfulness.

Evidential and strategic criteria are not usually distinguished. They are often classified, indiscriminately, as "methods" or "methodological rules". For example, when Larry Laudan (1984) distinguishes between the aims and the methods of science, "methods" seem to correspond to what I have called evidential and strategic criteria, whereas "aims" correspond to my ultimate criteria. Notice also that only the latter are strictly speaking good-making characteristics. Evidential and strategic criteria do not make theories good; they are only symptoms of, or in some sense guides to, something else that in turn makes theories good.

It may be wondered whether the criteria mentioned by Kuhn and Hempel are ultimate, evidential, or strategic. Hempel has suggested that they should be regarded as ultimate. He says:

The imposition of desiderata may be regarded, at least schematically, as the use of a set of means aimed at the improvement of scientific knowledge. But instead of viewing such improvement as a research goal that must be characterizable independently of the desiderata, we might plausibly conceive the goal of scientific inquiry to *be* the development of theories that ever better satisfy the desiderata. On this construal, the desiderata are different constituents of the goal of science rather than conceptually independent means for its attainment. (Hempel 1981, p. 404)[1]

Kuhn (1983, p. 565) has accepted this interpretation; he says that Hempel's formulation is an improvement on his own. On the other hand, some people seem to regard these criteria as evidential. For example, W. Newton-Smith (1981, p. 232) presents us with a list of factors which he regards as an extended version of Kuhn's list, and he says of them:

The factors relevant to theory choice in science are not constitutive of a good theory. The goodness of theories is constituted by their degree of verisimilitude. The factors are fallible inductive indicators of that. (1981, p. 225)

One factor mentioned by both Kuhn and Newton-Smith is simplicity. For Hempel and Kuhn, then, simplicity is constitutive of a good theory (in Laudan's terminology, it is an aim of science), whereas for Newton-Smith it is merely a "fallible indicator" of verisimilitude, which he regards as the aim of science. Like many others, I want to reject the idea that simplicity is an indicator of verisimilitude or truth, and hence an evidential criterion. If one does not want to follow Hempel and Kuhn in accepting it as an ultimate criterion, I suggest that one should regard it rather as strategic.

3. Complete and partial criteria.

Complete criteria of scientific value contain all the relevant factors and no ceteris paribus clauses. For example, Newton-Smith regards verisimilitude as a complete ultimate criterion of scientific value; according to him verisimilitude, and nothing but verisimilitude, is what constitutes the goodness of theories (see the quotation above). *Partial* criteria, on the other hand, contain an implicit or explicit ceteris paribus clause; they are relevant, but they may be outweighed in particular cases by other factors. For example, none of the five characteristics mentioned by Kuhn is regarded by him as a complete criterion, and neither is their conjunction. Kuhn does not regard them as jointly exhaustive. Similarly, the desiderata listed by Hempel are partial criteria.

It is important to notice, that even an exhaustive list of partial criteria is not the same as a complete criterion. The difference is, of course, that a simple list of partial criteria does not determine the relative *weights* of these partial criteria. For example, Kuhn's list of partial criteria does not tell us whether accuracy has the same weight as simplicity, or whether one of these is more important than the other. A given set of partial criteria may be aggregated in many different ways; if and only if the set is exhaustive, each of these ways corresponds to, or constitutes, a complete criterion.

4. Aggregation of partial criteria.

The distinction between a complete criterion of epistemic value and a complete list of partial criteria would not be of any practical importance if there were never any conflicts between the different partial criteria. But it seems that such conflicts may occur quite frequently. For example, if T_1 and T_2 are rival theories, T_1 may be less simple, but more accurate, than T_2. Another problem for someone who wants to apply criteria of this kind in practice is that they are often somewhat obscure or imprecise. Both these problems are emphasized by Kuhn. He says that

> two sorts of difficulties are regularly encountered by the men who must use these criteria in choosing, say, between Ptolemy's astronomical theory and Copernicus's, between the oxygen and phlogiston theories of combustion, or

between Newtonian mechanics and the quantum theory. Individually the criteria are imprecise: individuals may legitimately differ about their application to concrete cases. In addition, when deployed together, they repeatedly prove to conflict with one another; accuracy may, for example, dictate the choice of one theory, scope the choice of its competitor. (Kuhn 1977, p. 322)

I suggest that these "two sorts of difficulties" are essentially one and the same: it is the problem of aggregating several partial criteria. In particular, I would hypothesize that the "impreciseness" mentioned by Kuhn is mainly due to one or both of the following two factors. (1) It may be due to ambiguity or multi-dimensionality. For example, Kuhn himself (1977, p. 324) has pointed out that the simplicity criterion can be interpreted in different ways, so that Copernicus's theory is simpler than Ptolemy's in one sense, but equally simple in another. (2) Any remaining impreciseness is probably due to the possibility of giving different weights to partial criteria and to their different interpretations or dimensions. For example, simplicity may be given different weights by different people, and the two kinds of simplicity indicated by Kuhn may be aggregated in different ways depending upon the weights given to them.

Moreover, much the same problem may turn up in the case of several complete criteria. Such criteria may suffer from the same kind of impreciseness that Kuhn attributes to his partial criteria. They may contain different dimensions (or relevant interpretations) which may conflict with one another, and these dimensions must then be given weights in order for the conflicts to be resolved. For example, verisimilitude seems to contain at least two different and independent dimensions, viz. content and closeness to truth. The first of these dimensions concerns the proportion of "the whole truth" which is approximately captured by a theory. The second dimension would measure the relative distance, or the degree of approximation, between what the theory says about its subject matter and what is really true. Another example of a complete but multi-dimensional criterion would be problem-solving capacity. Here the number of problems solved, the importance of these problems, the number of new problems

created, and their importance would have to be weighed against one another,[2] and it is far from clear how this should be done.[3]

A central problem, then, is how to aggregate several partial criteria or several independent dimensions of one criterion (partial or complete). In order to see what is required for a solution to this problem, let us imagine that $C_1, C_2, \ldots, C_k$ are the relevant ultimate but partial criteria of scientific value (or the relevant dimensions of an ultimate and complete criterion), let W_i be a number which represents the relevant weight given to criterion C_i, and let $C_i(T)$ be a number which represents the degree to which a theory T satisfies criterion C_i (i.e., has the property C_i). We may then define the scientific value of a theory T, or V(T) for short, as a weighted sum:

$$V(T) = W_1 C_1(T) + W_2 C_2(T) + \ldots + W_k C_k(T)$$

and, in accordance with this, we may say that one theory, T_1, is better than another theory, T_2, if and only if $V(T_1) > V(T_2)$.

Obviously, in practice we do not use numerical measures of this kind when we estimate and compare the scientific value of theories. But the formula given above illustrates the problems involved. There are three different problems. First, we need an *exhaustive* list of relevant partial criteria. Second, we need a *weight* W_i for each of these criteria. Third, we need for each criterion an *interval scale* C_i on which different theories can be compared. The scales have to be interval scales for addition to make sense. (Of course, W_i and C_i may be combined into a single function. However, there are really two problems involved here. To determine the weights is one thing; to construct the interval scales is another.)

In practice, we may never have more than very rough and partial solutions to these problems. The questions I want to raise in the remainder of this paper are the following: Can we do better? Should we try to do better? If so, how? Or is it perhaps hopeless and/or undesirable to strive for more precision here? If so, does this undermine the notion of scientific value?

It should be noticed, that these questions may have quite different answers depending upon whether the criteria in question are ultimate or non-ultimate. If we have access to a justified complete and ultimate criterion of scientific value, the justification and aggregation of partial non-ultimate criteria would seem to be an ordinary scientific or empirical problem. Roughly speaking, what is needed then is just an investigation into various means-end relations or an analysis of certain statistical data which may be provided by historians of science. There is nothing particularly philosophical about that. On the other hand, to justify and aggregate ultimate criteria is philosophically much more problematic. This is what I am interested in. The distinction may be described as the distinction between the *methodology* and the *axiology* of science. Methodology is concerned with evidential and strategic criteria, whereas axiology is concerned with ultimate criteria. This terminology is adopted, e.g., in a paper by Laudan, where he also says that

> the axiology of inquiry is a grossly underdeveloped part of epistemology and philosophy of science, whose centrality is belied by its crude state of development. Methodology gets nowhere without axiology. (Laudan 1987, p. 29.)

I am inclined to agree. In particular, it seems quite impossible to solve the aggregation problem in methodology (i.e. for non-ultimate criteria) unless a fairly precise solution has already been given to the aggregation problem in axiology (i.e. for ultimate criteria). Anyway, this paper is concerned with the aggregation problem as it occurs in the axiology of science.

5. Practical skill.

It might be held that, in connection with criteria of scientific value, we have no need for anything so precise as numerical weights and interval scales, since good scientists tend to develop a practical skill which allows them to aggregate partial criteria without an

explicit algorithm. Something like this is held e.g. by Newton-Smith. He points out that "the scientist has to exercise judgment concerning the relative weight to be attached to the conflicting [methodological] rules", but he also holds that "the success of the institution of science gives us reason to have faith in the faculty of judgment, the exercise of which lies at the very heart of [scientific method]" (1981, p. 270). The practical skill of a good scientist is similar to that of a competent wine blender; in both cases the goods are delivered even though no one is able to articulate any precise rules for how this is done (see 1981, pp. 209, 225, 234, and 270).

This has a certain plausibility in the case of evidential and strategic criteria (which is what Newton-Smith has in mind), for in this case there is an independent goal of science (i.e., some ultimate criterion) which can serve as a standard for distinguishing skilful or sound judgments from unskilful or unsound ones. *If* science has been successful and progressive, relative to a given ultimate criterion, *then* we may conclude that the decisions, involving aggregation of partial evidential and strategic criteria, which have taken place throughout the history of science have been skilful (sound) to precisely this extent. But we cannot reason in the same way for partial criteria which are ultimate. For in this case there is no independent way of telling whether a given aggregation is skilful or not.

Suppose e.g. that verisimilitude is a complete ultimate criterion of scientific value, and that we have to aggregate different dimensions in order to decide whether T_1 has a higher degree of verisimilitude than T_2. If the scientific elite eventually agrees that T_1 has a higher degree of verisimilitude than T_2, we can hardly say that their judgment is skilful (sound, reliable, correct) because science is successful, for in order to know whether science is successful we must already know how to aggregate the partial criteria involved in verisimilitude.

In other words, we have no reason to suppose that scientists have a practical skill when it comes to aggregating ultimate criteria. No doubt they *do* (in a sense) aggregate ultimate criteria as well as non-ultimate ones. But the fact that they do this does not by

itself guarantee that they do it well or correctly or skilfully. So why should we trust their judgments?

6. The role of disagreement.

Besides, as pointed out above, different scientists tend to aggregate partial criteria in different ways. According to Kuhn, this is very fortunate, since it permits a certain division of labour which is essential for the development of science. He claims that scientific progress ordinarily

> *requires* a decision process which permits rational men to disagree, and such disagreement would be barred by the shared algorithm which philosophers have generally sought. (Kuhn 1977, p. 332)

The point is that the transition from T_1 to T_2 requires a period during which some scientists continue to work within the old theory T_1, while others simultaneously work within the new T_2. A new theory will not be accepted by the scientific community until it has been developed and tested to a certain extent. At the same time, it must be investigated whether the difficulties which motivated T_2 can in fact also be handled by the old T_1. In many cases, according to Kuhn, T_1 can indeed do the job, and T_2 does not survive (see 1977, p. 332). But the division of scientific labour which is required during such a period of competition would not obtain if an algorithmic decision procedure were available, by which it could be determined by everyone whether T_1 or T_2 is better than its rival.

This argument of Kuhn's suggests that we should be grateful that there is no precise and general solution to the problem of how to aggregate partial criteria of scientific value. It might seem that such a solution would be a threat to scientific progress. But this would be a mistake. Two points should be made here.

(1) The division of labour that is so desirable according to Kuhn, would no longer be desirable if an algorithm were available. Presumably, it is a good thing that people work with rival theories just because it cannot be known, in advance, which one is the best. If it could be known in advance that one of two rival theories is better than the other, then there would be no reason to continue to work with the second-best theory.

(2) More importantly, however, a precise and general solution to the aggregation problem is not the same as an algorithmic decision procedure. We have a solution to the aggregation problem if we have an exhaustive list of partial ultimate criteria with corresponding weights and interval scales. An algorithmic decision procedure, on the other hand, would include more than this. It would have to include a mechanical method for deciding to what extent a given theory satisfies—or will satisfy—certain criteria. Even if we have a general solution to the aggregation problem, it may be very difficult to find out, in a particular case, whether one theory is better than another. This may require a lot of further work with both theories. We may be completely unable to determine, in advance, whether one theory, if fully developed, would be better than another theory, if fully developed.

In other words, division of scientific labour seems a reasonable strategy because it cannot be known in advance how far a theory can be developed, and to what extent it will then satisfy the relevant criteria of scientific value. But this consideration would apply whether or not we have a precise solution to the aggregation problem. Therefore, Kuhn's observation provides no ground for not attempting to find such a solution.

7. Consensus.

However, it might be held that no precise and general solution to the aggregation problem is needed, since scientists are usually able to reach a consensus without it. It may be that, in every actual case, different scientists tend at first to aggregate partial criteria in different ways, but after some time their different judgments will converge towards a consensus. This is essentially Kuhn's picture. We have got along quite well without a general solution to the aggregation problem so far, and as long as we can reach consensus anyway, why should we not be satisfied with the usual imprecise criteria of scientific value?

One answer to this question is that consensus does not imply correctness. After a period of arguing back and forth, the scientific community may reach an agreement that T_1 is better than T_2, but this is hardly a guarantee that T_1 *is* better than T_2. The

consensus can be explained without the assumption that T_1 is better than T_2; hence, we cannot infer from the consensus to the assumption. (In fact, it is highly doubtful whether this assumption, being a value judgment, can play any role at all in a scientific explanation; and as long as an explicit solution to the aggregation problem is missing, it cannot be replaced by a naturalistic description.)

On the contrary, the consensus in question is easily explained by reference to ordinary psychological mechanisms. As long as a group of people believe that there is a correct answer to a question, they tend to work their way to a consensus even if there is no evidence one way or the other, and even if many different answers are in fact equally possible. Consider e.g. the classic experiments with the so-called autokinetic effect conducted by the social psychologist Muzafer Sherif (1936). When a spot of light is projected in a totally darkened room it will appear to move, but different individuals will perceive very different movements. When they are asked to describe the movement of the spot to the other members of a group, however, their judgments soon tend to converge towards a group norm. It seems that a consensus concerning the aggregation of partial criteria may arise in a similar way.

8. Idealism.

The argument just given might be rejected. In particular, it might be supposed that consensus does guarantee correctness after all, for could we not say that consensus within the scientific community is precisely what *constitutes* the truth condition of a scientific value judgment? One sometimes gets the impression that this is indeed what Kuhn means. For example, he says in one place that "in matters of theory-choice . . . trained scientists are . . . the highest court of appeal" (Kuhn 1970, p. 234). This might be taken to imply that T_1 is better than T_2 if there is a consensus within the scientific community (in the long run, or at the time in question) that T_1 is better than T_2. This is a kind of idealism: the facts depend upon what people think the facts are.

Surely, however, there is something odd about this view: it can hardly be the case that T_1 is better than T_2 *because* the scientific community thinks so. The reason the

scientists have for preferring T_1 is hardly the fact that they themselves prefer T_1. So perhaps what Kuhn means is simply that trained scientists are the best qualified to *find out* whether one theory is better than another. This is a claim that many people would accept. One should assume that Kuhn has something less boring in mind.

It might be held that a consensus within the scientific community can indeed support a certain kind of epistemic value judgments. In particular, we should distinguish here between *particular* and *general* value judgments. Particular value judgments are concerned (only) with particular cases; they claim that a particular theory is (or is not) better than a certain rival theory (or certain rival theories). General value judgments are concerned with properties which may be exemplified in many different cases; thus e.g. to state an ultimate criterion of scientific value is to make a general value judgment. In terms of this distinction it might now be suggested that a consensus within the scientific community constitutes the truth condition of—or explains the truth of—general value judgments, but not of particular ones.

For example, if there is a consensus within the scientific community (at a given time, or in the long run) that quantum theory is better than Newtonian mechanics, this fact is not what makes it true that quantum theory is better. In principle, this particular value judgment may even be wrong. Rather, if quantum theory is better, it is better because it is more accurate, more fruitful, and so on. On the other hand (it might be held), if criteria like accuracy and fruitfulness are ultimate criteria of scientific value, they are so just because there is a consensus within the scientific community concerning the general value judgments that accuracy and fruitfulness are ultimate criteria of scientific value.

However, idealism does not seem quite acceptable for general value judgments either. In particular, it seems that we should allow for the possibility of false consciousness among scientists; it is not inconceivable that the properties which are explicitly recognized as ultimate criteria within the scientific community are actually not those that characterize preferred theories. The reason for this might be that scientists sometimes or often have mistaken beliefs, even in the long run, about the relations that actually hold between rival theories. For example, many scientists may hold that truth is

the goal of science, and this claim may be partly caused by the belief that later theories are in general closer to the truth than earlier theories. But the latter belief may be false; later theories may not in fact be closer to the truth. If this is so, the general value judgments which are explicitly accepted by scientists would not correspond to actual scientific development. And perhaps such development should be more decisive for the identification of ultimate criteria than the beliefs and attitudes of scientists.

There is another problem with idealism. We have no guarantee that all scientists think alike. On the contrary, we may expect that different scientists have rather different ideas about what the ultimate criteria of scientific value are, and about the relative weights of these criteria. Even if there is always—sooner or later—a consensus about particular cases of theory-choice, there may be very little consensus about general criteria and their relative weights. One reason for this is that general criteria and weights may be underdetermined by particular value judgments. Another reason is that the acceptance of particular value judgments may often be causally determined, not only by the acceptance of general criteria, but also by all kinds of psychological and sociological mechanisms which have little to do with rational argument.

Besides, in order to find out whether there is a consensus within the scientific community, we need to know what "the scientific community" is. We need to know whether there can exist several different scientific communities simultaneously, one for each scientific discipline or for each sub-discipline, say. If there can be only one scientific community at any given time, should that be taken to include the social science and the humanities as well as the natural sciences? And how far should the scientific community extend over time? These questions are hard to answer. It may be expected that the more inclusive a "scientific community" is taken to be, the less are the chances that there will be a consensus concerning ultimate criteria and scientific value within it.

9. Social constructivism.

Perhaps the goal of science is to produce whatever it is that scientists do in fact produce. In other words, the good-making characteristics of scientific theories may be those properties which tend to characterize later rather than earlier theories. This means that scientists, in replacing old theories by new ones, also construct the goal of the social practice in which they are engaged. This view may be called "social constructivism".

Notice, that according to social constructivism, science is *necessarily* progressive. This is perhaps somewhat surprising, but it corresponds quite nicely, for example, to Kuhn's declaration that his remarks about scientific development should be read both as descriptions and as prescriptions, and that "scientists should behave essentially as they do if their concern is to improve scientific knowledge" (1970, p. 237).

However, there are at least three problems with social constructivism in this area. (1) It might well happen that some properties which characterize later theories are not really good-making properties. For example, it might turn out that, in a majority of cases, later theories are mathematically more complicated than earlier ones, but it seems absurd to claim that this shows that mathematical complication is in itself a goal of science. At most, it may be a means to some scientific goal. So how do we distinguish ultimate criteria, on the one hand, from non-ultimate criteria and contingently correlated properties, which are not criteria at all, on the other?

(2) Besides, it seems very difficult to state more precisely what is meant by the requirement that ultimate criteria "characterize later rather than earlier theories". For a property may satisfy this requirement, in the relevant sense, even if later theories very often have less of it than earlier theories. Consider simplicity, for example. As we have seen, simplicity may be a partial ultimate criterion of scientific value even if the scientific community often prefers theories which are less simple than their rivals. This is explained by the fact that the preferred theories have other virtues (accuracy, fruitfulness, etc) which outweigh simplicity in these cases.

(3) Finally, it seems to me that the goal of science should be defined in such a way that it is at least possible for the scientific community to go wrong in some cases, perhaps even in many cases. If this is not possible, it is hard to see why theory-choice

should be generally regarded as such an important matter. But it is not clear how the possibility of mistakes should be accommodated within the frame-work of social constructivism.

In order to solve or avoid these problems it seems that we will have to appeal to beliefs or attitudes within the scientific community. Perhaps a modified version of idealism is reasonable after all. More precisely, it seems to me that some combination of idealism and social constructivism is plausible in the axiology of science. For example, we might say that something is an ultimate criterion of scientific value if it satisfies the following two conditions: (i) it is regarded as such within the (relevant) scientific community and (ii) it is an essential part of some possible complete criterion which is in fact satisfied to a higher degree by later theories.

However, even if this is plausible, some possible confusion can be avoided if a further distinction is noticed.

10. Actual and ideal goals of science.

We may distinguish between what *is* and what *ought* to be the goal of science—or between what is and what ought to be an ultimate criterion of scientific value. The first may be called an "actual" goal; the second an "ideal" goal. These are conceptually different even if we may be so lucky that they happen to coincide in practice—at least at certain times, or in certain places. It is not that the former has no normative function. If you want to be a good scientist you should of course try to achieve the actual goal of science. (Compare Kuhn's words quoted above: "scientists should behave essentially as they do if their concern is to improve scientific knowledge".) But the actual goal of science (at a certain time) may be criticized and be replaced by something better. For example, Laudan claims that one may criticize certain scientific goals for being unrealizable or for failing to "accord with the values implicit in the communal practices and judgments we endorse" (1984, p. 50). I take it that such criticism is not meant to show that an alleged goal is not in fact a goal; rather, it is meant to show that an actual goal ought to be rejected and that some other goal ought to be accepted instead.

An *actual* goal of science, then, is determined by actual scientific practice and/or by dominant attitudes within the scientific community. On the other hand, an *ideal* goal of science is something that ought to be an actual goal of science. The reasons for accepting something as an ideal goal of science will of course have to be extra-scientific. For example, they may be reasons of utility. (It is possible that several distinct goals are ideal; it may be possible to construct more than one complete ultimate criterion of scientific value each of which is ideal in e.g. utilitarian terms. This would be a complication for someone who wants to identify (ideal) scientific value with closeness to the ideal goal of science. But I find such an identification implausible, partly for the very reason that this complication may occur.)

Some people may dislike the thought that the ideal goal of science should be determined by extra-scientific, perhaps even utilitarian, considerations. For example, it seems that many scientists and philosophers believe that scientific knowledge has *intrinsic* value, i.e. a kind of value which is independent of consequences and other more or less contingent relations, a kind of value that something may have "in itself" or "for its own sake". Presumably, someone who holds this view will also believe that some pieces of knowledge are intrinsically better than others. One might then go on to suggest that the ideal goal of science is to produce scientific knowledge (and/or scientific belief?) that has as much intrinsic value as possible. This would still be different from the actual goal of science, since intrinsic value would have to be independent of beliefs and practices within the scientific community. However, as I have argued elsewhere, scientific knowledge has no intrinsic value (Bergström 1987, pp. 53-63 and Bergström 1994, pp. 507-11). A further argument for this thesis might be the very fact that the alleged intrinsic value of scientific knowledge would be independent of the actual scientific value of what is known. This is surely odd, but it seems unavoidable as long the value in question is intrinsic.

One might wish to postulate that an ideal goal has to be something that it would not be irrational for scientists to try to achieve. For example, Laudan (1984, pp. 53-60) suggests that it would be irrational to pursue truth if we cannot ascertain that one theory

is closer to the truth than another, and that it would be irrational to regard it as an aim of science to find theories which make no claims about unobservable entities if one also believes that the best theories available at present make such claims. These examples seem to me to be rather dubious. For example, it may not be irrational to pursue truth for someone who *believes* that we can find out whether we approach it. However, Laudan is certainly right in holding that it may be irrational for a person to accept a certain value judgment (e.g. about the goal of science) under certain conditions (e.g. when he or she also has certain other beliefs and acts in certain ways). But this does not really disqualify anything from being an ideal goal of science. Whether it puts any restriction on what might count as an actual goal is perhaps more doubtful.

In any case, when we wonder whether or not one theory is better than another (from a scientific point of view), what is relevant is the actual goal of science, not the ideal goal. If T_1 is better than T_2 according to some ideal ultimate criteria of scientific value, but worse than T_2 according to the actual ultimate criteria, then it would be a mistake, and bad science, to prefer T_1 to T_2 (other things being equal). I have mentioned ideal goals of science here only to make the point that they are irrelevant for questions of scientific value. What determines scientific value is the attitudes and the behavior of scientists.

11. Nihilism.

The question, then, is whether the attitudes and behavior of scientists could determine a general solution to the aggregation problem. This seems extremely doubtful.

We have seen that the first part of such a solution can perhaps be found: we may be able to state what the relevant ultimate criteria are. More precisely, we may be able to state a necessary and sufficient condition for something to be a partial and ultimate criterion, but we may not be able to list all properties which in fact satisfy this condition. However, let us suppose that we can find such a list of criteria.

Our next step would be to construct interval scales for these criteria. Whether or not this is possible should depend to some extent upon what the criteria are. For example, if

they are similar to the criteria suggested by Kuhn, it is probably very difficult to construct such scales. (As far as I know, nothing like this has been constructed so far.) Besides, there is the danger that "rational reconstructions" of this kind would no longer be relevant according to the combination of idealism and social constructivism suggested above.

However, let us imagine that the scaling problem has been solved. Can we then determine the relevant weights? As far as I can see, the only way to do this would be to use something like the ordinary psychological methods for attitude measurement in order to determine the average weights actually given to the relevant criteria by the scientific community. Basically, we just ask individual scientists in a representative sample to compare the weights on some suitable scale, and we may then average over all individuals in the sample. This can be done, but a rather serious problem here would be to decide when a group of people is, for our present purposes, a representative sample.

The sample would have to be representative for some larger group. But what larger group? If we take the larger group to consist of all scientists at all times, we have simply ruled out the possibility that the (actual) goal of science may vary over time and from one discipline to another. This would be contrary to what many philosophers and historians of science claim. For example, Kuhn writes:

> If the list of relevant values is kept short (I have mentioned five, not all independent) and if their specification is left vague, then such values as accuracy, scope, and fruitfulness are permanent attributes of science. But little knowledge of history is required to suggest that both the application of these values and, more obviously, the relative weights attached to them have varied markedly with time and also with the field of application. (Kuhn 1977, p. 335)

Hence, there should be several different larger groups, i.e. different scientific communities, and these should be such that there are only small differences in attitude

within the groups and larger differences in attitude between groups. But this condition is vague. It seems quite likely that all kinds of differences occur, so that there is no clear boundary between small and large differences. Hence, we may expect that the condition can be satisfied in many different ways, and these will in turn determine different average weights.

These considerations indicate that the attitudes and behaviour of scientists underdetermine, and thus cannot determine, a solution of the aggregation problem. Moreover, since nothing else can determine such a solution (given a combination of idealism and social constructivism like the one suggested above), there simply is no solution. If this is correct, it seems that many particular value judgments of the form "T_1 is a better theory than T_2" are neither true nor false: there is no fact of the matter here. This is a form of *nihilism*. It is still possible that some judgments of this form, as well as many general value judgments concerning scientific theories, have truth-values. Hence, the nihilism in question is partial rather than total.

12. Consequences of nihilism.

If my argument is correct, there may often be no correct answer to the question of whether one scientific theory is better or worse than another. This need not prevent scientists from reaching a consensus in situations of theory choice. Sherif's experiments (mentioned above) exemplify that a consensus need not correspond to any fact of the matter whatsoever. In the case of scientific value, it seems that there are some facts of the matter, but that these leave many matters undecided. Does this matter?

It seems to matter in at least three ways. (1) First, if this kind of partial nihilism were accepted by the scientific community, it might put an end to science as we know it. As a matter of fact, scientists do discuss the relative merits of rival theories and try to find out which one is best. This practice seems to most of us to be important, but it would appear to be completely pointless if there is nothing to find out. However, if it were given up, it is hard to see that anything remotely similar to science as we know it could

survive. So even if partial nihilism is true, perhaps this should not be announced to the scientific community.

(2) Moreover, it might be held that actual theory choices are relevant to the determination of scientific value only if they can be taken as fairly reliable symptoms of correct particular value judgments. But if such value judgments are usually not correct (i.e. neither correct nor incorrect), it seems unnecessary to pay any attention to actual theory choices. If so, social constructivism should perhaps be given up completely, in which case we are back to a pure idealism concerning scientific value.

But in this case, idealism should perhaps also be accepted for particular judgments. This means that nihilism can be avoided after all. We might say, roughly speaking, that as far as scientific value is concerned, the majority is always right. If the majorities of different communities disagree, we get a form of *relativism*. Every majority is right from its own point of view.

It might be held that what is important here is that the general and the particular judgments of scientific value within a community are brought into agreement with one another so that a certain degree of coherence or equilibrium is achieved. However, this requirement is extremely problematic if there is no general solution to the aggregation problem. In the absence of such a solution, many different particular judgments are compatible with the general judgments. The requirement would be almost empty. Accordingly, coherence (or consistency) would not provide any justification.

(3) Finally, it seems that partial nihilism of the kind outlined here would create great problems for methodology. There are two points to be made. First, it is not clear that non-ultimate criteria can be justified at all if there does not exist a fairly precise complete ultimate criterion (which in turn seems to presuppose a general solution to the aggregation problem). If there is no clear goal, how should one decide what means would lead to it? There may of course be non-ultimate criteria connected with various partial and ultimate criteria, but these non-ultimate criteria may conflict in many cases, and in such cases they would give no clear guidance.

Second, if one methodology (i.e. one set of non-ultimate criteria) could be justified, there may be other rival methodologies that can be equally justified. This would be awkward. Laudan says in one place that "there are several rival methodologies in the market and they cannot all be right" (1977, p. 19). But if the goal of science is multi-dimensional, and if the different dimensions (partial ultimate criteria) cannot be aggregated to a single dimension, different sets of non-ultimate criteria may be equally "right". They may be equally right in the sense that they are equally good means to various goals, none of which is any worse than the others from a scientific point of view.

I conclude that nihilism of the kind described here has some rather unattractive consequences. But it is hard to see how it can be avoided. And it should be noticed that partial nihilism does not rule out scientific progress. For it allows the possibility that the last member of a sequence of theories is better than the first, even though no member is better than its immediate predecessor.[4]

Notes

1 See also Hempel (1983, p. 91). A similar point is made in Bergström (1980, pp. 2-3).

2 Compare Laudan (1977), p. 68.

3 See e.g. the discussion in McMullin (1979), pp. 637-43.

4 Earlier versions of this paper have been presented at Balliol College, the Graduate Center of CUNY, the University of Helsinki, Columbia University, the Inter-University Center in Dubrovnik, the University of Munich, and the Academy of Science in Moscow. I am grateful for comments made by various people on these occasions, as well as for comments on a written version by Henrik Bohlin, Richard Creath, Dagfinn Føllesdal, Matthias Kaiser, Peter Lipton, Wolfgang Balzer, Hugh Mellor, and Peter Pagin.

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