Socially Relevant Philosophy of Science: A Contextualist Program

Abstract. This paper introduces a heuristic framework for contextualist approaches in the philosophy of science. Starting with the contextualist proposals of Nancy Cartwright and Janet Kourany from the 2004 PSA Proceedings, the paper draws on ethnographic findings from the social sciences to argue that the contextualist elements of their proposals need not lead to the objectionable relativism associated with some forms of contextualism; rather, the proposals call for a multilayered, interdisciplinary framework for understanding and evaluating how evidence and argument warrant scientific claims in context-specific modalities. After briefly illustrating the use of this framework with a case study, the paper closes with suggestions for future research. (105 words)

1. Introduction. A symposium published in the 2004 PSA Proceedings addressed the following question: how should philosophy of science be socially relevant?[1] This question has a noble heritage: before the Second World War both logical empiricists and pragmatists had an interest in the political implications of their philosophies of science (Reisch 2005). In the last several decades, however, philosophers of science have largely concentrated on technical and abstract questions, leaving the social-political issues for the most part to feminists (e.g., Longino 1990; Harding 1998) or to the broader field of STS (science and technology studies), which has long had a social-critical branch (Hess 1997; Fuller 2000). More recently, however, one can detect shifts in both fields. Among STS scholars, interest in the social-political question has spread, embracing not only critical

theorists like Steve Fuller but also more empirically minded researchers (e.g., Evans and Plows 2007); it has also taken a turn toward more active involvement with policymaking and law (Lynch and Cole 2005; Woodhouse et al. 2002). Among philosophers, the interest has recently spread beyond feminists, capturing the attention of such prominent figures as Philip Kitcher (2001), not to mention historians interested in early logical positivism (e.g., Uebel 2000). I take the 2004 symposium as one sign of this development.

In responding to the symposium question, Nancy Cartwright (2006) called on philosophers of science to turn their analytic skills from abstract theories of evidence to situated studies of "evidence in use." Janet Kourany (2006), drawing on feminist philosophy of science, stressed the importance of the variable aims of science for any attempt to "societize" philosophy of science. A particular worry here, especially for those scarred by the "science wars," arises from the strong association between contextualism and relativist or social-constructivist approaches to the rationality of the sciences. In this paper I introduce a contextualist approach that can incorporate elements of Kourany and Cartwright's proposals in a broad framework that disarms objectionable forms of relativism—indeed, using tools one can find within sociological contextualism itself, whose case studies prove more directly useful here than the high-altitude reflections of analytic epistemologists (e.g., Gerhard and Preyer 2005). Consequently, the proposed framework has the advantage of drawing attention to the ground-level complexities of science in context; in addition, it opens up possibilities for the kind of interdisciplinary cooperation that Kourany and Cartwright's proposals require.

2. The Turn to Context. Cartwright and Kourany are both concerned with how various circumstances of use—natural or environmental conditions, particular social processes, social-political aims—can affect the evidence or warrant for scientific claims, thus shaping the very content of good science.

Kourany (2006) presents the more digestible view. Because scientists are "sometimes sensitive to the wider social context," social-political values (not to mention economic considerations) can constrain their methodological and theoretical choices (ibid., 993). The effect of such values on methodology should not surprise us, as it follows from familiar considerations of inductive risk (Douglas 2000); here one of the best-known cases is AIDS-treatment research, where patients' interest in access to possible cures led researchers to relax the usual experimental controls, among other things (Epstein 1995). The role of non-epistemic values in theory-formation—for example, scientists avoiding theories of sexual orientation that would pathologize homosexuality (Kourany 2006, 993)—is more problematic from a traditional epistemological standpoint, though feminists have long incorporated this phenomenon into their conceptions of scientific practice.

In any case, the presence of social-political values in scientific inquiry, according to Kourany, belies the prevalent philosophical assumption that truth (or the usual stand-ins such as empirical success, explanatory adequacy, etc.) constitutes the sole aim of science. As a descriptive claim about actual scientific practice, this assumption is simply false. Normatively, Kourany's observations point to something akin to Kitcher's idea of a

well-ordered science: "the needs of society, including the justice-related needs of society, and the ways that science might satisfy those needs are also relevant when determining what the aim of science ought to be" (Kourany 2006, 994; cf. Kitcher 2001). Insofar as social values enter into the very content of science—its methods and theorizing—epistemological questions about good science are inextricably intertwined with normative political questions regarding values and legitimate social aims. This need not devastate epistemologists of science: if political issues are open to rational deliberation as many democratic theorists insist, then the rationality of science remains intact, even if not "purely" epistemic.

Cartwright's view is more radical. She starts with Kitcher: a well-ordered science should "answer the *right* questions in the *right* ways, where value judgements and methodological issues are inextricably intertwined in determining what is *right*" (Cartwright 2006, 982). To answer scientific questions in the right ways, however, one cannot simply reflect on evidence in general; rather, one must attend to "evidence in use." She then draws two radical conclusions: (1) "what a claim means in the context in which it is first justified may be very different from what it means in the different contexts in which it will be put to use"; (2) "[w]hat justifies a claim depends on what we are going to do with that claim, and evidence for one use may provide no support for others" (ibid., 983). As she explains with an example from quantum physics, the different understandings of a given claim in two different contexts "implicate the claim in a radically different network of inference and assumption, so different that the claim must be assigned a different sense for the two groups" (ibid., 984).

Cartwright sees that her proposal calls for engagement with the social sciences. In fact, her Wittgensteinian approach to meaning and warrant has a natural ally in the ethnomethodology of scientific work (ESW), a radical brand of sociology that eschews the attempt to unify scientific practices under general norms of rationality or method (Lynch 1993). Rather, to understand the rationality of scientific claims, ethnomethodologists focus on local practices of science—the daily routines, say, in a particular laboratory (Lynch 1985). In doing so, they strive to notice and describe the patterns of behavior and associated modes of mutual accountability that make up the highly local, "situated rationality" of those routines, which includes the diverse, often ad hoc discursive and procedural moves that members of the practice employ to reach agreement on data interpretation and to maintain the ongoing mutual intelligibility of their practice. Many of these moves are taken for granted by practitioners: they constitute elements of a tacit know-how, noticed only when someone fails to perform.

What results from ESW is a highly contextualist approach to the rationality of science. Here the key idea lies in the mutual accountability that informs local scientific practices. Competent members know how to interpret each other's behavior and discourse, and they know how to criticize, justify, explain, and excuse deviations from expected patterns—to call for and provide "accounts" of their practice. These various methods of accounting certainly invoke general rules of method, established procedures, mathematical techniques, and the like. But such generalities always have an indexical character, such that their full sense for practitioners remains "reflexively tied" to the local occasions of their use (where "reflexivity" refers not to self-awareness but to a relation

between general accounts or discourse and practical engagement). Part of scientific competence consists in familiarity with these contexts of use, such that practitioners can contextualize, and thus appropriately interpret, each other's behavior and discourse.

Although ESW remains controversial, what interests me here is the plausible assumption that the interactions of scientists are not free of the ongoing interpretive demands that human beings generally face in everyday life: the need to fill in gaps in conversation, to negotiate ambiguities, to contextualize behaviors, to smooth over misunderstandings. Maintaining the ongoing intelligibility of everyday social interaction requires interpretive accomplishments on the part of actors, who draw on background know-how that is largely tacit and heavily local, rooted in the history of particular interpersonal relationships. The degree to which tacit background knowledge informs scientific reasoning becomes apparent in attempts at interdisciplinary communication. Successful communication involves far more than simply learning the other discipline's language; one must learn how to judge the force of its questions and arguments as its own members do, which is a far more difficult endeavor, as I suspect those who have attempted such communication have learned.

In any case, how far the indexical character of scientific statements extends, and exactly how it operates, varies with context and thus remains an open research question. But Eric Livinston's detailed studies of mathematical "cultures of proving" indicate that such reflexive ties to practice may be pervasive (1995, 1999). Mathematical proofs appear most universal, objective, and anonymous, as Livingston grants. Nonetheless, Livingston makes a plausible case, at least for some forms of proving, that the formulated

proof by itself (the "proof-account") is a partial description that gains its full force only in relation to the practices of a particular culture of proving and the actual "lived work" of proving. An effective proof-account supplies just those relevant concrete details that allow other members of the mathematical culture to contextualize the account and "do" the work of proving. These contextualizing moves involve ordinary bodily moves—for example, knowing the habitual sequence of physical actions involved in constructing a diagram—that competent members take them for granted (if they notice them at all)—to the point where drawing explicit attention to them appears incompetent or weird, and possibly creates confusion.

Similar to Cartwright, then, ESW assumes that the full sense of any rule of method, experimental procedure, theory, and so on—all of which scientists invoke in accounting for what they do—depends on its local context of use. The same holds for claims about the evidence or warrant for scientific results: one must always evaluate such claims in their local context and its particular forms of accountability. However, as Cartwright realizes, this radical contextualism creates an acute problem: if warrant is inextricably tied to particular contexts, "[h]ow then can warrant travel from experiment to use?" (2006, 984) Her question applies to a wide range of cross-context transitions: from the context of theorizing to laboratory or field practices, from one research site to another, from one discipline to the another, from science to policymaking: how do claims warranted in one context travel to other contexts? Like a shattered windshield, the rationality of science and its applications falls apart into a thousand shards, each context its own final authority on what counts as adequate warrant, or even what counts as "true."

The result seems to be some form of relativism or even a kind of nihilism about science as public knowledge: relativism, insofar as the warrant for, and probable truth of, scientific claims entirely depend on the contexts in which those claims are considered; nihilism, insofar as deep meaning variance isolates mutually incommensurable contexts from any genuine communication at all: public knowledge does not exist.

3. Toward a Non-Relativist Contextualism. I have started with Kourany and Cartwright because they provide forceful introductions to a contextualist philosophy of science. Rather than pursue the details of their research programs further, I now take a wider tack, setting the stage for a broad contextualist framework that can integrate proposals such as Cartwright's and Kourany's without falling into relativism or nihilism. Once again, ESW can serve as an ally: its own case studies provide the first clue to the solution, namely by helping us notice how ideas of objectivity and warrant function for practitioners within local contexts. In designing their experiments, interpreting data, and writing up results, members of a research team strive for accountability not only to each other—they also have in view broader contexts of accountability, above all possible audiences within their discipline or subdiscipline. Although this point is rather obvious, ESW arrives at it not by starting with assumptions about general rules of method but by carefully attending to local practices in particular contexts (e.g., Garfinkel et al. 1981).

The contextualism we get from ESW, in other words, has a multilayered character: on the one hand, as members of a given discipline, scientists working in an area of research are all more or less familiar with some set of general concepts,

mathematical formulations, ideas of evidence, types of instrumentation, and the like. On the other hand, given their indexical character, these shared resources do not fully dictate their meaning and use in local contexts of inquiry. Consequently, the rationality of scientific practices, and the warrant for scientific claims, cannot be completely understood in terms of such discipline-wide resources—one must see how practitioners situate them in local contexts of inquiry in their quest to develop fruitful research practices. For those familiar with the situated use of such resources, however, cross-site communication becomes possible insofar as members are able to contextualize statements about each other's local research activities. The shared general vocabulary employed in spoken and written accounts of research (at conferences, in articles) provides a set of cues that are semantically underdetermined at the general level, but contextualizable and thus understandable for experienced practitioners.

Radical contextualism itself, then, undoes the danger of encapsulation for scientific practices. Moreover, it uncovers the normative character of cross-context communication: in their communication of research, scientists hold each other accountable for justifying their claims, and they criticize each other for failing to do so. Further ethnomethodological studies (Pollner 1974) indicate that practices like that of the natural sciences depend on an idea of objectivity, i.e., a presumption that members share a common objective world and that statements about that world ought to cohere. Consequently, when results from two laboratories conflict, members hold themselves accountable for explaining the discrepancy (often by criticizing each other's practice; see Gilbert and Mulkay 1984). This idea of objectivity, which issues not from abstract

philosophical speculation but from studies of actual practice, supports a pragmatic understanding of truth as a property of statements that meet these cross-contextual demands of objectivity (McCarthy 1994, chap. 3). Given limitations of space, however, I focus here warrant.

We can now see the beginning of an answer to Cartwright's question regarding warrant in motion: the warrant for scientific claims can travel because scientists working in one context *design* their accounts for travel. They seek to account not only for the demands specific to their own context but also for those of other contexts. Again, this point is based on close empirical studies of the actual practice of science, and so answers to Cartwright's question call for attention, informed by input from the social sciences, to actual practice (2006, 984).

4. A Contextualist Framework for Assessing Warrant. The ethnomethodological research on scientific practices shows that scientists work simultaneously in local and disciplinary contexts of accountability. I now want to outline a contextualist framework for the study and evaluation of warrant and "evidence for use"—or more broadly, for the evaluation of scientific arguments, claims warranted by reasons that include empirical evidence.

The basic idea is to distinguish the different levels of context that concern scientists and those who use science in different ways and for different purposes. In general, I think one can identify at least three levels of context that are important for understanding how scientific warrant travels:

- The context given by the content of scientific arguments, that is, papers and articles that attempt to show that its conclusion is warranted by the evidence (along with other kinds of reasons); at this level of context, the sense of particular scientific statements depends on the set of assumptions and arguments that warrant it.
- The local or "transactional" context in which scientific inquiry is conducted (the research team) or evaluated (a particular conference, an expert committee, etc.): at this level, and only at this level, do scientists and those who evaluate or use science actually engage scientific arguments in a particular set of local circumstances and with specific aims in view; I take the paradigmatic example of transactional contexts to be the face-to-face setting in which a small group of participants construct or evaluate a scientific claim and its warrant.
- The broader public context for which scientific claims and arguments are designed, or to which they might eventually travel; the public context functions as a kind of intentional object—those to whom an argument is addressed—for members of a transactional setting, for example, the sub-discipline, technologists for whom a scientific claim is relevant, and so on; concretely, public contexts comprise an indefinite series of local transactional sites.

To understand how scientific warrant travels, one must analyze the interplay among these levels. The forms of interplay are as diverse as the forms and uses of science. The most straightforward case involves the following relationships: at a particular research site (transactional context of generation), scientists construct their

argument or evidence claim (content level) with a view toward some intended audience, normally other members of their discipline (public context). Those members, however, engage such arguments in further local settings (transactional contexts of reception and uptake), which diversely affect their understanding of the original content; insofar as they find the argument convincing and usable in their own research, the original content can be said to "travel" through the discipline. We may then say that such arguments demonstrate their "public merits," insofar as the further contexts are relevant, reasonable, and linked in a well-structured network or public space (see Rehg 2008).

This framework has a number of advantages for philosophers interested in a more socially relevant philosophy of science. First, it recognizes the complexity of the contexts in which arguments and evidence provide warrant for scientific claims. At one level, the content of the arguments themselves constitute a context: the set of statements that we take (or reconstruct) as an argument is a context for understanding the force of any particular statement. Thus the framework supports Kourany's prognosis that the abstract analyses of scientific arguments will not disappear with a more contextualist approach (2006, 997). However, such abstract analyses will have to prove their relevance in particular contexts, and so any generality they provide cannot be the last word. Rather, an adequate analysis and evaluation of the content of scientific arguments requires attention to the complex social context: both to the local demands on, and aims of, particular transactional contexts as well as the connections of relevance that link the original transaction to broader public contexts: at the least, other members of the discipline, but perhaps policymakers and lay publics as well.

The first advantage leads to the second: the framework naturally supports an interdisciplinary approach in which the understanding and assessment of warrant requires attention to social circumstances. (Indeed, the framework itself emerges from interdisciplinary engagement with ethnomethodology, argumentation theory, and rhetoric; see Rehg 2009.) Again, the matter of study is multilayered: one must attend both to the conditions shaping the local transactions in which warrant is at issue, as well as the ways in which arguments aim at publics of various sorts. Moreover, normative evaluation of warrant requires attention to how publics are reasonably linked or not. This kind of evaluation becomes especially important for anyone who accepts the idea of science as a kind of public knowledge (Ziman 1968). If science implies public knowledge, then claims that fail to travel beyond the original transaction fail as science. But that implication follows only on the assumption that publics, and the myriad transactions that constitute publics, are reasonably structured: that effective public venues exist, that communication processes are adequate, not subverted by strong biases, and so on. Investigation of these assumptions requires input not only from the social sciences and communication studies but also from normative theories of argumentation and the rhetoric of science.

Third, the tripartite appearance notwithstanding, the framework is open-ended, allowing for context-sensitive development and adaptation. I do not intend the triad of context-levels as an exhaustive grid into which all scientific argumentation should fit. For example, consider a plenary address at a large conference. Do we treat it as a single transaction (because everyone is present in the same place, hears what everyone else

hears, etc.), or as a set of transactions that form an occasional public (because each audience members brings his or her own background knowledge to the address, normally does not speak with other audience members, etc.)? Rather than force the choice, one might simply regard it as a distinct kind of context that makes its own demands on how scientific content is delivered and understood.

One should also be prepared to find interesting ways in which the levels of context are related, beyond the straightforward case sketched above. To illustrate this point, I provide a brief example of how the framework can illuminate contextual effects in science advisory committees.

5. An Example from Regulatory Science. Expert committees, such as those at the National Academy of Sciences (NAS), are transactional sites in which a small group of scientists (perhaps along with administrators) make a kind of argument, often an overview or report on the extant literature from which they draw conclusions about what the current science "says" on some question. Such reports are explicitly addressed to different publics: normally to policymakers, sometimes to health care practitioners, often to the broader public as well. In his study of NAS-sponsored reports on diet and health, Hilgartner (2000) shows that expert committees are concerned not only to present the scientific findings but also to present themselves as credible experts. The evidence for the latter—which Hilgartner finds in the front matter, preface, and appendix of the report, as well as in NAS procedural brochures and the like—lies in statements about committee design: citations of prestigious institutional affiliations, statements about the breadth of

disciplinary expertise and consultation, the objectivity of its procedures, and so on. Such statements attempt to show that the experts "do not speak for particular organizations or constituencies; they represent the viewpoints of their specific areas of knowledge" (ibid., 48).

In terms of the framework I propose, a committee that presents its report this way recognizes the importance of transactional quality for the public acceptability of its claims. The decisive feature of the transaction is this: that the committee members qualify as representatives of the larger "republic of science". As framing the report, this demonstration of transactional quality becomes part of the *content* of the argument that the committee makes to lay publics: specifically, the committee claims that its conclusions enjoy broad acceptability—i.e., public merits—within the relevant scientific disciplines, such that those conclusions deserve to travel widely in policymaking and lay publics. The idea of public merits operates here at two levels, as both warrant and conclusion: as a claim about the merits of the findings before scientific publics, which is supposed to warrant the conclusion that the argument deserves uptake before lay publics as well.

This case implies as well that warrant can shift with context as a function of audience. Within a given subdiscipline, scientists working on a particular problem typically judge evidence claims by scrutinizing the technical content of the argument; mainstream philosophy of science has supported this practice by attempting to understand warrant in such technical terms. The NAS case suggests that, when experts speak to non-expert policymakers and lay publics, warrant partly depends on evidence for the

transactional quality of the experts' engagement with the extant scientific literature, that is, on the evidence that their assessment of the literature is "well-traveled," as it were, within scientific publics.

Among STS scholars (e.g. Evans and Plows 2007; Jasanoff 2005), what I would call the "transactional merits" of expert advice has become an area of intense concern. If philosophers of science are to contribute to such research, they must move beyond their focus on the technical aspects of scientific warrant and evidence, and examine the ways that transactional and public contexts shape argument content for better or worse. And to understand and evaluate such contextual effects—evidence in use—they must seek input from the rhetoric of science, sociology, and normative theories of dialectical argumentation. Hilgartner's analysis of NAS committees, for example, integrates sociology and rhetoric. To be sure, such disciplinary cross-fertilization has been underway at least since the 1990s (e.g., Machamer et al. 2000). If Kourany and Cartwright's proposals win adherents, then we should expect more interdisciplinary initiatives.

6. Conclusion. In this paper I have taken the contextualist proposals of Kourany and Cartwright as a handle for introducing an interdisciplinary contextualist framework that does not fall prey to the forms of relativism or skepticism that is associated with many forms of contextualism in STS. This framework, I claim, opens up the analysis of warrant and evidence to the complex, case-specific effects of context on scientific argumentation. The case illustration, though condensed, gives a concrete example of how warrant can

shift with context.

The proposal raises a number of further questions for research. One of the more interesting, in my opinion, arises from the prescriptive potential of the framework. Although one can employ the framework simply as a descriptive heuristic for tracking the fate of scientific arguments as they originate and travel (or fail to travel) through different contexts, the concern with evidence and warrant calls for a normative evaluation of contextual effects. One of the issues here concerns how and how far warrant *ought* to travel beyond scientific points of origin. If science is public knowledge, then it seems that well-supported scientific claims ought to find public acceptance—they ought to have public merits. However, on a contextualist approach, one cannot infer universal acceptability simply on the basis of warrants specific to scientific contexts of origin. I have in mind here a stronger sense of "acceptability" than the layperson's blind faith that physicists, say, have good evidence for their claims about subatomic particles. On the contextualist approach, for a claim or argument to travel beyond its context of origin, and thereby demonstrate its broader public merits, means that it *proves itself relevant* for a new context and *holds up under reasonable conditions of warrant specific to that context*. By contrast, when I read a popular account of a scientific discovery, it is more accurate to say that the author has helped me understand the warrant in its original context, rather than to say that the warrant for the discovery has now traveled to my lay context.

This strong sense of the public merits of scientific arguments requires us to clarify at least two issues: (1) the idea of cross-contextual relevance, not only how arguments have relevance for new contexts but how a new context might be relevant for testing the

strength of a scientific claim; (2) what conditions of warrant are reasonable for testing the public merits of scientific claims. We do not consider every context as automatically relevant for testing the public merits of scientific claims, and not all the contexts that appear prima facie relevant set reasonable conditions for warrant. Working out detailed analyses of these issues has important implications, particularly in pluralistic societies whose citizens must somehow integrate diverse systems of meaning and discourse in grappling with the claims of science.

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[1]**Notes**

See the last page of Contents in Solomon (2006).