Appeared in: Heikki J. Koskinen et al. (eds.) *Science – A Challenge to Philosophy?*
Peter Lang GmbH, Frankfurt am Man, 2006.

The scope and limits of value-freedom in science

Panu Raatikainen

1. Introduction

The issue of whether science is, or can be, value-free has been debated for more than a century. The idea of value-free science is of course as old as science itself, and so are the arguments against this idea. Plato defended it against Protagorean relativism and skepticism. Bacon warned about "idols" that could divert scientific inquiry. Nineteenth century writers often used the term "ideology" as a sort of code word to underline the threat to the objectivity of science posed by the intrusion of values alien to science proper (cf. Proctor 1991).[1]

However, the dispute truly began with Max Weber's classical defense of value-freedom and the neutrality of social science in particular (see Weber 1904, 1917, 1919, 1949). For Weber, the fundamental issue was that objective knowledge was possible in social science. Accordingly, the arguments against value-freedom have often included the claim that scientific research and its results are so thoroughly value-laden that objective knowledge is not at all possible in science.

Weber wanted to defend the autonomy of science with respect to religion and politics. As is often noted, value-freedom for Weber was, at least in part, a shield against the accusations of socialism in social science. But it is important to note that Weber also wanted to protect values from *scientism*, that is, he opposed the view that one can derive all values from science — that science can answer all value questions. According to him, scientists should not say categorically what one ought to do.

In Weber's view, the scientist qua scientist should not make categorical value judgments. He was reacting to the "ethical economics" — popular at the time — which was quick to give all sorts of recommendations on what one should do. Weber, on the other hand, emphasized that one should keep science and politics separated, so that one would not disguise politics in science's clothing. He compared science to a map: it cannot tell us where to go, but it can tell us how to get there. One cannot derive ultimate values from scientific facts. Weber, however, admitted that values may affect the choice of a research problem.

Logical positivists defended a strong form of the value-freedom thesis. They held that moral statements are radically different from factual statements, as the former do not even have cognitive content — they endorsed emotivism in ethics. But of course, acceptance of the idea of the value-freedom of science does not require this radical view. In any case, logical positivism

thus differed from classical French positivism (e.g. Comte), which rather adhered to scientism, which is incompatible with the idea of value-free science.

Nazism, on the other hand, pressed that science is not, and should not be, value-free. According to it, there was Jewish science, and there was Aryan science. The ideal of value-freedom was also denied by the leftist Frankfurt critical school, by e.g. Horkheimer, Adorno, Marcuse — and later Habermas. So it was also in the Soviet Union, where one had to take a side between "bourgeois science" and "proletarian science". In 1948, Lysenko's totally unscientific theory of the inheritance of acquired characteristics was endorsed as the official state biology of the Soviet Union, and Mendelian genetics, as well as relativity theory and quantum mechanics, were denounced.

Richard Rudner (1953) famously argued against Weber that the scientist *qua* scientist does make value-judgments in a typically ethical sense. For example, if the hypothesis under consideration were to the effect that a toxic ingredient of a drug was not present in a lethal quantity, we would require a high degree of confirmation, whereas in some less critical case we may accept a hypothesis on the basis of a lower degree of confirmation. In general, Rudner stated, before a scientist can accept any hypothesis, the value decision must be made in the light of how serious it would be to make a mistake, and whether the evidence is strong enough to warrant its acceptance. How great a risk of being wrong one is willing to take will depend upon how seriously in the typically ethical sense one views the consequences of making a mistake, Rudner concluded.

The possibility of value-freedom was also questioned by Charles Taylor (1967). He argued that political science necessarily requires theoretical frameworks which always implicitly support some values. In his view, to say that something fulfills human needs and wants always constitutes a reason for calling it good. According to Taylor, political science cannot be morally neutral.

Sociologist Alvin Gouldner (1962) complained that "value-freedom" had become a hollow catechism, an excuse to not think seriously about social problems — that "value-freedom" had come to mean a willingness to sell one's skills to the highest bidder. According to Gouldner, "value-freedom" allowed one to believe that it is no worse to work to spread disease than to cure it; value-freedom meant that there was nothing wrong with doing market research for tobacco companies to help them sell cigarettes.

More generally, the extreme interpretation of "value-freedom" (it is hard to find anyone to defend it explicitly, but certainly the view has been out there) holds that, first, it is valuable to pursue any research problem — that all knowledge is positive — so it does not matter what research problem you choose, and, second, that a scientist is not responsible for the possible applications of his or her research, but that the responsibility lies with those who apply it in practice. In any case, this is not what Weber and the others following him defended as the value-freedom of science.

2. Some distinctions

The value-freedom of science has clearly meant different things to different people. I submit that one can only make progress in answering the question of whether science is value-free by making several clarifying distinctions.

First, it is important to distinguish *epistemic values* (such as explanatory power, simplicity, and empirical support) from *non-epistemic values* (e.g. moral and political values). Obviously, scientists are committed to such values as scientific ethos (in Merton's sense), honesty and regard for facts. More generally, science is characterized by the values of objectivity, openness, criticality and publicity. And certainly, epistemic values such as probability, simplicity, explanatory power, or support from observations are effective in the acceptance of theories and hypotheses.

None of this is really controversial, although such trivialities are sometimes offered as evidence for the conclusion that science is not value-free. In particular, such values in no way threaten the objectivity of science. The real and interesting question of the value-freedom of science therefore concerns the possible effect of non-epistemic values on science (and *vice versa*).

What exactly are the epistemic values of science and how they function is, of course, controversial, and the issue is debated in the philosophy of science. But it is clear that they are essentially different from such non-epistemic values as moral or political values. (See e.g. Levi 1967, Kuhn 1977, McMullin 1983, Laudan 1984, Niiniluoto 1984, 1993)

Second, one should keep in mind that "science" is an ambiguous term; by it, one may mean, for example: (1) science as an institution (scientific workers, the organization of research), (2) scientific research activity, or the research process, (3) results generally accepted in the scientific community at a particular moment; scientific knowledge, and (4) the scientific method as a critical and intersubjective method for accepting beliefs. (cf. Niiniluoto 1984, ch. 1).

Science understood as "the scientific method" is a strongly normative concept (although not in any moral sense) and not everything that claims to be science or is even widely taken as science really counts as science in this sense. Consequently, one can also understand the other senses of "science" in a normative way, e.g. delimit "the scientific institution" to only those instances that truly follow the scientific method. In that case, a community of investigators ceases to be a *scientific* community if it gives up, or is forced to give up, its autonomy and neutrality (cf. Niiniluoto 1984, ch. 1).

Alternatively, more external criteria can be used to limit the definition, so that workers whose work does not really satisfy the requirements of good science still count, if they are natural parts of the same institution of science as a whole. The same applies, *mutatis mutandis*, to the notions of "science" as research activity and as scientific knowledge, or to the results of science.

But especially in the case of the results of science, it may be more natural to think that only the outcomes of a critical discussion within the scientific community may be tentatively accepted as

the results of inquiry. In this sense, the real "subjects" of scientific knowledge are the scientific communities rather than individual scientists, as has been emphasized by e.g. Peirce, Weber and Popper (cf. Niiniluoto 1984, ch. 1).

Third, it is important to understand the difference between *basic research* and *applied research*. The traditional philosophy of science often totally ignores applied sciences. Various critical thinkers, on the other hand, assume that all science is applied, and thus at least implicitly deny the very existence of basic research.

The classical characterization of the distinction was presented by the OECD in 1966: Basic research is defined as "the systematic pursuit of new scientific knowledge without the aim of specific practical application", and applied research as "the pursuit of knowledge with the aim of obtaining a specific goal"; further, "development" uses the results of research to develop "new products, results, and means of production" (OECD 1966). This is certainly quite vague, for it is unclear whose aims are relevant here. Nevertheless, there are certainly clear enough examples of each; in Kitcher's words, "we can contrast extreme cases .... When any links to practical projects are buried in a distant past, with no bearing on contemporary applications, and when it's very hard to forecast how results from this inquiry could be adapted to novel practical problems, the researchers can quite legitimately declare their intentions to be thoroughly epistemic" (Kitcher 2000, p. 89).

Niiniluoto (1993) has presented a less vague approach: He suggests that the old basic-applied distinction is less fundamental than the distinction between what he calls *descriptive sciences* and *design sciences*. Descriptive sciences primarily aim to describe, explain and understand the reality surrounding us. Design sciences, on the other hand, aim at knowledge that is useful for the activity of design, i.e. aim to enhance human art and skill. The nature of design sciences can be schematically illuminated with "technical norms" (this notion is from G. H. von Wright, 1963):

> If you want A, and you believe that you are in a situation B,
> then you ought to do X.

Technical norms express the typical structure of the knowledge provided by design science. Such norms are factual, true or false statements about the relation between means and ends, and can be objective scientific results just like descriptive statements. One can accept or reject such norms through normal scientific methods.

Technical norms often follow from the corresponding causal claims:

> X causes A in circumstances B.

Statements of this form are typically results of ordinary descriptive science. In such a case, a design science really is applied science. In other cases, however, no general theory is at hand, and a technical norm is obtained by more practical trial-and-error procedures and experimental tests. Thus, not all design science is applied science. Design sciences are based upon manipulable causal relations in which we can do or bring about X. That many causal relations are not humanly

manipulable — one can think of astronomy — makes it clear that purely descriptive science also exists. Note, however, that the distinction between descriptive science and design science cuts across disciplines (see Niiniluoto 1993).

And fourth, the scientific research process has different phases. We can roughly divide it into three possible different phases:

(1) the choice of research problem;
(2) the acceptance of results; and
(3) the possible practical applications.

One can attribute value-freedom or value-ladenness to any or each of these. Originally, Weber only meant his thesis of value-freedom to apply to phase (2), and defended the objectivity requirement concerning results. Sometimes, however, both scientists willing to avoid any responsibility for the applications of their work and critics of the idea of value-freedom have assumed that the thesis of value-freedom applies to all these phases.

In consequence of all the above, the question of the value-freedom of science is multiply ambiguous.

3. Conclusions

With these distinctions at hand, let us now return to our main questions about the possibility and limits of the value-freedom of science. One can recap the content of Weber's original idea of value-freedom with the following two theses:

> **Objectivity Requirement:** In science, one should not, in accepting or refuting hypotheses, appeal to any non-epistemic (such as religious, political or moral) values.
>
> **Anti-Scientism:** Ultimate values (more exactly, unconditional value judgments) cannot be derived from the results of science.

I think that once the issue is clearly understood, the implausibility of scientism should be quite obvious. It is simply inconceivable that, say, human rights would be some day be considered to be unconditional facts of science. Likely assumptions of the contrary are usually only implicit and based on confusions — I doubt that very few nowadays really want to argue for scientism. Unlike conditional technical norms, the categorical unconditional imperative "You ought to do X" does not even have truth value. In other words, such unconditional recommendations are qualitatively different from the results of science.

How about the requirement of objectivity in accessing hypotheses? If we identify "science" by external criteria, and count e.g. Lysenko's theory or "ethical economy" (which was Weber's target) as science, it is rather trivial that science is *not* value-free — plenty of bad science and pseudo-science around violate that requirement. If, on the other hand, we understand science, and scientific knowledge, in a more normative way (see above), science is objective and value-free

by definition.[3] The question then is: Is there any science in this more demanding, normative sense? The extremely strong skeptical claim that there has never been, and could not ever be, good science is quite implausible and totally unjustified. It is rarely stated explicitly but is implicitly assumed in the many radical arguments against the value-freedom of science. The spectacular practical success of modern science strongly suggests that science has not done at all badly.

A further question is whether there is scientific research that is value-free also with respect to the choice of the research problem and applications. This turns to the question of whether there is non-applied basic research or descriptive science.

Behavioralism (e.g. Carnap, Jeffrey, Rudner) is the view which holds that scientists do not accept or reject propositions at all, unless such acceptance or rejection is related to practical objectives. Scientists are considered to be decision-makers or guidance counselors. From behavioralism's perspective, all the results of science should be viewed as mere recommendations for action in practical decision-making situations. More generally, *Instrumentalism* (some pragmatists and Marxists, e.g. Dewey, Habermas) treats all science as applied science, and denies that scientific knowledge of reality could be valued for its own sake. Science always has and must have instrumental value. Opposed to the both above views is *Cognitivism* (see Levi 1967), which recognizes the quest of truth as a legitimate human activity whose aims and products are not directly relevant to practical application — that at least *some* scientific inquiries may be concerned with accepting propositions as true, rejecting them as false, or suspending judgment on them (this acceptance may, of course, be tentative). Cognitivism appears to give a much more accurate picture of science than its opponents (see e.g. Levi 1967, Niiniluoto 1984). Consequently, there is also basic, descriptive science (cf. above).

For example, Rudner's much-cited argument against value-freedom is based on his assumption of behavioralism, and ignores the difference between basic and applied research. It shows, if anything, at best that in the practical application of scientific knowledge one has to make value-judgments. In basic descriptive science there are no such applications (cf. Levi 1960, 1967). More generally, many attacks on the idea of the value-freedom of science assume a behavioralist or instrumentalist view. But once we see the shortcomings of these views, many such critiques lose their power.

Every scientist must somehow select some specific topic of study; no one can study everything. Does this mean that all science must be value-laden at the point of selection? Not at all! That would be the case only if one wholly denied the existence of basic, descriptive science — but we have seen good reasons to rebut such instrumentalist views. In basic research, research problems are chosen primarily by their potential scientific relevance — that is, only epistemic values are involved. Such research is thus also value-free with respect to the choice of the research problem. And because phase (3) of the research process, applications, is missing by definition, one can say that such research is value-free in all three phases. In applied research, or design science, the situation is of course quite different.

At the level of results and their acceptance, all good science, whether basic or applied, descriptive or design science, is value-free and objective. On the other hand, in applied research, or design science, the question of which issues are studied is by no means morally neutral. The choice of research problem may be significantly value-laden. Further, in many situations it is reasonable to consider scientists morally responsible for the applications of their results. This is especially obvious if the morally questionable application has been clear from the beginning, e.g. in developing weapons of mass destruction, effective methods of torture, or marketing morally problematic products.

Further, as Kitcher points out, mere ignorance of morally problematic applications does not always free the scientist from moral responsibility: "when only little curiosity is needed to see the propensity of others to engage in morally consequential applications, the researcher who proclaims solely epistemic intent is guilty of self-deception (at the very least)" (Kitcher 2001, p. 89) Part of the moral duties of a scientists is to actively examine the question of potential ethically problematic applications of one's research. Only if one has done one's best in this respect, one may be considered to be not responsible for some unpredictable immoral applications of one's research.

Notes

1 Proctor (1991) is a comprehensive historical survey of the history of the controversy over the value-freedom of science.

2. Note, by the way, that also human sociobiology and orthodox evolutionary psychology, fields of study now often discussed in connection with the value-freedom thesis, do not at the moment, and in my mind probably never will, count as scientific knowledge in this sense. More generally, I think that many examples that the critics of science and the idea of value-freedom use do not count as scientific knowledge in this sense.

3. Many disputes between philosophers and sociologists of science seem to me only verbal, for the participants interpret "science" very differently.

References

Gouldner, Alvin (1962). "Anti-Minotaur: The myth of a value-free sociology", *Social Problems* Nr. 9, 199-213.

Kitcher, Philip (2001). *Science, Truth, and Democracy*. New York: Oxford University Press.

Kuhn, Thomas (1977). "Objectivity, value judgments, and theory choice", in *The Essential Tension*, Chicago: University of Chicago Press, 320-339.

Laudan, Larry (1984). *Science and Values*, Berkeley: University of California Press.

Levi, Isaac (1960). "Must the scientist make value judgments?", *Journal of Philosophy* 57, 345-357.

Levi, Isaac (1967). *Gambling with Truth*, Cambridge, Mass: MIT Press.

McMullin, Ernan (1983). "Values in science", *PSA 1982*, Vol. 2, 3-28. East Lansing: Philosophy of Science Association.

Niiniluoto, Ilkka (1984). *Is Science Progressive*, Dortrecht: Reidel.

Niiniluoto, Ilkka (1993). "The aim and structure of applied research", *Erkenntnis* 38, 1-21.

OECD (1966) *Ministerial Meeting on Science: Fundamental Research and the Policies of Government*, Paris, 1966.

Proctor, Robert N. (1991). *Value-free Science? Purity and Power in Modern Knowledge*. Cambridge, MA & London: Harvard University Press.

Rudner, Rudy (1953). "The scientist qua scientist makes value judgments". *Philosophy of Science* 20, 1–6.

Taylor, Charles (1964). "Neutrality in political science", in P. Lanslett and W. G. Runciman (eds.) *Philosophy, Politics and Society*, third series, Oxford: Basil Blackwell, 25-57.

Weber, Max (1904). "Die 'Objektivität' sozialwissenschaftlicher und sozialpolitischer Erkenntnis", in his *Gesammelte Aufsätze zur Wissenschaftslehre.* 3d ed. Tübingen, 1968. Translated as "'Objectivity' in social science and social policy" in Weber 1949.

Weber, Max (1917). "Der Sinn der 'Wertfreheit' der soziologischen und ökonomischen Wissenschaften", in his *Gesammelte Aufsätze zur Wissenschaftslehre*. 3d ed. Tübingen, 1968. Translated as "The meaning of 'ethical neutrality' in sociology and economics" in Weber 1949.

Weber, Max (1919). "Wissenschaft als Beruf", in his *Gesammelte Aufsätze zur Wissenschaftslehre.* 3d ed. Tübingen, 1968. Translated as "Science as a Vocation" in Hans Gerth and C. Wright Mills (eds.) *From Max Weber*, New York, 1958.

Weber, Max (1949). *The Methodology of the Social Sciences.* New York: The Free Press.

von Wright, G. H. (1963). *Norm and Action*, London: Routledge and Kegan Paul.