Imagining Responsibility, Imagining Responsibly: Reflecting on Our Shared Understandings of Science

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Abstract

If we cannot define science using only analysis or description, then we must rely on imagination to provide us with suitable objects of philosophical inquiry. This process links our findings to the particular ways in which we philosophers idealize scientific practice and carve out an experimental space between real world practice and thought experiments. As an example, I examine Heather Douglas' recent work on the responsibilities of scientists and contrast her account of science with that of "technoscience," as mobilized in nanotechnology, synthetic biology, and similar control-oriented fields. The difference between the two idealizations of science reveals that one's preferred imaginary of science, even when inspired by real practices, has real implications for the distribution of responsibility. Douglas' account attributes moral obligations to scientists, while a framework of "technoscience" spreads responsibility across the network of practice. I use this case to call for an ethics of imagination, in which philosophers of science hold themselves accountable for their imaginaries. We ought reflect on the idiosyncrasy of the philosophical imagination and consider how our idealizations, if widely held, would affect our fellow citizens.

1. Introduction

In 1938, *Philosophy of Science* published a talk by RK Merton, "Science and the Social Order." Merton captures, in his characteristically sweeping manner, the tensions inherent in nesting the institution of science within broader society. The scientific ethos, he suggests, requires scientists to focus on the immediate furtherance of knowledge, disregarding the way in which scientific results travel out into other "spheres of value and interest." The scientist justifies this epistemic myopia by believing that science is nonetheless a force for good in society, a rationale which Merton flags as a non-factual matter of faith. Though science is a solely truth-oriented practice, it is grounded in a commitment to social good. Merton calls this vision of scientific work "a confusion," an ethos that both reinforces the societal importance of an autonomous science and renders it incapable of addressing controversial social effects.

Merton's observations highlight the importance of collective self-understandings and of shared ideals. The ethos that he describes reserves a place for science in society, but at the cost of opening up scientists to responsibility attributions from their critics. The virtue of disinterestedness begins to look like negligence in the eyes of broader society, with consequential effects. Members of the public may voice dissatisfaction. Funding administrators might look elsewhere. And scientists might find themselves strategically reducing their autonomy in some ways, aiming for socially significant truths.[1] Even though an ethos is "in the head", so to speak, a careful sociologist or empirically-minded philosopher will see that cognitive visions for science are deployed, resisted, and modified in practice. As Merton shows so well, they are "ideal," loaded with propositions, values, and hopes, but are also often "real," providing meaning and guidance for the action of individuals and groups.

In 2016, this organizing feature of collective imagination is as active as ever, tying together activity as diverse as president's speeches, the BRAIN initiative, patient activism, and novel collaborations between engineers and scientists. Nevertheless, the action of collective imagination receives only indirect attention in the recent philosophical literature on science. I invoke 1930s Merton as a reminder that this sort of inquiry can be illuminating even within the narrow context of our field. The specific vision behind a practice serves as something to hold onto when we tackle tough questions of appropriate evidential relations, of scientific responsibility, or of technological development, to name a few. Meanwhile, work in science and technology studies (STS) has made collective imagination a core methodological focus. Most recently, Jasanoff and Kim (2015) direct our attention to the action of "sociotechnical imaginaries," defined as "collectively held, institutionally stabilized, and publicly performed visions of desirable futures, animated by shared understandings of forms of social life and social order attainable through, and supportive of, advances in science and technology." The authors effectively demonstrate how imaginaries provide a normative soundtrack or blueprint, so to speak, for the otherwise incomprehensible and deceptively mechanistic action of science in society.

This added perspective, I suggest, does two things. First, it prepares the philosophical observer of practice to think about the sciences as situated in a broader material-discursive context. Imaginaries highlight the ordered ways in which scientific reasoning is made simultaneously intelligible and possible through particular institutional arrangements, values, and visions of desirable futures. Philosophers can then engage with these normatively-laden features in a given context, inquiring into their origins

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[1]Indeed, in some respects this may be the situation we are in now.

and asking if they are worthy of our assent. Second, the framework of imaginaries can be turned back onto philosophical practice itself as a form of self-reflection. By identifying the content of philosophy's own internal imaginaries of science, whether idealizations or heuristic definitions, we can compare our discourse with the sociotechnical imaginaries that actually organize the sciences in society. Comparison can thus ground our own disciplinary habits of imagination.

To support these bold promises of utility and self-correction, I will provide here a more pointed example. The next few sections will examine more carefully the interaction between our idealizations of science and our intuitions about how responsibility is distributed. The first case is Heather Douglas' responsibilist framework. I argue that her framework is still beholden to more traditional Kuhnian idealizations of science as an isolated practice, which negatively affects the applicability of her prescriptions. Second, I will present a counter-imaginary from emerging technosciences, like synthetic biology or nanotechnology, in which the Kuhnian assumptions are not present; science is understood as entangled with society, with industry, government, and new technological applications. These contrasting cases, I will show, exhibit the idiosyncrasy of philosophical idealizations of science and encourage us to make imagination a more explicit object of inquiry. Our work on science will be better, I suggest, when we actively question the implicit sociotechnical arrangements and desirable futures that ground our normative work on science.

2. Old Ethics in New Bottles

Heather Douglas' recent work on responsibility begins where Merton leaves off in "Science and Social Order." Her arguments in *Science, Policy, and the Value-Free Ideal* (2009) continue that mid-20th century debate, an unfinished conversation over whether scientists deserve "autonomy" in their pursuit of knowledge. As Douglas describes it, one side emphasizes the ways in which science is and must be free of social or political values. These arguments for autonomy, relying on Michael Polanyi's "tacit knowledge" and scientific realism, represent several compelling possibilities. But value-freedom, even as an ideal, is not quite so simple. Douglas reminds us of the other side of the debate. Responses from Rudner (1953) and Churchman (1948) reveal that we cannot avoid judging the quality of a particular hypothesis or the risk we take on by accepting it. With these historical points and counterpoints laid out, Douglas presents her own analysis. Scientists, she suggests, have "general" moral responsibilities; they are responsible for their behavior in the way all social beings

are. And they have "role responsibilities," unique to the position of a scientist. She acknowledges that, sometimes, our roles can take priority and remove our general moral liability. Lawyers, she suggests, have a role that shifts responsibility onto the legal system and can disregard some general moral responsibility as they defend a guilty client. So we might think that the role of scientist is like that of a lawyer, such that general moral responsibility for mistakes and oversight is set aside. Douglas thinks not.

The problem, she suggests, is that usually there is no "rigid" institutional structure within science that can take morally significant decisions out of the hands of scientists.[2] Unlike the context of law, there is no adversarial system to ensure diverse or antagonistic perspectives are marshaled. There is no judge that at the end of the day has the power to decide whether or not a hypothesis deserves further testing, weighing its probability against the very real costs of a false positive. This is a decision that only the scientist and her immediate colleagues can make. Douglas, thus, argues that scientists have an obligation to consider the cost of error in their professional claims; they are not off the hook when it comes to general moral responsibilities. Douglas thus attacks the inconsistency in the traditional idealization of science and cuts through the "confusion" noticed by Merton in 1938; because scientific practice is sufficently unlike legal practice, its practitioners have to give up some autonomy.

This negative comparison, however, leaves Douglas' idealization of science somewhat implicit. Douglas' more constructive analysis can by found in *The Moral Terrain of Science* (2014), where she lays out a revised vision of science; it is a practice that is situated in and valued by society. She uses this fact to prescribe scientists' responsibilities beyond internal communal norms. There are, accordingly, three "bases" of responsibility to which scientists are accountable: "The first is to good reasoning practices; the second to the epistemic community of science; and the third to the broader society in which science functions and is valued." If we want to normatively evaluate a particular research project or field, we can run through each basis. *On the first basis:* did the research produce "reliable empirical knowledge"? According to Douglas, science exists because society values it as a source of "reliable empirical knowledge," and is thus beholden to that epistemic standard.[3] *On the second basis:* did the researchers support and enable their epistemic peers? As members of an interdependent community, scientists cannot act as if they work alone. *Finally, on the third basis:* Can we say that

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[2]SPVFI, p73

[3]ibid. p95

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a given area of research embodies and advances the values attributed to science by broader society? Since science does not occur in a vacuum, research and its reasonably foreseeable effects should not conflict with societal values.

As Douglas points out, this framework prevents anyone from problematically bracketing questions of knowledge from questions of morality; scientists can't excuse themselves from moral responsibility (i.e. basis 3) for the sake of empirical knowledge (i.e. basis 1). A neuroscientist, for example, cannot cite her dedication to good science simply to evade normative evaluation of her contribution to technological development, whether for military lie-detectors or novel treatments for depression. A philosopher of science is likewise encouraged to develop a sensitivity to the whole range of obligations that scientists might have, whether epistemic or ethical or in between. In this way, Douglas resolves the tension in the traditional idealization of science not by denying it outright but rather modifying it in favor of social responsibility. In her picture, science is still presumed to be a discrete practice, isolated from broader society by a set of internal epistemic norms, but with a few added ("general") moral responsibilities.

This strategy seems argumentatively effective against those who espouse the "value-free" ideal of science. However, Douglas' responsibilist approach still draws on her own philosophical imaginary of science, rather than an actual existing practice. Her descriptions of science seem to continue Kuhn's legacy of treating science as self-contained community, which operates according to internal norms. This choice is most evident in her chosen definition of science: "an iterative, ampliative process of developing explanations of empirical phenomena, using the explanations to produce predictions or further implications, and testing those predictions. In light of the new evidence from the tests, explanations are refined, altered, or further utilized." It is not clear if the scientific community, presumably as much an institution or a loose set of organizations as a process, would be correctly circumscribed by such a definition; if we took the process she describes as the definitive norm for scientists, then the "scientific" community might exclude scientists working in the context of biomedicine (as I will describe in the next section) as well as researchers pursuing what STS scholars highlight as "technoscience." Such a definition also fails to account for clinicians, DARPA, industry, the Market, bioethicists, scientist-engineers, and other things that are not self-evidently inside or outside science thus defined.

These exclusions from Douglas' framework may not be worrying in the context of internal philosophical

debates, but the values in science debate touches on fundamental questions of great practical import. Deciding on the proper role of science in society is tantamount to deciding which forms of life we want to foster and which futures we want to reach. If our goal is to realize this practical potential of philosophical thought, then Douglas idealization of science is unsatisfactory in a couple of ways. First, an ideal with little connection to actual practice may inhibit its utility. If a philosopher—Douglas included—uses a non-existent or ideal practice to set its value and its community structure, the resulting prescriptions may be inapplicable or practically useless. In the case of researcher responsibility, Douglas' prescribed obligations will have little or no force for technoscientific researchers, who may not recognize the imaginary being presupposed. "Who said we're all in the business of explaining?" a physicist might ask while working in the context of nanotechnology. Philosophical sophistication is of little value when the ideals under negotiation are unique to disciplinary philosophy. Second, "sociotechnical imaginaries" do not just assume how science should be organizationally configured; they also pair that configuration with some desirable future that we are working towards, whether that consists of avoiding climate change or creating healthy self-disciplining citizens. Even if Douglas admits the loftiness of her conception of science—maybe it's just a distant hope—then there remains the task of justifying that desirable state of affairs to persons who might not share Douglas' value judgments. Returning to some empirical findings from research practice will help make these criticisms more concrete.

3. Philosophical Imagination vs. "Mode 2" Science

We should primarily understand Douglas as responding to a particular debate about value-free science. In that sense, she presents a necessary correction to the ideal of science as value-free by formulating an opposing ideal. But if her mapping of the "moral terrain" is to provide a new responsibilist framework for philosophy of science, there are some additional obstacles to consider. In Douglas' main arguments, she treats science as a single identifiable practice, independent from university professorships, federal administration, and biotech start-ups. Recall, for example, that her imagined scientific community is circumscribed by its pursuit of "reliable empirical knowledge." Douglas also distinguishes between "the value of knowledge" and "social value," which (without some qualification) neglects her overarching insight that scientists frequently make policy and, in general, influence social order. This is disappointing given that Douglas herself acknowledges the extent to which philosophy

of science, under the influence of Thomas Kuhn, has artificially bounded science.[4] On one reading, Kuhn uses *The Structure of Scientific Revolutions* to hide a vision of autonomous science within socially-rich imagery, to create a "social" picture of science that neglects society. Steve Fuller, for instance, sees this as *the* consequential and anti-democratic misstep within philosophy of science.[5]
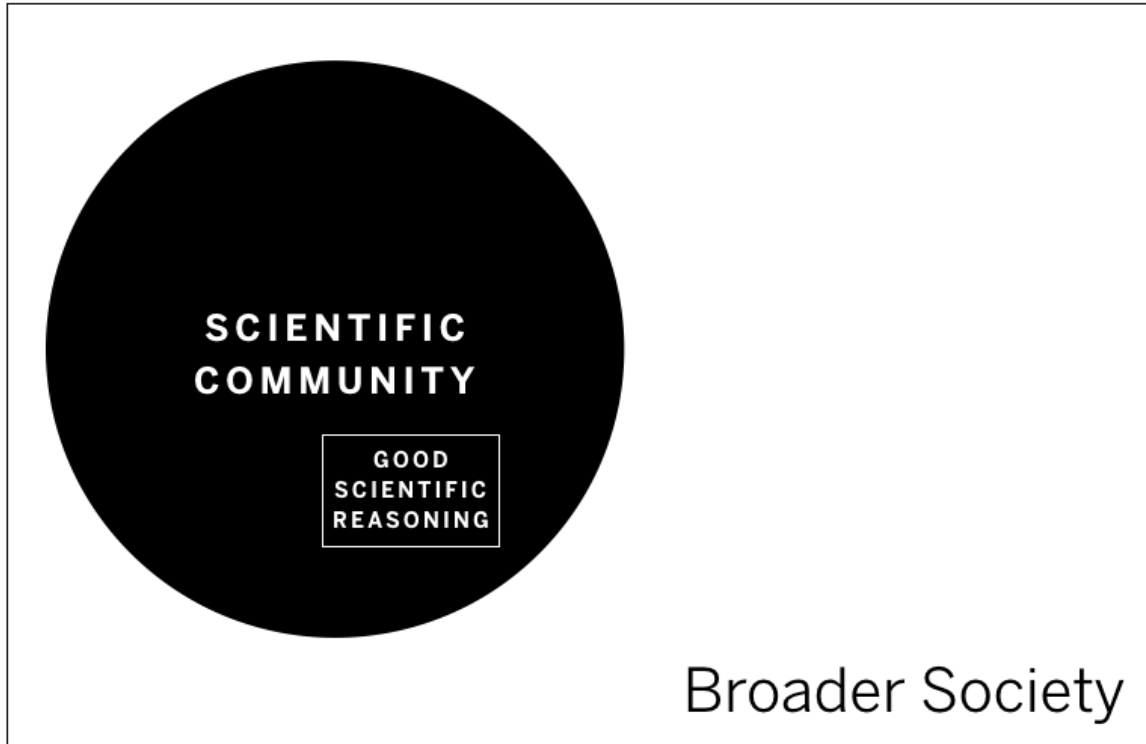

Figure 1: Basic "Kuhnian" Imaginary

Do these similarities reveal a shared mistake among philosophers? Do philosophers of science uncritically reinforce an idealization—we might say a Kuhnian imaginary—of science as an isolated practice? Only empirical work on philosophical discourse would do that question justice, but it is instructive to compare philosophers' implicit visions of science with those outside of philosophy of science. At the extreme, the Kuhnian idealization presupposes a clearly bounded scientific community, a consistent empirico-methodological core, and a society which surrounds it (see figure. 1). This caricature is surely not shared by all philosophers, but it should trigger a moment of reflection about what inspires or supports our own imaginaries. Even if it is used heuristically, as it may be in Douglas' arguments, idealizations are not neutral or inconsequential. Such reflection is especially

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[4]SPVFI, p61

[5]Fuller (1992) "Being There with Thomas Kuhn"

important, given that work in STS radically upsets the Kuhnian imaginary that underpins the debate over scientific autonomy.

Literature in STS typically portrays the boundedness of science as an achievement, the result of deliberate social and discursive work by practitioners. What is "inside" or "outside" of science is not given in advance, but actively contested and negotiated. Gieryn (1983) describes such "boundary work" as a practical problem for scientists, impacting the allocation of funding, respect, and epistemic credibility, among other things. Researchers can, for example, build up a distinction between pure and applied science, such that they are not responsible for failed technological applications. Or, as is more common today, they can undermine the distinction such that science can ally itself with industry. Latour (1991) takes the contingency of boundaries even further to suggest that the modern idea of Science stands or falls with our faith in a transcendent Nature that can be purified of culture or politics. Scientists then position themselves as the exclusive representatives of objects and natural things, portraying everyone else as operating in the messy world of human interests or passions. We need not, according to Latour, take this dichotomy for granted; through the lens of networks, the spheres of culture and nature are not so cleanly separated. Overall, these critical examinations of science challenge us to ask how an idealization is sustained and to inquire who benefits and who is harmed via a given idealization of science.

More to the point, the specific character of boundary drawing described in recent years seems to stray from the Kuhnian (or Modern, as Latour might say) ideal of science. Alfred Nordmann (2012), for example, stresses that much of contemporary science (i.e. "technoscience") is oriented towards "knowledge of control" and capacity-building, of which both evade narrower philosophical discussions of scientific knowledge. Bensaude-Vincent et al. (2011) argue further that scientists can now study and present objects explicitly in terms of their potential value to humans, non-scientists included. The authors suggest that much of today's most heralded research falls into this category of "technoscience", despite the fact that it clashes with the more "pure" vision of science proposed by philosophers like Francis Bacon, William Whewell, and Moritz Schlick. At a higher level of analysis, these phenomena could be described as part of the emergent "triple helix" of science, industry, and government collaboration (Etzkowitz and Leydesdorff, 1998) or the regime of "mode 2" science (Gibbons et al., 1994). Each of these empirically-based understandings of science show it to be more porous, less obviously bounded than we might think.

Most significant, for questions of responsibility, is the way in which some individual researchers in technoscience echo STS work in their self-understandings. Some deny their isolation from society by invoking an anti-Kuhnian imaginary. Previous empirical studies have shown that technoscientific researchers will often describe themselves as cogs in a machine, lacking true control—and thus responsibility—over the direction of their projects.[6] In these cases, describing science as hopelessly entangled in society has the same effect as an autonomous science; it becomes difficult to hold researchers accountable for the values they espouse or the technologies they enable. Boundary work, thus, can function just as well removing distinctions as it does building them up. The action of institutional structure, funding priorities, and disciplinary identities, among other things, can complicate Douglas' partially *a priori*, partially empirical attributions of scientific responsibility.

Directed back at Douglas' "bases of responsibility", the anti-Kuhnian imaginary of science can be used to pose a very simple critique of her prescriptive claims. An individual can't be held responsible without some combination of freedom, causal sufficiency, and factual understanding of possible consequences. And technoscientific actors might deny having any of the three conditions! If, for example, funding agencies only further unethical values, then the individual researcher might cite this as a severe constraint on their agency. Or similarly if the disciplinary identity of the neuroscientist is subsumed by pressure to become an excellent entrepreneur, than the failing to fulfill "scientific responsibility" may be unavoidable for some. Scrambling for intellectual property might be expected, despite the negative effects on an open community of inquiry. One could thus read the technoscientific imaginary as disarming Douglas' responsibility attributions.

This critique, however, is overly simple for a couple of reasons. Douglas acknowledges that individuals may have to "collectivize" certain responsibilities that are too big for one person, as in the creation of the National Science Foundation or the National Institutes of Health;[7] a lack of causal sufficiency is not an automatic excuse for the individual. Thus, individuals may have a second-order responsibility to work around and/or change institutional obstacles to their fulfilling their obligation to the "three bases." Moreover, she acknowledges that we need to recognize the range of between "minimum standards" that are not "absurdly strict" and "ideal behavior" that may be unattainable for anyone. Douglas, thus, acknowledges the existence of social constraints on individuals and attempts to integrate that insight into her prescriptions. In other words, the mere fact that some scientists or

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[6] see for example Swierstra and Jelsma (2006)

[7] Moral Terrain, section 4

engineers deny responsibility is not particularly significant.

The problem with simply presupposing a Kuhnian imaginary is more general and more pervasive than a first-order disagreement about scientists' agency. Douglas' account of responsibility depends on a certain way of thinking about science and the way that it is ordered. Like much of philosophy of science, this account is partially empirical, relying on the author's familiarity with real world practice, and partially *a priori* holding some features as fixed or as definitive. Douglas' choices have not, to my knowledge, been controversial among philosophers of science. And her account can only get better as details from actual sciences are filled in, as the "moral terrain" is given some much-needed topographical detail. But by juxtaposing the implicit Kuhnian imaginary of science with the STS literature on technoscience, the idiosyncrasy of philosophical imagination is revealed. Our go-to idealizations of science or quick definitions should be more controversial than they are. While there is nothing inherently wrong with being idiosyncratic, philosophy of science can benefit from confronting its choice of imaginaries.[8]

4. Towards Responsible Imagining

Despite the "demise of the demarcation problem" (Laudan, 1983), philosophers still talk about "Science" or a science or "the sciences." Even without a clear definition, we charge full force into discussions of "well-ordered" science or the value-free ideal. It is imagination, I suggest, that has supplanted the analytic task of definition. Even when an philosopher provides an nominal definition, the arguments that follow often play fast and loose with ideals and actualities, switching seamlessly between features that we want science to have and features that it has in spite of us. Though Laudan has banished words like "unscientific" to the world of politics or Edinburgh SSK, many philosophers rely on an ability to inject their hopeful vision of science into discussions of practical import and real world problems. Douglas does so in order to start a conversation of responsibility. Helen Longino's (2002) critical-contextual empiricism, another example, turns her commitment to liberal society into an account of good science. In this sense, philosophers of science are not so different from everyone else. Sociotechnical imaginaries help scientists, policy-makers, and publics think about and organize science in society, even as its exact definition or boundary is contested.

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[8]More politically-oriented philosophers of science, Popper and the Vienna Circle included, show that there is a precedent for this sort of reflection; each tied their visions for science to a distinct set of ethico-political values.

It is a strength of our field that we can use imagination in a similar way, crafting visions of science that are not limited by rigid adherence to empirical detail or by the immediate practical needs of institutions. For this reason, I am not recommending that all philosophers of science become sociologists of knowledge or that they substitute all idealization with description. But the process of imagining Science, as good or as bad, is tied to the context of imagination. As Dewey puts it, "[The] ideals that guide us are generated through imagination. But they are not made out of imaginary stuff. They are made of the hard stuff of the world of physical and social experience" (LW 9:33). The philosophical form of life shines through when we write about science. Arguments and evidence, texts and disciplines, these things are present but so much else can be left out, from the 1980 Bayh–Dole Act to the regime of technoscientific promises (Joly, 2010). These are more likely to be found in the self-understandings of synthetic biologists or in the STS literature.

As mentioned earlier, this fact creates a practical challenge; philosophers' insights and prescriptions might go unheeded by non-philosophers. Our old fashioned examples, thought experiments, and writing might fail to resonate with the worldly scientist of today, who may be as concerned with collecting patents as with the context of justification. But more fundamentally, the barren ontology of the Kuhnian imaginary fails to direct our attention to the countless ways in which society serves as a condition of possibility for science. "Good" scientific reasoning is situated in a constellation of widely-shared implicit normative commitments. Public discourse, for example, shapes what types of bodies or minds are deemed broken and what projects deemed fund-able or ethical. One need only look at the beginning or end of papers in *Science* or *Nature*, where the authors echo and reinforce these commitments (e.g. 'x number of people suffer from condition y'). As suggested by the aforementioned STS work on technoscience, these interactions between society and science are as consequential as they are ubiquitous. And such interactions should not be excluded without some reason, which brings me to the second sort of worry mentioned earlier: the ethics of imagination.

Defaulting to a Kuhnian imaginary may run afoul of our own disciplinary standards. Though philosophy is often associated with careful reflection, our choice of idealization may have become unreflective over time. Most philosophers do not spend time in labs or tune into congressional hearings on science policy, so there is little that could contradict implicit imaginaries. At the same time, many philosophers of science can recite the general idea of *The Structure of Scientific Revolutions*, whether admiringly or not. This form of life may be well-suited to avoiding a foundational question:

what would it take to explicitly justify our chosen imaginaries of science? How do we pick what goes into and what is left out of our idealization? More STS-minded philosophers might stress the importance of observation; start with the what exists outside the office, they might suggest. Such a re-orientation towards the science-in-the-world is an excellent place to start, and some philosophers have a substantial head start in this respect.[9] However, even the devoted armchair philosopher could benefit from spelling out out exactly which desirable futures she is taking for granted when doing analytic work.

In the case of the Kuhnian imaginary, the maintenance of a epistemologically-bounded scientific practice fulfills our modern aspirations of a purified nature, where citizens can watch carefully crafted experimental performances and ascertain the truth for themselves. Consequently, the scientist earns a special role in democracy, providing ostensibly apolitical facts to feed into the processes of governance and policy-making. "Scientific" responsibility, in this picture, can be stringent within the community, but it does not easily extend to the grey areas or "trading zones" (Galison, 2010) where theory and application blur. Not so in the technoscientific imaginary. Technoscientific responsibility could explicitly include applications, but perhaps at the cost of disappearing the individual researcher in a vast network of technological development. Accordingly, the technoscientific imaginary includes a place for publics, who demand a voice in previously closed processes of sociotechnical change. These are just some of the trade-offs and distinctive features that deserve consideration.

In either case, an ethics of imagination does not dictate that we discuss only one, good imaginary or only the "accurate" imaginary of science. Accuracy is probably a poor fit for something that is at least partially ideal and almost completely perspectival. Instead, I propose an ethics of imagination in which we take up or reject these imaginaries with care and deliberation. As in Gieryn's work on boundaries, we should ask who wins and who loses within our favorite imaginary. We should ask how responsibility might be distributed, who gains epistemic authority, and how democracy is re-configured. There are surely many other questions to be asked, but most of all we should imagine science or technoscience with a keen awareness of how could affect our fellow citizens, especially those with the least power. As Merton, Jasanoff and Kim, and Dewey all suggest, collective imagination matters, structuring identities, practices, and society at large. To the extent philosophers are part of this process, we must take care that our contributions are as responsible as they are creative.

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[9]see Bernadette Bensaude-Vincent, feminist philosophers of science, like Helen Longino, among others.

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