What STS and Philosophy of Science Can Offer Each Other

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Joyce C. Havstad
University of Utah
joyce.havstad@utah.edu

Matthew J. Brown
Southern Illinois University
matt.brown@siu.edu

1. Introduction

Philosophy of science has long had a vexed relationship with sociology of scientific knowledge (SSK) and science and technology studies (STS). But in our view, there are key insights in STS that philosophers of science have not fully appreciated, as well as key resources from philosophy of science that could be combined with those of STS to do valuable work for both philosophers and STS scholars. Here we appreciate and attempt to integrate these two traditions.

We begin with a pair of ideas from Bruno Latour in *Science in Action*: (1) under conditions common in scientific practice "there is no other way open to the dissenters than to building another laboratory" (79) and (2) "all laboratories are counter-laboratories" (79). Latour's insights entail the following: material resources and institutional factors have a significant influence on the development of inquiry, argument, justification, and credibility in science. The distribution of resources in science is far from equitable or even meritocratic, and this affects how science proceeds as well as what it produces. In particular, the hyper-specialized, technically-intensive research at the cutting edge of many sciences, with the expensive equipment and complex distribution of labor it requires, gives scientists with superior resources significant competitive advantages at various stages of the scientific process.

This leads to *problems* for the practice of science: problems of replicability, limitations on cognitive diversity, financial susceptibilities, and lack of genuine, effective critical debate. Concentration of resources thus makes science more vulnerable to unintentional bias, error, and dead ends, as well as intentional fabrication, fraud, manipulation, and misuse. Although STS has done crucial work debunking myths about science held by scientists, philosophers, and the general public, as well as provided more descriptively adequate accounts of scientific practices, these achievements by themselves do not make it easy to identify the problems described above *as problems*. The desire to identify, diagnose, and address such problems is present and is reflected in the semi-recent self-reflective moment

in STS itself, as seen in the wake of Latour's "Why Has Critique Run out of Steam?" (2004). This concern carries into the elective modernism of Collins and Evans (2017). What philosophy of science has to offer is a *normative* perspective on scientific practices that can help us recognize and diagnose such problems *as* problems. Philosophy of science seeks to understand whether and why science is successful, what conditions support that success, and what goes wrong when science fails.

Two normative stances from philosophy of science which are highly pertinent here are (a) the social dimensions of scientific objectivity (Longino 1990, 2002) and (b) feminist standpoint theory (Hartsock 1983; Crenshaw 1989; Collins 1991), especially that developed for philosophy of science in particular (Harding 1986; Wylie 2003; Intemann 2010; Crasnow 2014). These prescriptive contributions by philosophers and social epistemologists entail conditional recommendations for scientific practice, such as: if scientific practitioners desire to aim for the achievement of objectivity in their domain, then they will need to establish avenues for critique, shared standards, uptake of criticism, and egalitarian intellectual authority. Or: if scientific practitioners hope for robust exploration of the possibility space in their domain to occur, and for the generation of resilient claims from within it, then they will need to establish and support a diversity of epistemic agents who are engaged in the exploration of that space and the generation of candidate claims from within it, abolishing material barriers to diverse participation.

In this chapter, we adopt the normative stance more characteristic of philosophy of science than of scholarship in STS, in order to update and recommend further engagement of what we see as key lessons from the anthropological strain of STS. Our aim is to engage both philosophy of science and STS in our discussion of ideas about how science works, grounded in the material practices of science (drawing on STS), while exploring what these ideas imply about how science ought to work (drawing on normative philosophy of science). We have observed that practitioners and meta-commentators alike are in today's climate seeking to establish and support a science that is expert (Collins and Evans 2017; Oreskes 2019), that is objective (Longino 1990, 2002), that is resilient (Harding 1986; Wylie 2003), and that is salient to contemporary problems (Intemann 2010; Wylie 2012).

We argue that a long-forgotten lesson of Karin Knorr-Cetina's *The Manufacture of Knowledge* (1981) needs to be remembered: a lesson that reminds us to move away from a primarily discursive approach to understanding scientific practice (common in both STS and philosophy of science) which stresses consensus and dissemination, towards a more embodied, economic, and material approach to scientific practice, which stresses resource incorporation and deployment instead. The language of "incorporation" carries two distinct connotations which are essential to our proposal: (i) it reminds us of material incorporation, of the enfolding and uptake of objects into collectives and the metabolism of resources into bodies; and (ii) it reminds us of economic incorporation, of the transformation of an organization into a company and of the possibility of investors, the need for capital, and the entry of a new player into a market.

First, we explore past contributions in STS and philosophy of science, showing appreciation for what is offered as well as identifying trajectories that we think have been either

neglected or overplayed. By the end of the paper we hope to have charted a path forward that directs us to move away somewhat from too narrow a focus on discourse and discussion, towards the material conditions and political economy of scientific practice.

2. The Anthropological Strain in STS

As various chapters of this volume demonstrate, *science and technology studies* and the *sociology of science and technology* encompass a diverse variety of methodological and theoretical approaches to the social study of science. (We use the acronym "STS" to encompass the entire domain.) Many of these approaches use the concepts and tools of general sociology to study science specifically; their radical potential stems from the idea that the activities of science might be explained by social forces—norms, interests, class conflict, politics—rather than abstract logical and epistemological frameworks. Many of these approaches focus on broadly discursive elements of science, especially controversies and consensus. A major element of the social study of science has been the attempt to explain how the scientific community comes to agree (on facts, theories, or paradigms) in light of the phenomena of underdetermination and contingency in science (Latour 1987, 260n4).

One tradition in STS we find fruitful for challenging the preconceptions of philosophy of science is what we will call *the anthropological strain* in STS, as it has played out in the area of *laboratory studies* originating in the work of Bruno Latour and Steve Woolgar (1979) and Karin Knorr-Cetina (1981). This tradition stands out in two important respects: (1) its focus on the *non-discursive* material and cultural aspects of scientific practice, and (2) its eschewing of high-concept "social forces" in favor of detailed attention to the social interactions.

In *Science in Action* (1987), Bruno Latour explores the stance of a determined dissenter to gradually shift attention from the *content* of scientific publications, to their role in social discourse, to the material-cultural contexts of the laboratories that produce them. Latour treats the scientific article not as merely the result of the scientific process of investigation but as a token *in* the scientific process that serves a persuasive function. In analyzing the rhetorical structure of the scientific paper, Latour uncovers much about the social and material bases of scientific practice. To dispute a scientific claim, it is not enough to master the technical literature in which it appears,[1] nor to use one's logical and rhetorical skills to produce a counter-argument. In this way, scientific argument differs from philosophical argument, for example, where such knowledge and skills suffice. As Latour puts it:

> Dissenters have now done all they can do to disbelieve, disaggregate and disassociate what is mustered behind the claim. …At this point they have to take another step—either give up, or find other resources to overcome the author's claim… we will concentrate on the rarest outcome, when, all else being equal,

[1] A point that has figured prominently in recent work by Collins and Evans (2017).

> there is no other way open to the dissenters than to building another laboratory. The price of dissent increases dramatically and the number of people able to continue decreases accordingly. This price is entirely determined by the authors whose claims one wishes to dispute. The dissenters cannot do less than the authors. …This is why all laboratories are counter-laboratories just as all technical articles are counter-articles. So the dissenters do not simply have to get a laboratory; they have to get a better laboratory. This makes the price still higher… (79)

What Latour demonstrates here is that science is a material practice, a practice that comes with a material cost. A particular claim in a particular scientific paper exists in a net of interactions involving laboratory workers, instruments, materials, resources, energy architecture, among others; disputing the claim potentially brings in all of those factors. Dissent and controversy in science literally do not come cheap.

The more technical a scientific claim is; the more its establishment and contention require instrumentation; the higher the cost to build, maintain, and access that instrumentation. All these literal costs have to be accounted for in the decision whether to engage and contest a claim. This means that one way to inhibit a scientific challenge to one's ideas is to make the cost of objecting prohibitively high; it also means that the lower the cost of producing a particular claim, the easier it will be for objectors to purchase a chance to challenge it. The fact that the availability of sufficient capital (in the literal economic sense) is required and must be incorporated into decisions about whether and how to challenge knowledge claims in science, is a dimension that has not been sufficiently appreciated by philosophers of science.

Karin Knorr-Cetina's masterful and underappreciated[2] *The Manufacture of Knowledge* (1981) goes further in exploring the material culture of laboratory science. She contextualizes scientific publication and discourse within laboratory practice as follows:

> [T]he link between the laboratory and the scientific paper cannot be established by rules of cognitive transformation. The scientists who write a manuscript do not recall the research process and then proceed to summarise their recollections. …The gap which exists between the dynamics of the research process and the literary dramatics of the paper is bridged by a *double mode of production* rather than by cognitive transformation. The scientific paper is the *product* of this double mode of production, not its reflection, or summary description. The instrumental mode of production which results in laboratory measurements involves an almost total decontextualisation, relieved only by the rationales found in the scientists' written notes. The literary mode of production which results in the published

[2] We were shocked to discover while working on this chapter that this important book is effectively out of print, only available as a pricey ebook from Elsevier. The book is badly in need of a reprint edition.

> paper offers a recontextualisation, but as we have seen, not one which brings back the memory of laboratory work. (130)

Here again is a crucial lesson: the scientific *literature* does not by itself constitute scientific practice, nor does it merely reflect a tidied up version of that practice. Rather, it exists as one piece in a more complex process of production that includes the material-instrumental, non-discursive aspects of practice, with which the discursive-literary mode of practice is in constant interaction.

In the next section, we explore the parts of philosophy of science that have done the most to incorporate the lessons of STS, as well as feminist scholarship, and what they have to offer in return.

3. Philosophy of Science and the Rational-Social Dichotomy

Much helpful and important work—often STS inspired, or declaredly feminist—has been done by philosophers of science attempting to problematize that oh-so-beloved distinction between the rational and the social. While there are social-epistemic elements already in the work of Karl Popper (1934, 1945) and Thomas Kuhn (1962), it is the contemporary philosopher of science, Helen Longino (1990, 2002), whose work characterizes scientific objectivity as a thoroughly *social* product and widely changes how objectivity—that paragon of *rationality*—is understood by philosophers today.

As early as 1990, Longino argued that:

> Scientific communities will be objective to the degree that they satisfy four criteria necessary for achieving the transformative dimension of critical discourse: (1) there must be recognized avenues for the criticism of evidence, of methods, and of assumptions and reasoning; (2) there must exist shared standards that critics can evoke; (3) the community as a whole must be responsive to such criticism; (4) intellectual authority must be shared equally among qualified practitioners. (76)

For Longino, the rational and the social must work together in the achievement of scientific objectivity. She mentions a community (paradigmatically a social object) working toward objectivity (paradigmatically a rational product); and she speaks of critical discourse (a hyper rationalist activity) being achieved via the sharing of standards and authority (where sharing is a fundamentally social exercise).

In her 2002 book, Longino takes explicit aim at the way philosophers of science have tended to view social aspects of science as undermining, or in tension with, its rational aims. In taking a fundamentally rational part of science (objectivity) and making it social (not just descriptively, but properly and prescriptively so), she offers a challenge to the presumption that sociality in science is a flaw or problem. On Longino's view, sociality is a necessary good for science—at least during pursuit of objectivity. There is no objectivity in science without sociality.

So, Longino did not hesitate to offer a normative characterization of scientific objectivity, even back in the 1990s, and despite the trajectory of STS in that moment (Barnes, Bloor, & Henry 1996). She took a stand on what good, well-functioning science looks like. Her characterization is one which supposes that science can function well or poorly; that it has standards of its own making, which it endorses, and that it can either succeed or fail to live up to those standards. Longino's account of scientific objectivity furnishes a yardstick which can be held up to various episodes—from within *or* outside science—in order to ask, "Does this community measure up, in this instance, to the demands of objectivity?" When science measures up, that is a good thing, a mark in its favor. Objectivity is no small achievement.

There is a sense in which Longino comes off looking pretty good, in this respect, given what we might call the "oh wait, what did we do?" turn in semi-recent STS (e.g., Latour 2004, Collins 2014, Oreskes 2019). Only if one admits science can sometimes earn our trust does it make sense to ask "how does science earn our trust?" when it does so. However, although Longino elides the perennial, stubborn distinction between the rational and the social, there is another distinction whose boundaries she does not so clearly traverse. We might identify this boundary as the intellectual versus the material or, in the context of persuasion, the discursive versus the embodied.

Consider how Longino characterizes the four criteria that determine whether scientific communities achieve objectivity. When she glosses the first—recognized avenues for criticism—she focuses on presentation of criticism in public forums such as "journals, conferences, and so forth." With the second—shared standards—she emphasizes public-ness (again) as well as shared concerns and obtaining "a hearing." She also mentions minority voices and "appeals." It is very much about conversation, out in the open; it is about speaking and listening in the agora, as it were. When Longino glosses her third criteria—community response—she focuses on belief change, critical discussion, and responsiveness measured by (once more) *public* phenomena such as textbooks, funding, and awards. Paying attention to critical *discussion*, and logical sensitivity to that discussion, are also mentioned. Finally, her gloss of the fourth criteria—equality of intellectual authority—is explicitly Habermasian, and Longino speaks again of voices and critical scrutiny.

It is all very *discursive*, with a strong focus on ideas, talk, voices, discussion, debate, and scrutiny. It is also very *public*, and wide open: gone are the small research groups and often-private conversations between lab mates, usually numbering in the tens at most. Also gone is the physical laboratory of STS ethnography, which you often cannot enter without a literal badge, or some other form of physical access card or key. Above all, Longino's picture of what generates objectivity in a scientific community is *academic*. This is interesting, as it explains the objectivity of science in a manner that would similarly apply to any scholastic research community with these features, not just scientific research. Objectivity becomes a facet of open, discursive, receptive, intellectual communities.

Is this how science gets its objectivity? Perhaps the objectivity of science really is down to community debate in a sort of ideal or imagined seminar room. Maybe the way objective

science works is via critical conversation. But the lessons of the anthropological strain of STS suggest that this picture is missing something. For instance, what if anyone can read a published paper in a journal, but no competitors can obtain a physical sample of (say) the restriction enzyme needed in order to challenge a claim made within it? What happens when the instrument needed to test, replicate, or challenge a particular scientific claim costs hundreds of thousands of dollars? What if it is subject to intellectual property constraints, and the company making the instrument decides to restrict access to only those collaborators who agree to advance their interests? What happens when a scientific posit relies on access to one specific instrument (such as a massive astronomical telescope) and access is controlled by an agent with their own "investment portfolio," when it comes to the various claims being made and challenged in that intellection space? There are material and economic realities, as well as non-ideal sociality, to consider.

In the next section of the paper (§4), we take a look at two classic cases of consensus-building and challenge-marshaling in science. We notice that there are importantly non-discursive elements to each, which are essential to understanding how these episodes actually unfolded and were resolved (or not). We also emphasize the ways in which these episodes are departures from, rather than exemplars of, business as usual in science, and not in ways that serve as models of good practice to be more widely adopted. Eventually (in §5), we argue that a better way to understand what is happening in science is via the concepts of embodied, material incorporation and resource deployment instead.

4. Transformative Critical Discourse is Not Normal Science

We have by now identified a focus on the discursive, to the exclusion of the material and embodied, as a common and problematic issue in the social epistemology of science. Although it is standard to gesture towards the material practices and tacit knowledge in science, many philosophers of science are nonetheless guilty of what we might call "seminar-room idealism." This is the implicit assumption that the ideal epistemic situation is like a lively department seminar, based in discursive practices of reading, discussing, arguing, interrogating, etc. Seminar-room idealism ignores the fundamental material-cultural practices of science so ably uncovered by STS scholars like Latour and Knorr-Cetina.

Seminar-room idealism is "idealism" in at least two senses. First, it is *idealistic* about the critical discursive practices of science. Idealized accounts of transformative discourse provide an overly-rosy picture of processes like peer review and credentialing, giving the false impression that every idea in science is subjected to rigorous organized skepticism, potentially leading us to overestimate the credibility of scientific results in general or at least to misunderstand the source of their credibility. Second, it is *idealism* in a metaphysical sense, in that it treats the core objects of science as ideational (or linguistic) rather than material in nature. Science to the seminar-room idealist consists of claims, theories, arguments, criticisms, etc. Material objects, instruments, substrates, etc. fall out of focus.

Social epistemologists of science differ as to the aims of transformative criticism. One possible outcome of transformative critical discourse is building a scientific consensus. Indeed, Oreskes (2019) hangs her argument on why we should trust science on the idea that science already approximates Longino's ideal of transformative criticism and reaches a well-formed consensus on topics like climate change. A surprising amount of work on a variety of problems of science and society depend on the idea of a scientific consensus (e.g., Miller 2013; Kahan 2015; Chinn et al. 2018; Stegenga & Menon 2023). Another possible outcome of transformative criticism is a stable pluralism of competing approaches. This is highlighted by Longino (2002, 2013), and in this sense she is building on prior work by John Stuart Mill (1859) and Paul Feyerabend (1975). While Longino gives no *a priori* argument that pluralism is more likely than consensus in general, she gives reasonable motivation that we should expect pluralism to do better than monism in a variety of fields, and thus we should remain open to it. Either way, for Longino, it is the social process of transformative criticism, irrespective of any degree of consensus, that generates the objectivity of science. In this sense, it may be a variety of competing and incommensurable theories or approaches, and not any specific scientific results, that counts as "objective scientific knowledge."

Longino's account emphasizes the discursive in a way that does not *necessarily* rule out the additional importance of the economic and the material. It is a question of emphasis—of representation and focus—instead. We have no desire to quibble over framing, or to get embroiled in an uncharitable and unending relative significance debate (à la Beatty 1997). A more urgent concern is that, by and large, transformative critical discourse is not a central feature of actual scientific practice. It is not a feature of normal science (in the Kuhnian sense). Research in laboratory studies has shown that the idea of rational consensus formed through social discourse is problematic. And the examples of transformative discourse in the literature are typically paper thin—Oreskes (2019), for example, cites peer review and tenure evaluation as examples of how the scientific community approximates Longino's ideal. This ignores the vast literature on the inadequacies of these systems of review. Peer review is a relatively low bar to clear, and is far from an ideal deliberative process, and tenure and promotion review is no better. Heesen and Bright (2021) argue that pre-publication peer review is such a bad system that it should be abolished (cf. Atkinson 1994; Lee 2012; Jukola 2017).

The actual cases approximating consensus-building transformative discourse that can be found show themselves on further inspection to be the exceptions that prove the rule, as they are clearly abnormal procedures. In this section, we will look at two different cases of what looks like transformative discourse working to achieve the type of consensus prized in this genre. We look at the International Astronomical Union discussion and vote on demoting the planethood of Pluto and the Asilomar conference on recombinant DNA. In each case, we show that the discursive and consensus-forming practices at work are not those of idealized or ideal science-as-usual. At a minimum, this means that those processes should not inherit whatever credibility we attribute to ordinary scientific processes as we think they ought to occur.

4.1. The Consensus on Deplanetizing Pluto

Infamously, in 2006, we lost our beloved ninth planet, Pluto. This was not due to some rogue comet, asteroid, or extrasolar object, a collision with Neptune, or any such astronomical disaster. Rather, the blame lay squarely on scientists, on the community of astronomers. In August of that year, the International Astronomical Union voted on a definition of "planet" according to which Pluto no longer counted as one, thereby demoting Pluto and reducing the count of planets in our solar system to eight.[3] Here, it would seem, we have a key example of a scientific community coming together and forming a consensus on a fundamental question through discursive processes. But do these processes live up to Longino's ideal of transformative criticism? Do they exhibit science at its best, or even science as it is ordinarily practiced?

The incitement towards providing a formal definition of "planet" and potential reclassification was brought to a head by two incidents at the turn of the century. First, Neil deGrasse Tyson, director of the Hayden Planetarium at the American Museum of Natural History, decided to exclude Pluto from their renovated display of the planets that reopened in 2000. Second, the discovery of Eris (then UB_{313}), which then needed to be named—but whether it was a planet determined who had naming rights. These are less conceptual issues in science proper, or even empirical issues (though the discovery of Eris put some empirical pressure on the category); they are questions about how science is communicated to the public and the institutional politics and prestige attached to naming and curating objects in the solar system. On narrowly scientific grounds, it is not obvious that a more rigid definition of "planet" is a good thing. Flexibility and ambiguity also have their benefits in scientific language, especially concerning terms that act as "boundary objects" (Star & Griesemer 1989) between different groups of scientists and nonscientists (Messeri 2010, 189). The goal here seems as much the presentation of science having more rigor and precision than it in fact has and the promotion of certain scientists and institutions. The forging of discursive consensus around a strict definition thus seems far from a necessary move, epistemically speaking. In this same vein, it is noteworthy that a leading opponent of the reclassification is Alan Stern, the head of NASA's New Horizons mission to Pluto, who stands to lose much if the public significance of Pluto wanes.

Importantly, the vote at the meeting only constituted a minority of the members of the IAU, itself only a portion of working astronomers. There are a number of ways in which this fails to meet the conditions of transformative criticism that would be necessary for saying that this was a well-ordered or objective consensus-forming process. A first pass at a definition was made by two different committees in 2005–2006, to be presented at the IAU meeting in August 2006. Their recommended definition was hastily reworked and almost completely revised in the course of the ten day meeting, voted on in the final session of the conference by those members still in attendance at the end of the meeting. It was a rushed process, and unrepresentative in various ways, driven by those scientists with time and

[3] We take the details of this case from Messeri (2010).

resources to be full members of the IAU, to attend the final day of the conference, and to participate in the debate until the end.

Finally, it is uncertain whether what took place in the IAU vote resolved anything in a lasting way; the controversy over Pluto has not quieted much, either within different sub-disciplines of astronomy, or within society more broadly. And it is far from clear that this redefinition has had much impact on how research is conducted on Pluto or other sub-planetary bodies in our solar system. Happily for NASA, the fact that the demotion of Pluto has not been readily accepted by the wider culture might mean that the IAU's decision will not have the material economic impact feared by those like Stern.

4.2. Asilomar and Epistemic Insider Trading

In 1975, a group of scientists along with some lawyers and policy makers met in Pacific Grove on the central coast of California. The subject of the meeting was recombinant DNA techniques and technology, and the meeting has since become known simply by the name of the beautiful venue at which it occurred: Asilomar.

Gene-splicing shifts from mere possibility into actual practice in the early 1970s. By the summer of 1974, a *Committee on Recombinant DNA Molecules* had been formed by the Assembly of Life Sciences within the National Research Council of the National Academy of Sciences. Said committee simultaneously announced in *Nature*, *Science*, and *PNAS* their call for a moratorium on recombinant DNA work in molecular biology until proper safeguards against potential biohazards could be put into place. Asilomar occurred over three days in late February 1975, and a resulting set of tentative recommendations regarding under what conditions work could responsibly proceed were announced (e.g., by Berg et alia 1975 in *Science*). By late 1975, the National Institutes of Health (NIH) had drafted a candidate proposal for discussion and review, and on June 23rd, 1976, the agency issued their official *Guidelines for Research Involving Recombinant DNA Molecules*. These guidelines were published in the federal register just a couple of weeks later, on July 7, and all new proposals as well as renewals involving recombinant DNA research received on or after December 1 of that same year had to be in compliance with the guidelines in order to be considered for funding by the NIH.

It is helpful to divide the history of how Asilomar has been presented and understood into several distinct waves. In its initial wave of public relations and perception, Asilomar is typically seen as a model of reflective, philanthropic scientific self-regulation conducted voluntarily in the public interest. Although there are a few vehement detractors (e.g., Chargaff & Simring 1976), the presentation and press covering Asilomar in this first wave tends to laud the effort as a model of scientific precaution and civic duty. The fact that the moratorium was self-elected and widely followed; the manner in which discussion of risks and benefits was openly conducted and widely attended; and the corresponding development of actual regulation, put into place and then followed and regularly updated by the NIH—all these features were regularly pointed out by scientists and science journalists alike, in their presentation of Asilomar and what made it special.

A second wave of Asilomar commentary crashes onto the metascientific shore as quickly as the historical and sociological scholarship can catch up and begin to scrutinize the phenomenon this conference turned out to be. This wave is driven more by science studies than by scientific practitioners themselves or the usually quite friendly establishment journalists at, e.g., *Science* and *Nature*. In this wave, assessment of Asilomar is markedly less credulous when it comes to uptake of what the conference organizers and principal actors themselves declare that they are doing. Was the aim of the conference to discover whether or not recombinant DNA work could be safely and ethically performed; or, was the aim of the conference to ensure that when recombinant DNA work did occur, there would be no major public outcry or panic (à la Wright 1994)? Was there sufficient, broad representation of ethical, social, and political considerations by actors from across diverse sectors; or, was this a discussion led entirely by molecular biologists with a few token actors from other sectors selected for their amenability, without any meaningful representation from policy makers, environmental scientists, or clinical and health researchers (à la Krimsky 1982)? Was this a public-interest driven attempt to ensure the safety and responsibility of science; or, was this a scientifically self-interest driven attempt to ensure management of the issue stayed in the hands of practitioners rather than in those of politicians or the public?

This second wave comprising more mixed takes on the ethicality and (self)interestedness of Asilomar has endured for quite some time (e.g., Hughes 2011; Yi 2015). Perhaps the most important thing to note about the transition from the first wave to the second wave is that Asilomar moves from being glossed as, fundamentally, for the public (in the first wave) to for the practitioners (in the second wave). But politicians and the public may not have been the only ones left behind by the meeting and its resulting guidelines.

It is past time to introduce a third wave of Asilomar assessment in STS: one that pays attention to the way that those scientists who participated in the conference and were the architects of the eventual requirements for recombinant DNA work were positioned, by the way this situation unfolded, to leap ahead in the race for a succession of discoveries which quickly followed the lifting of the moratorium in the summer of 1976. One of us worked at the Salk Institute for a handful of years, where many of the key players in this recombinant regulatory debate worked at the time it occurred. The story told there, in the early 2000s, was that at midnight one night, when the moratorium was officially lifted and the rest of the country learned what size of clean room, what degree of ventilation, and what design and orientation for the physical laboratory space was required in order to responsibly conduct NIH-fundable recombinant DNA work, several of the architects of the regulation were able to begin work at 12:01am. That is because, as architects of the regulation, they knew in advance what laboratory spaces to physically construct. Following the ban meant waiting to do any recombinant DNA work until the ban was officially lifted; but it did not necessarily mean waiting to physically build the clean rooms required. For a few months, while the rest of the field were rushing to construct the laboratories in which such work could be conducted, those who already had their physical spaces built could begin the experimentation—and race ahead. Lest we forget, this is an area in which several Nobel Prizes were eventually awarded. It is also the area in whence occurred, shortly after the

ban was lifted, those developments which led to (e.g.) the founding of Genentech and its resulting earnings derived from patents produced in this era.

A third wave of Asilomar scholarship might thereby look at the way Asilomar drew a circle around participating molecular biologists, leaving other competitors from within the field outside the boundary. Group making is fundamentally both inclusionary (of those inside the circle) and exclusionary (of those outside it), and it is not just members of the public, policy-makers, and those scientists from areas other than molecular biology who were excluded from the conference's inner circle. Some molecular biologists were on the inside, others were on the outside; it may even be the case that something like "epistemic insider trading" occurred—as those on the inside used their knowledge to leap ahead in the extremely prestigious and highly profitable recombinant DNA race.

5. From Consensus and Dissemination to Incorporation and Deployment

The lesson of our first case (Pluto's deplanetization) is that the process of consensus formation in scientific practice tends to fall short of the ideal of transformative criticism that the seminar-room idealist projects. If transformative criticism is regularly approximated by the discursive activity of the scientific community, then we should expect the process of defining "planet" should exemplify that ideal. The IAU is an august organization, in operation for over a century, with members on six continents. It formed a committee well ahead of time, in response to concerns that had been raised in the literature. The committee presented its results at a large meeting, in a discursive forum, to be discussed and voted on by attendees. And yet, there are many flaws in the process and product of this deliberation. It is not clear that the question at hand was an important scientific one, nor that it was settled in a way that resolved the controversy satisfactorily. The careful work of the committee was thrown out in favor of a rushed and *ad hoc* process. The prestige and power of certain key movers in the field seemed as central to the issue as any empirical, conceptual, or pragmatic value of the competing definitions. And while the case might seem to have serious repercussions for the field, the lack of public, non-scientific acceptance of the decision, along with the ongoing scientific controversy, mean that in practice the decision is relatively idle. It was a "seminar room" discursive process, but it was far from ideal in execution or outcome.

The complementary lesson provided by our second case (Asilomar's insideriness) is that the conceptualization of the scientific process of consensus-building and discovery as seminar-room discussion usually presumes that the room in which discussion is to occur has already been built. It often leaves out who can get to the table, or even pay tuition to enroll in the hypothetical seminar in the first place. Literal rooms—laboratory spaces with all the physical-economic requirements therein—might need to be built. What those blueprints will look like and who can afford the construction costs affects who gets to eventually and discursively debate the chosen scientific question. These material and economic issues can even affect which questions will be selected as worthy of discussion! If

the lesson of our first case is that seminar-room discursive-style scientific processes can be far from ideal, the lesson of our second case is that the action often happens elsewhere: not in a seminar room, or even in a hotel's conference rooms. It happens in laboratories with rooms, workers, instruments, and materials that all need to be acquired and paid for.

Let's return to the early anthropological work of Knorr-Cetina, which importantly reframes concepts like validation, acceptance, the context of justification, and scientific consensus in terms of laboratory practice, contents, and location:

> But where do we find the process of validation, to any significant degree, if not in the laboratory itself? If not in the process of laboratory decision-making by which a previous result, a method or a proposed interpretation, comes to be preferred over others and incorporated into new results? What is the process of acceptance if not one of selective incorporation of previous results into the ongoing process of research production? To call it a process of opinion formation seems to provoke a host of erroneous connotations… To view consensus as the aggregate of individual scientific opinions is misleading, since (a) short of regular opinion polls we have no access to the predominant, general or average opinions of relevant scientists, and (b) it is a commonplace in sociology that opinions have a complex and largely unknown relationship to action. So even if we knew what scientists' opinions were, we would not know which results would be consistently preferred in actual research. What we have, then, is not a process of opinion formation, but one in which certain results are solidified through continued incorporation into ongoing research. This means that the locus of solidification is the process of scientific investigation, or …the selections through which research results are constructed in the laboratory… Consequently, it is the process of production and reproduction of research in the laboratory we must look at in order to study the very "context of justification." (8)

Philosophy of science has sometimes erred in taking the explicit justifications found in scientific publications as a model for the context of justification, and then compounded the error in the social epistemology of science by socializing that process as a discursive one. But STS scholarship demonstrates that the social epistemology of science is realized in material practices through which results become incorporated and deployed in new contexts.

Conclusion

Empirical scientific results propagate not merely through rational discourse, but through selective incorporation of past results due to their fruitfulness for future results in combination with the ability that scientific players have to access the arena in which results are produced and contested. This pay-to-play constraint has a rhetorical dimension, as results are marshaled for support of new arguments in the literature (Latour 1987), as well as a practical dimension, in which past results are incorporated into new laboratory work, insofar as they promise to further those activities (Knorr-Cetina 1981), and finally an

economic dimension, given that the chance to challenge past results or generate future ones is conditioned on the ability to afford the cost of entry to the field.

The key lesson of the anthropological strain of STS, from our perspective, is that the non-communicative, non-discursive elements of scientific practice are crucial to its successes, constrain its capacities, and condition its failures. Any purely discursive approach (whether it be the philosophers' rational reconstruction or the sociologists' discourse analysis) will fail to account for the nature of scientific practice and the content of scientific results. It has become commonplace in both STS and philosophy of science to acknowledge the role of tacit knowledge. The so-called "practice turn" in the philosophy of science pays increased attention to the material conditions of scientific practice (e.g., Chang 2004; Mirowski 2011; Leonelli 2016; Massimi 2022). But the complex materiality of laboratory practice, including its human and political economy, has still not received sufficient uptake in either field, particularly in discussions where notions of dissent and consensus loom large.

Achieving objectivity in scientific communities requires the removal of certain barriers to access and participation. STS scholars have tended to shy away from talk of scientific progress, but a science deficient in objectivity is bad for all of us. We do not need a vision for what counts as ideal science, or even belief that such a thing is possible, in order to nonetheless defend the notion that there are better and worse ways for our non-ideal science to be.

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