

Trust, expertise, and the philosophy of science

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Abstract Trust is a central concept in the philosophy of science. We highlight how trust is important in the wide variety of interactions between science and society. We claim that examining and clarifying the nature and role of trust (and distrust) in relations between science and society is one principal way in which the philosophy of science is socially relevant. We argue that philosophers of science should extend their efforts to develop normative conceptions of trust that can serve to facilitate trust between scientific experts and ordinary citizens. The first project is the development of a rich normative theory of expertise and experience that can explain why the various epistemic insights of diverse actors should be trusted in certain contexts and how credibility deficits can be bridged. The second project is the development of concepts that explain why, in certain cases, ordinary citizens may distrust science, which should inform how philosophers of science conceive of the formulation of science policy when conditions of distrust prevail. The third project is the analysis of cases of successful relations of trust between scientists and non-scientists that leads to understanding better how ‘postnormal’ science interactions are possible using trust.

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1 Introduction

Trust is a central concept in the philosophy of science. Its centrality is often overlooked when philosophers reduce science to a mechanical process of hypothesis proposal and

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confirmation. But when philosophers take their point of departure from what scientists actually do and the contexts within which they work, the centrality of trust in scientific communities is undeniable (Hardwig 1985, 1991). Scientists have to put trust in the data and products of others' research, in their colleagues' testimony, and in the disciplinary and institutional structures encompassing them. Neither scientific research in laboratories nor the institutions that govern scientific conduct would be possible without trust (Hardwig 1985, 1991).

We are concerned with highlighting another area in which trust is important—the wide variety of interactions between science and society. We claim that examining and clarifying the nature and role of trust (and distrust) in relations between science and society is one principal way in which the philosophy of science is socially relevant. Such endeavors contribute to better understanding and resolving public controversies where issues of trust and distrust impede deliberative decision making and limit the public benefits that can be provided by scientists and scientific research activities. Among the many possible contributions to this area, we argue that, in particular, philosophers of science should extend their efforts to develop normative conceptions of trust that can serve to facilitate trust between scientific experts and ordinary citizens. We suggest three normative projects. The first project is the development of a rich normative theory of expertise and experience that can explain why the various epistemic insights of diverse actors should be trusted in certain contexts and how credibility deficits can be bridged by rethinking expertise. The second project is the development of concepts that explain why, in certain cases, ordinary citizens may distrust science, which should inform how philosophers of science conceive of the formulation of science policy when conditions of distrust prevail. The third project is the analysis of cases of successful relations of trust between scientists and non-scientists that leads to understanding better how 'postnormal' science interactions are possible using trust, where 'postnormal' simply refers to scientific research activities that require the involvement of actors who are outside of the scientific peer community.

2 Trust in the interactions between science and society

The significance of trust in the relations between science and society has been claimed by philosophers of science. We will review some claims made about the significance of trust in the relations between science and society by philosophers of science, and suggest that more attention needs to be paid to specific projects intended to improve trust between science and society.

For our purposes, trust means deferring with comfort and confidence to others, about something beyond our knowledge or power, in ways that can potentially hurt us. Sometimes we have options as to who to trust and exercise a degree of control over our choices. This is often the case when we inquire into which doctors we would have treat us. At other times we have no control about those to whom we defer, such as when we are wheeled, helpless, into an emergency room. There is much at stake in who we defer to given that our trust puts us in a position of vulnerability. When we trust our doctors, our health, in a sense, is in their hands. When we trust our collaborators, the

viability of our project, and our reputation, is on the line based on whether they fulfill their obligations.

Trust in epistemic matters is particularly important. We cannot be fully informed about everything—even in our own fields of specialization. In some circumstances that call for action, such as knowing when food exposed to toxins is safe, whether a shipment of nuclear fuel is harmless, or what is the impact of global climate change on our tribe, it is impossible for us as individuals to work out the answers for ourselves. We have to trust what experts and other epistemic authorities say, or be unable to act.

Philosophers of science have observed that trust has important epistemic functions in scientific research activities (Hardwig 1985, 1988, 1991; Kitcher 1990, 1992, 1993). John Hardwig's well-known example of the physics paper with ninety-nine authors is an example of a scientific research activity that would not have been possible without high degrees of trust. Some scientific projects are so large that it would be impossible to achieve results without complicated networks of trust relations (Hardwig 1991; Wilholt 2009).

Another way in which trust has an epistemic function is in the relation between science and society. Naomi Scheman has argued that one of the purposes of science is to provide publically beneficial research products. That is to say, one purpose of scientific research activities is to produce reliable knowledge that can be used by more than just scientists (Scheman 2001; Wilholt 2009). Scientific research is not only important insofar as it tests the questions that interest scientists, but also insofar as the results benefit members of the public. But if science is to provide these public benefits, then "science" and scientists must be trustworthy in the eyes of ordinary citizens. This raises the philosophical question of whether ordinary citizens have good reasons to believe that scientists have good reasons for whatever the latter report as the results of their research.

An initial way of understanding this question is that ordinary citizens and other scientists are well advised to defer to scientists owing to the latter's competence and expertise in the particular area in question (Hardwig 1985; Rolin 2002). Scientific training, research experience, and sustained participation in the research community affords scientists specialized knowledge of particular areas. These factors give scientists good reasons to make judgments in their domains of expertise. It is perfectly rational to trust scientists on matters that ordinary citizens could not possibly know any better about (Hardwig 1985).

But whether scientists actually are trusted by ordinary citizens is a different issue than whether scientists should be trusted. There are good arguments for why scientists, in many contexts, should be trusted. That does not mean that scientists who should be trusted are indeed trusted by ordinary citizens who may benefit from the formers' research and expert advice. Consistent with some social epistemologists, Kristina Rolin has argued that there is a difference between credibility and trustworthiness (Craig 1990; Fricker 1998; Rolin 2002). Credibility has to do with what indicators are actually used by others as the basis for trusting scientists. Trustworthiness has to do with what indicators actually constitute why someone should be trusted, like their competence and sincerity (Craig 1990; Fricker 1998). Feminist scholars have pointed out that social factors like racism and sexism often influence credibility (Anderson 1995; Code 1991; Hill Collins 2000; Rolin 2002; Scheman 1993; Williams 1991).

Science and technology studies scholars have also shown that credibility was determined by social factors such as one's class position (Shapin 1994). In this sense, social arrangements such as power and prestige can influence who is considered to be credible (Addelson 1983). Likewise, social factors are often behind whether scientists are perceived as credible in the eyes of ordinary citizens.

It is important to note that often what goes into making scientists appear credible or uncredible is not related to scientists' values or research. Scientists who are performing research the results of which will be invaluable to ordinary citizens may not be trusted because they suffer from credibility deficits. Ordinary citizens might believe scientists to be biased based on their class positions, their employers, and the like. Scheman has pointed out that in societies in which scientists are members of privileged classes it is likely that people will not trust them. Moreover, if people have had bad experiences with science before—consider the “Tuskegee Effect”—then they will be unlikely to trust scientists again (Scheman 2001). In these cases, scientists lack credibility owing to social factors, which may render scientists unable to provide public benefits. There are many areas where members of society distrust scientists and scientific institutions. Sometimes this distrust is purely based on advancing a political agenda, making it possible for individuals to use predictions and false claims against science to advance their policy agendas. The goal of such predictions and claims is to foster unwarranted distrust among science, communities, and public institutions. Politicians can use this strategy, on the left and right, such as when they promote the uncertainty of scientific findings in global warming, or when they under-hype the dangers of low-level toxins by cultivating distrust of measurements and data. The danger of doing the latter is to create *environmental Maginot lines*, which squander resources on the wrong places, lull citizens into a false sense of security, are vulnerable to political manipulation, and contribute negligibly to the wider social purpose—public safety—that is the ostensible goal. If scientists are not considered to be credible by ordinary citizens, then the latter will be unable to reap the public benefits of scientific research activities. In this sense, we understand the issue of whether ordinary citizens trust scientists as concerning both whether scientists are trustworthy (that they should be trusted), and whether they are perceived as credible. The latter affects whether ordinary citizens actually do trust scientists, which impacts the degree to which scientific research activities are permitted to benefit non-scientists.

Scheman argues that if science is to fulfill its role in society, that is, of providing the public benefits of objective results on various matters, then it will have to be both trustworthy and credible (Scheman 2001). Scientific results should be trustworthy because they are reliable; they should also be packaged to be credible. This means that those of us, from scientists to policy makers to philosophers of science, should also explore what social grounds and institutions should be established to facilitate credibility between scientists and ordinary citizens, and not just explore why scientists should or should not be trusted by ordinary citizens in various cases.

The philosophy of science has social relevance when it comes to tackling problems relating to how trust and credibility should be built between science and society. Scheman argues that “if we cannot trust those on whom we are epistemically dependent, we will not believe when we should; and if we ought not to trust them, we risk believing when we should not” (Scheman 2001, pp. 42–43). For Scheman, we should

explore what goes into both the “psychological possibility” and “rational justifiability” of trust and credibility even when the grounds are social and fall outside the features that make scientific norms themselves trustworthy (Scheman 2001, p. 43). Hardwig (though speaking mainly of relationships between scientists) notes that trust is an integral part of epistemology and that “untrusting, suspicious attitudes” impede the growth of knowledge (Hardwig 1991). We believe that philosophers of science should follow up on these claims and endeavor to pursue projects that will contribute to enhancing the public benefits of science in the appropriate contexts. In the following sections, we will suggest three such projects that have to do with trust between science and ordinary citizens.

3 Project #1 and unrecognized contributor cases

The first project is the development of a rich normative theory of expertise and experience that can explain why the various epistemic insights of diverse actors should be trusted in certain contexts and how credibility deficits can be bridged by rethinking expertise. The scientific aspects of some technical decisions are best carried out by both credentialed scientists and lay people. Heather Douglas claims that one of the ways in which ordinary citizens can improve technical analyses is by providing “key knowledge of local conditions and practices relevant to the analyses” (Douglas 2005, p. 158). In cases where such knowledge is critical for good scientific analysis, scientists should proceed together with the involvement of ordinary citizens. However, in some contexts this is prevented because scientists do not recognize the need for citizen inputs in technical analyses and discredit them owing to their lack of credentials. In contexts like these, the ordinary citizens and direct stakeholders do not trust the scientists because they see that the scientists’ conception of expertise is so narrow as to exclude obviously important inputs from citizens. We will call case contexts like these ‘unrecognized contributor cases’, or ‘UC cases.’

UC cases occur when certain individuals with scientifically relevant knowledge, experiences, and perspectives are excluded as potential contributors to scientific activities. These contributors are often unrecognized because scientists and other actors in the controversy hold a narrow conception of scientific expertise. A problem in these cases is that because scientists do not acknowledge lay persons’ ability to contribute, the latter use that as an indicator for distrusting the scientists. The scientists’ failure to include them breeds distrust of the competence of the scientists. A classic example occurred in Great Britain in the 1980s involving Cumbrian sheep farmers and government officials following the Chernobyl nuclear reactor accident. We can be brief in our exposition, because the case has been much discussed in the literature on public controversies involving scientific expertise (Collins and Pinch 1998; Wynne 1996).

Days after the Chernobyl meltdown in the Soviet Union in April 1986, rain deposited radioactive material, including cesium, on portions of Great Britain. Sheep ingested contaminated grass, and the British Ministry of Agriculture, Fisheries, and Food (MAFF) found levels of radioactivity in samples of lamb meat from the Cumbrian fells to be 50% greater than the “action level,” or the maximum permissible amount before official intervention is required. As a result, that June the MAFF posted a ban on

the movement and slaughter of sheep in parts of Cumbria and North Wales. This ban was intended to last three weeks, for the contaminated material was expected to pass rapidly through the sheep by normal metabolic processes and the sheep were not expected to absorb new radioactive materials. However, levels of radiation in the sheep unexpectedly continued to increase, and in July the ban was extended indefinitely. These restrictions had a dramatic effect on the sheep farmers' flourishing, for the sale of spring lambs provided the farmers with their only significant yearly income.

Scientific experts sent by MAFF to assess the radioactivity levels relied on their own theoretical models for doing field research, and did not solicit the advice of the sheep farmers. These factors resulted in several scientific mistakes. One was that the scientists assumed that the amount of radioactivity would correspond with the level of rainfall, but this proved to be mistaken, for rainwater does not flow or gather uniformly. The scientists also assumed that cesium would not be absorbed by plants after the initially contaminated grass was eaten by the sheep; this assumption proved incorrect because it had been based on cesium's behavior in a different kind of soil than common in the area under scrutiny. Finally, the scientists were using models that had been developed for the threat cesium posed to humans rather than sheep.

The sheep farmers had key knowledge of these local conditions that could have been used by the scientists to perform sound assessments. But the scientists did not recognize the knowledge of the sheep farmers. The problems caused by the failure of the scientists to respect the local experience of the farmers was highlighted by one episode in which the scientists were exploring ways of getting rid of the radioactive cesium. The scientists spread different concentrations of an absorbing mineral on the ground in fenced-in plots. Sheep were allowed to graze on the plots and then tested for contamination, with the results compared with those of a control group of sheep that had grazed on the fells. The farmers criticized the experiment, pointing out that sheep usually graze over open tracts of fell land, and if fenced in they "waste," or go out of condition. These criticisms were ignored, but the farmers were ultimately vindicated when the experiments were abandoned for the reasons outlined by the farmers. The scientists also overlooked the farmers' local knowledge of the land, such as observations of where water accumulated and thus potential radiation hot-spots. The biggest source of antagonism between the two groups, however, resulted from the lack of understanding that officials displayed about fell sheep farming as a way of life, and their lack of familiarity with the subtleties of producing sheep for sale at a market. Again and again, the government experts dealt with this problem in a way which demonstrated their lack of familiarity with fell farming, and their ignorance of the impacts of the restrictions on the normally flexible and informal system of hill farm management.

Part of why the public benefits of scientific analysis could not be realized is that the scientists were unwilling to consider the knowledge of local conditions had by the sheep farmers. This is not to say that had the scientists engaged the sheep farmers that every problem would have been solved, or that the sheep farmers' testimony contained the "one" missing piece of evidence required for drawing good conclusions. Indeed, the scientists were using theoretical models that were problematic for other reasons—and such problems would likely have persisted with or without the contributions of the sheep farmers. However, the participation of the sheep farmers may have introduced

questions that might have helped the scientists adjust their models and methods, have a better grasp of how local conditions differ from the conditions under which they were trained, and understand what was at stake for the sheep farming community.

One of the reasons why none of these possible contributions were explored is because of lack of trust between the parties. UC cases like this one occur when some actors operate with too narrow a conception of scientific expertise. Actors with relevant knowledge and experience, but no formal credentials, are not recognized as potential contributors; credentialed scientists, that is, overlook relevant types of experience and knowledge. Such misrecognition promotes the intractability of the dispute and precludes the possibility that certain kinds of relevant information and experience will play a role in its closure—especially the contributions of those without credentials. The unrecognized contributors then tend to acquire distrust of the credentialed experts. These actors are unable to see the value the experts' models and methods given that the latter were unwilling to engage local experience and knowledge. Though, as previously mentioned, such lay contributions may not improve all aspects of the scientific analysis, the fact that they are outright rejected by scientists breeds distrust, regardless of whether we can precisely foresee the improvements. This distrust is not conducive to resolving scientific controversies like that between the MAFF scientists and sheep farmers.

Philosophers of science are in the position to analyze scientific controversies like this one and argue that such controversies are or have elements of UC cases. This might involve several socially relevant projects. First, there should be a normative theory of scientific expertise that can clarify the different kinds of epistemically relevant contributions that different actors can make to technical analyses, as Heather Douglas has begun to do in her work (Douglas 2005). Second, this normative theory should include possible strategies for bringing these actors together in ways that bridge the credibility deficits.

Some important questions for this normative theory are as follows: Are there different kinds of scientific and *scientifically-relevant* expertise? How are degrees of relevance between an expertise and a problem established? How should interactions between scientific experts and members of the public with relevant, local knowledge and experience be arranged? And how can different experts be inserted into controversies in ways that will eliminate the credibility barriers of trust?

These are the kinds of questions that motivated Collins and Evans in their normative theory of expertise developed in *Rethinking Expertise* (Collins and Evans 2007). There they formulate a “Periodic Table of Expertises” to describe various contributions and limits of different kinds of scientific and non-scientific knowledge. These kinds include “interactional expertise,” or knowledge of a scientific field that is sufficiently advanced to understand and communicate within the discourse yet unable to contribute to research. Such a concept is potentially quite useful for interpreting and correcting UC cases. Had an interactional expert in environmental sciences been present, either among the MAFF scientists or as an external mediator, perhaps this expert could have bridged gaps separating the experts from the sheep farmers. An interactional expert could have been able to make the case that the local experience and knowledge of the sheep farmers should be considered, and that the MAFF scientists would do good to facilitate cooperation and collaboration with them. In this sense, a concept such as

interactional expertise can be used to understand some of what it takes to bridge credibility deficits in public interactions between science and society. An implication of the Periodic Table is that it can serve as a sorting mechanism for controversies like UC cases, where normative criteria would be useful for better understanding why closure on appropriate scientific and technical grounds is elusive, and what norms governing expertise should be employed for such ideal closure.

We mention the example of these cases because a normative theory could provide justification for why an interactional expert could be helpful in establishing trust. An interactional expert could communicate with scientists and lay people and show the importance of the different contributions. It is precisely here that philosophers of science can become socially relevant. In the case of interactional expertise, arguments need to be provided concerning how trust could be built using the mediation of interactional experts, or why it is not a useful avenue. Also, there are other creative approaches to creating a normative theory of expertise beyond Collins and Evans' approach that philosophers of science could explore.

An additional insight is that philosophers of science could perhaps play the role of interactional experts in certain cases. Many philosophers of science have spent years developing expertise of a certain science but do not become full practitioners of it. If interactional experts can serve as mediators and translators, then perhaps philosophers of science should be compelled to enter UC cases and establish conditions of trust between scientists and ordinary citizens.

4 Project #2 and poisoned-well cases

The second project is the development of concepts that explain why, in certain cases, ordinary citizens may distrust science based on social factors. The purpose of understanding this is to inform how philosophers of science conceive of the formulation of science policy when conditions of distrust prevail. Such a project would be very important for what we will refer to as 'poisoned-well cases' or 'PW cases,' where trust and distrust in experts is an explicit and irreducible element in multilateral negotiations over scientific and technical issues. In PW cases, there is no hope for a technical argument to succeed. An example is an episode that took place at Brookhaven National Laboratory (BNL) in 1976, involving a controversy over the shipment of spent nuclear fuel, which began with a shipment of spent nuclear fuel from the High Flux Beam Reactor (HFBR) at BNL in 1976. The case information has been part of the research of one of the authors (Crease 1999).

The HFBR, commissioned in 1965, normally had about two dozen fuel elements replaced in shifts about every four weeks. The spent fuel was "cooled" in a storage pool for about nine months, then put in casks, loaded on trucks, and shipped to a reprocessing plant. The HFBR, commissioned in 1965, normally had about two dozen fuel elements replaced in shifts about every four weeks. The spent fuel was "cooled" in a storage pool for about nine months, then put in casks, loaded on trucks, and shipped to a reprocessing plant. By 1975, 330 shipments had been made from the HFBR over two decades without incident, and 10,000 shipments from other reactors had been made in the United States without incident.

But by the mid-1970s, several factors had combined to help reshape the public's attitude toward science and technology. One was the passage of the National Environmental Policy Act (NEPA) of 1969, which mandated a role for public participation in risk-related processes. Another was increasing appreciation of social dimensions and involvements of science in ways that often tarnished its image, involving the Vietnam War, pollution, and the actions of certain institutions with regulatory responsibilities, including the government agency charged with supervising BNL, the Atomic Energy Commission (AEC). Yet another change was a shift from the "human exceptionalist paradigm" to the "ecological social paradigm," in which modern industrial-technological practices are seen as potentially destabilizing nature (Catton and Dunlap 1980). Fiction and nonfiction accounts, meanwhile, popularized the idea that when humans introduce small changes in the environment these may connect with other processes to have major and even disastrous consequences. Such factors helped spark several major public controversies in 1976, including the banning of recombinant DNA research in Cambridge, MA, and protests against the nuclear freighter *Mutsu* by Japanese fisherman. In addition to the above three factors important for understanding the BNL spent fuel shipment controversy, a fourth was local. In 1965, the year the HFBR began operation, the Long Island Lighting Company (LILCO) announced plans to build a nuclear power plant in the town of Shoreham. The NEPA-mandated hearings in the early 1970s attracted national protests, and while the HFBR's construction permit was approved, and its reactor pressure vessel set in December 1975, an increasingly organized and vocal opposition mobilized.

These four developments form important background for understanding what happened in March 1976, when a flatbed truck bearing a fuel shipment reached a dock in Orient Point—the previous overland route, through New York City, had been ruled out—to encounter a crowd of protesters. The protesters, who urged cessation of the shipments and closure of the reactor, carried placards bearing portraits of mushroom clouds and skull and crossbones, handed out anti-nuclear energy leaflets, and other material that compared nuclear waste to cigarettes and bombs. The result was a public controversy involving technical dimensions that was inextricably infused with social dimensions of distrust. Much was at stake, for the activists, scientists, and public at large: if spent nuclear waste cannot be shipped, then there can be no reactors, and without reactors, nuclear research virtually stops and nuclear medicine is severely harmed.

To lab scientists, administrators, and local health officials, the statements and claims of the protesters were irrelevant to the shipments of HFBR's spent fuel. The protesters' signage and material invoked catastrophic accidents like weapons explosions—nothing like what had happened in the past, or what could conceivably happen to the spent fuel rods in their thick, well-tested casks in trucks escorted by motorcades moving along carefully chosen routes. And equating the HFBR, a research reactor, with nuclear power plants orders of magnitude more powerful and under a completely different kind of management and constraints, was simply wrong, a category mistake. For the protesters, on the other hand, the signs and flyers did address relevant issues, and the shipments were intimately related to nuclear power. To them, it was all part and parcel of the same technical and moral problem, the growing nuclearization of modern life. To them, both the technical calculation and the judgment

of the risk were untrustworthy. Were this a different sort of controversy, the warning flag posed by the controversy ought to have called for a restructuring of the technical decision-making to make it transparent to the public what the activists were claiming to represent. But the activists' distrust of the technical aspects of spent fuel shipments did not lead to this kind of correcting or democratically constraining activity.

The difference of perspectives between lab scientists and protestors was highlighted when a state official boarded the truck and measured the cask's radioactivity: 1.5 mrem/h at the surface, with the inverse-square law reducing that to undetectability within a short distance. No one present appears to have challenged this measurement. The closed doors that might have shrouded the original risk estimation thus seem to have been lifted. But the controversy remained. To the lab scientists, this amount was safe, comparable to half a normal chest X-ray and a hundredth of an average tooth X-ray of the time, and the amount was about a hundred and thirtieth of existing federal regulations. The protestors saw it differently. A representative of Friends of the Earth claimed no level of radioactivity was safe and called for a Congressional investigation. A headline in a local newspaper asked, "Are we expendable?" And a local district attorney asserted that he would press criminal charges, calling the shipping of the cask whose surface emanations were a hundredth of a tooth X-ray an act of "genocide." Critical framing issues were thus in place that admitted of no tacit standards of agreement on technical issues. To the scientists, 1.5 mrem/h supported the shipments' safety; to the activists, it confirmed the material presence of radioactivity, a known health hazard.

Hardened positions like these generally lead to legal/political solutions that are undesirable from a technical standpoint. The high level of distrust makes it so that scientific results cannot factor in to resolving the controversy. And after this and five more shipments using the same route, Associated Universities, Inc. (AUI), Brookhaven's then-contractor, pursued a legal solution by seeking to have the prohibition of shipping through New York City declared inconsistent with the federal Hazardous Material Transportation Act (HMTA), to avoid having to make more shipments by ferry at Orient Point. The key players continued to regard those on the other side as irrational, as not worth having a dialogue with, or even worth acknowledging. The resulting legal snarls held up BNL's spent fuel shipments until the middle of the next decade.

In this case, the scientific and technical details should matter in the resolution of the controversy; communities ought to know what shipments pass through them, what the risk is, and how the assessment was made. But high levels of distrust precluded scientific and technical issues from weighing in, which should have happened in order to advance the interests of all actors. Each side, instead, saw the other as irrational. To the scientists, many of the claims forwarded by the activists were scientifically illiterate, based on a confused jumble of irrelevant and inaccurate information; the most charitable way of viewing the activists, in this view, is that they were making a disguised and dishonest attack on nuclear power by attacking the shipments of spent fuel—a reactor's Achilles Heel—of a research instrument. To the activists, the scientists' claims that the cask was safe were arrogant, patronizing, dangerous, and politically motivated. In some cases public opinions about safety are more appropriate than those of scientists (Shrader-Frechette 1991). The difference cannot be fruitfully understood

as the product of scientific illiteracy. Instead, statements about the risk of the shipments acquired meaning for the scientists and the activists through an interpretive process in which actions, information, and motives were endowed with prejudices, thus ‘poisoning the well’ for possible technical dialogue. The activists’ claims involved not the risk estimations, but the social value of reactors. There was thus no spoken or implicit rule agreed to by the opponents about the role of science and its potential benefits for closing the controversy. The opposition was total, admitting of no shared standards.

PW cases are scientific and technical disputes that are entirely overshadowed by social matters rooted in distrust. The distrust is largely based on social grounds. There is hardly any room for improved scientific or technical reasoning to weigh in on the different decisions made by the actors. Positions are hardened to the point where even a change in perspective will not mean much if it comes with no change in position on the big issues. For example, any trust of scientific experts, from the standpoint of activists, would mean the same thing as trust in those who are also considered to be responsible for technological harms and putting societies at great risk.

One role for philosophers of science is to better understand why there is so much distrust of scientists, which blocks the possibility that science can provide public benefits. An example of a concept that could be useful, which comes from social sciences research, is Robert Benford’s concept of ‘movement narrative.’ A movement narrative is a story told about a cause that introduces and links together what look to others as unrelated objects and issues. Movement narratives “function as internal social control mechanisms, channeling and constraining individual as well as collective sentiments, emotions, and actions” (Benford 2002). The formation and maintenance of movement narratives helps explain the coherence of many judgments that, to the other side, appeared mistaken or irrational. Movement narratives provide a way to understand why people discredit scientists owing to social factors. The narrative by which the scientists understood their work and its opposition by activists helped close off the possibility that scientists could at least identify with or understand the activists. Meanwhile, the activists’ narrative allowed them, *prima facie*, to dismiss the scientists’ reassurances as dangerous. This does much to explain why conventional expert advice has so little traction in many PW cases, for the movement narrative works on several levels. One involves a narrative calling for distrust in the risk assessment deemed safe by the authorities—in the risk assessment of oil spills, small amounts of toxins and other forms of radiation, and so forth—for this assessment is alleged to be deliberately underestimated. Another is a call to oppose this reactor by whatever means necessary, inasmuch as it is part of the nuclearization of American society. Still another element is neutralization of the other side’s experts—even experts whose values are in line with that of the activists. For any expert advice favoring one side, a story will always be available to be told from the other side to neutralize it, which is a process of destroying credibility that we call ‘expert tainting.’ Here, for instance, any expert advice that seems to be in the service of allowing shipments can be tainted as linked with the military-industrial complex, while other experts will be dismissed as charlatans or misguided. This creates conditions of distrust where expert opinions are judged based on the conclusions, not on the processes used to establish these conclusions.

A movement narrative poses problems for would-be mediators. In such cases, mediators cannot be credentialed scientists employed by or directly or indirectly associated

with some interest, and thus vulnerable to distrust and accusations of taint. There are no “freelance” or “at large” credentialed experts. But to be listened to and effective, a credentialed expert must be (1) free of taint, yet (2) manage to support a counter position while appearing to remain untainted, but also (3) manage to connect the technical information with the values that underlie the narrative in a way contrary to the narrative’s direction, requiring expertise in rhetoric, pedagogy, etc. This implies another kind of expertise—that of an expert in public relations. But the exercise of expertise raises unacceptable moral problems of paternalism and autonomy that haunt everything from the physician–patient relationship to scientist–public issues. We wind up with the same obstacles to conflict resolution, only reinscribed.

Concepts that can explain why science is distrusted based on social factors can be used as ingredients for formulating criteria for how ordinary citizens should interact with scientists in certain kinds of public controversies like PW cases. There are many other possible relevant concepts that could be used for this purpose—the concept of movement narrative is just an example. In terms of developing criteria, some philosophers of science have begun to develop more specific policies for how ordinary citizens should interact with scientists in situations like PW cases. Kristin Shrader-Frechette, for example, has articulated principles of justice that have scientific and technical traction in PW cases with high levels of distrust. Her conception of participative justice and theory of scientific proceduralism articulate reforms that will change how scientists and members of the public work together even when high levels of distrust and private interest are prevalent (Shrader-Frechette 1991, 2002). For example, in *Environmental Justice*, she writes that the only way in which decision making can be said to offset private interests and demystify expert or other political claims is if the opinions of stakeholders and experts are given equal weight (Shrader-Frechette 2002, p. 28). In PW cases, any unjustified privileging of perspectives or opinions of any side will not help to improve conditions of trust. Policies should be sensitive to that and understand that citizens, whose participation is required for close of the controversy, cannot have their concerns shelved simply because scientists experts disagree with them. For Shrader-Frechette, only reforms that guarantee informed consent and the equal weighting of opinions promise to be able to fairly regulate situations like this. Shrader-Frechette’s ideas can be understood as constraints that shield all actors from the effects of political and financial interests, which are often the basis of credibility deficits. Shrader-Frechette’s approach is a well-articulated beginning. Further philosophical work is required to apply concepts like movement narratives and Shrader-Frechette’s reforms on a case-by-case basis, the overall goal being to reframe how scientists and citizens should interact in controversies with high levels of distrust. The philosophy of science is socially relevant as a forum for discussing why ordinary citizens may distrust science, and how understanding that should inform how we conceive of the formulation of science policy when conditions of distrust prevail.

5 Project #3 and trusted mediator cases

The third project is the analysis of cases of successful relations of trust between scientists and non-scientists that leads to understanding better how postnormal science

interactions are possible using trust. Postnormal science refers to a scientific methodology for certain difficult problems where scientific research activities must extend beyond the peer community and include lay people and other stakeholders (Bidwell 2009; Funtowicz and Ravetz 1990, 1992, 1993; Norton 2005). Are there touchstone cases where ordinary citizens with varying degrees of epistemic insights are involved in trusting relations with scientists? Let us call these Trusted Mediator (TM) cases. These cases would involve the active development of social relationships that promote trust based on both social and scientific grounds. Distrust is managed based on an arrangement of actors that ensures a collaborative flow of relationships focused on achieving better scientific and technical knowledge on a problem. We take our cue on this from an example between an indigenous group and scientific experts in Canada that was reported in *Indian Country Today*.

The Nunavik Research Center (NRC) in Northern Quebec evaluates the impact of environmental change on the Inuits in Nunavik and suggests changes that might be needed to adapt to threats (Woodard 2005). Nunavik is a region where global climate change is undeniable. Their winters begin later in December instead of October; ice melts earlier in the Spring, which brings polar bears inland in May instead of June or July. Moreover, pollution resulting from local mining and worldwide industrial growth is on the rise. The Inuits cannot adapt to these changes alone; extensive scientific and technical advice in addition to the mobilization of local knowledge is much needed. They require reliable information on what food is edible, how much game to hunt, and what other threats are arising. But how is this possible given the history of oppression against indigenous peoples in Canada and the current political structure which makes indigenous participation difficult? The Inuits, indeed, have myriad reasons to distrust Western institutions, both horizontally and, of course, vertically, considering the history of Canadian colonial oppression of indigenous people. The Inuits' perception of scientific experts was strained by the fact that, normally, the environmental scientists used to arrive, collect data, and leave, without sharing what they had found.

In response, the Inuits designed the NRC to change that, using their financial resources to hire laboratory scientists. The Center set up conditions of interaction between laboratory scientists and the population that seek to filter the effect of distrust. For example, lay suspicions about food contamination or game scarcity are conveyed to the Center. The Center's health board conveys the lay concerns to the laboratory scientists. The latter group, in turn, devises studies. The design, execution, and results of studies are constantly checked against indigenous standards of knowledge as well as the discrimination of elders. This ensures that technical decision-making does not fall prey to obvious problems of local compatibility or indigenous participation. Most of the scientists depend on the development of an understanding of indigenous knowledge and lifeways to communicate to lay people how to gather data for the different studies. This involves attaining a kind of expertise that arguably could be interactional expertise. Once the results of the studies are in, they are sent back to the health board, which makes the technical results useful for village members through linguistic and symbolic translation—impedance matching. This translation works precisely because it is well known that the entire process blends Western science, indigenous sovereignty, local concerns, and indigenous knowledge together into a system of checks and balances.

The process just described does not aim naively to remove distrust—this would be impossible—but rather develops critical checks and balances that ensure the importance of technical matters. Distrust is effectively managed. Having these data is critical for the Inuits in order to be able to negotiate with the Canadian government over environmental matters such as hunting quotas. Cases like this are important places to consider why these relationships are successful and whether there are lessons to be transferred to other similar cases. Philosophers of science ought to take up the analysis of such cases and explain how the interactions facilitate trust without ignoring the social factors that may be responsible for discrediting scientists and ordinary citizens. It is important to understand how social and technical factors should be balanced in order that the public benefits of science are better realized, especially in situations where the results of scientific research activities are greatly needed.

6 Conclusion

In this essay we have shown that the philosophy of science can be socially relevant regarding the problem of trust in the relations between science and society. The public benefits of scientific research activities are often compromised when ordinary citizens distrust scientists. Philosophers of science should examine the various ways in which social factors lead to distrust and credibility deficits. Three particular projects are relevant. For unrecognized collaborator cases, philosophers of science should develop a rich theory of expertise and experience that can serve as part of the basis for bridging the credibility deficits caused by narrow conceptions of expertise. For poisoned well cases, philosophers of science should develop concepts of science policy that are sensitive to irreducible elements of distrust. Finally, philosophers of science should analyze successful cases of managing distrust thanks to the implementation of jointly social and technical solutions.

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