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THE DIALECTICS OF SCIENTIFIC KNOWLEDGE AND PRAXIS

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ANNOTATION

Study of the social dimensions of scientific knowledge encompasses the effects of scientific research on human life and social relations, the effects of social relations and values on scientific research, and the social aspects of inquiry itself. Several factors have combined to make these questions salient to contemporary philosophy of science. These factors include the emergence of social movements, like environmentalism and feminism, critical of mainstream science; concerns about the social effects of science-based technologies; epistemological questions made salient by big science; new trends in the history of science, especially the move away from internalist historiography; anti-normative approaches in the sociology of science; turns in philosophy to naturalism and pragmatism. This entry reviews the historical background to current research in this area and features of contemporary science that invite philosophical attention.

Keywords: knowledge, social aspects of knowledge, praxis, knowledge and practice, social dimensions of cognition, *cogito*.

INTRODUCTION

The philosophical work can roughly be classified into two camps. One acknowledges that scientific inquiry is in fact carried out in social settings and asks whether and how standard epistemology must be supplemented to address this feature. The other treats sociality as a fundamental aspect of knowledge and asks how standard epistemology must be modified or reformed from this broadly social perspective. Concerns in the supplementing approach include such matters as trust and accountability raised by multiple authorship, the division of cognitive labor, the reliability of peer review, the challenges of privately funded science, as well as concerns arising from the role of scientific research in society. The reformist approach highlights the challenge to normative philosophy from social, cultural, and feminist studies of science while seeking to develop philosophical models of the social character of scientific knowledge and inquiry. It treats the questions of the division of cognitive labor, expertise and authority, the interactions of science and society, etc., from the perspective of philosophical models of the irreducibly social character of scientific knowledge. Philosophers employ both formal modeling techniques and conceptual analysis in their efforts to identify and analyze epistemologically relevant social aspects of science.

MATERIALS AND METHODS

Philosophers who study the social character of scientific knowledge can trace their lineage at least as far as John Stuart Mill. Mill, Charles Sanders Peirce, and Karl Popper all took some type of critical interaction among persons as central to the validation of knowledge claims.

DISCUSSION AND RESULTS

Mill's arguments occur in his well-known political essay On Liberty, [1] rather than in the context of his logical and methodological writings, but he makes it clear that they are to apply to any kind of knowledge or truth claim. Mill argues from the fallibility of human knowers to the necessity of unobstructed opportunity for and practice of the critical discussion of ideas. Only such critical discussion can assure us of the justifiability of the (true) beliefs we do have and can help us avoid falsity or the partiality of belief or opinion framed in the context of just one point of view. Critical interaction maintains the freshness of our reasons and is instrumental in the improvement of both the content and the reasons of our beliefs. The achievement of knowledge, then, is a social or collective, not an individual, matter.

Peirce's contribution to the social epistemology of science is commonly taken to be his consensual theory of truth: "The opinion which is fated to be ultimately agreed to by all who investigate is what we mean by truth, and the object represented is the real." [2] While often read as meaning that the truth is whatever the community of inquirers converges on in the long run, the notion is interpretable as meaning more precisely either that truth (and "the real") depends on the agreement of the community of inquirers or that it is an effect of the real that it will in the end produce agreement among inquirers. Whatever the correct reading of this particular statement, Peirce elsewhere makes it clear that, in his view, truth is both attainable and beyond the reach of any individual. "We individually cannot hope to attain the ultimate philosophy which we pursue; we can only seek it for the community of philosophers." [3]. Peirce puts great stock in instigating doubt and critical interaction as means to knowledge. Thus, whether his theory of truth is consensualist or realist, his view of the practices by which we attain it grants a central place to dialogue and social interaction.

Popper is often treated as a precursor of social epistemology because of his emphasis on the importance of criticism in the development of scientific knowledge. Two concepts of criticism are found in his works [4] and these can be described as logical and practical senses of falsification. The logical sense of falsification is just the structure of a modus tollens argument, in which a hypothesis is falsified by the demonstration that one of its logical consequences is false. This is one notion of criticism, but it is a matter of formal relations between statements. The practical sense of falsification refers to the efforts of scientists to demonstrate the inadequacies of one another's theories by demonstrating observational shortcomings or conceptual inconsistencies. This is a social activity. For Popper the methodology of science is falsificationist in both its logical and practical senses, and science progresses through the demonstration by falsification of the untenability of theories and hypotheses. Popper's logical falsificationism is part of an effort to demarcate genuine science from pseudo-science, and has lost its plausibility as a description of scientific methodology as the demarcation project has come under challenge from naturalist and historicist approaches in philosophy of science. While criticism does play an important role in some current approaches in social epistemology, Popper's own views are more closely approximated by evolutionary epistemology, especially that version that treats cognitive progress as the effect of selection against incorrect theories and hypotheses. In contrast to Mill's views, for Popper the function of criticism is to eliminate false theories rather than to improve them.

The work of Mill, Peirce, and Popper is a resource for philosophers presently exploring the social dimensions of scientific knowledge. However, the current debates are framed in the context of developments in both philosophy of science and in history and social studies of science following the collapse of the logical empiricist consensus. The philosophers of the Vienna Circle are conventionally associated with an uncritical form of positivism and with the logical empiricism that replaced American pragmatism in the 1940s and 1950s. According to some recent scholars, however, they saw natural science as a potent force for progressive social change. [5] With its grounding in observation and public forms of verification, science for them constituted a superior alternative to what they saw as metaphysical obscurantism, an obscurantism that led not only to bad thinking but to bad politics. While one development of this point of view leads to scientism, the view that any meaningful question can be answered by the methods of science; another development leads to inquiry into what social conditions promote the growth of scientific knowledge. Logical empiricism, the version of Vienna Circle philosophy that developed in the United States, focused on logical, internal aspects of scientific knowledge and discouraged philosophical inquiry into the social dimensions of science. These came into prominence again after the publication of Thomas Kuhn's *Structure of Scientific Revolutions*. A new generation of sociologists of science, among them Barry Barnes, Steven Shapin, and Harry Collins, took Kuhn's emphasis on the role of non-evidential community factors in scientific change even further than he had and argued that scientific judgment was determined by social factors, such as professional interests and political ideologies [6]. This family of positions provoked a counter-response among philosophers. These responses are marked by an effort to acknowledge some social dimensions to scientific knowledge while at the same time maintaining its epistemological legitimacy, which they take to be undermined by the new sociology. At the same time, features of the organization of scientific inquiry compel philosophers to consider their implications for the normative analysis of scientific practices.

The second half of the twentieth century saw the emergence of what has come to be known as Big Science: the organization of large numbers of scientists bringing different bodies of expertise to a common research project. The original model was the Manhattan Project, undertaken during the Second World War to develop an atomic weapon in the United States. Theoretical and experimental physicists located at various sites across the country, though principally at Los Alamos, New Mexico, worked on sub-problems of the project under the overall direction of J. Robert Oppenheimer. While academic and military research have since been to some degree separated, much experimental research in physics, especially high energy particle physics, continues to be pursued by large teams of researchers. Research in other areas of science as well, for example the work comprehended under the umbrella of the Human Genome Project, has taken on some of the properties of Big Science, requiring multiple forms of expertise. In addition to the emergence of Big Science, the transition from small scale university or even amateur science to institutionalized research with major economic impacts supported by national funding bodies and connected across international borders has seemed to call for new ethical and epistemological thinking. Moreover, the consequent dependence of research on central funding bodies and increasingly, private foundations or commercial entities, prompts questions about the degree of independence of contemporary scientific knowledge from its social and economic context.

John Hardwig [7] articulated one philosophical dilemma posed by large teams of researchers. Each member or subgroup participating in such a project is required because each has a crucial bit of expertise not possessed by any other member or subgroup. This may be knowledge of a part of the instrumentation, the ability to perform a certain kind of calculation, the ability to make a certain kind of measurement or observation. The other members are not in a position to evaluate the results of other members' work, and hence, all must take one anothers' results on trust. The consequence is an experimental result, (for example, the measurement of a property such as the decay rate or spin of a given particle) the evidence for which is not fully understood by any single participant in the experiment. This leads Hardwig to ask two questions, one about the evidential status of testimony, and one about the nature of the knowing subject in these cases. With respect to the latter, Hardwig says that either the group as a whole, but no single member, knows or it is possible to know vicariously. Neither of these is palatable to him. Talking about the group or the community knowing smacks of superorganisms and transcendent entities and Hardwig shrinks from that solution. Vicarious knowledge, knowing without oneself possessing the evidence for the truth of what one knows, requires, according to Hardwig, too much of a departure from our ordinary concepts of knowledge.

The first question is, as Hardwig notes, part of a more general discussion about the epistemic value of testimony. Much of what passes for common knowledge is acquired from others. We depend on experts to tell us what is wrong or right with our appliances, our cars, our bodies. Indeed, much of what we later come to know depends on what we previously learned as children from our parents and teachers. We acquire knowledge of the world through the institutions of education, journalism, and scientific inquiry. Philosophers disagree about the status of beliefs acquired in this way. Here is the question: If *A* knows that *p* on the basis of evidence *e*, *B* has reason to think *A* trustworthy and *B* believes *p* on the basis of *A*'s testimony that *p*, does *B* also know that *p*? Some philosophers, as Locke and Hume seem to have, argue that only what one has observed oneself could count as a good reason for belief, and that the testimony of another is, therefore, never on its own sufficient warrant for belief. Thus, *B* does not know simply on the basis of *A*'s testimony but must have additional evidence about *A*'s reliability. While this result is consistent with traditional philosophical empiricism and rationalism, which emphasized the individual's sense experience or rational apprehension as foundations of knowledge, it does have the consequence that we do not know most of what we think we know.

A number of philosophers have recently offered alternative analyses focusing on one or another element in the problem. Some argue that testimony by a qualified expert is itself evidential, [8], others that the expert's evidence constitutes good reason for, but is not itself evidential for the recipient of testimony, others that what is transmitted in testimony is knowledge and not just propositional content and thus the question of the kind of reason a recipient of testimony has is not to the point [9].

However this dispute is resolved, questions of trust and authority arise in a particularly pointed way in the sciences, and Hardwig's dilemma for the physics experiment is also a specific version of a more general phenomenon. A popular conception of science, fed partly by Popper's falsificationism, is that it is epistemically reliable because the results of experiments and observational studies are checked by independent repetition. In practice, however, only some results are so checked and many are simply accepted on trust. Not only must positive results

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be accepted on trust, but claims of failure to replicate as well as other critiques must be also. Thus, just as in the non-scientific world information is accepted on trust, so in science, knowledge grows by depending on the testimony of others. What are the implications of accepting this fact for our conceptions of the reliability of scientific knowledge?

The philosopher of biology, David Hull, argued in his book [10] that because the overall structure of reward and punishment in the sciences is a powerful incentive not to cheat, further epistemological analysis of the sciences is unnecessary. What scientists have to lose is their reputation, which is crucial to their access to grants, collaborations, prizes, etc. So the structure itself guarantees the veridicality of research reports. But some celebrated recent episodes, such as the purported production of "cold fusion" were characterized by the failure of replication attempts to produce the same phenomenon. And, while the advocates of cold fusion were convinced that their experiments had produced the phenomenon, there have also been cases of outright fraud. Thus, even if the structure of reward and punishment is an incentive not to cheat, it does not guarantee the veridicality of every research report.

On Hull's view, the scientific community seeks true theories or adequate models. Credit, or recognition, accrues to individuals to the extent they are perceived as having contributed to that community goal. That is, individual scientists seek reputation and recognition, to have their work cited as important and as necessary to further scientific progress. Cheating, by misreporting experimental results or other misconduct, will be punished by loss of reputation. But this depends on strong guarantees of detection. Absent such guarantees, there is as strong an incentive to cheat, to try to obtain credit without necessarily having done the work, as not to cheat.

Both Alvin Goldman and Philip Kitcher have treated the potential for premature, or otherwise (improperly) interested reporting of results to corrupt the sciences as a question to be answered by means of decision theoretic models [11]. The decision theoretic approach to problems of trust and authority treats both credit and truth as utilities. The challenge then is to devise formulas that show that actions designed to maximize credit also maximize truth. Kitcher, in particular, develops formulas intended to show that even in situations peopled by non-epistemically motivated individuals (that is, individuals motivated more by a desire for credit than by a desire for truth), the reward structure of the community can be organized in such a way as to maximize truth and foster scientific progress. One consequence of this approach is to treat scientific fraud and value or interest infused science as the same problem. One advantage is that it incorporates the motivation to cheat into the solution to the problem of cheating. But one may wonder how effective this solution really is. Increasingly, we learn of problematic behavior in science based industries, such as the pharmaceutical industry. Results are withheld or distorted, authorship is manipulated. Hot areas, such as stem cell research, cloning, or gene modification, have been subjected to fraudulent research. Thus, even if the structure of reward and punishment is an in principle incentive not to cheat, it does not guarantee the reliability of every research report. The decision theoretic model needs to include at least one more parameter, namely the anticipated likelihood of detection within a relevant timeframe.

Community issues have also been addressed under the banners of research ethics and of peer review. One might think that the only ethical requirements on scientists are to protect their research subjects from harm and, as professional scientists, to seek truth above any other goals.

This presupposes that seeking truth is a sufficient guide to scientific decision-making. Heather Douglas, in her critical study of the ideal of value-freedom, rejects this notion. Douglas draws on her earlier study of inductive risk to press the point that countless methodological decisions required in the course of carrying out a single piece of research are underdetermined by the factual elements of the situation and must be guided by an assessment of the consequences of being wrong. Science is not value-free, but can be protected from the deleterious effects of values if scientists take steps to mitigate the influence of inappropriate values. One step is to distinguish between direct and indirect roles of values; another is the articulation of guidelines for individual scientists. Values play a direct role when they provide direct motivation to accept or reject a theory; they play an indirect role when they play a role in evaluating the consequences of accepting or rejecting a claim, thus influencing what will count as sufficient evidence to accept or reject. The responsibility of scientists is to make sure that values do not play a direct role in their work and to be transparent about the indirect roles of values. A number of writers have taken issue with the tenability of Douglas's distinction between direct and indirect. Steel and Whyte examine testing guidelines developed by pharmaceutical companies to point out that the very same decision may be motivated by values playing a direct role or playing an indirect role. If the point is to prohibit practices such as withholding negative results, then it shouldn't matter whether the practice is motivated by values functioning directly or indirectly. Elliott questions whether only harmful consequences should be considered. If science is to be useful to policy makers, then questions of relative social benefit should also be permitted to play a role. Finally the cognitive activities demanded by Douglas's ethical prescriptions for scientists seem beyond the capacities of individual scientists. This point will be pursued below.

Torsten Wilholt argues that the research situation is more complicated than the epistemic vs. nonepistemic tradeoff implied by the decision theoretic approach. In part because of the difficulties in achieving the degree of knowledge required to realize Douglas's ethical prescriptions, he argues that the reliance called for in science extends beyond the veridicality of reported results to the values guiding the investigators relied upon. Most research involves both results expressed statistically (which requires choice of significance threshold and balancing chances of Type I vs. Type II error) and multiple steps each requiring methodological decisions. These decisions, Wilholt argues, represent trade-offs among the reliability of positive results, the reliability of negative results, and the power of the investigation. In making these tradeoffs, the investigator is per force guided by an evaluation of the consequences of the various possible outcomes of the study. Wilholt extends the arguments about inductive risk offered originally by Richard Rudner and elaborated by Heather Douglas to propose that, in relying on another's results I am relying not only on their competence and truthfulness, but on their making methodological decisions informed by the same valuations of outcomes as I have. This attitude is more than epistemic reliance, but a deeper attitude: one of trust that we are guided by the same values in a shared enterprise. For Wilholt, then, scientific inquiry engages ethical norms as well as epistemic norms. Formal or mechanical solutions such as those suggested by the application of decision theoretic models are not sufficient, if the community must be held together by shared ethical values.

Peer review and replication are methods the scientific community, indeed the research world in general, employs to assure consumers of scientific research that the work is credible. Peer review both of research proposals and of research reports submitted for publication screens for quality, which includes methodological competence and appropriateness as well as for originality and significance, while replication is intended to probe the robustness of results when reported experiments are carried out in different laboratories and with slight changes to experimental conditions. Scholars of peer review have noted various forms of bias entering into the peer review process. In a review of the literature, Lee, Sugimoto, Zhang, and Cronin report documented bias along gender, language, nationality, prestige, and content as well as such problems as lack of inter-reviewer reliability consistency, confirmation bias, and reviewer conservatism. Lee argues that a Kuhnian perspective on values in science interprets lack of inter-reviewer consistency as variation in interpretation, applicability, and weight assigned to shared values by different members of the scientific community. Lee and colleagues argue that journal editors must take much more action than is currently taken to require that researchers make their raw data and other relevant trial information available to enable peer reviewers to conduct their work adequately.

One issue that has yet to be addressed by philosophers is the gap between the ideal of replication resulting in confirmation, modification, or retraction and the reality. This ideal lies behind the assumptions of efficacy of structures of reward and sanction. Only if researchers believe that their research reports will be probed by efforts at replication will the threat of sanctions against faulty or fraudulent research be realistic. John Ioannidis and collaborators have shown how infrequently attempts to replicate are actually made and, even more strikingly, how contradicted results persist in the literature. This is an issue that goes beyond individuals and beyond large research collaborators to the scientific community in general. It underscores Wilholt's contention that the scientific community must be held together by bonds of trust, but much more empirical and philosophical work is needed to address how to proceed when such trust is not justified. The demonstration of widespread lack of replicability on studies in psychology and in biomedical research has prompted debate about the causes and the seriousness of the alleged crisis.

CONCLUSION

As a conclusion, let us draw attention to a different kind of supra-empirical, ethical issue raised by the contemporary situation of multiple authorship. What they call "radically collaborative research" involves investigators with different forms of expertise, as in Hardwig's example, and as is now common across many fields, collaborating to generate an experimental result. The question is not merely reliability, but accountability. Who can speak for the integrity of the research when it has been conducted by researchers with a variety not just of interests, but of methodological standards, opaquest one to another? Many argue that a model of the social collaboration is needed as much as a model of the data or of the instruments. They argue further that the laissez-faire Wisdom of Crowds model (according to which local differences in methodological standards will cancel each other out), while perhaps adequate if the question is one of reliability, is not adequate for addressing these issues of accountability. They do not themselves, however, offer an alternative model.

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