The Handbook of Rationality

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14.1 Scientific Rationality and Objectivity

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Summary

This chapter provides an overview of the accounts of scientific rationality and objectivity that have been offered by philosophers of science during the 20th century. We begin by presenting different accounts of how *individual* scientists should act to be rational and objective. In the latter half of the 20th century, some philosophers argued that the rationality and objectivity of science are obtained at the level of communities of scientists rather than at the level of individual scientists. Hence, we also present accounts of how *communities* of scientists can be rational and objective. Finally, we illustrate how the philosophical discussions about scientific objectivity may be relevant for scientific practice.

1. Models of Individual Rationality and Objectivity

This chapter provides a brief overview of the various accounts of scientific rationality and objectivity that have been offered by philosophers of science during the 20th century. We start with models of individual rationality and objectivity, which dominated philosophy of science until the mid-20th century. We then move to models of group rationality, which became popular during the second half of the 20th century. We also examine recent models of group objectivity, focusing on feminist approaches to objectivity in science. Finally, we illustrate how the philosophical discussions about objectivity may be relevant for scientific practice. In line with most English-language accounts of rationality and objectivity in science, "science" is used to refer to the natural sciences.

Science is often considered the epitome of rationality and objectivity. The view that science is *objective* can be understood in two different ways. It can be understood either in the way that scientific *theories* faithfully describe facts about the world or in the way that the *methods* employed in science are independent of social and personal values and biases (see Reiss & Sprenger, 2014/2017). In this section, we focus on models of rationality and objectivity from the first half of the 20th century, and at that time, science was generally seen as objective in both these ways. The methods employed by scientists were usually seen to be objective and to lead to objective theories. In fact, insofar as scientists acted *rationally*, they were usually considered to adhere to objective methods that led to objective theories. Thus, the central question was what it means for scientists to act rationally.

In the first half of the 20th century, many philosophers of science linked questions about scientific rationality to the question of how individual scientists validate their scientific beliefs. This approach was based on the assumption that there is a clear distinction between discovery and justification in science.[1] On this view, how scientists *form* new scientific hypotheses is seen as an empirical question to be relegated to disciplines such as psychology, sociology, and history of science, while in contrast, the question of how scientists should *evaluate* scientific hypotheses and make choices between competing theories is seen as a normative question, to be answered by philosophy. The standard view was that science could be characterized by its special method that specified how scientific theories could be assessed. On this view, scientists act rationally by adhering to this scientific method. Consequently, an important philosophical question was how to define this method.

Some of the dominant accounts of the unique scientific method in the 20th century have been Hempel's (1966) hypothetico-deductive model, Popper's (1963/1972, 1934/2014) falsificationist model, and Bayesianism (we will not examine Bayesianism in the present chapter but refer the reader to chapter 4.1 by Hájek & Staffel, this handbook).

Hempel's hypothetico-deductive, or H-D, model presents a logic of confirmation. On this model, scientists assess a hypothesis by first deducing from it the observable events that would take place if the hypothesis were true and then conducting experiments to observe whether the predicted events occur. If the predicted events occur, this provides supporting evidence for the hypothesis, where the degree of support depends on the quantity, variety, and precision of the supporting evidence. In contrast, if the predicted events do not occur, scientists should reject the hypothesis in question.

In contrast to the H-D model's logic of confirmation, Popper's falsificationist model presents a logic of refutation. On the falsificationist model, like on the H-D model, scientists start by deducing from the hypothesis under test the observable events that would occur if the hypothesis were true. However, Popper stressed that, regardless of the amount of supporting evidence for the hypothesis, the hypothesis can never be confirmed with certainty. By contrast, it follows logically that the hypothesis is false if it has false implications: if events can be deduced from it that do not in fact occur. This led Popper to suggest that, for scientists to act rationally, they must proceed by trying to *falsify* hypotheses. Thus, Popper focused on the potential falsifiers of a hypothesis and applauded scientists' sincere attempts at falsification. On this view, the more a hypothesis claims, the better it is, in the sense that it is more open to falsification. If a hypothesis has been subjected to numerous tests and still has not been falsified, this does not justify scientists in accepting the hypothesis. It only means that the hypothesis should be refined to be more open to falsification and be made subject to even more stringent tests. On this view, the scientific method can be described as a method of conjectures and refutations.

Both the H-D model and falsificationism have had a huge influence on the perception of science and scientific rationality. In general, the view that there is a unique scientific method, to which all rationally behaving scientists adhere, has been strong not only in philosophy but also in other fields. For example, in science education, the scientific method is often presented as a fixed procedure from the description of a phenomenon, over the formulation of a hypothesis that explains the phenomenon, to the conduct of an experiment that tests the hypothesis (Blachowicz, 2009; see also Bauer, 1992, for a historical overview).[2] The literature on scientific method is vast, but overviews can be found, for example, in H. Andersen and Hepburn (2015/2016), Laudan (1968), and Losee (2004, 2005).

We end this section by discussing some challenges to the presented models of individual rationality.

First, it has been argued that the rational acceptance or rejection of a hypothesis is not solely based on experiments but is to be seen as a value-based decision between different courses of action, where the values may be epistemic or nonepistemic, namely, social, ethical, and political values. An aspect of this debate is therefore also whether to understand rationality in *epistemic* terms as a question of which theory or hypothesis to accept, given the evidence, or if rationality can also be understood in *instrumental* terms as a question of which actions to perform, given specific beliefs and ends. Among philosophers of science, Churchman (1948) and Rudner (1953) have argued that an analysis of the methods of scientific inference includes both an assessment of the strength of the evidence in favor of the hypothesis and an assessment of the consequences of making a mistake in accepting the hypothesis. A similar debate has been seen within statistics. Here, Fisher (1955) has argued that a theory of inductive inference should provide a numerical expression of disbelief in a hypothesis, while Neyman (1956) and Pearson (1955) have argued that the consequence of error also needs to be included (for details on this debate, see, e.g., Howie, 2002; Lenhard, 2006; Marks, 1997). In recent decades, these issues have been pursued by discussing whether there is a distinction between rational and irrational influences on theory choice and, more specifically, between the roles played by epistemic and nonepistemic values (see, e.g., Douglas, 2009; Machamer & Wolters, 2004). For further reading on this topic, the reader is referred to chapter 14.2 by Bueter (this handbook) on the value-freedom of science.

Second, it has been argued that there is no single rational method for scientists to follow, because scientific norms and aims differ over time and across disciplines. Hence, it has been argued, it is not possible to provide a universal and ahistorical account of the rational method of scientific inquiry. During the 1960s and 1970s, historically inclined philosophers of science such as Feyerabend, Hanson, Kuhn, Laudan, Shapere, and Toulmin argued from studies of the history of science that scientific norms and aims may differ across disciplines or over time.[3] The most radical position was advanced by Feyerabend, who in a monograph with the provocative title *Against Method* (1975/1993) argued for a methodological anarchism on which any methodological rule can be violated fruitfully in some contexts.

Finally, some, such as Hanson (1958), argued that observations are often theory-laden (i.e., they depend on theoretical assumptions). The relation between observation and theory is therefore more complex than assumed both by the accounts of the rationality of science examined above and by the view that science is objective in the sense that scientific theories faithfully describe facts about the world. Like the previous paragraph, this provides a reason for believing that it is not possible to provide a universal and ahistorical account of the rationality of science.

2. Models of Group Rationality

In this section, we consider models of rationality that can accommodate the challenges to the traditional models of rationality just presented. As opposed to the traditional models, these are models of group rationality. In the next section, we consider models of group objectivity. During the second half of the 20th century, philosophers of science started questioning whether the rationality of science should be addressed from the level of the individual scientist and not rather from the level of the scientific community.

Especially the work of Thomas S. Kuhn was foundational for this discussion. Kuhn developed an account of the scientific enterprise on which individual scientists work on a set of received beliefs that they do not question. An individual scientist will sometimes replace this set of beliefs with another set of beliefs, and the time at which she does so is partially determined by personal preferences. Given the traditional conception of scientific rationality and objectivity introduced in the previous section, this makes science seem irrational and subjective. However, on Kuhn's model, science *is* a rational and objective enterprise, but it is the scientific community that serves as the agent responsible for the rationality and objectivity of science.

Kuhn is well known for the phase model of the development of science that he advanced in his monograph *The Structure of Scientific Revolutions* (1962/2012). On this model, science develops through successive periods of cumulative "normal science" separated by noncumulative "revolutions." Normal science is dependent on some set of received beliefs, a "paradigm," which marks out what the acceptable research problems are and what acceptable approaches and solutions to these problems must look like. Yet, some of the scientific problems defined by a paradigm may turn out recalcitrant. If such anomalies cannot be resolved, they may cause a crisis in the scientific community, and this crisis may eventually lead to a revolution, after which the community will have adopted a new paradigm. Such a revolution is not a cumulative process: the new paradigm is not just an extension of the old; on the contrary, the previous paradigm may have precluded solutions of the sort provided by the new. Thus, for Kuhn, competing paradigms were incommensurable and could not be compared in any straightforward manner.

This phase model of the development of science has often by itself been taken to imply that science is irrational. To understand why it does not, one must look at the "essential tension," as Kuhn called it, between the tradition-*preserving* activity of normal science and the tradition-*shattering* activity of scientific revolutions. On the one hand, the tradition-preserving phases of normal science are very effective for solving the problems that the existing paradigm defines. When scientists within a community adhere to the same paradigm, science can move faster and penetrate deeper than if they continuously questioned the paradigm. In this respect, normal science increases the efficiency with which scientific problems are solved.

On the other hand, science would not be able to produce fundamental innovations if normal science were the only mode of doing science. A tradition-shattering complement is needed as well. Kuhn introduced a mechanism that could ensure that the change from one paradigm to another happens neither too quickly nor too late. Thus, if there is a distribution of conservative and innovative dispositions among the members of a scientific community, individual scientists may react differently to encountered anomalies, such that only some start considering alternatives to the reigning paradigm while others continue working within the established paradigm.[4]

To explain both how individual scientists can come to divergent decisions and how the scientific community can, in the end, come to a common decision on which theory to prefer, Kuhn suggested that theory choice was guided by a set of epistemic values that the members of the scientific community share, such as accuracy, consistency, scope, simplicity, and fruitfulness. While these values provide the basis for theory choice, they do not determine it: individuals may legitimately interpret and weigh the values differently. The fact that individuals may weigh the values differently matters to theory choice because the values may conflict. For example, the simpler of two competing theories is not necessarily also the most accurate. Further, Kuhn even claimed that not only epistemic values such as accuracy, consistency, scope, simplicity, and fruitfulness may play a role in theory choice; also, personal preferences about, for example, originality, risk taking, aesthetics, or philosophical convictions may influence theory choice (see Hoyningen-Huene, 1993, chapter 4.3.c). As a result, although individual members of the scientific community may employ the same set of shared epistemic values, they can nevertheless still reach different conclusions as to which of the competing theories to pursue.

Hence, Kuhn's model of the development of science explicitly "*requires* a decision process which permits rational men to disagree" (Kuhn, 1977, p. 332). This stands in stark contrast to the accounts of philosophers like Hempel, who, as described above, linked the rationality of science to individual scientists' adherence to a unique method that determines the rational theory choice. On this view, two rational scientists will always choose the same theory. On Kuhn's view, the fact that theory choice is guided by values that differ from scientist to scientist, as well as by shared values that may be interpreted differently by different scientists, contributes to the development of science. This does not mean that science is irrational; rationality just has to be seen from a social rather than from an individual perspective. As a new, alternative theory is developed, it may prove capable of solving encountered anomalies in such a way that it is gradually accepted by more and more scientists until, eventually, a consensus on the new theory is established among members of the scientific community. Dissent continues until all members of the community, based on their individual interpretation and weighing of the shared values, prefer the same paradigm. Hence, "it is the community of specialists rather than its individual members that makes the effective decision" (Kuhn, 1962/2012, p. 199). A new consensus in the scientific community reflects that arguments in favor of the new paradigm have proliferated.

It is not clear where this leaves us in terms of how exactly to conceive of scientific objectivity. Kuhn does not go much further than to suggest that an account of scientific objectivity should be informed by the fact that the criteria of accuracy, consistency, scope, simplicity, and fruitfulness constrain, but do not determine, theory choice:

> Objectivity ought to be analyzable in terms of criteria like accuracy and consistency. If these criteria do not supply all the guidance that we have customarily expected of them, then it may be the *meaning* rather than the limits of objectivity that my argument shows. (Kuhn, 1977, p. 338, emphasis added)

Hence, Kuhn claims to have shown that science is objective in a different sense than traditionally thought, rather than having shown that science is subjective. But while Kuhn develops an account of scientific rationality on which the rationality of science is obtained at the level of scientific communities, he at most hints at an account on which the objectivity of science is likewise obtained at the level of scientific communities. As will be described in the next section, others have developed such accounts of scientific objectivity.

When Kuhn introduced his model, many critics found that it made theory choice seem irrational (see, e.g., Lakatos, 1970; Scheffler, 1967/1982; or Shapere, 1966). This perception was further amplified when sociologists of science, partially inspired by Kuhn's account, started arguing that scientists' beliefs should be explained in terms of the social conditions that bring them about. This "strong" program of sociology of science claimed to be impartial with respect to rationality and irrationality, meaning that it set out to explain successful as well as unsuccessful knowledge claims in science in the same type of way, with reference to the same set of social causes (Bloor, 1976/1991).

However, other philosophers of science, such as David Hull (1988) and Philip Kitcher (1993), have followed Kuhn in arguing that the social structure of a scientific community contributes to the effective realization of the epistemic goals of science (for a detailed analysis, see also Wray, 2011). Hull (1988) argued that scientists are motivated by the credit they get for contributing true theories. If they would get credit for contributing fraudulent theories, they would cheat. But the structure of reward and punishment in science ensures that scientists will not cheat, because the punishment if they are caught is very severe. On Hull's account, science is so successful because the social structure of science is such that it is in the self-interest of scientists to pursue true theories.

Kitcher (1993) has similarly argued that nonepistemic pressures on individuals, such as desire for fame, can help a scientific community to effectively realize its epistemic goals. It may be tempting for all the scientists working on a given problem to use the same method to pursue the problem: the available method whose intrinsic qualities are highest. But for a scientific community to effectively realize its epistemic goals, it is important that scientists choose *different* methods to pursue a problem. For it may be an unorthodox method that can provide the solution. The nonepistemic pressures can help prevent that all scientists working on the problem use the same method. For example, if a scientist has a desire for fame, it may be better to pick an unorthodox method since there *is* a chance that it will provide the solution, and there are fewer direct competitors in the sense of other scientists pursuing the same strategy. In this way, nonepistemic considerations at the level of individual scientists can contribute to the attainment of the epistemic goals of the scientific community.

On the accounts considered in this section, the agent that is responsible for the rationality of science is the scientific community as a whole, and consequently, the rationality of scientific beliefs should be examined using the tools of social epistemology. For the topic of social epistemology, the reader is referred to chapter 10.1 by Dietrich and Spiekermann (this handbook).

Similarly, philosophers of science such as D'Agostino (2010), De Langhe (2010, 2012, 2014; see also De Langhe & Rubbens, 2015), Weisberg (2013; see also Weisberg & Muldoon, 2009), or Zollman (2007, 2010, 2018; see also Mayo-Wilson, Zollman, & Danks, 2011) have drawn on game theory, economics, and mathematical modeling of social behavior in examining how various forms of heterogeneity in the scientific community can be beneficial or detrimental for the progress of science.

3. Models of Group Objectivity

In recent decades, feminist philosophers of science have examined different ways in which traditional models of the objectivity of science have disadvantaged women and other minority groups (for a review, see Anderson, 2000/2020). In their view, scientific knowledge has a situated and perspectival nature. What is known reflects the perspective of the knower; there is no view from nowhere. This has led some to embrace subjective ways of knowing nature (Irigaray, 1987). Others have argued, similarly to Kuhn, that the objectivity of science should be assessed at the level of scientific communities (Haraway, 1988; Longino, 1989, 1990; Solomon, 2001).

In Helen Longino's view, observational data are evidence for a theory only relative to a context of background assumptions. This is so because the theory says more than the statements describing the data. The theory is, in other words, underdetermined by the data (see Longino, 1990, pp. 38–61, for her defense of a contextualist account of epistemic justification). On Longino's account, the background assumptions are often biased.[5] So she raises the question of how scientific inquiry can be objective if the background assumptions of individual scientists are biased. She argues that the production of scientific knowledge can be said to be objective only if scientific results are subjected to evaluation by a plurality of individuals and formed by the ensuing discussions. Only then can we see how the influence of subjective assumptions and preferences can be blocked or mitigated (Longino, 1989, p. 266).[6]

Like Kuhn, Longino believes that scientific inquiry as practiced *is* generally objective. Longino has pointed out that scientific knowledge is not produced by just collecting the products of individuals but through a process in which those products are critically examined and modified by other scientists. Experiments thus "get repeated with variations by individuals other than their originators, hypotheses and theories are critically examined, restated, and reformulated before becoming an accepted part of the scientific canon" (Longino, 1989, p. 265). Hence, scientific inquiry is objective because what is ultimately accepted as a piece of knowledge is the result of a process formed by multiple individuals with different perspectives.

At the same time, Longino makes the case that scientific communities should strive for a *higher* degree of objectivity. Objectivity comes in degrees, and the more perspectives represented in the critical discussions that form scientific results, the more objective the process. This has important implications for the organization of science. The degree to which scientific inquiry is objective depends on the extent to which the scientific community is organized in such a way that a multiplicity of perspectives on a theory is elicited. Longino tentatively suggests a list of criteria that a scientific community must meet in order to support effective criticism. Included on the list are appropriate venues for criticism and that the community responds to criticism (Longino, 2006, pp. 172–173).

Other scholars have argued for a conception of objectivity comparable to Longino's (e.g., Haraway, 1988; Harding, 1992; Solomon, 1992, 2001; Wylie, 2013). For example, Donna Haraway (1988) speaks of "feminist objectivity" and states, "Feminist objectivity means quite simply *situated knowledges*" (p. 581). Haraway insists on "the embodied nature of all vision" and rejects that there is such a thing as "a conquering gaze from nowhere" (p. 581). It is the recognition of the contingency of knowledge claims that enables objectivity in science. Sandra Harding's (1992) "strong objectivity" requires not just the recognition of the situatedness of knowledge claims but also the recognition that certain marginalized perspectives are epistemically privileged.

If we accept a Longino-style account of scientific objectivity, peer review and post-publication criticism are important parts of the processes that secure the objectivity of science. Peer review serves not only to check that data seem right and that arguments are sound but also to introduce alternative viewpoints and thereby to minimize subjective biases. Similarly, post-publication reception in the form of replications, modifications, integration with other results, and so on also contributes to refining new pieces of knowledge and shaping them into what is ultimately accepted (Longino, 1990, p. 69). The process of peer review is examined in more detail in the next section.

4. Objectivity in Philosophy of Science in Practice

In this section, we provide an example of how our conception of scientific objectivity matters to current discussions of how science should be organized. We focus on the question of how peer review should be organized, a question that has attracted a lot of attention in the past four decades.

There are three common types of peer review: single-blind, double-blind, and open. The most commonly used is single-blind peer review, where the identity of the reviewer is concealed from the author, but the reviewer knows the identity of the author (Lee, Sugimoto, Zhang, & Cronin, 2013, p. 10; Ware, 2008, pp. 6–7). The main argument made for concealing the reviewer's identity to the author is to allow the reviewer to provide honest commentary without fear of repercussions. Double-blind peer review is also common, especially in the humanities and social sciences (Ware, 2008, pp. 6–7).[7] The main argument made for concealing the identity of the author to the reviewer is to protect the author against social bias on the part of the reviewer. When peer review is open, finally, authors and reviewers know each other's identities, and reviews may even be put online. In most such systems, the name of the reviewer is made public (Lee et al., 2013, p. 10).

Peer review can now be considered from two perspectives: (a) from the point of view that the objectivity of science is a function solely of the methods employed by the individual scientists and (b) from the point of view that the objectivity of science is also a function of how values and biases are distributed in the scientific community and brought to bear on the process of knowledge creation.

From the former perspective, concealing the identity of the author and/or reviewer contributes to the objectivity of science. This is so because in practice, scientists are not as objective as legend has claimed. Scientists sometimes fail to follow the scientific methods that ensure the objectivity of science on the former conception of objectivity. If reviewers always assessed papers with no regard to who the authors were, and if reviewers had no reason to fear retaliation from authors, there would be no need for concealing the identities of either author or reviewer. In sum, blinding the peer-review process contributes to maintaining the objectivity of science because scientists do not always follow scientific methods.

From the latter perspective, the objectivity of science is a function of whether the perspectives of multiple individuals with different values and biases are brought to bear on the process of knowledge creation. Hence, concealing the identities of authors and/or reviewers may mask the distribution of values and biases and may therefore *not* support the effort to maintain the objectivity of science. Not knowing who the author is will make it harder for the reviewer to uncover problematic background assumptions of the author. Likewise, not knowing who the reviewer is will make it harder for the author to uncover problematic background assumptions of the reviewer. Hence, on a Longino-style account of objectivity, disclosing author and reviewer identities can be an advantage. On the other hand, disclosing reviewer identities may work against the honest articulation of criticism because the reviewer fears retaliation from the author. Weighing such considerations is a complex task, and we shall not do so here. We merely want to emphasize that different conceptions of objectivity call for different approaches to the question of how peer review should be organized.

The last example we will consider involves open peer review of a certain kind. Some journals with open peer review make reviewer names and reviews *publicly* available. For example, the prominent medical journal *BMJ* publishes everything online: the reviewer comments and identities, the authors' responses, and the different versions of the articles (Groves & Loder, 2014; Smith, 1999, 2006). The aim of publishing everything online is to promote transparency and accountability. This transparency about reviewer comments and identities allows members of the community to assess the review while taking into account the theoretical and cultural perspective it represents, that is, while having a way of recognizing the background assumptions of the reviewer. In this way, reviews can be perspectives in the critical discussion of research on a par with other perspectives that are presented publicly (see also Lee et al., 2013, p. 11). These perspectives may even be especially helpful voices in the discussion since public peer review provides an incentive for reviewers to be both thorough and fair. Hence, if one conceives of objectivity as obtained at the level of scientific communities and as a function of the different perspectives that are brought to bear on the process of knowledge creation, this extreme kind of open peer review may help promote objectivity.

How we conceive of scientific objectivity not only matters to the question of how peer review should be organized but also to the question of how to *measure* the reliability of peer review of the different kinds. In particular, whether the level of agreement among reviewers is a good measure of the reliability of peer review depends on how we conceive of objectivity. Much theorizing about peer review is based on the assumption that agreement among reviewers is something desirable (cf. Lee, 2012, pp. 861–862). However, Kuhn's and Longino's accounts of science indicate that this is not necessarily the case. With reference to Kuhn, Lee (2012, pp. 863–867) argues that reviewers can disagree in normatively appropriate ways about how to interpret and apply evaluative criteria such as soundness, significance, and novelty. To use Kuhn's (1977, p. 325) own example, some scientists value originality more and are accordingly more prepared to take epistemic risks. They may reasonably have a lower evidential threshold for accepting a hypothesis. Furthermore, if Longino is right about the role of biases in the choices of individual scientists, a high level of inter-reviewer agreement could just as well be an indication that the referees have common biases. By the same token, low inter-reviewer agreement may be an indication that the scientific community in question supports effective criticism. Consequently, the fact that inter-reviewer agreement has been found to be low in empirical studies of peer review is not necessarily a cause for concern.

5. Conclusion

In this chapter, we have reviewed a number of accounts of scientific rationality and objectivity. Some philosophers consider scientific rationality and objectivity to be obtained at the level of individual scientists when the individual scientists follow a particular method for doing science. The chapter has presented divergent views on what that method consists in. Others argue that scientific rationality and objectivity are obtained at the level of communities of scientists. These accounts emphasize that individual scientists will be guided by different values, but that this in fact contributes to the rationality and objectivity of science at the level of scientific communities. We have presented different views on what this process looks like.

Notes

1. See Hoyningen-Huene (1987) for a detailed account of different interpretations of the distinction between the context of discovery and the context of justification.

2. For an example of a textbook aimed at university students that avoids conveying the idea that there is one scientific method, see Potochnik, Colombo, and Wright (2019).

3. For detailed accounts of the various positions advanced during these decades, see, for example, Nickles (2017/2020), Losee (2004, 2005), or Newton-Smith (1981).

4. For further details, see H. Andersen (2013).

5. Longino examines numerous cases of research in which she claims biases have played an important role. Among other things, she studies cases of research in human evolution and behavioral neuroendocrinology (see Longino, 1990, pp. 103–161). For a review of empirical studies of the role of bias in the peer review process in particular, see Lee, Sugimoto, Zhang, and Cronin (2013).

6. Popper (1934/2014) similarly claimed that "the objectivity of scientific statements lies in the fact that they can be *intersubjectively tested*" (p. 44). There is also an interesting similarity between Longino's account of scientific objectivity and Jürgen Habermas's account of how truth claims are justified. On Habermas's (1973) account, truth claims are justified in a critical discussion among free and uncoerced participants. The reasons for the claim should in principle be acceptable to any reasonable person (see also Bohman & Rehg, 2007/2017).

7. It is worth noting that double-blind peer review is sometimes not possible: the content of a manuscript will sometimes give away the author, which is presumably particularly true in highly specialized disciplines (L. E. Andersen, 2017, p. 178; Brown, 2007, p. 133; for further references, see Lee et al., 2013, p. 10).

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