14.2 Rationality and the Value-Freedom of Science

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Summary

The value-freedom of science has traditionally been regarded as a presupposition of scientific rationality. However, during the past decades, in addition to numerous empirical counterexamples of value-laden science, systematic arguments have put the adequacy of value-freedom as an ideal into doubt. This chapter presents the most important debates on the ideal of value-freedom, which concern the epistemic impact of values in the discovery and justification of theories, the distinction between epistemic and nonepistemic values, and the argument from inductive risk. Taken together, these arguments call for new normative models of how to deal with values in science, which no longer equate value-laden science with bad science or irrationality. Rather, they suggest that scientific rationality is highly complex, since epistemic issues are interwoven with practical, sociopolitical, institutional, and ethical ones.

1. Values and the Rationality of Science

Science is often seen as an exemplarily rational endeavor, ideally generating knowledge untainted by extra-scientific interests, prejudices, or wishful thinking. As such, it can support rational beliefs about the world and serve as the basis for rational decision making in policy matters (e.g., concerning the mitigation of climate change). As Newton-Smith (1981) puts it, “The image that the scientific community likes to project of itself, and indeed the image that most of us accept of that community, is that of rationality par excellence” (p. 1).

This rationality of science is secured by the methodical production and testing of scientific knowledge that minimize random errors as well as systematic distortions—for instance, by value-laden perspectives. The value-freedom of science is therefore traditionally considered a precondition of its rationality.[1] Such a view is particularly plausible if one takes value judgments to be mere expressions of subjective preferences, incapable of rational justification. It is, however, equally compatible with all positions that consider value judgments to allow for rational discussion, yet not as the kind of judgment that can be supported by scientific methods. Essential to the ideal of value-free science is a separation of the realms of *is* and *ought*: empirical conclusions do not follow from normative premises, and neither can empirical judgments legitimize normative conclusions.

1.1 From Is to Ought

Important predecessors of the current debate on science and values have often focused on this latter question of whether science can provide us with normative guidance in the ethical or political realm. David Hume is famous for criticizing deductions from *is* to *ought* (Hume, 1739/1975, p. 469). In the context of the developing social sciences in the 19th century, Max Weber has argued for the importance of keeping science and politics separate. Expressing normative, political judgments in the role of (social) scientist, he says, is misguiding and only serves to immunize the respective judgments from criticism by other citizens (Weber, 1917/1973). The moral and rational thing to do would be to keep the roles of scientist and citizen distinct and to clearly separate descriptive and normative judgments (on the relation of normative and descriptive theories, see also section 4 of the Introduction by Knauff & Spohn, this handbook). What science can do, according to Weber, is to study values and value systems. Their relations and coherency can be analyzed, and science can clarify whether and how certain political goals are empirically realizable. For example, economics might inform us about the most effective ways to increase production, but it cannot tell us whether or not we should prioritize productivity over fair working conditions. Science may give us the means to realize certain practical ends, but it cannot justify these ends (cf. Weber, 1904). In other words: science should be politically neutral.

This aim of neutrality has also been debated in the positivism dispute between proponents of critical rationalism (Popper, Albert) and the Frankfurt school in sociology (e.g., Adorno, Habermas) during the 1960s. The latter argued that a critique of the societal status quo is an important task of the social sciences, and the scientific justification of political goals is possible through historical materialism. Critical rationalists criticized this approach for purporting normative assertions to be scientifically justified even though they are not testable by scientific methods. The Frankfurt school, they argued, borrowed the authority of science for their political positions but without submitting them to the rigorous critical process that gives science its epistemic authority. Adorno and others replied that a political neutrality of science serves and perpetuates given power structures by failing to offer a critique and an alternative (cf. Adorno et al., 1969; see also Bueter, 2012).

1.2 From Ought to Is

Since the 1980s, the value-freedom of science has yet again been subjected to intense debate. The current debate mostly focuses on the impact of value judgments on scientific theories and processes, asking whether such influences are avoidable—and, if not, how to deal with them. Here, the terms "value" and "value judgment" are often used in a very broad sense, in which "a value is something that is desirable or worthy of pursuit" (Elliott, 2017, p. 11). This may include religious or social values but also economic interests, political goals, or individual preferences. Often, "value judgment" is characterized in a negative manner as comprising all judgments that cannot be justified by logic and empirical evidence alone. Prima facie empirical judgments can be "value-laden"; for instance, if they rely on an interpretation of the evidence from a certain interested perspective or result from biased methodological standards.

Much research in philosophy and history of science as well as in science studies has brought forward examples of science that is value-laden in this sense. In particular, feminist scholars have exposed a multitude of cases where social prejudices related to gender have found their way into accepted scientific theories (cf., e.g., Keller, 1985; Kourany, 2010; Schiebinger, 1999). While such *empirical* cases of value-ladenness were acknowledged, they have often been taken to underline, not undermine, the importance of value-freedom from a *normative* point of view. In other words, the *ideal* of value-freedom is often held on to: "Although complete value-freedom cannot be achieved, it ought to be a goal or ideal of science" (Shrader-Frechette, 1991, p. 44).

This contemporary ideal of value-freedom[2] is usually based (implicitly or explicitly) on a distinction between the contexts of discovery, justification, and application.[3]

> There should be no compromise when it comes to the proposal that ideological factors be invited into the Context of Justification. . . . We should make every attempt to keep politics and religion out of the laboratory. We may not always be successful, but that simply means that we should try harder, not that we should give up the attempt. (Koertge, 2000, p. 53)

"Context of discovery" usually refers to the generation of hypotheses. Whether these hypotheses are the result of a long thinking process, misogynism, or a nightmare about snakes is considered irrelevant for their epistemic status, which has to be assessed via rigorous and systematic testing in the "context of justification." Finally, empirically supported hypotheses and theories can be implemented in practice ("context of application"). Yet again, such an implementation is not considered a scientific decision. Instead, it is delegated to policy-makers and democratic deliberation, as it often requires the negotiation of different values and practical goals.

The current ideal of value-freedom thus does not require science to be completely independent of any values or interests; rather, these may impact the direction and application of research as well as decisions on the ethical admissibility of methods. However, the epistemic assessment (i.e., justification) of theories is supposed to be value-free.

In the past decades, the feasibility of this normative ideal has been thoroughly questioned by systematic arguments that concern both the value-freedom of justification and its independency from values at play in discovery and application. In the following, I will present the most prominent debates on value-freedom: from the relevance of (values in) the context of discovery (section 2) and the distinction between epistemic and nonepistemic values (section 3) to the problem of inductive risk (section 4). The respective criticisms of the value-free ideal call for a refined understanding of scientific rationality that does not presuppose value-freedom. I will conclude with a brief outlook on resulting questions and emerging topics.

2. Discovery, Pursuitworthiness, and Significance

Restricting value-freedom to the testing and evaluation of theories presumes that this stage of the research process is epistemically independent from discovery and application. To put it another way, it assumes that there is a rational, scientific core of research, on the one hand, and more external stages not amenable to scientific rationality, on the other. Accordingly, the context of justification has often been treated as the sole proper subject of philosophy of science.

Against this, it has been argued that while there may not be an unambiguous *logic* of discovery, the generation of hypotheses and the selection of research questions are also not totally random or even irrational. Rather, they depend on ascriptions of significance that can be influenced by epistemic as well as sociopolitical, economic, or ethical considerations.

Philip Kitcher argues that any assumption of a superordinate goal of science (such as truth, successful predictions, etc.) is incapable of efficiently directing research. Scientists are not equally interested in anything that may be true; rather, they search for *significant* truths (or significant predictions, explanations of significant phenomena, etc.). Such ascriptions of significance depend on more specific epistemic goals (e.g., making successful predictions about climate change or explaining the existence of our solar system). These specific epistemic goals are often informed by values. As Kitcher argues, even what seems epistemically significant (rather than practically important) today does so as result of a specific scientific history, which may have been influenced by value-laden concerns in earlier phases (Kitcher, 2001, chapters 6 and 7).

Based on this, Kitcher develops the ideal of a *well-ordered science*, which broadens the focus of philosophy of science to include questions traditionally relegated to the context of discovery. Well-ordered science calls for political responsibility and accountability in the direction of research, making it subject to a process of democratic deliberation by representatives of different social interests (to be informed by impartial scientific experts; Kitcher, 2001, chapter 10; cf. also Kitcher, 2011).

An important point to make here is that ascriptions of significance do not stop at the choice of a research question. To get from a specific question to a testable theory is a process that involves more than just the logical deduction of empirical consequences from a certain hypothesis. It may involve questions of how exactly to frame the hypothesis, which variables to focus on, what concepts to employ,[4] and which empirical consequences of a hypothesis to test and how. Developing a testable theory, moreover, also depends on sufficient funding as well as the time and willingness to persevere in the face of setbacks.

This has two important consequences: first, the distinction between discovery and justification tends to make all of these important steps invisible, even though many of them are capable of rational evaluation. We could thus add a *context of theory pursuit*, where we judge theories not in terms of their eventual justification but their (epistemic and/or practical) prospects and fertility as well as initial plausibility (cf., e.g., Kordig, 1978; Nickles, 1980; Šešelja & Straßer, 2014; Whitt, 1992).

> [A] careful examination of scientific practice reveals that there are generally *two* quite different contexts within which theories are evaluated. . . . Even if we had an adequate account of theory choice within the context of acceptance . . . we would still be very far from possessing a full account of rational appraisal. (Laudan, 1977, pp. 108–109)

Second, several authors have argued that values in discovery and/or pursuit have impacts on the evaluation of theories, putting the epistemic independence of the context of justification in doubt. The general idea here is that ascriptions of significance impact which theories are developed and tested (and which not), as well as which data are generated and what is considered noise. This, in turn, impacts the assessment of existing theories. For example, Okruhlik (1994) argues that theory assessment is usually comparative: what is the best theory depends on the range of available theories. In a thoroughly sexist culture, though, "[n]on-sexist rivals will never even be generated. Hence, the theory which is selected by the canons of scientific appraisal will simply be the best of the sexist rivals; and the very *content* of science will be sexist" (Okruhlik, 1994, pp. 201–202).

The choice between competing theories may thus be based on rational criteria and yet unable to screen out earlier social impacts—a point that Ohkruhlik takes to underline a need for more enhanced models of scientific rationality, which include the contexts of theory generation and attend to the social embeddedness of science. A similar point has been made by Elliott and McKaughan (2009). Based on case studies from toxicology, they show how social values and interests impact the availability of theories and data—and, consequentially, theory assessment:

> The degree of evidential support for a theory clearly depends both on the *array of available theories* and on the *set of data at hand*. Therefore, to the extent that the nonepistemic values associated with discovery and pursuit influence the available theory and data, they affect theory appraisal. (Elliott & McKaughan, 2009 p. 600)

It is necessary to make choices about significance in scientific research. However, if all of these choices are guided by the *same* value-laden background assumptions, this can lead to bias in our results, even if these are tested methodically and meticulously. As Anderson (1995) explains, a partial truth can be thoroughly misleading. Bueter (2015) argues that values in discovery and pursuit can create blind spots such that alternative theories or data, which would undermine currently accepted theories if they existed, are never even generated. For instance, this can happen due to widely shared sexist or racist prejudices, or due to a domination of research fields by agents with particular interests, for example, when medical research is driven by the pharmaceutical industry that aims to make profits and therefore does not explore nonpharmaceutical treatment alternatives (cf. Jukola, 2019).

3. Values in Justification: The Social, the Epistemic, and the Nonepistemic

Section 2 summarized arguments against a restriction of scientific rationality and value-freedom to the context of justification. For one, decisions concerning agenda-setting and pursuitworthiness are said to allow for rational discussion; for another, value-impacts on these decisions have been argued to affect theory assessment via the (non)existence of alternatives. However, a large part of research on science and values has focused on questions relating to justification. Prominently, it has been debated whether epistemic values can be distinguished from nonepistemic ones and, if so, whether the latter can or should be eliminated from theory assessment.

The modern ideal of value-freedom is often taken to be essentially this: the evaluation of scientific contents is supposed to rely exclusively on epistemic criteria. Kuhn (1977) has been the first to call such criteria *values*, thereby starting a still-ongoing debate on whether one can distinguish between epistemic (or cognitive)[5] and nonepistemic (noncognitive) values. Kuhn's original list contained five such criteria that serve as indicators of good theories: *accuracy* (or *empirical adequacy*), requiring that a theory's deducible consequences are in line with empirical observations; *consistency* (*internal*, i.e., logical, as well as *external*, i.e., in relation to other currently accepted theories); *scope*; *simplicity*; and *fruitfulness*.

Kuhn calls these criteria "values" because they do not provide algorithmic rules for theory choice, only guidance. They need to be interpreted and balanced against each other in specific research contexts. Moreover, their interpretation and weighing can be influenced by other factors such as the personality of a researcher (for a more detailed account of Kuhn's position, see chapter 14.1 by Andersen & Andersen, this handbook).

McMullin (1982) calls Kuhn's values *epistemic*, arguing that they "promote the truth-like character of science" (p. 18), and distinguishes them from nonepistemic (e.g., religious or sociopolitical) values. Against this, Rooney (1992) points out the existence of different lists of such epistemic values, taking this variety to indicate a lack of clarity in the distinction between epistemic and nonepistemic. She further argues that this lack of clarity can sometimes turn sociopolitical (e.g., androcentric) values into part of what it means to be epistemically successful.

This resonates with Longino (1996), who argues that the function of epistemic values is to promote the goal(s) of science.[6] Such goals can be value-laden, for instance, when feminists aim to understand mechanisms of gender oppression or to produce nonsexist theories about women's health. Thus, we could have research goals that are both feminist and epistemic—a status that translates accordingly to the values guiding theory evaluation. Starting from case studies on research with feminist goals, Longino creates an alternative list of such (feminist) epistemic values. She agrees that *internal consistency* and *empirical adequacy* are necessary features of a good theory.[7] In contrast to Kuhn, however, she adds *novelty*, *ontological heterogeneity*, *complexity of relation*, *applicability to human needs*, and *diffusion of power*. Furthermore, she argues that the Kuhnian values can function as promoters of androcentric theories in particular research contexts (e.g., when simplicity serves as a justification of considering only men's perspectives and concerns).[8]

Others have defended the distinction between epistemic and nonepistemic values. For instance, Ruphy (2006) proposes an empirical evaluation of epistemic values in terms of their contribution to empirically successful theories. Douglas (2013) argues that a more fine-grained classification of kinds of epistemic values can help both to reduce tensions between them and to support their epistemic status. She does so by distinguishing epistemic values on two axes: (1) ideal desiderata versus minimal criteria and (2) values that apply to theories per se versus to theories in relation to evidence. Some, but not all, of the resulting four groups of epistemic values are genuinely truth-assuring; others can be considered strategic or pragmatic epistemic values in that they make it more likely (e.g., via testability) that flaws will be discovered (Douglas, 2013).

Hugh Lacey argues that, when considering the role of epistemic and nonepistemic values in justification, it is necessary to distinguish between different kinds of attitudes with regard to the assessment of theories, which tend to be blurred under the common term of "accepting" a theory. According to him, a theory may be "adopted" (i.e., used to frame and direct future research), it can be "endorsed" (i.e., considered well enough supported to put it into practical use), or it may be "impartially held of a set of phenomena" (i.e., considered so well established that it does not need further testing). In this third case, only epistemic values have legitimate roles to play (Lacey, 2017; cf. also Lacey, 1999). Rooney (2017) replies that Lacey's argument is question-begging, because it presupposes, rather than grounds, the distinction between epistemic and nonepistemic values.

Steel (2010) distinguishes between "intrinsic" and "extrinsic" epistemic values. *Intrinsic* epistemic values, such as empirical adequacy, are those that are necessary for the attainment of truth. *Extrinsic* epistemic values promote such an attainment only indirectly; examples here are testability or the ability to obtain funding. For Steel, nonepistemic values are values that are either irrelevant or detrimental to the attainment of truth.

Even if we assume the possibility of distinguishing epistemic from nonepistemic values though, further problems remain. In recent years, a controversy has emerged about the appropriate normative role of nonepistemic values in theory assessment, which focuses on the priority of epistemic over nonepistemic values. For instance, Steel's account represents an "epistemic constraints" approach: nonepistemic values are illegitimate if they interfere with or override the standards of adequate science expressed in extrinsic and intrinsic epistemic values. On the other hand, they are allowed to play a role as "tie-breakers" if epistemic values are insufficient to guide theory choice in particular cases.

Steel (2017) contrasts such epistemic-constraints approaches with aim-oriented ones, which do not ascribe unambiguous priority to epistemic values. For instance, Elliott and McKaughan (2014) argue that the goals of inquiry are underdetermined if characterized as attainment of truth in general—rather, research aims for truths that are considered significant. The relevant goals here are therefore particular research goals that integrate practical aims and concerns, which can be promoted by both epistemic and nonepistemic values. Nonepistemic values may therefore have legitimate roles to play in theory assessment, as long as scientists are maximally transparent about their goals and values. Relatedly, Brown (2013) argues that it is unnecessary to assign priority to epistemic values in order to avoid wishful thinking, as long as the value judgments involved are subjected to rational discussion and are considered in principle revisable (cf. also Brown, 2017). A question for future research that arises here is how to determine the legitimacy of such epistemic-social goals (and thus values) and how exactly to avoid a corruption of research by political or economic interests.

4. Application, the Consequences of Error, and Inductive Risk

Another important strand in the debate on value-freedom focuses on the problem of inductive risk. The central question here is whether all responsibility for decisions on applying research results can be externalized or whether scientists themselves need to take into account potential practical consequences of their work—at least in the case of uncertainty. The argument from inductive risk says that practical consequences of mistaken scientific results are relevant to the setting of evidentiary standards, thereby rejecting the epistemic independence of justification from the context of application.

Rudner (1953) famously argued that in deciding to accept or reject any hypothesis, scientists always face two different kinds of potential errors: false-negative results (rejecting a true hypothesis) versus false-positive results (accepting a false hypothesis). This gives rise to the question of how much evidence is enough to decide upon the acceptance or rejection of individual hypotheses in the light of error risks. Consequently, Rudner argues, scientists need to refer to value judgments concerning the potential consequences of different kinds of errors. For instance, if the acceptance of a hypothesis about the safety of a new drug will likely lead to its approval and use in a large population, a false positive may lead to many patients experiencing severe side effects, while a false negative would mean the loss of a potential treatment to them and of financial profits to the pharmaceutical company. Which kind of error is considered worse in a specific situation depends on nonepistemic value judgments and impacts the epistemic standards for acceptance (e.g., if it is most important to us to avoid a false-positive result, these standards will be set very high).

Rudner (1953) concludes that this means that the scientist *qua* scientist necessarily makes value judgments. Against this, Levi (1962) has argued that Rudner conflates accepting a hypothesis in terms of its epistemic validity and thus *believing* in its truth, with *acting* based on such a belief. Relatedly, Jeffrey (1956) has objected that scientists often cannot foresee potential practical applications of their work and thus consequences of error. Therefore, they can hardly decide upon the acceptance/rejection of a hypothesis based on values referring to these consequences. Instead, Jeffrey proposes that scientists should not make such decisions but confine themselves to ascribing probabilities to hypotheses—leaving it to policy-makers to decide how high a probability is high enough in a particular situation. Pointing out the problems with leaving such value judgments to scientists rather than democratic decision making, Betz (2013) similarly argues that scientists should not accept or reject plain hypotheses but rather "hedged hypotheses," which articulate the amount and kind of uncertainty involved in making a certain claim (for a response, see John, 2015a).

In one of the most influential articles in the debate, Douglas (2000) has both resuscitated Rudner's argument and strengthened it. First, she confines the argument to policy-relevant research, where practical consequences of scientists' assertions often are predictable. Second, she shows how trade-offs between the probabilities of false positives versus false negatives occur not only at the point of a hypothesis's final evaluation but at several methodological stages before. For instance, such trade-offs relate to the setting of a significance level, the gathering and interpretation of data, or the interpretation of results. This makes it more difficult to externalize all the decisions facing inductive risk and implies a thorough value-ladenness of science. For Douglas, this amounts to an ethical obligation of scientists to consider the potential practical consequences of errors. For her, the respective ethical considerations *should* have an impact on evidential standards in science. She distinguishes such legitimate *indirect* value influences from illegitimate ones, where nonepistemic values *directly* impact the rejection/acceptance of a hypothesis:

> Two clear roles for values in reasoning appear here, one legitimate and one not. The values can act as reasons in themselves to accept a claim, providing direct motivation for the adoption of a theory. Or, the values can act to weigh the importance of uncertainty about the claim, helping to decide what should count as sufficient evidence for the claim. (Douglas, 2009, p. 96)

Douglas's work has been the beginning of an ongoing and very lively debate about inductive risk in science. For one, the argument of inductive risk has been applied in a variety of case studies from medicine over climate science to theoretical physics (cf., e.g., Elliott & Richards, 2017). For another, its soundness, its scope, and its impact on the ideal of value-free science continue to be discussed (cf., e.g., Biddle, 2013; de Melo-Martín & Intemann, 2016; Hicks, 2018). Biddle (2016) as well as Biddle and Kukla (2017) have recently called for, and started on, a more fine-grained differentiation of kinds of risk involved in inductive risk.

Douglas's normative answer—distinguishing between indirect value influences on evidential standards as legitimate and direct influences on acceptance decisions as illegitimate—has also been called into question, in terms of both its conceptual clarity and its normative sufficiency (e.g., Elliott, 2011). In particular, indirect influences can also sometimes be problematic, for example, if they overly tip the balance between error probabilities in favor of sponsor interests (cf. Wilholt, 2009, for an example). Wilholt, moreover, provides an alternative proposal for identifying biased research, which relies on the role of conventions in the scientific community. Such conventions serve as a means to standardize methodological decisions that can indirectly affect acceptance decisions. Deviations from such commonly employed standards can thus make value influences visible. In addition, Wilholt argues that the distribution of error probabilities is not only a problem in cases of applied research with foreseeable social consequences. It is rather an in-principle epistemological issue, since false-positive or false-negative results differ in relevance and importance even if we consider only epistemic utilities, such as the overall importance of a certain hypothesis for our belief systems and theories (Wilholt, 2009, 2013).

5. Conclusion

To sum up, the value-freedom of science has been the subject of intense debate in the past decades—both in terms of numerous empirical counterexamples and regarding its adequacy as a normative ideal. At the same time, most philosophers do not equate value-ladenness with bad science (anymore). Instead, they aim for more sophisticated approaches, supposed to be able to distinguish between illegitimate and legitimate value influences; for example, in terms of kinds of values (i.e., epistemic vs. nonepistemic), indirect or direct roles of values, or stages of scientific research in which they may or may not play a role. While this debate on normative alternatives is still ongoing, the emerging consensus seems to be both that the traditional value-free ideal is doomed—and that this does not imply that science lacks rationality. Rather, it indicates that the kind of rationality involved is highly complex, since epistemic issues are thoroughly interwoven with practical, sociopolitical, institutional, and ethical concerns.

Since one of the purported functions of the ideal of value-free science was to provide the public with factual, untainted information to be used in democratic deliberations on practical concerns, an emerging topic now is how this new image of science relates to issues of public trust (see also chapter 14.3 by Bromme & Gierth, this handbook). In particular, if science is shot through with ethical and social values, the question arises whose values these are, how they are to be legitimated, and how this can be compatible with rational trust in the results of such science. Furthermore, this leads to the question of how to communicate to the public the role of values in scientific research. While, for instance, McKaughan and

Elliott (2013) have pleaded for transparency here, John (2015b) warns against a further erosion of public trust caused by such transparency. To conclude, the question at this point in time no longer seems to be whether science is value-free or whether it must be so in order to be rational—but rather how to deal with value-laden science in a sensible manner, not least on the institutional level of organizing scientific research and science–public relations.

Notes

1. It has also usually been treated as a necessary, yet not sufficient, condition for objectivity. For a more detailed look at objectivity, see chapter 14.1 by Andersen and Andersen (this handbook).

2. Regarding different elements and historical versions of the value-free ideal, see, for example, Proctor (1991).

3. For a closer analysis and discussion of the context distinction, see Schickore and Steinle (2006). I am referring to it here not in order to support it but because it has had a considerable impact on the ideal of value-freedom and constitutes a useful means to organize different discussions in the science-and-values debate.

4. Another discussion I will not go into here for reasons of space concerns the semantic value-ladenness of scientific concepts, classifications, and terminologies (cf., e.g., Dupré, 2007).

5. The terminology differs here. For the sake of readability, I will use "epistemic" throughout in the following.

6. Helen Longino's work has played a very decisive role in debates on values in science; this holds in particular for her account of *social objectivity*, which does not presume value-freedom. Rather, social objectivity requires scientific communities to be organized in a way that enhances the efficiency of critical discourse (Longino, 1990; for more details, see chapter 14.1 by Andersen & Andersen, this handbook).

7. Similar to Kuhn, Longino takes consistency and empirical adequacy to be insufficient to determine theory choice. In both cases, this assumption is based on moderate versions of the underdetermination thesis. Kuhn argues that rival theories are often not empirically equivalent, yet both more or less empirically adequate, differing in what areas of evidence they match better (Kuhn, 1977, p. 323). Longino proceeds on the basis of an underdetermination thesis regarding evidential relations. As she argues, the relation between theoretical hypotheses and (significant) empirical consequences is mediated by substantial and methodological background assumptions, which are possibly value-laden (Longino, 1990, pp. 40–48).

8. Such a diversity in values guiding theory assessment might be taken to undermine the possibility of rational discourse in the case of dissent. Carrier (2013) argues that pluralism extending to the epistemic values used in theory assessment is unproblematic if the scientific community shares a fundamental "epistemic attitude" toward the goals of research, which enables a joint striving for consensus and rational discussion. This epistemic attitude is not implemented at the level of standards for theory assessment but at the level of procedural rules for debating knowledge claims.

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