Challenges for 'Community' in Science and Values: Cases from Robotics Research

Charles H. Pence[*]
charles@charlespence.net

Daniel J. Hicks[♦]
hicks.daniel.j@gmail.com

ABSTRACT

Philosophers of science often make reference – whether tacitly or explicitly – to the notion of a *scientific community.* Sometimes, such references are useful to make our object of analysis tractable in the philosophy of science. For others, tracking or understanding particular features of the development of science proves to be tied to notions of a scientific community either as a target of theoretical or social intervention. We argue that the structure of contemporary scientific research poses two unappreciated, or at least underappreciated, challenges to this concept of the "scientific community" in the philosophy of science. In particular, we will present two case studies from robotics research, broadly construed, which show that (1) the *boundedness* of the scientific community is threatened when private citizens can develop scientific and technological advances at minimal expense (*democratization*), and (2) the *discreteness* of scientific research programs is threatened by the complexly interrelated environment of contemporary scientific work (*interconnectivity*). Taken together, the extent of democratization and interconnectivity present a significant challenge for any practically oriented philosophy of science, one which we hope will be taken on directly by philosophers in the future.

1. Introduction

What are we studying when we are studying science? One obvious answer might be that we are studying the practices, claims, beliefs, theoretical or material products, and so forth, of something called *the scientific community.* Any

[*] Université catholique de Louvain, Belgium.
[♦] University of California Merced, USA.

such concept is, to be sure, historically situated. Over the course of the twentieth century, paralleling the development of what we now call the received view in contemporary philosophy of science, we have seen a broad standardization of what it has meant to be a member of such a community (Shapin, 2010). The notion of the scientific community thus became deeply integrated in a whole generation of philosophy of science. Discussion of scientific paradigms in the Kuhnian sense, Lakatos's research programmes, or even Bachelard's picture of science as a series of epistemic ruptures all rely to varying extents upon our ability to distinguish such communities and their parts, track their change over time, and so forth.

As we move well into the twenty-first century, we find that both the scientific and philosophical components of this view look quite different. First, the structure of science has radically shifted. Breakdowns in traditional institutions of academia (including changes as diverse as the increasing precarity of the workforce and persistent calls for interdisciplinarity) have been matched by breakdowns in those same structures in private industry (see, e.g., Mirowski, 2012). Second, the philosophy of science has itself changed. Two such trends, among many others, are worthy of note for us here. An increasing emphasis has been placed on the philosophy of science in practice, attempting to derive philosophical conclusions in the closest possible dialogue with practicing scientists themselves (Soler et al., 2014). The Socially Relevant Philosophy of Science movement, or SRPOS, has aimed to supplement and challenge mainstream philosophy of science by investigating novel objects of study and developing novel conceptual frameworks for understanding those objects, with the goal of offering philosophical insight on topics of contemporary social or political relevance.[1]

This quickly changing landscape, we believe, calls for a reevaluation of the notion of the role of the scientific community itself and its uses within philosophical work, and our project here constitutes some first steps in that direction. In this paper, we want to argue for two related claims. First, the notion of the scientific community remains important for the philosophy of science – even in its most contemporary or innovative incarnations. Second, this concept suffers from two significant but underappreciated weaknesses that cast doubt

[1] Paradigms of SRPOS for us include Cartwright (2007), Mitchell (2009), Parker (2010) and the contributions to Plaisance & Fehr (2010).

on its ability to play the roles that are demanded of it. If these two claims are true, then the place which such a concept takes in our understanding of science itself needs to be carefully rethought, work which we hope philosophers will increasingly tackle head-on.

We will proceed as follows. First, in §2, we will consider whether or not a notion of the scientific community is really necessary in contemporary philosophy of science, arguing that indeed it is, whether references to it are made tacitly or explicitly. Next, we turn to the two problems with this notion that we believe contemporary science poses. In §3, we discuss research on drones – autonomous or remotely piloted aerial vehicles – and argue that the rise of *democratized* or "garage-scale research" challenges what we will call *boundedness,* or the notion of a well-defined, clearly bounded scientific community. We show how this breakdown of the scientific community leads to conceptual and practical problems for Janet Kourany's recent work on ethics codes. In §4, we discuss control systems research as an example of what we will call *interconnectivity.* We show that this poses a problem for Heather Douglas' account of the responsibilities of scientists. We conclude in §5 by suggesting that things may not be so dire. The problems we identify are conceptual or philosophical, but also practical and institutional, and this means that there may be practical and institutional responses to the philosophical problems.

2. What Good is a Scientific Community?

Why has the notion of the scientific community been useful thus far to philosophers of science? We argue that this notion is implicated in most of our usual understandings of theory choice and scientific change. In the Postscript to *The Structure of Scientific Revolutions,* Thomas Kuhn recognizes that his central notion of "paradigm" is crucially connected to the notion of a scientific community:

> The term 'paradigm' enters the preceding pages early, and its manner of entry is intrinsically circular. A paradigm is what the members of a scientific community share, *and,* conversely, a scientific community consists of [people] who share a paradigm. (Kuhn, 2012, p. 175)

For Kuhn, then, the theoretical work of isolating a paradigm is coextensive with the sociological or empirical work of isolating a scientific community. Kuhn presents what we might call a "traditional" view of the nature of these communities: they consist of "the practitioners of a scientific specialty," who have "undergone similar educations and professional initiations; in the process they have absorbed the same technical literature and drawn many of the same lessons from it" (Kuhn, 2012, p. 176). Such an idea of the scientific community thus describes such communities in terms of patterns of training, institutional structures, university departments, and similar traditional markers. Similar analyses were offered by the likes of Ludwig Fleck (Fleck, 1979), Imre Lakatos (Lakatos, 1978), and Marjorie Grene (Grene, 1985). All of these authors treat scientific communities as reasonably well-delimited entities.

Near the end of the twentieth century, feminist philosophers of science developed analyses of concepts such as objectivity and evidence that explicitly refer to scientific communities. In an early contribution to what is now called social epistemology, Lynn Hankinson Nelson argued that individual knowledge claims depend on "community criteria, public notions of what constitutes evidence"; claims that do not satisfy the criteria of the relevant community are either rejected – do not constitute knowledge – or require "a change in our standards of evidence." In either case, "I *can* know only what *we* know, for some we" (Hankinson Nelson, 1990, p. 255). In her influential account of objectivity, Helen Longino proposes to treat it as a property of scientific communities rather than individual scientists. Such communities are objective insofar as they exemplify certain features that promote a critical exchange of views among (groups of) individual scientists (Longino, 1990, p. 76ff). Notably for our purposes, Longino brackets consideration of what constitutes a scientific community, but recognizes that its boundaries may be difficult to pin down: "The precise extension of 'scientific community' is here left unspecified. If it includes those interested in and affected by scientific inquiry, then it is much broader than the class of those professionally engaged in scientific research" (Longino, 1990, p. 69n10).

Contemporaneously, Joseph Rouse argued that the isolated instances of experiment, observation, or data collection of which the scientific process is made up only make sense – only "acquire their intelligibility and significance," in Rouse's words (1990, p. 181) – in the context of a narrative, a story that scientists tell each other about the field's past, present, and future. "The intelligibility, significance, and justification of scientific knowledge," he claims, "stem from their already belonging to continually reconstructed narrative contexts supplied by the ongoing social practices of scientific research" (Rouse, 1990, p. 181). Since these narratives are to be understood as the shared epistemic context of the entire scientific community, which creates and continually reconstructs them, analysis of that community becomes essential for any further analysis of the epistemic commitments of its members. Despite the similarity to Hankinson Nelson's view, in this particular piece Rouse engages primarily with the work of ethicist and philosopher of social science Alasdair MacIntyre and empirical researchers in the then-new Strong Programme for the Sociology of Knowledge. This indicates that, by the end of the twentieth century, science studies scholars from multiple fields were deploying the concept of the scientific community in a variety of ways.[2]

To expand our view beyond the question of scientific theory change, consider science, values, and policy, a burgeoning field in the last several decades. Recent work here makes no less use of the idea of the scientific community. A common refrain in this literature (especially when it is written by practitioners or targeted at governmental or regulatory contexts) appeals to the example of the Asilomar conference on recombinant DNA technology (Berg et al., 1975). Recognizing the potential ethical ramifications of the combination of genetic material from different organisms, the research community working on such techniques launched a voluntary moratorium on any further research in the area, and then came together to draft a set of recommendations for experiments falling into various categories of risk, along with procedures for the implementation of these recommendations. Work resumed the following year under structures of regulation and oversight largely drawn from the biologists' recommendations.

Asilomar has become something of a trope in the decades since, despite serious worries concerning its long-term effectiveness and the extent to which regulations were relaxed in its immediate aftermath [particularly as possibilities for commercial exploitation of these technologies were uncovered; see Rasmussen (2015)]. Calls for "another Asilomar" have been made across contemporary science and technology. Examples may be found in the field of

[2] While it is not our aim here to evaluate these different approaches to the concept of the scientific community in the extant literature, we note that in what follows, we have been largely inspired by Rouse's understanding of scientific communities as epistemic communities which share a common narrative context and demand constitutive mutual accountability (see below). We do not believe, however, that the importance of or response to the problems that we raise in this paper will be significantly different depending on the definition of "scientific community" that one adopts; the sense of "community" required for our argument to go through is a minimal one, instantiated by any such concept that hopes to unambiguously demarcate the extent of such communities.

synthetic biology, which promises a novel "engineering approach" to the construction and genetic modification of organisms (Ferber, 2004); in the National Science Advisory Board for Biosecurity's call for "another Asilomar-type moment" in response to the controversy surrounding human transmissibility of avian flu viruses (Berns et al., 2012; Nature Editorial Board, 2012, p. 154; Yen & Peiris, 2012); or genetic modification of human embryos (Vogel, 2015). An Asilomar-like conference (at Asilomar, no less) was even arranged by the Future of Life Institute, which produced a series of twenty-three principles for beneficial AI (Future of Life Institute, 2017).

One obviously necessary condition for any "Asilomar-type moment" is the ability to bring together a well-defined group of researchers who may plausibly be said to be the scientific community at issue – with the expectation that decisions made by this community will be enacted as community standards or norms governing the future work of these scientists.[3] Rouse has offered an account of such communities, with an emphasis on the way in which these kinds of community norms might relate to future behavior, and hence on exactly how Asilomar-type self-regulation could be expected to proceed (Rouse, 2002, 2006, 2007). A central feature of this account is what we will call *constitutive mutual accountability.* This has three components: (a) there are implicit or explicit rules for the behavior of members of the community *qua* members, (b) these rules include reflexive rules of accountability and enforcement, i.e., rules regulating when and how members of the community identify and punish other members who violate the rules, and (c) to be a member of the community is, in part, to be accountable to other members for one's behavior *qua* member, according to these rules, and to hold other members accountable for their behavior *qua* members. In short, the members are mutually accountable to one another *qua* members, and this mutually accountability constitutes both their individual membership and the existence of the community *as* a community.

The success, then, of an Asilomar-type event depends on having effective enforcement mechanisms in the scientific community – on having constitutive mutual accountability. Traditionally, enforcement mechanisms in the scientific community include the provision of such scarce resources as research

[3] We take for granted that scientists are indeed responsible for the potential moral harms arising from their work, and discuss here only how and for which particular harms they should in fact be held responsible; for an explicit argument for this claim, see Douglas (2003).

funds, graduate student research assistants, and high-status career positions. Further, in the last several decades, Institutional Review Boards (IRBs) and ethics training requirements have been introduced explicitly to enforce various ethical regulations. There is thus a clear chain of necessary conditions here: without mutual accountability, there is no hope for an Asilomar-type intervention. Without a community with reasonably clear rules for membership, there is no mutual accountability. The scientific community is thus a crucial actor in any such regulation of the practice of science.

Rouse's mutual accountability is not the only approach to ethics in scientific practice that makes important use of the idea of the scientific community. Another example can be found in Janet Kourany's ideal of socially responsible science (Kourany, 2010). In the final chapter of her book, Kourany offers a series of powerful criticisms of ethics codes promulgated by such scientific associations as the American Chemical Society and American Physical Society: these codes leave such key terms as fabrication, plagiarism, and professional development undefined; place only weak or insubstantial responsibilities on scientists, such as "chemists should understand and anticipate the environmental consequences of their work"; fail to recognize conflicts of interest within their own guidelines; omit responsibilities to society, future generations, and the environment, and to promote diversity within science; inadequately assess interventions and other activities; and are either unenforceable or lack mechanisms of enforcement (Kourany, 2010, pp. 110–113). This last point is especially important, since without enforcement even blatant violations of the existing codes continue (Kourany, 2010, p. 114).

Kourany goes on to argue that the scientific community should regulate itself with "clear, accessible, well-publicized ethics codes with clear modes of enforcement" (Kourany, 2010, p. 117), before public outrage prompts government regulation:

> [I]f the scientific community fails to regulate its own activities, other bodies will....
>
> Adequate ethics codes...would be constructed by scientists and enforced by scientists, and they could be revised...when conditions require it or new knowledge enables it. Adequate ethics codes, in short, would represent scientists regulating themselves....
>
> In addition, adequate ethics codes self-imposed by the scientific community and enforced by the scientific community would inspire public trust in science

> and, in fact, would earn that trust; by contrast, the need for continuing – indeed, increasing – regulation of science by external authorities can have no other effect than the erosion of public trust in science. (Kourany, 2010, p. 117)

Kourany's emphasis on the role of the scientific community is thus quite direct. If scientists want to engage in profitable self-regulation, and thus avoid the imposition of draconian regulation from outside science, the most profitable locus at which to do so, she argues, are the codes of ethics promulgated by scientific community organizations themselves.[4]

While examples of this sort could be proliferated *ad nauseam,* to close this section we would like to gesture at a handful of further cases where the concept of the scientific community seems to play an ineliminable role. Work in the sociology of science on knowledge transmission and the global structure of science, especially discussion of the "center-periphery" model of the dissemination of science, are explicitly framed in terms of the structure of global scientific communities (Gizycki, 1973; Schott, 1991). Questions about the ways in which theory change in fact happens over time sometimes link such change with the changing character of the scientific community – for instance, "Planck's Principle," the idea that younger members of a community will be the easier converts to a novel scientific theory, and hence a significant part of scientific change is simply waiting for the death of one's opponents (Hull et al., 1978).

3. Democratization: The Drone in My Garage

If the arguments of the last section have succeeded, then we have demonstrated that a significant quantity of work across a variety of areas of philosophy of science, both historical and contemporary – though we have focused in particular on the values in science literature, for reasons that will soon become clear – make crucial reference to a concept of the scientific community. In this section and the next, we now want to present two case studies drawn from contemporary work on robotics and intelligent systems that, we think, make serious

[4] Notably, a number of universities have also turned toward these sorts of explicit codes of ethics in the context of research financing and industry relations (Hillerbrand & Werker, 2019, p. 1637), and even citizen-science organizations such as DIYbio have expended extensive effort developing and publicizing codes of conduct (Eggleson, 2014).

trouble for at least any simplistic notion of the scientific community. First, consider the extent to which scientific research is *democratized*.[5]

In an article considering the expansion of the use of drones in domestic law enforcement for *Time* magazine, Lev Grossman begins rather poetically – describing his own, household drone:

> A few months ago I borrowed a drone from a company called Parrot. […] The Parrot went on sale last May and retails for about $300. It's a quadcopter, meaning it's a miniature helicopter with four rotors; basically it looks like a giant four-leaf clover designed by Darth Vader. It's noisy and a bit fussy, … [b]ut when it's on its best behavior, the Parrot is a little marvel. You control it with an app on your smart phone, to which it feeds real-time video in return. (Grossman, 2013)

Lest we get swept away by the technological novelty, make no mistake: "the Parrot is recognizably genetically related to some very efficient killers" (Grossman, 2013) – the Predator, Reaper, and other drones in use over Afghanistan, Pakistan, Yemen, and other theaters in the United States' "war on terror" (Alston, 2010). The widespread use of these technologies is by now unsurprising – drones are now a familiar feature of our local parks and every television or movie recording set worldwide.

Perhaps more problematically, ethically significant military or terrorist uses of these technologies are also widespread.[6] Drones place technology that was once the exclusive province of the military-industrial complex of large nations into the hands of everyday civilians. One of the first high-profile cases of a (failed) terrorist use of such an aerial vehicle occurred in 2011, when Rezwan Ferdaus was arrested outside Boston after having scouted locations in Washington, DC, from which to launch explosive-laden toy aircraft at the Pentagon

[5] In choosing the term "democratization," we mean only to imply widespread access to these technologies with low barriers to entry, not a normative claim that such access is intrinsically good (e.g., as discussed by Simons, 2021, pp. 171–172).

[6] This concern obviously brings us close to the problem of *dual-use* research – research that could simultaneously be used to benefit or harm humanity (Ehni, 2008; Evans, 2014; S. Miller, 2013, 2018; S. Miller & Selgelid, 2007) – and the moral responsibilities of scientists faced with such research. The two case studies that we will discuss are indeed drawn from the dual-use literature, though we do not have the space here to consider the definition and impact of dual-use in general. This also means we will not take a position here on whether dual-use is a valuable frame in the first place, or rather "distorts the debate about bioterrorism and truncates discussion of the moral issues" (Buchanan & Kelley, 2013, p. 195).

and US Capitol Building (Goodnough, 2011). The first successful use of such technologies in a terrorist context occurred in 2013, when Hezbollah delivered explosives via drone against Syrian targets; further uses in 2015 and 2017 by ISIS and an effort to assassinate Venezuelan president Nicolás Maduro using two modified DJI camera drones in 2018 are also notable (Braun, 2020; New America, 2020). And it is not only actions by "rogue" or "terrorist" groups that might (or should) concern scientists: one must also evaluate the open use of such technologies by democratic governments to determine whether or not those uses, controlled though they may be by democratic processes, in fact merit changes in the scientific process.[7]

For scientists working in drone research (and philosophers of science interested in understanding them), these incidents raise an obvious and pressing question. How might we intervene in order to preserve ethical uses of drone technology – of which there are, of course, many! – while preventing, to the extent that we are able, (mis)use of such robotic systems for unethical purposes? As we have discussed in the preceding section, there is an extensive discussion of precisely this question in the literature on values in science. But this literature, in turn, has relied in many cases on a concept of the "scientific community" as a well-defined entity that can be the target of an intervention designed to control unwanted uses of technology, the adopter of an effective code of ethics, or the arbiter of punitive action designed to enforce community standards.

It is here that we believe this case study demonstrates the first major problem for such notions of the scientific community. Widespread possibilities for *democratized* technology pose a threat to what we will call the *boundedness* of the scientific community, potentially rendering these enforcement mechanisms ineffective.[8] In these cases, it becomes difficult to draw clear boundaries

[7] For the uses of such technologies in practice, see, e.g., International Human Rights and Conflict Resolution Clinic and Global Justice Clinic (2012); for more on the question of the relationship between democratic decision-making and science, see Kitcher (2001, 2006). We are inspired in this point by the distinction between "dual use problem 1" and "dual use problem 2" drawn by Buchanan & Kelley (2013), p. 196.
[8] Democratization is particularly visible in the context of drone research, but noteworthy trends toward democratization can be found elsewhere, even in more traditionally "laboratory" driven fields like molecular biology (see Ledford, 2010; Simons, 2021; Talbot, 2020). One frequently raised point concerning the ethics of CRISPR-Cas9 gene editing is precisely that its low cost and ease-of-use make controlling it difficult (what Mariscal & Petropaganos, 2016 call the "power

around a given scientific community – to determine who counts as a member of that community and what kinds of research should be taken to constitute it.[9] As we will see, this failure of boundedness causes acute problems for several of the ways in which we saw scientific communities being put to philosophical use in the last section.

Consider, for instance, the ways in which the agreed-upon standards following from an Asilomar-type event might be promulgated in the scientific community. As we discussed above, many of the enforcement mechanisms that arise in this context involve the distribution of scarce resources such as grants, jobs, and high-profile journal publications. A democratized scientific community in which boundedness fails tends to lose the need for such scarce resources. Development of novel applications of drone technology now requires, at most, a small business loan and some bright undergraduates, not an extensive research and development program. This enables such researchers to operate beyond the purview of IRBs and other enforcement bodies.[10]

problem”). Versions of democratization can even be envisaged for the manufacture of chemical weapons (S. Miller, 2018, p. 68).

[9] While we lack the space to explore the question here, it is notable as well that there might be a sort of feedback process between communities and technologies in this sense. The presence of a DIY-drone community online, or even a community constituted by the users of a particular off-the-shelf technology, could be precisely the thing that *makes* drone technology democratically accessible. Such communities will also, we think, echo the problems we will discuss here of diffuse boundaries and lack of incentive structure. Thanks to Massimiliano Simons for raising this idea.

[10] One might want to distinguish between researchers and users of a technology, with distinct sets of norms for the two sets of actors. Why think it a problem if military users of drones in warfare don’t comply with the rules of human subjects research? But “democratization” means that this traditional distinction between researchers and users can easily break down in practice. Consider a group of hobbyists at a makerspace who modify commercial drones in ways that extend their flight time, allow them to coordinate autonomously in a swarm, allow them to track individuals as they enter and travel in vehicles, or the like. Then these hobbyists test these new capabilities by deploying the drones in public areas (that do not have drone restrictions). Should these hobbyists be considered users or researchers? Their activities seem to be going beyond “mere application,” from use into research. But insofar as they are considered “researchers,” how should IEEE identify and enforce an ethics code on their research? And why think that IEEE has the authority to do so? If not IEEE, then who would have authority, and which norms apply? Our point is that these questions do not have clear answers because of the way “non-researchers” can engage in drone research. We thank an anonymous reviewer for raising this objection.

The failure of boundedness also threatens Rouse's notion of constitutive mutual accountability in a more fundamental way. Insofar as the relevant enforcement mechanisms break down, individual scientists are not *de facto* accountable to other scientists, whether or not they are accountable *de jure*. Then, by Rouse's condition (c) above (that being a member of a scientific community just is, in part, being accountable to its other members), these individuals are not *de facto* members of the scientific community. Taken to the extreme, a scientific community in which boundedness has entirely broken down has no members, and so does not exist. The enforcement problem created by democratization is, at the same time, an ontological problem for the scientific community.

From Kourany's perspective, if we hope to find a place where institutional codes of ethics might produce constructive intervention in drone research, the Institute of Electrical and Electronics Engineers (IEEE) Robotics and Automation Society would seem to be the place to start looking. Indeed, they have a "Technical Committee for Robot Ethics" that has published a number of articles and features a number of philosophers – but includes no concrete code of conduct for IEEE members (beyond the code of conduct that applies to the society as a whole, see Institute of Electrical and Electronics Engineers, 2021). However, even if the IEEE Robotics and Automation Society did have a more substantive code of ethics addressing drone research, it would not seem to be enforceable in a democratized context. The society is a professional organization, not an accreditation or licensing body; membership is voluntary, and the primary benefits of membership are access to society publications, conferences, and social events run by local chapters. Any attempt to censure unethical "rogue" drone research would, we expect, simply prompt the researcher to leave the organization. It is simply too easy for the rogue engineer to continue their work "in their garage" and outside of the scientific community.

Kourany might respond that mutual accountability is often managed implicitly or informally. For example, a lab group might have a rule against extended conversations in the main lab space when other people are trying to work there. This rule need not be explicit for the members of the group to enforce it. People trying to work might put in earplugs or put on headphones in an exaggerated way; violators might receive dirty looks or be asked (more or less directly) to take their conversation out into the hallway; and persistent violators might be punished in indirect ways, such as by being assigned desks in undesirable locations outside of the main lab space.

On a larger scale, ethics codes might also be enforced informally and indirectly. Single violations might mean that one's paper is not accepted in the professional society's journal or annual conference; persistent violations might mean that one's work is not taken up by one's colleagues, one has a higher burden of proof in peer review situations, and so on.

However, we believe that, on the larger scale, even informal and indirect enforcement mechanisms require a certain degree of explicit, formal, or institutionalized enforcement (and vice versa). For instance, if an individual journal editor or granting agency program manager tried to adopt a higher burden of proof for persistent violators, we expect that many of her colleagues would be outraged. This mechanism would only be viable if either (a) the mechanism were kept secret or (b) the board of directors (or analogous governing body) wrote the mechanism explicitly into the journal or agency's peer review guidelines. Option (a) is obviously problematic in numerous ways; option (b) makes the mechanism formal or explicit.

In addition, the problem of an unbounded community can reappear at the institutional level. In the US, the National Robotics Initiative [NRI] has been a major source of federal funding for robotics research (Hicks & Simmons, 2019). Multiple agencies of the US federal government participate in NRI, and so it might seem to be an ideal agency to enforce a code of ethics for robotics research. Because NRI is primarily administered by the US National Science Foundation, several of its program managers and many of its peer reviewers have been active academic researchers; NRI thereby seems to combine both the external coercive power of government regulation and the sense of community self-enforcement. However, not all US agencies that fund robotics research participate in NRI. While the Office of Naval Research in the US Department of Defense [DOD] participated in the past, DOD is no longer listed as a participating agency in the most recent NRI solicitation (National Science Foundation, 2021). So a code of ethics enforced by NRI might not apply to DOD-funded research.

On Rouse's account, the normative authority or force of the rules – more broadly, of the system of mutual accountability among members of the community – comes from within the system of mutual accountability itself: these rules govern our behavior because they were established by the process that the rules have laid out for establishing our rules (Rouse, 2007, p. 48).

This is circular, but not viciously so, as long as the system is self-correcting over time (where self-correction is, again, a reflexive feature of the community, Rouse, 2007, p. 53). There is a similar non-vicious-because-self-correcting circularity in the history of science: these observations are trustworthy because our best theories tell us they were made in the correct way, and our best theories were established by trustworthy observations. Rouse emphasizes that this account of normative authority is naturalistic, and we add that it is democratic in a sense: these rules have normative authority over us, not because they have been handed down by some higher authority (God, pure practical reason, or the state), but because we collectively recognize them as having normative authority over us (Rouse, 2006, p. 504).

But the breakdown of constitutive mutual accountability – as, we claim, happens when boundedness fails as a result of democratization – takes us out of this circularity, and it is difficult to see how we are supposed to get back in. Suppose the IEEE Robotics and Automation Society or NRI develops a substantive ethics code for drone research in line with Kourany's recommendations.[11] The committee may be able to claim normative authority over members of the society or recipients of funding, insofar as to be a member of the society or accept such money is to agree to have one's behavior regulated by the society's rules (and likewise for the funding program and the researchers it supports). But it's hard to see how they could claim such authority over individuals who choose to leave the society or seek funding elsewhere because their drone research would violate the new ethics code.

Note that this is not the problem of enforcement discussed above. Enforcement is a practical problem: in what concrete ways shall we punish people who violate these rules? The problem here is the prior or more abstract problem of normative authority: roughly, why is anyone *morally obligated* to obey these rules, and why is it *morally permissible* to punish violators?

4. Interconnectivity: Control Systems Research

Researchers are, and ought to be, held responsible for the consequences of their research – practically, professionally, as well as morally. But, insofar as ought implies can, this entails that individual researchers are able to make

[11] Proposals for such a code in the field of robotics more broadly have been presented by, for instance, Riek & Howard (2014) and, as already discussed, Future of Life Institute (2017).

themselves sufficiently aware of the future practical, professional, and moral impacts of their laboratory work. In this section, we make trouble for this idea in the context of contemporary scientific research, by pointing to another feature of the scientific community – its *interconnectivity* – which problematizes the extent to which any individual researcher could have epistemic access to the future results of her work, even those results for which she might well be held accountable.[12]

Consider research into control systems. Control systems are ubiquitous – any time that one finds a set of "subsystems and processes assembled for the purpose of obtaining a desired output with desired performance, given a specified input," a control system may well be at work (Nise, 2011, p. 2). Everything from elevators and hot-water heaters to airplanes, rockets, and robots are governed by control systems, which ensure that their output states remain within desired parameters in response to inputs and disturbance from the external environment.

A researcher working on cutting-edge problems in control systems research would generally work with some kind of model system – whether a physical device or a computer model. This model system probably does not in itself result in any ethical consequences of note; it likely would not be able to function outside its limited laboratory context. Some nearly-immediate applications would, of course, be reasonably obvious. Were our researcher working on how to process input from gyroscopic stabilizers, she would know that her work had ready application to the aerospace realm. However, once published, this work can be used by anyone with access to the publication. Such research could thus be used for anything from hobbyist remote-controlled helicopters, to the next generation of NASA launch craft, to the newest iteration of the Predator or Reaper drone. In the latest version of the United Kingdom's autonomous-drone test system, Taranis, expected to enter service around 2030, one of the

[12] As was the case with democratization, a number of other fields have exhibited pronounced trends toward interconnectivity. To mention only a few, recent defenses of the importance of sciences as diverse as natural history (Greene, 2005; Tewksbury et al., 2014) and systems biology (Calvert & Fujimura, 2011; Eddy, 2005; O'Malley & Soyer, 2012) have appealed to the many and often unexpected ways in which their fields are connected to a host of neighboring disciplines, and have consequences which regularly spill over from purely scientific concerns into society more broadly. More broadly, Sabina Leonelli has highlighted a host of such connections that arise across the contemporary use of "big data" in the life sciences (Leonelli et al., 2013; Leonelli, 2016).

Ministry of Defence's explicit goals is "the successful integration of off-the-shelf technologies, including automation, command and control, sensor integration, and payload integration" (Larrinaga, 2013) – that is, the defense industry is actively working to integrate more technologies developed by third parties and published publicly. In this kind of case, it is much more difficult for the researcher to anticipate how her work will be used, even just a short distance downstream.

Control systems, therefore, demonstrate what we will call the extreme *interconnectivity* of contemporary scientific research.[13] In an environment where technological innovation often takes the form of the combination of a variety of "off-the-shelf" scientific and technical components deriving from a whole host of *a priori* unrelated domains of study, it becomes genuinely difficult to determine the downstream consequences of a particular piece of research. This forms half of the classic "Collingridge dilemma" for the regulation of technology – our relatively poor performance at, as Lautenschläger puts it, "correlating research and development with results" (Lautenschläger, 1985, p. 699). If the interconnectivity of science rises to such a degree that these correlations become practically impossible to draw, we argue that we find a new kind of threat to the scientific community – the *discreteness* of such communities is called into question, as there are too many avenues of scientific research with too many interacting downstream consequences for us to be able to readily separate their influence and impact. Control systems research, for instance, creates an environment where it becomes difficult to know whether a particular piece of technology "belongs" to the study of control systems, robotics, artificial intelligence, aerospace engineering, mechanical engineering, and so forth. The complex web of interconnections between such domains makes it increasingly difficult to conceive of such research as cleanly composed of discrete sub-communities.

[13] Interconnectivity, in our sense, is different from interdisciplinary collaboration. Collaboration is "synchronic," with team members of different disciplinary backgrounds working together on the same project. Interconnectivity, by contrast, is "diachronic," with research outputs from field A being taken up and utilized for a different project in field B. (Or, indeed, research outputs from fields $A_1, \ldots, A_n$ taken up and utilized by field B.) Interdisciplinary collaboration is also interactive within a single project, with ongoing exchanges across disciplinary boundaries (although see Brister, 2016), while interconnectivity is often a unidirectional transmission of information from A to B, at least within the scope of a single project. We thank an anonymous reviewer for encouraging us to clarify this contrast with interdisciplinarity.

We are not the only authors to have noticed this feature of contemporary science, which is now increasingly ubiquitous. As Justine Johnstone has written in the context of the ethics of information technologies:

> Even where agents can be individuated, features of systems such as non-linearity, opaqueness, positive feedback loops and complexity mean that agents are frequently unable to predict the outcomes of actions, assess the potential for unintended negative consequences, or even clearly distinguish causes and effects. (Johnstone, 2007, p. 74)

Among other mechanisms, interconnectivity frustrates the anticipation of downstream research by facilitating novel applications of research programs, and especially novel combinations of emerging technologies. Control-systems research, for example, might be combined with work on automotive pathfinding, range detection, and navigation to produce automated vehicles (Fountain, 2012), or with innovations in stable quad-rotor helicopter production and mobile web applications to create a helicopter-based taco delivery system (TacoCopter, Inc., 2012). Both of these applications combine recent developments in, to take only a few examples, control systems, mobile internet, and precision mapping technologies. While many researchers in each of these three areas may have been able to anticipate the developments in their specific field that are applied in these examples, and several may have been able to anticipate that technologies in these areas would be combined, we suggest that almost no individuals would have been able to anticipate these specific technologies a few decades ago, when the groundwork for such developments was initially laid.

Novel combinations are not a new product of interconnectivity. Instead, we suggest that interconnectivity facilitates novelty. It is of course the case that no technological innovation starts "from scratch" – engineers always build upon the resources available to them at the time. But interconnectivity, by giving engineers a wide array of stable, readily available, off-the-shelf technologies that are very broadly applicable (such as internet access, GPS, freely available maps and other forms of public data, 3D printing, general-purpose microcontrollers, and a wide array of open-source software products), allows the initial phases of product development, from concept to initial prototype, to proceed extremely rapidly, scaffolding on these other freely available technological developments.

Heather Douglas has argued that scientists are morally responsible for the reasonably foreseeable consequences of their research (Douglas, 2009, ch. 4; Douglas, 2014). That is, scientists have moral responsibilities to anticipate consequences of their research (avoiding negligence) and to properly weigh the benefits and harms of those consequences (avoiding recklessness). Both negligence and recklessness are indexed to the community of scientists, e.g., non-negligence requires anticipating consequences that could have been foreseen by the community of scientists.[14]

In the previous section we challenged the assumption that there is a community of scientists. Here we consider an argument that the failure of discreteness renders impossible anything more than short-term foresight. As the case of control systems research shows, it will often not be possible for scientists to carry out this evaluation of the downstream consequences of their research, because they are not in a position even to be aware of the downstream connections of that research. Put briefly, where should such a researcher look in order to find those consequences? A traditional rule of thumb – something like, within one's own community and perhaps a few neighboring communities with which one has well-defined connections – seems to break down. As discreteness begins to fail, the range over which a potential researcher must look becomes prohibitively large.[15] The control-systems researcher from the case study above cannot know whether her work will be used to stabilize a rocket to Mars, a missile over Pakistan, or the TacoCopter. As noted above, she may have some idea about immediate applications or domains of interest, as well as some information from her laboratory group's own testing. But after the research is published or sold, predicting the work's future trajectory becomes nearly impossible. Indeed, she can reasonably expect that, after her research is completed, unanticipated and even unanticipatable field applications of the code will be developed. Thus it seems to be impossible for her to satisfy her responsibilities to avoid negligence and recklessness.

[14] Such accounts become more complex – though still ascribe significant moral responsibility to the relevant scientists at issue – if we take more seriously the fact that scientific research is a collective action, and hence the moral responsibility at issue is also in an important sense collective (see S. Miller, 2013, pp. 199–202; S. Miller, 2018, ch. 4). Such collective responsibility also exacerbates the reliance on concepts of the scientific community, for it is important to know who forms the relevant collective.

[15] Notably, part of what makes this foresight difficult is the fact that, according to all of the conceptions of scientific community discussed in the introduction, scientific communities are (or at least are nearly) mono-disciplinary entities. Looking beyond one's community thus demands difficult interdisciplinary translation work.

One response to this argument would concede that only the obvious, immediate applications of a given research program are reasonably foreseeable; maintain that her responsibilities to avoid negligence and recklessness apply only to the reasonably foreseeable downstream consequences; and conclude that she is responsible only for the obvious, immediate applications.

We find this response far too undemanding, for two reasons. First, a simple-minded reading implies that the scientist can satisfy her responsibilities simply by limiting her ability to exercise foresight. Any number of skeptical arguments could be used to claim that no one can *really know* the consequences of some new technology; thus all the downstream consequences are unforeseeable and so she is not responsible for any of them. A similar version of such skepticism has been taken up by Bruno Latour, who has argued that, faced with the threat that the problem of foresight will be read instead "as proof that no action was possible any more" (Latour, 2011, p. 25) because the task of weighing consequences has become impossible, we should respond instead by reinforcing the responsibility of scientists "to care for *our own* creations," to ensure that "unexpected consequences are *attached* to their initiators and have to be followed through all the way" (Latour, 2011, pp. 20, 25).[16]

Second, and less trivially, in numerous historical cases scientists have been held responsible (albeit not criminally liable) for failing to properly manage uncertainty about the consequences of their work. Consider the public controversy surrounding chemical pesticides after the publication of Rachel Carson's *Silent Spring* and the contemporary controversy over genetically modified foods.[17] In both cases, a major concern is not so much that scientists have failed to exercise reasonable foresight, but rather that they have failed to acknowledge the limitations of reasonable foresight. That is, in both cases, while there have been some efforts to exercise foresight regarding certain harmful downstream consequences – using animal models to test for carcinogenicity, for example – there has been relatively little attention to the possibility of harmful consequences that could not be, or even simply were not, foreseen by anyone. While it seems too strong to hold scientists responsible for all of the downstream consequences of their work – no matter how far into the fu-

16 Thanks to Massimiliano Simons for pointing us toward this source.
17 Civilians can even, in some circumstances, be charged with war crimes, as exemplified by the famous "Zyklon B" case; see United Nations War Crimes Commission (1947).

ture – it seems too weak to hold them responsible *only* for the foreseeable consequences.

It therefore seems likely that researchers working in domains where interconnectivity is high enough to challenge the discreteness of scientific communities will not be able to avoid the problem of foresight as we have described it here. Such interconnectivity makes it incredibly unlikely that a researcher will be able to reasonably foresee the consequences of her work, including consequences for which she may well be responsible.

5. Conclusion

We have, we recognize, been primarily critical in our work here. We have described two ways in which the contemporary landscape of scientific research makes trouble for the notion of the scientific community in the philosophy of science – including research in the context of the science and values debate that has endeavored to be sensitive to precisely these complexities. The increasingly common features of *democratization* and *interconnectivity* have contributed, we argue, to the failure of *boundedness* and *discreteness* in scientific communitites. To be sure, some of the questions we have raised wait on further empirical research, which we keenly recognize. Just how seriously should we take the possibility of democratization in various scientific fields? What are the best ways to hold scientists accountable? How can "garage-scale" researchers best be persuaded to see themselves as members of the broader scientific community? We hope that sociological studies of science can help shed light on these concerns (see, for example, Cech, 2014; Shilton, 2013).

But to conclude on a more constructive note, what can be done by professional scientists to deal with the ethical and practical issues in play here, and how can philosophers of science contribute? As Miller and Selgelid note, creativity is required – any analysis of the kind of ethical dilemmas raised here must take into account the fact that "the dilemma must, if possible, be resolved by *designing* a new third or fourth option, i.e., by bypassing the dilemma" (S. Miller & Selgelid, 2007, p. 543). Our arguments above indicate that the breakdown of constitutive mutual accountability among scientists *qua* scientists amounts to the breakdown of the scientific community. This breakdown is driven by technological developments that make it possible for researchers and technologists to evade the established institutions of accountability. This suggests that responding to the problem requires reforming existing institutions and establishing new ones, and thereby maintaining the existence of the scientific community.

We draw on a model for legitimizing values in fields such as environmental public health, sketched by Fernández Pinto & Hicks (2019). They conceptualize legitimacy in terms of relationships of mutual accountability between scientists, policymakers, and publics, and argue that institutions for participatory governance, adaptive management, and participatory research can be effective for strengthening these relationships. As we have argued here, failures of boundedness and discreteness mean that we can't draw the sharp boundaries around scientific communities that Rouse's relationships of constitutive mutual accountability require. But the model of Fernández Pinto & Hicks (2019) suggests that such relationships can extend beyond the boundaries of the scientific community. That is, scientists can be held accountable for their actions as scientists – and thus constituted as scientists, according to Rouse's account – *not just by other scientists, but also by policymakers and publics*. In the remainder of this paper, we apply these ideas to the two problems of democratization and interconnectivity.

Consider first democratization. The Stop LAPD Spying Coalition is a collection of antiracist, police abolitionist, and homeless advocacy organizations that work together to criticize the militarization of the Los Angeles Police Department, especially in its use of surveillance technologies such as "predictive policing" and drones. In their "Drone Report" (Stop LAPD Spying, 2015), Stop LAPD Spying draws connections between the military use of drones in the US war in Afghanistan as surveillance and weapon platforms, on the one hand, and LAPD's longstanding use of novel surveillance technologies, on the other. The report argues that drones, as police surveillance technology, are subject to "mission creep": "While the LAPD claims to only want drones for limited situations, evidence of domestic drone usage in the U.S. shows a much different trend – one which parallels the habit police forces have of expanding the use of novel techniques and technologies beyond their original stated intent" (Stop LAPD Spying, 2015, p. 10). The report draws a parallel to Special Weapons and Tactics (SWAT) teams. While originally "authorities claimed SWAT teams would only occur [sic] in high risk situations such as confronting barricaded/armed suspects and active shooter scenarios," in fact "a recent report by the ACLU found that 79% of SWAT deployments were for executing search warrants. Sixty-two percent were for drug searches and 65% of SWAT raid targets were unarmed" (Stop LAPD Spying, 2015, p. 12).

This critique of LAPD's use of drones was remarkably prophetic. Four years after its publication, in August 2019, LAPD adopted a policy that drones would be regularly used by the department, but only "in specific situations, including active shooters, barricaded suspects and search warrants." The drone policy explicitly states that the drones would not be "equipped" with weapon systems or "facial recognition software or analysis capabilities" (Moore, 2019). However, in October 2020, the LAPD Police Commission approved a policy that would allow the department to record and store "aerial footage of protests and other large gatherings from its helicopters," apparently including drones (Rector, 2020).

Stop LAPD Spying has been sharply critical of PredPol, a predictive policing technology developed by a partnership between LAPD and Jeffrey Brantingham, an anthropologist at UCLA. For our purposes, there are a number of notable features of Stop LAPD Spying's campaign to end the use of PredPol. First, Brantingham is an academic, and so his work developing PredPol might not seem to fit our notion of "democratization." But PredPol certainly exhibits breakdowns of both boundedness and discreteness: an anthropologist who originally specialized in human habitation in Asia in the Paleolithic (e.g., Brantingham et al., 2001) adapting mathematical methods that were designed to predict earthquake aftershocks (Mohler et al., 2011) in order to develop "crime" prediction software for use by police departments. Second, Stop LAPD Spying draws on and extends several academic critiques of predictive policing in general and Brantingham's work in particular, including critiques of reductionism in social science, the "feedback loop" between police-generated data and overpolicing, and the social construction of crime (Stop LAPD Spying, 2018). Stop LAPD Spying has also developed their own critical framework, "The Algorithmic Ecology," which views PredPol (or other technology) in the social context of its development and use. As the authors put it, PredPol is "designed to *operationalize* the *ideologies* of the *institutions* of power to produce intended *community impact*," with significant elaboration for each emphasized term (Stop LAPD Spying, 2020). These sophisticated critiques hold Brantingham accountable *as a scientist* for his role in the broader network of actors that support and benefit from predictive policing. The fact that he operates (in part) outside of the traditional scientific community does not render him beyond scientific accountability. Indeed, Stop LAPD Spying can hold Brantingham accountable as a scientist *because* he's not an insulated, ivory tower academic pursuing research with no social consequences. Third and finally, Stop LAPD Spying's campaign against PredPol shows that such campaigns to hold scientists accountable can be successful: after defending the technology for years, LAPD abandoned the use of PredPol in April 2020 (L. Miller, 2020).

Next, the foresight problem. Within scientific communities, reforming practices of peer review could improve foresight and provide institutions tools for deliberating about the ethical uses of technologies. For example, the US National Science Foundation currently uses peer review panels to evaluate the Intellectual Merit and Broader Impacts of research proposals (Holbrook, 2005); an ethical evaluation of the downstream consequences might be included in the Broader Impacts criterion or as a third merit criterion. Or, at present, the US National Research Council uses peer committees to produce documents for policymakers that describe the state of scientific knowledge about a wide variety of policy-related issues. These committees could also produce discussions of the future trajectory of current research and offer advice about which paths should and should not be pursued.

Participatory technology assessment [pTA] provides an important model for participatory governance in science policy (Kaplan et al., 2021). pTAs are highly structured, deliberative activities, designed to develop and solicit the opinions and concerns of members of the general public. Participants may be screened for *low* levels of prior knowledge on a topic, in order to promote views that are often marginalized in the conventional policymaking process (Hicks, 2017; Steel et al., 2020; Tomblin et al., 2015, p. 7), then provided background reading materials and access to both credentialed experts and policymakers during the deliberative activity. While organizers work with stakeholders to develop focal questions for deliberation, the pTA structure typically includes some flexibility that allows participants to refocus and reframe to their own concerns (Weller et al., 2021). In terms of foresight, pTA embodies the longstanding insight from science and technology studies that publics are likely to identify negative impacts of a technology or policy that have been overlooked by credentialed experts (Epstein, 1996; Wynne, 1989).

Even if these kinds of measures successfully improve the foresight of the scientific community, we expect that foresight will still be limited, and again, we believe that scientists' responsibilities extend beyond the boundaries of reasonable foresight. Sandra Mitchell has argued that the complexity of biological and social systems imposes serious limitations on our ability to control and predict their behavior (2009). Thus, "once-and-for-all" policy decisions prevent us from responding appropriately to the development of scientific knowledge and unforeseeable consequences of our policies. Instead, policy should be based on "adaptive management schemes, which require monitoring, updating, and revision of actions on an ongoing basis" (Mitchell, 2009, p. 17). She discusses at length the importance of integrating ongoing scientific research into the processes of making and *remaking* public policy (see also Norton, 2005).

We suggest that adaptive management is essential for managing the limitations of foresight beyond ecology and environmental policy, and that the integration of scientists into adaptive management schemes is one important way for the scientific community to exercise its responsibilities with respect to unforeseeable consequences. Adaptive management for autonomous vehicles, for example, will require the ongoing contributions of control systems researchers, along with other experts in the technical capabilities and limitations of such systems. Thus, these researchers cannot wash their hands of the unforeseeable consequences of their research as a problem for politicians, regulators, and social scientists.

Douglas has emphasized that some of scientists' responsibilities may be better satisfied collectively than individually (Douglas, 2014, especially pp. 19ff). Thinking in terms of community reform emphasizes social institutions and practices rather than individual bad actors. And Douglas is keenly interested in moving forward with institutional changes rather than retrospectively identifying blameworthy individuals. We agree, but extend this thought. Where Douglas moves from individuals to communities of scientists, we move further to networks of communities, a rich ecology that includes scientific communities as well as government agencies and public groups. But we also caution that the mere existence of many different communities is insufficient to address the challenges raised by democratization and interconnectivity. Stop LAPD Spying's analysis of the "algorithmic ecology" surrounding predictive policing (Stop LAPD Spying, 2020) shows that certain configurations of communities and institutions can be vicious (in the ethical sense) by reinforcing unjust power hierarchies and marginalizing the communities that are most harmed by overpolicing. More virtuous configurations will likely require deliberate engineering, as in the way pTAs select participants who are likely to be excluded from traditional policymaking (Weller et al., 2021).

REFERENCES

Alston, P. (2010). *Report of the special rapporteur on extrajudicial, summary or arbitrary executions, addendum: Study on targeted killings*. Human Rights Council, United Nations General Assembly.

Berg, P., Baltimore, D., Brenner, S., III, R. O. R., & Singer, M. F. (1975). Summary statement of the Asilomar conference on recombinant DNA molecules. *Proceedings of the National Academy of Sciences*, *72*(6), 1981–1984.

Berns, K. I., Casadevall, A., Cohen, M. L., Ehrlich, S. A., Enquist, L. W., Fitch, J. P., Franz, D. R., Fraser-Liggett, C. M., Grant, C. M., Imperiale, M. J., Kanabrocki, P. S., Joseph ' Keim, Lemon, S. M., Levy, S. B., Lumpkin, J. R., Miller, J. F., Murch, R., Nance, M. E., Osterholm, M. T., Relman, D. A., … Vidaver, A. K. (2012). Policy: Adaptations of avian flu virus are a cause for concern. *Nature*, *482*(7384), 153154. https://doi.org/10.1038/482153a

Brantingham, P. Jeffrey., Krivoshapkin, Andrei I., Jinzeng, L., & Tserendagva, Ya. (2001). The Initial Upper Paleolithic in Northeast Asia. *Current Anthropology*, *42*(5), 735–747. https://doi.org/10.1086/323817

Braun, T. (2020). Miniature menace: The threat of weaponized drone use by violent non-state actors. *Wild Blue Yonder*. https://www.airuniversity.af.edu/Wild-Blue-Yonder/Article-Display/Article/2344151/miniature-menace-the-threat-of-weaponized-drone-use-by-violent-non-state-actors/

Brister, E. (2016). Disciplinary capture and epistemological obstacles to interdisciplinary research: Lessons from central African conservation disputes. *Studies in History and Philosophy of Biological and Biomedical Sciences*, *56*, 82–91. https://doi.org/10.1016/j.shpsc.2015.11.001

Buchanan, A., & Kelley, M. C. (2013). Biodefence and the production of knowledge: Rethinking the problem. *Journal of Medical Ethics*, *39*(4), 195–204. https://doi.org/10.1136/medethics-2011-100387

Calvert, J., & Fujimura, J. H. (2011). Calculating life? Duelling discourses in interdisciplinary systems biology. *Studies in History and Philosophy of Biological and Biomedical Sciences*, *42*(2), 155–163. https://doi.org/10.1016/j.shpsc.2010.11.022

Cartwright, N. (2007). Are RCTs the gold standard? *BioSocieties*, *2*, 11–20. https://doi.org/10.1017/S1745855207005029

Cech, E. (2014). Culture of disengagement in engineering education? *Science, Technology, & Human Values*, *39*(1), 42–72. https://doi.org/10.1177/0162243913504305

Douglas, H. (2003). The moral responsibilities of scientists (tensions between autonomy and responsibility). *American Philosophical Quarterly*, *40*(1), 59–68.

Douglas, H. (2009). *Science, policy, and the value-free ideal*. University of Pittsburgh Press.

Douglas, H. (2014). The moral terrain of science. *Erkenntnis*, *79*. https://doi.org/10.1007/s10670-013-9538-0

Eddy, S. R. (2005). "Antedisciplinary" science. *PLOS Computational Biology*, *1*(1), e6. https://doi.org/10.1371/journal.pcbi.0010006

Eggleson, K. (2014). Transatlantic divergences in citizen science ethics—comparative analysis of the DIYbio code of ethics drafts of 2011. *Nanoethics*, *8*, 187–192. https://doi.org/10.1007/s11569-014-0197-7

Ehni, H.-J. (2008). Dual use and the ethical responsibility of scientists. *Archivum Immunologiae Et Therapiae Experimentalis*, *56*(3), 147. https://doi.org/10.1007/s00005-008-0020-7

Epstein, S. (1996). *Impure Science*. University of California Press.

Evans, N. G. (2014). Dual-use decision making: Relational and positional issues. *Monash Bioethics Review*, *32*(3-4), 268–283. https://doi.org/10.1007/s40592-015-0026-y

Ferber, D. (2004). Time for a synthetic biology Asilomar? *Science*, *303*(5655), 159.

Fernández Pinto, M., & Hicks, D. J. (2019). Legitimizing Values in Regulatory Science. *Environmental Health Perspectives*, *127*(3), 035001. https://doi.org/10.1289/EHP3317

Fleck, L. (1979). *Genesis and development of a scientific fact* (T. J. Trenn & R. K. Merton, Eds.). University of Chicago Press.

Fountain, H. (2012, October 26). Yes, driverless cars know the way to San Jose. *The New York Times*.

Future of Life Institute. (2017). *Asilomar AI principles*. https://futureoflife.org/ai-principles/

Gizycki, R. von. (1973). Centre and periphery in the international scientific community: Germany, France and Great Britain in the 19th century. *Minerva*, *11*(4), 474–494.

Goodnough, A. (2011, September 28). Man is held in a plan to bomb Washington. *The New York Times*.

Greene, H. W. (2005). Organisms in nature as a central focus for biology. *Trends in Ecology & Evolution*, *20*(1), 23–27. https://doi.org/10.1016/j.tree.2004.11.005

Grene, M. (1985). Perception, Interpretation, and the Sciences: Toward a New Philosophy of Science. In D. J. Depew & B. H. Weber (Eds.), *Evolution at a crossroads: the new biology and the new philosophy of science* (pp. 1–20). MIT Press.

Grossman, L. (2013, February 11). *Drone home*. Time. http://content.time.com/time/magazine/article/0,9171,2135132,00.html

Hankinson Nelson, L. (1990). *Who Knows: From Quine to a Feminist Empiricism*. Temple University Press.

Hicks, D. J. (2017). Genetically Modified Crops, Inclusion, and Democracy. *Perspectives on Science*, *25*(4), 488–520. https://doi.org/10.1162/POSC_a_00251

Hicks, D. J., & Simmons, R. (2019). The National Robotics Initiative: A Five-Year Retrospective. *IEEE Robotics Automation Magazine*, *26*(3), 2–9. https://doi.org/10.1109/MRA.2019.2912860

Hillerbrand, R., & Werker, C. (2019). Values in university-industry collaborations: The case of academics working at universities of technology. *Science and Engineering Ethics*, *25*, 1633–1656. https://doi.org/10.1007/s11948-019-00144-w

Holbrook, J. B. (2005). Assessing the science–society relation: The case of the US National Science Foundation's second merit review criterion. *Technology in Society*, *27*(4), 437–451. https://doi.org/10.1016/j.techsoc.2005.08.001

Hull, D. L., Tessner, P. D., & Diamond, A. M. (1978). Planck's principle. *Science*, *202*, 717–723.

Institute of Electrical and Electronics Engineers. (2021). *Policies, 7.8: IEEE code of ethics*. https://www.ieee.org/about/corporate/governance/p7-8.html

International Human Rights and Conflict Resolution Clinic and Global Justice Clinic. (2012). *Living under drones: Death, injury, and trauma to civilians from US drone practices in pakistan*. Stanford Law School; NYU School of Law.

Johnstone, J. (2007). Technology as empowerment: A capability approach to computer ethics. *Ethics and Information Technology*, *9*, 73–87. https://doi.org/10.1007/s10676-006-9127-x

Kaplan, L. R., Farooque, M., Sarewitz, D., & Tomblin, D. (2021). Designing Participatory Technology Assessments: A Reflexive Method for Advancing the Public Role in Science Policy Decision-making. *Technological Forecasting and Social Change*, *171*, 120974. https://doi.org/10.1016/j.techfore.2021.120974

Kitcher, P. (2001). *Science, truth, and democracy*. Oxford University Press.

Kitcher, P. (2006). Public knowledge and the difficulties of democracy. *Social Research*, *73*(4), 1205–1225.

Kourany, J. (2010). *Philosophy of science after feminism*. Oxford University Press.

Kuhn, T. S. (2012). *The structure of scientific revolutions* (50th anniversary ed.). University of Chicago Press.

Lakatos, I. (1978). *The methodology of scientific research programmes: Philosophical papers, volume 1* (J. Worrall & G. Currie, Eds.). Cambridge University Press.

Larrinaga, N. de. (2013, October 24). *Taranis makes maiden flight*. IHS Jane's 360. http://www.janes.com/article/28899/taranis-makes-maiden-flight

Latour, B. (2011). Love your monsters: Why we must care for our technologies as we do our children. *Breakthrough Journal*, *2*, 19–26.

Lautenschläger, K. (1985). Controlling military technology. *Ethics*, *95*(3), 692–711. https://doi.org/10.1086/292667

Ledford, H. (2010). Garage biotech: Life hackers. *Nature*, *467*, 650–652. https://doi.org/10.1038/467650a

Leonelli, S. (2016). *Data-centric biology: A philosophical study*. University of Chicago Press.

Leonelli, S., Smirnoff, N., Moore, J., Cook, C., & Bastow, R. (2013). Making open data work for plant scientists. *Journal of Experimental Botany*, *64*(14), 4109–4117. https://doi.org/10.1093/jxb/ert273

Longino, H. E. (1990). *Science as social knowledge: values and objectivity in scientific inquiry*. Princeton University Press.

Mariscal, C., & Petropaganos, A. (2016). CRISPR as a driving force: The *Model T* of biotechnology. *Monash Bioethics Review*, *34*(2), 1–16.

Miller, L. (2020, April 21). LAPD will end controversial program that aimed to predict where crimes would occur. *Los Angeles Times*. https://www.latimes.com/california/story/2020-04-21/lapd-ends-predictive-policing-program

Miller, S. (2013). Moral responsibility, collective-action problems and the dual-use dilemma in science and technology. In M. J. Selgelid & B. Rappert (Eds.), *On the dual uses of science and ethics: Principles, practices, and prospects* (pp. 185–206). ANU E Press.

Miller, S. (2018). *Dual use science and technology, ethics and weapons of mass destruction*. Springer.

Miller, S., & Selgelid, M. J. (2007). Ethical and philosophical consideration of the dual-use dilemma in the biological sciences. *Science and Engineering Ethics*, *13*(4), 523–580. https://doi.org/10.1007/s11948-007-9043-4

Mirowski, P. (2012). The modern commercialization of science is a passel of Ponzi schemes. *Social Epistemology*, *26*(3-4), 285–310. https://doi.org/10.1080/02691728.2012.69721

Mitchell, S. (2009). *Unsimple truths: Science, complexity, and policy*. University of Chicago Press.

Mohler, G. O., Short, M. B., Brantingham, P. J., Schoenberg, F. P., & Tita, G. E. (2011). Self-Exciting Point Process Modeling of Crime. *Journal of the American Statistical Association*, *106*(493), 100–108. https://doi.org/10.1198/jasa.2011.ap09546

Moore, M. R. (2019). *Small unmanned aerial system program deployment guidelines and procedures*. Los Angeles Police Department. http://www.lapdpolicecom.lacity.org/091019/BPC_19-0248.pdf

National Science Foundation. (2021). *National Robotics Initiative 3.0: Innovations in Integration of Robotics (NRI-3.0) (nsf21559)*. https://www.nsf.gov/pubs/2021/nsf21559/nsf21559.htm

Nature Editorial Board. (2012). Publishing risky research. *Nature*, *485*(7396), 5. https://doi.org/10.1038/485005a

New America. (2020, July 30). *World of drones: Non-state actors with drone capabilities*. https://www.newamerica.org/international-security/reports/world-drones/non-state-actors-with-drone-capabilities/

Nise, N. S. (2011). *Control systems engineering* (6th ed.). John Wiley & Sons.

Norton, B. G. (2005). *Sustainability: a philosophy of adaptive ecosystem management*. University of Chicago Press.

O'Malley, M. A., & Soyer, O. S. (2012). The roles of integration in molecular systems biology. *Studies in History and Philosophy of Biological and Biomedical Sciences*, *43*(1), 58–68. https://doi.org/10.1016/j.shpsc.2011.10.006

Parker, W. (2010). Predicting weather and climate: Uncertainty, ensembles and probability. *Studies in History and Philosophy of Modern Physics*, *41*(3), 263–272. https://doi.org/10.1016/j.shpsb.2010.07.006

Plaisance, K., & Fehr, C. (Eds.). (2010). Making philosophy of science more socially relevant. *Synthese*, *177*(3).

Rasmussen, N. (2015). DNA technology: "Moratorium" on use and Asilomar Conference. In *eLS*. John Wiley & Sons. https://doi.org/10.1002/9780470015902.a0005613.pub2

Rector, K. (2020, October 27). LAPD gets approval to begin recording, storing aerial footage of protests. *Los Angeles Times*. https://www.latimes.com/california/story/2020-10-27/lapd-to-begin-recording-aerial-footage-of-protests

Riek, L. D., & Howard, D. (2014). A code of ethics for the human-robot interaction profession. *WeRobot 2014*.

Rouse, J. (1990). The narrative reconstruction of science. *Inquiry*, *33*(2), 179–196.

Rouse, J. (2002). *How scientific practices matter: Reclaiming philosophical naturalism*. University of Chicago Press.

Rouse, J. (2006). Practice theory. In S. Turner & M. Risjord (Eds.), *Handbook of the philosophy of science, vol. 15: The philosophy of sociology and anthropology* (pp. 639–681). Elsevier.

Rouse, J. (2007). Social practices and normativity. *Philosophy of the Social Sciences*, *37*(1), 46–56. https://doi.org/10.1177/0048393106296542

Schott, T. (1991). The world scientific community: Globality and globalisation. *Minerva*, *29* (4), 440–462.

Shapin, S. (2010). *The scientific life: A moral history of a late modern vocation*. University of Chicago Press.

Shilton, K. (2013). Values levers: Building ethics into design. *Science, Technology & Human Values*, *38*(3), 374–397.

Simons, M. (2021). "Science without scientists": DIY biology and the renegotiation of the life sciences. In D. Houtman, S. Aupers, & R. Laermans (Eds.), *Science under siege: Contesting the secular religion of scientism* (pp. 155–178). Palgrave Macmillan.

Soler, L., Zwart, S., Lynch, M., & Israel-Jost, V. (Eds.). (2014). *Science after the practice turn in the philosophy, history, and social studies of science*. Routledge.

Steel, D., Bolduc, N., Jenei, K., & Burgess, M. (2020). Rethinking Representation and Diversity in Deliberative Minipublics. *Journal of Deliberative Democracy*, *16*(1), 46–57. https://doi.org/10.16997/jdd.398

Stop LAPD Spying. (2015). *The Drone Report*. https://stoplapdspying.org/wp-content/uploads/2015/12/The-Drone-Report.pdf

Stop LAPD Spying. (2018). *Before the Bullet Hits the Body: Dismantling Predictive Policing in Los Angeles*. https://stoplapdspying.org/wp-content/uploads/2018/05/Before-the-Bullet-Hits-the-Body-May-8-2018.pdf

Stop LAPD Spying. (2020, March 3). *The Algorithmic Ecology: An Abolitionist Tool for Organizing Against Algorithms*. Medium. https://medium.com/@stoplapdspying/the-algorithmic-ecology-an-abolitionist-tool-for-organizing-against-algorithms-14fcbd0e64d0

TacoCopter, Inc. (2012). *TacoCopter: One-click taco delivery in the SF bay area*. http://www.tacocopter.com/

Talbot, M. (2020). The rogue experimenters. *The New Yorker*, 40–49.

Tewksbury, J. J., Anderson, J. G. T., Bakker, J. D., Billo, T. J., Dunwiddie, P. W., Groom, M. J., Hampton, S. E., Herman, S. G., Levey, D. J., Machnicki, N. J., Rio, C. M. del, Power, M. E., Rowell, K., Salomon, A. K., Stacey, L., Trombulak, S. C., & Wheeler, T. A. (2014). Natural history's place in science and society. *BioScience*, *64*(4), 300–310. https://doi.org/10.1093/biosci/biu032

Tomblin, D., Worthington, R., Gano, G., Farooque, M., Sittenfeld, D., & Lloyd, J. (2015). *Informing NASA's Asteroid Initiative: A Citizen Forum*. http://www.nasa.gov/sites/default/files/atoms/files/ecast-informing-nasa-asteroid-initiative_tagged.pdf

United Nations War Crimes Commission. (1947). Case IX: The Zyklon-B case, trial of Bruno Tesch and two others. In *Law reports of trials of war criminals* (Vol. 1, pp. 93–103). Her Majesty's Statonery Office.

Vogel, G. (2015). Embryo engineering alarm. *Science*, *347*(6228), 1301. https://doi.org/10.1126/science.347.6228.1301

Weller, N., Govani, M. S., & Farooque, M. (2021). Thinking Like a Citizen. *Issues in Science and Technology*. https://issues.org/thinking-like-citizen-participatory-technology-assessment-weller-govani-farooque/

Wynne, B. (1989). Sheepfarming after Chernobyl: A Case Study in Communicating Scientific Information. *Environment: Science and Policy for Sustainable De-*

velopment, *31*(2), 10–39. https://doi.org/10.1080/00139157.1989.9928930

Yen, H.-L., & Peiris, J. S. M. (2012). Virology: Bird flu in mammals. *Nature*, *486*(7403), 332333. https://doi.org/10.1038/nature11192