# SCIENCE, ENDS, AND DEMOCRACY
# A DISSERTATION PROPOSAL

DAN HICKS

## Introduction

Philosophy of science, strictly speaking, was never completely apolitical. While philosophy of science was closely tied to political projects in both Germanic and Anglophone countries in the 1920s, and later faded from mainstream work[1], there were always at least clusters of philosophers and other thinkers – especially Marxists, socialists, feminists, and other leftists – whose work had an explicitly political component.

Since, roughly speaking, the publication of Helen Longino's *Science as social knowledge*[2] in 1990, politically-relevant philosophy of science has gradually returned to the mainstream of philosophy from the Marxist and feminist margins. It has, for example, attracted the attention and qualified support of more politically moderate philosophers, including Philip Kitcher[3] and Ronald Giere[4]. At the 2008 biennial meeting of the Philosophy of Science Association, a lunchtime session on 'Philosophy of science in the public domain, Bending scientific inquiry: Legal, historical, and philosophical concerns' filled the room beyond capacity, and a workshop on 'How can philosophers of science take up more socially relevant roles?' was extremely well-attended. I think that it is therefore fair to say that the turn of the new century has seen the rebirth of a movement in philosophy of science that I will call the Science and Values movement, or SAV.

But not all of the attention to the political dimensions of scientific inquiry has been positive, and there is a pervasive distrust of SAV, in both its mainstream and radical forms. At the center of the resulting disputes has been the notion of

*Date*: Draft of April 2, 2009.
[1]Delaney, 1972; Howard, 2003; Reisch, 2005; McCumber, 2001
[2]Longino, 1990
[3]Kitcher, 2001
[4]Giere, 2003

underdetermination. As I will argue in the part 1, underdetermination has been a key concept for both SAV and its ancestors in the nineteenth and early twentieth centuries, but its use has led to an unproductive deadlock between proponents and critics of SAV and a focus in the literature on issues that are tangential to the primary aims of SAV. I will therefore go on to suggest that SAV back off somewhat from underdetermination, and take up a new, broader framework. The ultimate aim of my dissertation will be to provide such a framework; I start to lay it out in part 2. In part 3, rather than completing this framework, I turn to work in political philosophy, to discuss how its completion can be an ongoing public project, rather than something done by a single philosopher once and for all. Finally, in part 4, I apply the work of parts 2 and 3 to consider a case study.

## 1. Underdetermination

1.1. **Some history.** The earliest *locus classicus* of underdetermination is in Pierre Duhem's *La théorie physique: Son object et sa structure*[5]. Duhem argues that the design of an experiment in physics requires the use of background theory or auxiliary hypotheses concerning the operations of the experimental equipment; hence an experimental outcome contradicting the predictions of one's physical theory in fact only contradicts the conjunction of the physical theory with the auxiliary hypotheses.[6] Therefore logic and observation alone cannot determine whether the physical theory or the auxiliary hypotheses (or both) are epistemologically unacceptable. Duhem goes on to posit an epistemological virtue, *bon sens*, by which 'good' scientists choose whether to reject the theory or auxiliary hypotheses.[7] This way of filling the logical gap between evidence and theory has explicit political-cultural implications for Duhem, as he adds that the superior *bon sens* exhibited by French scientists makes French scientific inquiry epistemologically superior to English and German scientific inquiry.[8]

[5]Duhem, 1908, 2.VI, especially §II, *'Qu'une expérience de Physique ne peut jamais condamner une hypothèse isolée, mais seuelement tout un ensemble théorique'.*
[6]*Ibid.*, 303
[7]*Ibid.*, 356ff.
[8]*Op. cit.*

A similar argument appeared a short time later in the work of left-wing logical empiricists. Otto Neurath[9] appealed[10], not to formal logical considerations, but instead to practical exigencies, arguing that 'since often, in actual living, the requirements of action allow of no delay, it is very certain that when it is not in our power to determine which opinions are truest, we ought to follow those seemingly most likely'[11]. We rarely (if ever) have the luxury of delaying action until we have absolutely conclusive evidence supporting one hypothesis over another; we typically must act, and act decisively, based on preliminary results and after only relatively brief consideration of our options. Instead we take such evidence and rational evaluation as we have we been able to acquire, and make a decision to act by appeal to an 'auxiliary motive'[12]. An auxiliary motive can include appeals to instinct, superstition, tradition, authority, or random chance, but most importantly appeals to our ethico-political values.[13]

The structure of the argument is the same in both cases: evidence and logic are insufficient to rationally determine – in the sense of either theoretical or practical reason – which of two or more theories to adopt, and hence there is logical room – again in the sense of either theoretical or practical reason – for ethico-political values in the choice of theory. This same argument reappeared in feminist philosophy of science at the end of the twentieth century. In 1990, for example, both Helen Longino[14] and Lynn Hankinson Nelson[15] used underdetermination to argue for a role for ethico-political values in theory choice. More recently, underdetermination has been used by Kourany[16], Howard[17], Hands[18], Uebel[19], and others to argue for a similar role for ethico-political values in theory choice. In short, underdetermination

[9]Philip Frank also made use of underdetermination. See Uebel, 2005, 759n.
[10]Neurath, 1913/1983
[11]*Ibid.*, 1
[12]*Ibid.*, 4
[13]*Ibid.*, 4-7 and 11
[14]Longino, 1990, ch. 3
[15]Hankinson Nelson, 1990, ch. 6
[16]Kourany, 1998, Kourany, 2003
[17]Howard, 2009
[18]Hands, 2005
[19]Uebel, 2005

is often and rightly taken to be one of the central arguments against an ideal of value-free scientific inquiry.[20]

At the same time, underdetermination has been a major point of attack for critics of SAV. Critics of the use of underdetermination by feminist philosophers have argued that the resulting picture is of scientific inquiry that is 'ideologically driven' and constrained by 'political correctness'.[21] Less hyperbolic opposition to SAV has appeared in the form of more careful and direct challenges to, for example, the account of induction assumed by appeals to underdetermination.[22] Kitcher and Giere have explicitly rejected underdetermination, and thus distanced their views from more politically radical members of SAV.[23] Even some feminist members of SAV have given arguments against the use of underdetermination.[24]

The result has been a messy proliferation of accounts of the actual and legitimate role of ethico-political values in scientific inquiry, and an increasingly tendentious debate that does not further any actual aims of SAV. Radicals appeal to underdetermination without qualm. Moderates make distinctions between 'cognitive' and 'non-cognitive' values, and try to identify 'moments' at which different values may play a 'legitimate' role[25], or gesture vaguely at a notion of 'significance' for choosing among research programs[26]. Radicals retort – often in explicit defense of underdetermination – by attacking the moderates' distinctions[27] and charging their accounts of 'significance' with being either empty or incoherent[28].

1.2. **A broader perspective.** Underdetermination is undoubtedly an important concept for SAV. But this importance does not warrant the attention underdetermination has received. Rather than actually engaging in socially-relevant philosophy of science, the focus on underdetermination means that we spend most of our time

[20]See, for example, Kindcaid, Dupré, & Wylie, 2007, 14.
[21]Haack, 2003 and Koertge, 2003; see Anderson, 2006 for a negative review of these two essays and their containing anthology.
[22]Norton, 2008
[23]Kitcher, 2001 and Giere, 2003.
[24]See, for example, Anderson, 2004 and Intemann, 2005.
[25]Cf. Lacey, 2005
[26]Kitcher, 2001, ch. 6
[27]Machamer & Douglas, 1999: Longino, 1995: Longino, 1996; Longino, 2002a, appendix to ch. 3; Kourany, 2003; Howard, 2009; Biddle, 2006; &c.
[28]Longino, 2002b and Brown, 2008

on debates over the epistemology of theory choice. As philosophically valuable as the discussion of underdetermination may be, it is often as practically irrelevant as the debates over confirmation and explanation that were philosophy of science in Hempel's day. We have made little to no progress in thirty years. If SAV wants to be an agent of genuine *social* progress, it must first be an agent of *philosophical* progress. This means getting beyond the Hempelian epistemological issues, which in turn means finding a broader theoretical framework within which to consider the relation between science and society.

This broader theoretical framework comes, I think, from looking more carefully at the aims[29] of scientific inquiry, and the standards of success for those aims. At the end of her critical discussion of uses of underdetermination in feminist philosophy of science, Kristen Intemann suggests that 'contextual [ethico-political] values might legitimately play a role in theory justification . . . if such values were somehow inextricably connected to the aims of (at least some) scientific research contexts'[30]. That is, '[i]f there are cases where the goals of a research context presuppose or depend on endorsing certain contextual values, then such values will play a legitimate role in applying and adjudicating between constitutive values.'[31] Intemann cites both Longino and Kitcher making similar suggestions, indicating that a connection between ethico-political values and the ends, aims, goals, or purposes of scientific inquiry may be an important point of agreement for both radical and moderate members of SAV. We can therefore say that articulating and defending the following *first key claim* is an important aim of SAV:

> **First key claim:** There is a connection between our ethico-political aims and the aims of scientific inquiry.

Will it also placate the critics of SAV? Elizabeth Anderson argues that opposition to and criticism of SAV (in the form of defense of an ideal of value-free scientific inquiry) is largely motivated by a fear that value judgments are 'dogmatic', so that 'scientists who bring to inquiry value judgments concerning the subject of

[29]Throughout this proposal, I use the terms 'aims', 'ends', and 'goals' interchangeable. The concept of 'internal goods', introduced in part 2, is closely related, and I sometimes slide between this and the above terms when I do not feel there is a dangerous ambiguity.
[30]Intemann, 2005, 1010; my brackets, her parentheses
[31]*Op. cit.*

investigation . . . will be unable to impartially assess empirical theories concerning the subject'[32]. That is, opponents (like Haack and Koertge) are worried that, since value judgments (or at least ethico-political value judgments) are not subject to rational criticism and debate, they are held dogmatically; and then, if they play a role in scientific inquiry, scientific inquiry will be dogmatic as a result. This suggests that SAV can best assuage its critics by defending a *second key claim*:

> **Second key claim:** The role ethico-political values play in scientific inquiry is subject to rational criticism and debate, and hence scientific inquiry is not dogmatic as a result.

An appeal to underdetermination will still play a role in defending these two key claims, but only as one small move in a much larger argument. Instead of focusing on underdetermination, what I need primarily is an account of the aims of scientific inquiry, how they are related to the aims of our other activities (including our ethico-political activities), and how we rationally evaluate these aims. By showing how the aims of scientific inquiry are connected to the aims of ethico-political activities, this will articulate and defend the first key claim. And showing how they can still be evaluated rationally will articulate and defend the second.

Rather than focusing so much on epistemology, what SAV needs is, first, an account of *science as a practice* and, second, an account of *the rational evaluation of science as a practice*. And this is exactly what I aim to give in my dissertation. As I envision it now, this project will have four basic functional parts.[33,34] The content of this section, and some of the introductory material from the previous section, comprise the first part: a discussion of SAV, a discussion of the importance of and problems with underdetermination, and a sketch of this alternative route to the aims of SAV. The second part deals with scientific inquiry as a practice, laying out the terminology I will use and then developing an account of the aims of scientific inquiry and their 'connection' to our ethico-political aims. The third functional part

[32]Anderson, 2004, 4

[33]Each of these parts is rather large; I suspect they will have to be further subdivided into chapters. Hence 'parts' rather than 'chapters'.

[34]It's not that I've been *reading* too much Aristotle, or even MacIntyre. It's that I've been spending too much time with graduate students who are *teaching* – and consequently *constantly talking about* – Aristotle and MacIntyre.

considers how the sketch of the second part can be developed further. Rather than doing this development myself, I turn it over to the public, using resources from political philosophy to consider whether and how public deliberation on scientific inquiry would work. Fourth and finally I show that my work is not merely a theoretical abstraction, lacking traction with real life and the concrete aims of SAV, by developing some institutional guidelines for this public deliberation.

## 2. Scientific inquiry as practice

2.1. **Theory and practice.** First, let me say a few words about theories and models. In Hempel's day, the 'science' with which philosophy of science was primarily concerned was one or several theories, in the sense of one or several sets of propositions or sentences structured by logical relations and investigated using the tools and techniques of twentieth-century mathematical logic. Starting with the work of Patrick Suppes and Bas van Fraassen, however, a growing contingent of philosophers saw scientific knowledge as a collection of models rather than sets of propositions or sentences. Variations on this *semantic approach* developed in the '80s and '90s, and there is some recent work trying to reconcile these variations.[35]

Despite their differences, all the variations of the 'syntactic' and 'semantic' approaches to the philosophy of science focus on the knowledge produced by scientific inquiry. I call this focus *science-as-theory*. According to science-as-theory physics is the theory – or collection of theories or models or what-have-you – that is the content of physics textbooks and physics journals, chemistry is similarly the content of chemistry textbooks and journals, and so on. Science-as-theory generally excludes both the concrete activities that produced that content and the later activities that use or otherwise assume that content. At its closest approach to these concrete activities, science-as-theory may deal with the formal or logical relations between a body of evidence and the theory it confirms.

Science-as-theory is contrasted with *science-as-practice*. My definition of practice comes from MacIntyre:

[35]See, for example, Suárez & Cartwright, forthcoming; Giere, forthcoming; and Cunningham, 2009.

> By a 'practice' I am going to mean any coherent and complex form of socially established cooperative human activity through which goods internal to that form of activity are realized in the course of trying to achieve those standards of excellence which are appropriate to, and partially definitive of, that form of activity, with the result that human powers to achieve excellence, and human conceptions of the ends and goods involved, are systematically extended.[36]

Physics, chemistry, and biology are among the handful of examples MacIntyre gives of practices. Rather than viewing these sciences as objects or quasi-objects – knowledge thought of as an entity, whether in the form of theory or model – and, hence, things that human beings *have*, we view them as activities, processes, or other things that human beings *do*. MacIntyrean practices have three crucial components:

(1) a socially organized set of practitioners,
(2) a set of 'goods internal to that form of activity', and
(3) a set of 'standards of excellence' for those goods.

As a first pass, biology has all three: the biologists themselves, organized into a network of labs, departments, and institutions; biology-as-theory; and the epistemic standards by which biological theory is judged.

MacIntyre makes an important distinction between two kinds of good in terms of practices:

> There are thus two kinds of good .... On the one hand there are those goods externally and contingently attached ... to ... practices by the accidents of social circumstance ... such as prestige, status and money. There are always alternative ways for achieving such goods, and their achievement is never to be had *only* by engaging in some particular kind of practice. On the other hand there are the goods internal to the practice ... which cannot be had in any way but by ... [engaging in practices] of that specific kind.[37]

[36]MacIntyre, 1984, 187. Science – especially social science – was an important touchstone for MacIntyre's development of this conception of practice; cf. MacIntyre, 1967; MacIntyre, 1977; MacIntyre, 1978; MacIntyre, 1979; MacIntyre, 1986; and MacIntyre, 1987.
[37]*Ibid.*, 188, his emphasis

For a given practice, call the first type of goods *external goods*, and the second type its *internal goods*. If science is indeed a practice, and does indeed have a distinctive set of internal goods according to MacIntyre's definition, then science-as-theory is the most obvious candidate for those internal goods.[38] While external goods are certainly important for science-as-practice – one needs a great deal of money if one is going to build a superconducting supercollider, after all – they are not its *raisons d'être*. Instead, the fundamental aim of science-as-practice is the pursuit of excellent science-as-theory.

Along with its internal goods, science-as-practice also has standards of excellence for those goods. I will refer to these standards as *the internal standards of science-as-practice*, or just internal standards for short. If the internal goods of science-as-practice are science-as-theory, then the internal standards are the standards by which science-as-theory is judged. As a first pass, these are the standards given by normative epistemology: one might say, for example, that excellent biological theory is biological theory produced by a reliable belief-forming mechanism. Just as choosing between definitions of theory was not necessary above, choosing between normative epistemologies is not necessary here.

Internal goods and their internal standards serve two important normative or evaluative functions. First, the internal goods and standards are used to evaluate the success of a particular *institution*, as an instantiation of the (abstract and idealized) practice. A successful institution is one that produces internal goods that are, in turn, evaluated highly according to the internal standards. For example, as part of our first pass gloss, a successful biology lab might be one that produces biological theory in a reliable way. Furthermore, a biology lab that is *not* producing biological theory in a reliable way can, *ipso facto*, be criticized as unsuccessful and, hopefully, reformed in such a way that it does end up producing biological theory in a reliable way. The internal goods and standards thereby give a practice and its institutions a kind of normative instrumental rationality, and hence standards for successful practice *on its own terms*.

[38]There are subject-verb agreement problems here: 'science-as-theory' is a mass noun, and hence has no plural, while 'internal goods' is, grammatically speaking, the plural of a count noun. I hope the reader will indulge the occasional awkward phrase that results from this usage.

We can also use the internal goods and standards of a practice to evaluate it *more generally*. If the internal goods and standards of one practice can help us achieve the internal goods of another practice, for example, this gives us some reason to engage in the first practice. More concretely, to the extent that the knowledge produced by biological inquiry can help us grow more nutritious food in greater quantities at a lower cost, and thereby help us achieve the goods of relieving hunger and preventing starvation, we have some reason to engage in biological inquiry. On the other hand, to the extent that the knowledge produced by a line of inquiry interferes with our ability to pursue our other valuable activities (say, research into nuclear physics that will only be useful for producing weapons), this gives us some reason not to pursue that line of inquiry. By examining its internal goods, and how they relate to other goods, we can consider whether a practice is worth pursuing at all.

All together, then, the first pass gloss of science-as-practice focuses on ideal or concrete communities of scientists pursuing excellent science-as-theory, where this excellence is understood epistemologically. By appeal to its epistemological aims, we can ask and start to answer a wide array of important questions:

- What aims should scientific inquiry attempt to realize? (Answer: Science-as-theory)
- What are the standards of excellence for scientific inquiry? (Answer: Those of normative epistemology)
- How should the community of scientists be organized in order to realize these aims according to these standards? (Answer: Using reliable methodology)
- Why are these aims worth the investment of our resources? (Answer: Because they will help us pursue other activities that we find valuable)

Even with this first pass gloss, the wider perspective of science-as-practice is clearly more useful for SAV than the narrow perspective of science-as-theory. Science-as-theory and normative epistemology provide only the first step in showing a connection between our ethico-political aims and the aims of scientific inquiry; the further steps taken by science-as-practice give us a way to argue for the first key claim of SAV, and show that there is a connection between our ethico-political aims and the aims of scientific inquiry.

The first pass gloss I have given here is inadequate, however, as it presents an overly simplified account of the internal goods of scientific inquiry. We do not engage in scientific inquiry *only* because it produces epistemologically good theory, or *only* because epistemologically good theory happens to be useful. There is, I claim, a much more sophisticated relationship between the internal goods and standards of science-as-practice and the goods of our other practices.

2.2. **The internal goods of science.** To develop this more sophisticated account, I turn from MacIntyre to Dewey.[39] In the first half of the twentieth century, Dewey was a prominent proponent of a close connection between science and values, and in particular defended versions of all three of the key claims.[40] Dewey views science as practice, not theory, including all the major features of MacIntyre's account of practices several decades later. He identifies a 'basic problem of the relation of common sense material and methods to that of scientific subject-material and method, can be explicitly discussed'[41]. By 'common sense', Dewey means our ordinary, everyday practices and activities – what I have called our ethico-political projects, aims, and values. The 'basic problem' is therefore how the aims of science-as-practice relate to our ethico-political values. That is, Dewey is deeply concerned with what I have called the first key claim of SAV, and in a way that provides us with a more sophisticated account of the relationship.

Dewey's philosophy of science is therefore extremely relevant to the task I have set for myself in articulating and defending the first key claim. His solution, however, is cryptic:

[39]I consider both Dewey and MacIntyre to be among the most important philosophers of the twentieth century, and among the most neglected. Due perhaps to the common influence of Hegel, there are deep parallels between Dewey and MacIntyre, but also equally deep differences. See Carden, 2006.

[40]For the first key claim in particular, chapter 4 of Dewey's *Logic* (LW 12) is the obvious touchstone. About a decade later, in *Knowing and the known* (LW 16), written with Arthur Bentley, Dewey wrote another chapter on the same topic. Citations to Dewey will be to the appropriate volume and page of his *Collected works*, divided into Early (EW), Middle (MW), and Late (LW) series. Citations to Dewey with an asterisk (*) are to an earlier edition; I have not yet had time to cross-reference my notes with the *Collected works* edition.

[41]LW 12:71*

> (1) Scientific subject-matter and procedures grow out of the direct problems and methods of common sense, of practical uses and enjoyments, and (2) react into the latter in a way that enormously refines, expands and liberates the contents and the agencies at the disposal of common sense.[42]
>
> For purposes of illustration, it may be supposed that primitive astronomy and primitive methods of keeping track of time (closely connected with astronomical observations) grew out of the practical necessities of groups with herds in care of animals with respect to mating and reproduction, and of agricultural groups with reference to sowing, tilling and reaping.[43]

I suggest that Dewey's view is of a 'feedback loop' between science and common sense, with each informing and constraining the other. To develop this vague metaphor more concretely, I suggest we turn to the work of some contemporary members of SAV. Martin Carrier's work on applied science and technology will give us a better understanding of the internal goods of science-as-practice, while Heather Douglas' approach to SAV will help us identify the interactions between the internal standards of science-as-practice and common sense. I will spend the remainder of this subsection discussing Carrier's work, and turn to Douglas in the next.

Martin Carrier has argued for a sophisticated view of the interplay between 'pure' and 'applied' science.[44] In contrast to a view of research into applied science as simply 'employ[ing] the toolkit of established principles and bring[ing] general theories to bear on technological challenges'[45], uninterested in robust empirical adequacy (much less truth) and giving explanations, Carrier argues that applied science does not enjoy the luxury of choosing 'simple' or 'tractable' problems. Instead, the need for reliable practical results – artefacts and techniques that mostly work in stable or predictable way – means there is genuine need for robust empirical adequacy, and

[42]LW 12:66*
[43]LW 12:71-2*
[44]Carrier, 2004; Carrier, 2007; Carrier, 2008
[45]Carrier, 2004, 3

the complexity of the 'intractable' problems requires bringing to bear a wide variety of theories and models, which in turn requires a genuine understanding of the content of and conflicts between those theories and models. Carrier argues that the development of applications and technology in applied scientific enquiry proceeds with a certain degree of independence from the development of pure theory. Applied science acts as an interface between practical demands and desires, on the one hand, and the work of abstract theory-builders, on the other, and cannot be reduced or divided cleanly between them. Applied science is not simply the combination of society proposing and science disposing.

Furthermore, Carrier identifies a phenomenon he calls 'application innovation'[46], in which the results of *applied* research create new possibilities for *pure* scientific inquiry. He gives the example of the discovery of prions and retroviruses, which was the result of epidemiological research.[47] Another important example is the development of the personal computer, without which research into nonlinear dynamics would be all but impossible (to say nothing of the use of computer simulations in a wide range of disciplines).

There are two lessons I want to take from Carrier. First, not all of the internal goods of science-as-practice are *representational knowledge*. That is, the aims of science go beyond just producing theories or models that serve as (partial) representations of parts of the world. Instead, the internal goods of science-as-practice include both *technology* – concrete physical objects that are useful as tools and instruments, and are not representations – and *technical knowledge*, also called *know-how* and *tacit knowledge*. This is the knowledge involved in performing certain actions and sequences of actions, performing them well, and performing them in novel ways or situations.

Second, all three of these internal goods are interdependent. While we can make distinctions between them, each requires the other two.[48] Technical knowledge,

[46]Carrier, 2007, 10

[47]*Op. cit.*

[48]They may therefore be an example of the kind of system Dewey and Bentley call a 'transaction' (LW 16). As a gloss, a transaction is a system in which parts, elements, or individuals can be differentiated, but these parts cannot be adequately understood by examining their intrinsic properties. Instead, they must be understood relationally, ie, in terms of their relations to the rest of the system. Other examples of transactions from Dewey's *œuvre* would then include: an

paradigmatically, is knowledge of how to use a piece of technology; we cannot use the piece of technology without the corresponding know-how. Similarly, as Carrier points out, developing a piece of technology requires the application of a complex array of representations. Finally, developing new representational knowledge – at least, new *empirical* representational knowledge – requires experimentation and observation, which in turn require the coordinated operation of technological equipment, including both the technology itself and the technical knowledge of its operation. Instrumentation, after all, is itself technology.

All together, Carrier gives us a picture of a spectrum of interdependent internal goods for science-as-practice, ranging from the conceptual and abstract (in terms of *content*, not nature) representational knowledge to concrete and immediately useful pieces of technology. On this view, science is much more than just theory – and comes quite close to our ordinary, everyday activities.

2.3. **Douglas and the feedback loop.** With Carrier's richer account of the internal goods of science-as-practice in hand, we can return to Dewey's 'feedback loop' between science and our ordinary everyday activities. As I construed it above, in this loop, each of science and common sense inform and constrain each other. While the phrase 'inform and constrain' suggests that there will be two relations in each direction – an informing relation and a constraining relation – this is mistaken. Rather, informing and constraining are two different ways of understanding a single activity. A better gloss might be to say that the relation is one of guiding – or, since it is a reciprocal relation, *mutual guiding*. If I guide you on a hike up a mountain, for example, I inform your actions, literally, by giving you information about, eg, good places to rest, or especially difficult sections of the trail. By doing so I also constrain your actions. For example, you take a rest here, as opposed to there, or avoid that seemingly-easy-but-actually-quite-difficult scramble up the rocky outcropping.

So, to cash out Dewey's metaphor, there are two questions to ask:

- How does science guide common sense?
- How does common sense guide science?

organism and its environment; an individual human being and her society; and science and common sense.

Carrier has given us a preliminary answer to the first question: science guides common sense by providing technology and knowledge (both technical and representational) that promotes common sense projects and aims, and also identifies limitations on our ability to carry out our common sense projects. For example, despite occasional suggestions to the contrary, research into nuclear fusion appears to have shown that a fusion-based power plant is not a feasible replacement for fossil fuels.

I turn, therefore, to the second question. And to answer this question, I draw on the work of Heather Douglas.[49] Following a handful of philosophers of science of the 1950s and '60s[50], Douglas has developed an argument that runs something like the following:

(1) The standards for successful science-as-practice include accepting and rejecting hypotheses.
(2) Accepting or rejecting a hypothesis depends, in part, on whether the available evidence is sufficiently strong to support the hypothesis.
(3) Whether the available evidence is sufficiently strong to support the hypothesis depends, in part, on the significance of the consequences of accepting a false hypothesis.
(4) The significance of the consequences of accepting a false hypothesis depends on an ethico-political value judgment.
(5) Hence, whether the evidence is sufficiently strong to support the hypothesis depends, in part, on an ethico-political value judgment.
(6) Hence, accepting or rejecting a hypothesis depends, in part, on an ethico-political value judgment.
(7) Hence, the standards for successful science-as-practice depend, in part, on ethico-political value judgments.

The first four steps are best illustrated by walking through an example. Say we have some hypothesis $H$, and as scientists we are considering whether or not to accept it. More specifically, we are trying to choose between $H$ and the relevant null hypothesis $N$ on the basis of some set of statistical evidence and analysis $E$.

[49]Douglas, 2000; Douglas, 2006; Douglas, forthcoming
[50]Especially Rudner, 1953.

Let us further suppose that $E$ includes both some observations $o$ and the claim that, for $o$, $p < 0.1$. That is, the probability of observing $o$, given $N$, is at most 10%.

Now, first, as scientists we are tasked with deciding whether or not to accept $H$. Second, this acceptance depends, at least in part, on whether $E$ is sufficiently strong to support $H$. That is, is $p < 0.1$ a good reason to accept $H$? Douglas argues that, third, we must answer this question by considering the potential consequences of adopting $H$, should $H$ turn out to be false. Let us make this more concrete: Suppose $H$ is the claim that a certain level of exposure to bisphenol A (BPA) causes a significant increase in the incidence of certain kinds of cancer.[51] If we accept and act on $H$, and $H$ turns out to be false, then we will implement some unnecessary regulations on plastics manufacturers, at some economic cost. On the other hand, if we reject $H$ and act on the null hypothesis $N$, and $N$ turns out to be false, then we will have failed to prevent some cases of cancer, and the cost will most likely be in terms of human lives.

The considerations of the last few sentences are operationalized by considering whether $p < 0.1$ means $E$ is sufficiently strong to support $H$. If we are wary of unnecessary regulation and the economic costs thereby incurred, we will have reason to say that $p < 0.1$ is not stringent enough, and prefer a stronger test, say $p < 0.05$. That is, the observations must have a probability of only 5%, given $N$, in order for them to provide good reason to support $H$. On the other hand, if we are wary of additional deaths due to cancer, we will have reason to say that $p < 0.1$ is stringent enough, and hence $E$ is sufficiently strong, and so we should accept and act on $H$. So the choice to accept $H$ depends on whether we are more wary of unnecessary regulation or additional cancer deaths. But, fourth, this is a question of ethico-political values. It is an ethical question whether a small hit to the economy is more important than tens of thousand of additional deaths due to cancer.

There are three things I wish to point out about Douglas' argument. First, it does not apply only to science-as-theory. Consider a piece of technology, such as a new insecticide. We evaluate it by considering several of its properties: its reliability

[51]Debates over this claim were prominent in the US news about a year ago. Bisphenol A was, until recently and among other things, a common ingredient in rigid, clear, plastic water bottles.

(in eliminating insect pests), its side-effects (whether it will also kill beneficial or neutral insects, and whether it will end up being ingested by birds), its affordability or how much it costs to produce, and so on. Here, as per Douglas' argument, the standards for assessing the insecticide must be set by appeal to ethico-political value judgments. If we care quite a lot about affordability and not so much about killing beneficial and neutral insects, a cheap and general insecticide will be more valuable than if we care less about affordability and more about not killing beneficial and neutral insects. Furthermore, the judgment of which properties of the insecticide to examine at all is itself dependent on ethico-political value judgments. Similar considerations come into play when evaluating the success of an effort to develop a piece of practical knowledge. Hence, the standards of success for all three internal goods of scientific inquiry depend, in part, on ethico-political value judgments.

Second, on Douglas' account, ethico-political values are indeed guiding scientific inquiry. They are doing so by informing and constraining the choice of evidentiary standards. In MacIntyrean language, the internal standards of science-as-practice are constituted, in part, by ethico-political values, and not *only* a matter of normative epistemology. But this is so in such a way that the production of those goods is not simply wish-fulfillment. To use an automotive metaphor: The standards of a good, useful, and well-functioning car must be responsive to the needs of the driver and passengers, but significant aspects of the standards for the operation of the car do not depend directly on those needs. To say that the cooling system is working properly is not to say that the cooling system simply and directly makes the driver and passengers happy. But this means that the involvement of ethico-political values in scientific inquiry does not lead directly to dogmatism, in any kind of wish-fulfillment sense. This provides at least partial support for the second key claim. Hence, while Douglas' argument is one version of underdetermination, it should not worry the critics of SAV; it does not give science over to political correctness.

Third, I cannot simply take up Douglas' account without criticism. Douglas allows ethico-political values to play a role only in the evaluation of the consequences of epistemic error, not in the evaluation of the consequences of epistemic correctness. That is, she has us ask 'what if we accept $H$, and $H$ is false?' and 'what if we reject

$H$, and $H$ is true?', but not 'what if we accept $H$, and $H$ is *true*?' This is quite deliberate, not just an accident of focus or presentation:

> Valuation of the consequences of accepting a true theory (or rejecting a false one) are irrelevant; valuation of the consequences of accepting a false theory (or rejecting a true one) for scientists and society as a whole are not.[52]

But the first part of this sentence seems to me to be clearly false. Consider[53] into links between 'innate intelligence' and race, and consider a hypothesis $H$ that, say, people of African descent are, on average, innately significantly less intelligent than people of European descent. Following Douglas, we ask how much evidence we need to accept $H$. Because of the severe consequences of acting on $H$ *whether or not it is true*, we should require a great deal of evidence before accepting it. In this case, either we do not need to take into account the possibility that $H$ is false (the consequences of our belief being false would be trivial compared to the consequence of acting on the belief itself), or we need to take into account *both* the possibility that $H$ is false and the possibility that $H$ is true. Assuming these issues are addressed, however, I can give a sketch of science-as-practice that clearly identifies a connection between our ethico-political aims and the aims of scientific inquiry, as per the first key claim.

## 3. Science and deliberative democracy

3.1. **Some worries.** The critics of SAV will probably not be satisfied with the sketch I have given of science-as-practice. While I did give a brief argument that the picture Douglas gives us is not one on which science becomes nothing more than wish-fulfillment, it is still one on which science can be held hostage by 'special interests', whether polluting industries or political correctness. The critic starts by conceding Douglas' point, if only for the sake of argument: regarding a particular claim (say, the safety of BPA), the choice between different standards of evidence (say, $p < 0.1$ and $p < 0.05$) depends on ethico-political value judgments (say, how

[52]Douglas, 2006, 11

[53]Another relevant example would research into nuclear physics that would be useful for building weapons, mentioned above.

concerned we are about economic inefficiency and cancer). But then, she continues, the epistemic debate over the particular claim becomes a partisan dispute over ethico-political values. We cannot expect industry representatives (who, I suppose, are more concerned about economic inefficiency than an increase in the incidence of cancer, so long as both are small or moderate) and environmentalists (who, I suppose, are more concerned about cancer than economic inefficiency) to agree on what's more important, and hence what evidentiary standards we ought to adopt for judging the safety of BPA. At the end of the day, the decision to accept the claim or not will be a 'political' decision, in the most derogatory sense of political, and not actually based on reason or evidence.

This argument can be further developed in one of two ways. First, the critic may be arguing that our actual political institutions have been captured by 'special interests' and corrupt politicians, who will simply ignore countervailing evidence and reasons for the benefit of their wealthy benefactors. This is a cynical position, but not one based primarily on principle; this version of the critic should allow that reform (albeit possibly very difficult reform) of our political institutions would remove the difficulty.

More seriously, the second version of the critic argues that the sort of deadlock of conflicting value judgments is inevitable and hence that the decision to accept the claim concerning the safety of BPA is, on the sketch I provided in the last section, necessarily or as a matter of principle 'political' in the most derogatory sense. In particular, no matter how much or how good evidence we have supporting the claim that BPA increases the incidence of certain kinds of cancer, industry representatives can always simply insist that economic inefficiency is of such enormous importance that a more stringent threshold is needed.

I strongly feel that this more serious version of the objection is based on antiquated meta-ethical views – namely, Ayerean-Humean non-cognitivism, or some closely related version of relativism.[54] In particular, it seems that the critic must claim

[54]I don't think that philosophers of science who hold these meta-ethical views – and I stress that not everyone involved in the debate over SAV does – are especially intellectually culpable in that respect. The specialization of philosophy as a discipline simply means that philosophers of science do not regularly engage in issues in ethics, and vice-versa.

that the industry representatives' belief in the enormous importance of economic efficiency cannot be rationally criticized or subjected to debate, on the basis of a more general belief that ethico-political value judgments cannot be rationally criticized or subjected to debate. Without this claim, I can respond to the objection by arguing that the industry representatives are being irrational (at best) in holding economic efficiency to be so much more important than public health.

Assuaging the critic of SAV therefore requires developing an alternative meta-ethics, one on which ethico-political value judgments *are* subject to rational criticism and debate. However, such a project, at this broad level of description, would be too ambitious for me to undertake here. Instead, using the framework of science-as-practice, I will focus on how the determination of the internal goods and standards of scientific inquiry can be subject to rational criticism and debate. That is, I will articulate and defend the second key claim.

I have already given a preliminary account of the internal goods and standards of scientific inquiry. The critics' response has shown that this account is, indeed, preliminary: it does not have enough detail to actually be useful in, say, designing a research program, or assessing its success afterwards. Operationalizing my account therefore requires further developing it in at least two ways, which I will call *specification* and *connection*. By 'specification' I mean the activity of articulating vague, open-ended, or general concepts with more specific, determinate, or specific concepts.[55] For example, the aim of 'theoretical knowledge' can be specified by replacing it with an aim of 'methodologically robust data regarding the claim that BPA increases the incidence of certain kinds of cancer such that $p < 0.01$'. By 'connection' I mean the activity of identifying relations of promotion or interference between the internal goods and standards of one practice (whether general or specified) with those of other practices, including ethico-political aims. For example, the specified aim regarding BPA of the last example can be related (in a fairly direct way) to promoting our aim of reducing the incidence of cancer, and interfering (in a more remote way) with the aim of increasing economic efficiency.[56]

[55]Cf. Richardson, 2002, 104-5.

[56]These two activities are closely related to the two normative or evaluative functions of internal goods and standards I identified above. The first of these functions was an internal or instrumental

Complete specification and connection for all of scientific inquiry – or even one particular research program – is inappropriate here, for several reasons. First, it would simply be an enormous undertaking, much more than I could accomplish on my own in a couple years. Second, and more importantly, these activities are highly context-sensitive. Specification in terms of particular statistical methods, for example, presupposes that the society in question has the theoretical and practical knowledge of those methods. Similarly, identifying a connection between two practices only makes sense in a social context where at least one of the two is seen as important. For example, connecting theoretical knowledge with knowledge of God's providential ordering of the world only makes sense in a social context with certain background theological assumptions, and mostly does not make sense in the context of contemporary secular Western societies.[57]

Instead of a complete specification and connection, in this third part of the dissertation I will give an account of how public deliberation can engage in specification and connection. To borrow some jargon from political philosophy, instead of further developing a substantive account of the aims of scientific inquiry, I will instead give a procedural account, examining how a given community can determine for themselves what counts as scientific success. This account will draw heavily on recent work on deliberative democracy, especially that of Henry Richardson[58] and Joshua Cohen, as well as looking back to Dewey's political philosophy for inspiration. Taking this approach addresses the concerns of the last paragraph. First, identifying the features of productive deliberation – especially in a way that lends itself to constructive criticism of existing institutions – is easier than developing a complete specification and connection. Second, the procedural account provides normative

evaluation of a particular institution: how well does this lab achieve its research aims? Answering this question – and hence evaluating an institution – requires specification of the internal goods and standards. Vague, open-ended, or general concepts like 'theoretical knowledge' are too vague, open-ended, and general to actually assess a concrete institution. The specified internal goods and standards – as the example I gave above – by contrast, are operationally useful. The second function was to ask, from an external perspective, whether a given practice was worth pursuing at all. This function is served in part by specification – it's easier to assess the value of more specifically-identified goods – but first and foremost involves connection, identifying how the proposed practice relates to our other practices.

[57]Cf. Habermas, 1968/1971, appendix, and Kitcher, 2001, 67

[58]Richardson, 1994; Richardson, 1998; Richardson, 2002

constraints on specification and connection, but flexible constraints that allow for contextual variation.

This part of the proposal is more programmatic than the others, as I am less familiar with the literature on deliberative democracy than the literature I have discussed in the other parts. Still, the view I have begun to develop both draws on and stands in contrast with the views of others. In the next three subsections, I will briefly discuss the work of some proponents of deliberative democracy, and explain how my view differs from theirs.

3.2. **Henry Richardson and John Dewey.** My first two deliberative democrats are John Dewey and Henry Richardson. Public deliberation over means and ends is at the center of Dewey's meta-ethics and moral epistemology, and his political philosophy is deeply democratic.[59] More concretely, Dewey's meta-ethics look, in a paragraph, something like this:[60] We are initially confronted with some objective disruption to our existence and activities (a 'problem-situation' or 'problematic scenario'). Where other organisms may react automatically (as in the case of plants) or on the basis of instinct (as in the case of lizards), human beings (like many other mammals) have the ability to interrupt immediate response to the disruption and engage in reflective deliberation. As a first result of this deliberation, we identify several proposed courses of action. Each of these courses of action consists of, first, an action or series of actions on our part, and an anticipated end-state, or what Dewey calls an end-in-view. We then evaluate each course of action by considering two empirical claims about it:

(a) The action or series of actions is likely to result in the end-in-view.

[59]Dewey's major works on ethics, meta-ethics, and political philosophy include: *Lectures on Ethics, 1900-1901* (ed. Koch, published 1991); 'The evolutionary method as applied to morality', MW 2; 'The logical conditions of a scientific treatment of morality', MW 3; *How we think*, MW 6; 'The logic of judgments of practice', MW 8; *Democracy and education*, MW 9; *Reconstruction in philosophy*, MW 12 (esp. ch. XX); 'Valuation and experimental knowledge', MW 13; *Human nature and conduct*, MW 14; 'Value, objective reference, and criticism', LW 2; *Ethics* (with James Tufts), LW 7; *The public and its problems*, LW 2; 'Three independent factors in morals', LW 5; and, most importantly, *Theory of valuation*, LW 13. There have been numerous books and articles written over the last several decades on Dewey's ethics and political philosophy, including Anderson, 2005/2008; Campbell, 1995, ch. 4; Welchman, 1995; Putnam, 1995a; Putnam, 1995b; West, 1989; Gouinlock, 1972; and Bernstein, 1966.

[60]The sketch of the rest of this paragraph is, in essence, a summary of *Theory of valuation*.

(b) The problematic feature of the problem-situation is not part of the end-in-view.

That is, we determine empirically[61] whether the series of actions can reasonably be expected to alleviate the problem. Of course, the end-state will not be perfect and eternal. Our actions are liable to lead to new problems – I can successfully make it up the tree, pick the apple, and eat it, thereby relieving my hunger, but then I will need to get back down without injuring myself. We therefore must also consider a third empirical claim, which I think can best be presented as something like the following:

(c) The problematic features of the end-in-view are less problematic than those of the current problem-situation.

That is, we evaluate a course of action by considering its ability to relieve a given problem (its end), but also the other effects this course of action is liable to have. By examining its end and effects, we thereby evaluate each end-in-view of a proposed course of action as being itself a 'means', in the sense of a medium or middle state, between our current and future situations. This trick of empirically evaluating a proposed end by considering it as a means Dewey calls the *continuity of means and ends*, and it is the central move in his response to non-cognitivism. This approach to rationally evaluating ends should sound familiar; it is roughly the account of connection or 'external' evaluation of practices I sketched above.

Henry Richardson has taken up Dewey's meta-ethics, and used it to develop his own approach to deliberative democracy.[62] On Richardson's account, public deliberation is, in the first instance, 'truly collective reasoning about public ends', in that it 'involves multiple citizens attempting to forge joint intentions that will determine institutional decisions'[63]. The product of this collective reasoning is a conception of the 'public good' that can be used to guide public policy and collective action, 'a view about how public action should be regulated'[64].

[61]The qualification is important in context, where Dewey's primary foil is Ayer's empiricist-motived non-cognitivism.
[62]See, for example, Richardson, 2002, 121-2.
[63]Richardson, 2002, 19
[64]*Ibid.*, 40

Richardson does not borrow from Dewey uncritically, however. He argues that Dewey was ambiguous about the legitimacy of *final ends*, ends that are pursued for their own sake (as opposed to being pursued only for the sake of some other end), and was wrong to reject *ultimate ends*, ends that are pursued only for their own sake.[65] Here Richardson and I disagree. Dewey's ambiguity about final ends and rejection of ultimate ends is due, most importantly, to the continuity of means and ends. Treating some proposed end as ultimate makes it, we might say, discontinuous with other ends: something that is to be pursued only for its own sake cannot be evaluated as a means for achieving other ends. Similarly, we cannot determine whether to treat a proposed end as final or not by considering its connection to other ends; at the very least, Dewey's meta-ethics does not seem to provide a method for assessing final ends *qua* final.

Richardson's primary foils in his account of deliberative democracy come from welfare economics and social choice theory approaches to the public good. These approaches deny the value of public deliberation in producing the public good. In the spirit of Bentham, they consider the use of formal methods to aggregate or collect together lists of prior individual preferences into an overall list of group preferences, which is then identified with the public good. Hence public deliberation is superfluous, and plays no role in constructing or developing the public good; at best, it's just a useful method for identifying the independently existing public good.

Importantly, the individual preferences are taken to be fixed prior to the aggregation procedure; individuals cannot, or cannot normally, compromise, in a 'deep'[66] way that involves modifying their preferences. These approaches are therefore related to the meta-ethical views I identified earlier in this section – namely, the views that implied that ends (and other ethico-political value judgments) are not subject to rational deliberation and debate. Richardson's criticisms of welfare economics and social choice theory, and his contrasting account of deep compromise and institutions for deliberative democracy, should therefore be straightforwardly useful in responding to the critic of SAV.

[65]Richardson, 1998
[66]Cf. *ibid.*, ch. 11

3.3. **Joshua Cohen.** The second proponent of deliberative democracy that I will consider is Joshua Cohen. Cohen has written an enormous amount on deliberative democracy[67]; I focus here on just one germinal article, 'Deliberation and democratic legitimacy'[68]. In this piece, Cohen identifies five features of a 'formal conception' of deliberative democracy[69] and four of an 'ideal deliberative procedure'[70]. Like Richardson, Cohen rejects an assumption that individual preferences are fixed; deliberative democracy is 'likely to require a willingness to revise one's own understanding of one's own preferences and convictions'[71].

However, Cohen's account of deliberative democracy is far too sweeping for my purposes, in several respects. First, it is comprehensive, in that it deals with all aspects of the social order (including, as I read him, the institutions for deliberation themselves): 'deliberative arenas which are organized exclusively on . . . issue-specific lines are unlikely to produce the open-ended deliberation required to institutionalize a deliberative procedure'[72]. My target is much narrower, dealing only with scientific inquiry. Second, on similar grounds, Cohen requires that deliberation be, in a sense, presuppositionless. Two features of his ideal procedure, for example, are that the 'consideration of proposals is not constrained by prior norms or requirements' and that '[t]he participants . . . do not regard themselves as bound by the existing system of rights'.[73] 'Communitarians', including Alasdair MacIntyre, Charles Taylor, and Michael Sandel[74] have argued that practical reason cannot operate in this presuppositionless way, and MacIntyre has related this point to the views of Kuhn and Michael Polanyi in the philosophy of science[75].

Above, I argued that public deliberation about science should engage in the activities of specification and connection, and I argued that these had a contextual

[67]His website CV lists 78 articles, including works in progress. A significant percentage of these deal with deliberative democracy.
[68]Cohen, 1997
[69]*Ibid.*, 72-3
[70]*Ibid.*, 74-5
[71]*Ibid.*, 75
[72]*Ibid.*, 85
[73]*Op. cit.*
[74]None of these writers claim the label 'communitarian' for themselves; hence the scare quotes. For Sandel's reasons for rejecting the label, see Sandel, 1994, §I.
[75]MacIntyre, 1978

aspect: facts about the social and scientific status quo can and should be taken into account when we deliberate over the aims of scientific inquiry. This means that this deliberation cannot be presuppositionless in the way that Cohen requires. Hence, my account will differ from Cohen's by incorporating the work of 'communitarian' critiques of liberalism. Overall, I will have to find a way to balance the willingness to compromise and revise one's ends (vital for deliberative democracy) with the rejection of presuppositionless deliberation.

3.4. **David Estlund.** Finally, I turn to David Estlund's work on the epistemic aspects of deliberative democracy.[76] One of Estlund's primary concerns – and the one that is relevant for my project – is whether deliberative procedures successfully produce truth (for example, whether a determination that a certain public policy is just is true, ie, that the policy is indeed just), and how this question is related to the justification of the procedure and the status of experts. On one view, which he calls Rational Deliberative Proceduralism, this need not be the case: Rational Deliberative Proceduralism does 'not claim that the procedure produces outcomes that (tend to) approximate some standard . . . that is independent of actual procedures'[77]. Estlund rejects this view, arguing that it cannot give an account of 'better reasons', as such presuppose 'procedure-independent standards'.[78] Estlund's alternative to Rational Deliberative Proceduralism, which he calls Epistemic Proceduralism, does not appear to have a clear definition, but presumably he thinks that a procedure for public deliberation will be at least partly justified (or not) by its ability to arrive at the truth (or not). In later work, he asks whether the well-educated and knowledgeable, as technocrats or 'epistocrats', should be given more political power than the less-educated and ignorant, on the grounds that the former will be able to run things better than the latter.

The critic of SAV may raise similar worries for my account. First, she may worry that public deliberation on the ends and means of scientific inquiry will result in the devaluation of truth and 'pure' research, in favor of more immediately useful research. If a research program can only be valued for its contribution to other

[76]Estlund, 1997; Estlund, 1998; and Estlund, 2003
[77]Estlund, 1997, 179
[78]*Op. cit.*

goods, as in my reading of the continuity of ends and means, then research into fundamental physics, say, that does not have much of a contribution to make, will turn out to be less valuable, and consequently the pursuit of such knowledge will be neglected. Or, more radically, what is to prevent the internal good of 'theoretical knowledge' from being specified in such a way that it does not include truth at all?

Second, why should we turn the issue of the ends and means of scientific inquiry over to the public at large at all? The vast majority of the citizenry are profoundly scientifically ignorant. At best, they will simply dumbly go along with the decisions made by scientific experts – actual practicing scientists themselves. Or worst, if given a large degree of control over determining the course of scientific inquiry, they will send it off in trivial or dangerous directions.

I don't yet have good responses to these worries. But Estlund's work, in the context of deliberative democracy, gives me a way to take them up, in the context of philosophy of science. Indeed, I expect that working on these problems in the context of philosophy of science will identify weaknesses with Estlund's own account. The result may be a productive synthesis of political philosophy and philosophy of science.

## 4. An application: Institutions for bioethics

Putting the last two parts together, I believe that I can give an adequate articulation and defense of the two key claims I identified at the end of the first part. Of course, following Dewey, we should think that the real test of a piece of philosophy is its ability to solve problems. And, in this final section, I am to do just that, by using the tools I have assembled in the first three parts of the dissertation to identify some criteria for an institution for public deliberation over bioethics.

Raymond Geuss has recently argued[79] that liberal political theory has adopted an approach that he, following Rawls, calls *ideal theory*. In ideal theory, one first uses a relatively small set of principles to sketch a picture of the perfect, or ideal, just society, and then tries to act in the world in such a way so as to bring about this society, or at least get as close as possible. Geuss goes on to point out that this

[79]Geuss, 2008

philosophical method is profoundly inadequate to the problems faced by a society as dynamic as ours. For example, scientific innovations bring about dramatic changes in almost all aspects of our lives on a semi-regular basis. The internet has dramatically changed the way we communicate and coordinate activity with our friends, family, and coworkers (not to mention creating novel forms of activity, such as blogging). Over the last few decades, and most likely for the next several as well, biology and biotechnology has had and will have an even more profound impact, challenging our notions of personhood and creating ethical debates over reproductive technologies, cloning, and the genetic modification of non-human (and human) organisms. The vision of Utopia created by an ideal theory, Geuss argues, is too distant from our messy and dynamic reality to be of any use in actually guiding action.

Geuss then appears (from my brief look at his book) to go on to treat this criticism of ideal political philosophy as a criticism of *liberal* political philosophy. But this is too hasty: Dewey and Richardson are both liberal political philosophers, and neither adopts the method of ideal theory. They embrace both the messiness of reality and the dynamism of contemporary human society, including as that dynamism is driven by scientific innovation. Rather than ideal theory, we could say that they attempt to develop *melioristic theory*, theory that is designed to address the most pressing problems we face today, with no expectation that the solutions will be final or work for all time.

Certainly SAV should have similar aims and ambitions – it should be construed as melioristic philosophy of science. So we can test my approach by applying it to one of the most pressing problems we face today in science policy. And the problem I want to tackle here are the new controversies in bioethics, such as those mentioned a few paragraphs ago. As in the last part, I do not want to do this by directly taking up questions like 'Should we clone human beings?' First, because answering this sort of question tells almost nothing about what we should do when it comes to cloning. An answer won't tell us, for example, who we should or may clone, or for which reasons, or under which circumstances, or in which institutions. Second, because answers to both the should-question and proposals for dealing with the

issues of the last sentence are, in a liberal democracy, best developed as a product of public deliberation.

Instead, I will take up the question 'What features should our institutions for the public deliberations about human cloning have?' My highly provisional answer is that these institutions should have the following features:

- They should have the power to allocate funding directly to research programs, and a budget drawn from an appropriate source of funds.
- They should, collectively, have the power to regulate research funded by other institutions (private businesses, for example).
- The participants of these institutions should include a wide variety of experts – including scientists, physicians, lawyers, philosophers of science, and ethicists – as well as politicians (or other representatives of the citizenry at large) and private citizens (as representatives of social groups who take a particular interest in these issues). More generally, the institutions should aim to be as inclusive of all relevant views as possible while still able to engage in productive deliberation.
- The issues these institutions deliberate over should range from the very general (should there be a blanket ban on human cloning?) to the very specific (should we fund this particular research program?) and even the epistemic (is the evidence sufficiently strong to support the hypothesis that this particular method of human cloning is safe?).
- Deliberation within these institutions should endeavor to achieve a reasonable consensus whenever possible but should also have concrete mechanisms – such as a majority vote – by which proposals can be adopted despite a persistent reasonable dissensus. Participants should be open to the possibility of deep compromise, revising their ends for the sake of a reasonable consensus.
- The activity of participants in these institutions should be governed by both implicit and explicit norms of intellectual honesty and charity.

This list of features need not be read as a piece of useless ideal theory. It is not a picture of how science policy will be done in a perfect world, but instead a resource for generating constructive criticism and identifying specific ways to improve institutions that we already have. In particular, it can be used to critically assess the President's Council on Bioethics (PCB), and offers specific recommendations for reform. For example, almost none of the members of the PBC are scientists actively involved in the areas of research with which the PCB deals, all members have advanced degrees, and all had to be appointed personally by the President. During the Bush Administration, little effort was made to make the PCB inclusive of all relevant views. This inadequacy has an obvious remedy: the PCB, or a successor institution, should be opened to a wider range of participants.

If these critiques can be developed, then my account can also be used to generate useful conclusions in social and political philosophy. Optimistically, it can also be extended and easily adapted to other problems. But I think that what I have laid out in this proposal is sufficiently ambitious for now.

## References

Anderson, E. (2004, Winter). Uses of value judgments in science. *Hypatia*, *19*(1), 1-24.

Anderson, E. (2005/2008). Dewey's moral philosophy. In E. N. Zalta (Ed.), *The Stanford encyclopedia of philosophy.* Available from `http://plato.stanford.edu/archives/fall2008/entries/dewey-moral/`

Anderson, E. (2006). *How not to criticize feminist epistemology: a review of* Scrutinizing feminist epistemology. Available from `http://www-personal.umich.edu/{~{}}eandersn/hownotreview.html`

Bernstein, R. (1966). *John Dewey.* Washington Square Press.

Biddle, J. (2006). *Why transient underdetermination matters: Values and social organization in Kitcher's* Science, truth, and democracy. Presentation at the 2006 Philosophy of Science Association biennial meeting.

Bohman, J., & Rehg, W. (Eds.). (1997). *Deliberative democracy: Essays on reason and politics.* MIT Press.

Brown, M. (2008). *Scientific significance and genuine problems: Fundamental troubles in Kitcher's social theory of science.* Presentation at the 2008 Philosophy of Science Association biennial meeting.

Campbell, J. (1995). *Understanding John Dewey.* Open Court Publishing Company.

Carden, S. (2006). *Virtue ethics: Dewey and MacIntyre.* Continuum international publishing group.

Carrier, M. (2004). Knowledge gain and practical use: Models in pure and applied research. In D. Gillies (Ed.), *Laws and models in science* (p. 1-17). King's College Publications.

Carrier, M. (2007). *Theories for use: On the bearing of basic science on practical problems.* Presentation at the 2007 European Philosophy of Science Association conference.

Carrier, M. (2008). Science in the grip of the economy: On the epistemic impact of the commercialization of research. In M. Carrier, D. Howard, & J. Kourany (Eds.), *The challenge of the social and the pressure of practice: Science and values revisited* (p. 217-34). University of Pittsburgh Press.

Carrier, M., Howard, D., & Kourany, J. (Eds.). (2008). University of Pittsburgh Press.

Cohen, J. (1997). Deliberation and democratic legitimacy. In J. Bohman & W. Rehg (Eds.), *Deliberative democracy: Essays on reason and politics* (p. 67-92). MIT Press.

Cunningham, T. (2009). *To save the semantic view.* Unpublished draft posted to the PhilSci archive.

Delaney, C. (1972). The changing temper of American philosophy. *The review of politics*, *34*(4), 129-37.

Douglas, H. (2000). Inductive risk and values in science. *Philosophy of science*, *67*(4), 559-79.

Douglas, H. (2006). Norms for values in scientific belief acceptance. *PSA*.

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

Duhem, P. (1908). *La théorie physique: Son objet et sa structure*.

Estlund, D. (1997). Beyond fairness and deliberation: The epistemic dimension of democratic authority. In J. Bohman & W. Rehg (Eds.), *Deliberative democracy: Essays on reason and politics* (p. 173-204). MIT Press.

Estlund, D. (1998, January). The insularity of the reasonable: Why political liberalism must admit the truth. *Ethics*, *108*(2), 252-75.

Estlund, D. (2003). Why not epistocracy? In *Desire, identity and existence: Essays in honor of TM Penner* (p. 53-69). Academic printing and publishing.

Geuss, R. (2008). *Philosophy and real politics*. Princeton University Press.

Giere, R. (2003, January). A new program for philosophy of science? *Philosophy of science*, *70*, 15-21.

Giere, R. (forthcoming). An agent-based conception of models and scientific representation. *Synthese*.

Gouinlock, J. (1972). *John Dewey's philosophy of value*. Humanities press.

Haack, S. (2003). Knowledge and propaganda: Reflections of an old feminist. In C. Pinnick, N. Koertge, & R. Almeder (Eds.), *Scrutinizing feminist epistemology: An examination of gender in science* (p. 7-19). Rutgers University Press.

Habermas, J. (1968/1971). *Knowledge and human interests*. Beacon Press.

Hands, D. (2005). You want the social? You can't handle the social! Mirowski on the secret history of scientific philosophy. *Studies in the history and philosophy of science A*, *36*(4), 726-33.

Hankinson Nelson, L. (1990). *Who knows: From Quine to a feminist empiricism*. Temple.

Howard, D. (2003). Two left turns make a right: On the curious political career of North American philosophy of science at midcentury. In G. Hardcastle & A. Richardson (Eds.), *Logical empiricism in North America* (Vol. XVIII, p. 25-93). University of Minnesota Press.

Howard, D. (2009, February). Better red than dead – Putting an end to the social irrelevance of postwar philosophy of science. *Science and education*, *18*(2), 199-220.

Intemann, K. (2005, December). Feminism, underdetermination, and values in science. *Philosophy of science*, *72*, 1001-12.

Kindcaid, H., Dupré, J., & Wylie, A. (Eds.). (2007). *Value-free science?* Oxford University Press.

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

Koertge, N. (2003). Feminist values and the value of science. In C. Pinnick, N. Koertge, & R. Almeder (Eds.), *Scrutinizing feminist epistemology: An examination of gender in science* (p. 222-33). Rutgers University Press.

Kourany, J. (1998). A new program for philosophy of science, in many voices. In J. Kourany (Ed.), *Philosophy in a feminist voice: Critiques and reconstructions* (p. 231-62). Princeton.

Kourany, J. (2003). A philosophy of science for the twenty-first century. *Philosophy of science*, *70*, 1-14.

Lacey, H. (2005). *Values and objectivity in science, and current controversy about transgenic crops*. Lexington Books.

Longino, H. (1990). *Science as social knowledge*. Princeton.

Longino, H. (1995, September). Gender, politics, and the theoretical virtues. *Synthese*, *104*(3), 383-97.

Longino, H. (1996). Cognitive and non-cognitive values in science: Rethinking the dichotomy. In L. Hankinson Nelson & J. Nelson (Eds.), *Feminism, science, and the philosophy of science* (p. 39-58). Kluwer.

Longino, H. (2002a). *The fate of knowledge*. Princeton.

Longino, H. (2002b). Science and the common good: Thoughts on Philip Kitcher's *Science, truth, and democracy*. *Philosophy of science*, *69*, 560-8.

Machamer, P., & Douglas, H. (1999). Cognitive and social values. *Science and education*, *8*, 45-54.

MacIntyre, A. (1967). Review of *Philosophy of social science* by Richard Rudner. *British journal for the philosophy of science*, *18*, 344-5.

MacIntyre, A. (1977). Epistemological crises, dramatic narrative and the philosophy of science. *The monist*, *60*, 453-72.

MacIntyre, A. (1978). Objectivity in morality and objectivity in science. In H. T. E. Jr. & D. Callahan (Eds.), *Morals, science and sociality* (p. 21-39). Hastings Center.

MacIntyre, A. (1979). Medicine aimed at the care of patients rather than what . . . ? In E. Cassell & M. Siegler (Eds.), *Changing values in medicine* (p. 83-96). University publications of America.

MacIntyre, A. (1984). *After virtue* (Second ed.). Notre Dame.

MacIntyre, A. (1986). Positivism, sociology and practical reason: Notes on Durkheim's suicide. In A. Donagan, J. Anthony Perovich, & M. Wedin (Eds.), *Human nature and natural knowledge: Essays presented to Marjorie Grene on the occasion of her seventy-fifth birthday* (p. 87-104). Reidel.

MacIntyre, A. (1987). Post-Skinner and post-Freud: Philosophical causes of scientific disagreements. In H. T. E. Jr. & A. L. Caplan (Eds.), *Scientific controversies: Case studies in the resolution and closure of disputes in science and technology* (p. 295-311). Cambridge University Press.

McCumber, J. (2001). *Time in the ditch: American philosophy and the McCarthy era*. Northwestern University Press.

Neurath, O. (1983). The lost wanderers of Descartes and the auxiliary motive. In R. Cohen & M. Neurath (Eds.), *Philosophical papers 1913-1946.* D. Reidel publishing company. (Original work published 1913)

Norton, J. (2008). Must evidence underdetermine theory? In M. Carrier, D. Howard, & J. Kourany (Eds.), *The challenge of the social and the pressure of practice: Science and values revisited* (p. 17-44). University of Pittsburgh Press.

Pinnick, C., Koertge, N., & Almeder, R. (Eds.). (2003). *Scrutinizing feminist epistemology: An examination of gender in science*. Rutgers University Press.

Putnam, H. (1995a). Pragmatism and moral objectivity. In M. Nussbaum & J. Glover (Eds.), *Women, culture, and development* (p. 199-224). Oxford University Press.

Putnam, H. (1995b). *Pragmatism: An open question*. Blackwell publishing.

Reisch, G. (2005). *How the Cold War transformed philosophy of science: To the icy slopes of logic*. Cambridge University Press.

Richardson, H. (1994). *Public reasoning about final ends.* Cambridge University Press.

Richardson, H. (1998). Truth and ends in Dewey's pragmatism. *Canadian journal of philosophy (Supplement)*, *24*, 109-47.

Richardson, H. (2002). *Democratic autonomy.* Oxford University Press.

Rudner, R. (1953). The scientist qua scientist makes value judgments. *Philosophy of science*, *20*(1), 1-6.

Sandel, M. (1994, May). Review of *Political liberalism. Harvard law review*, *107*(7), 1765-94.

Suárez, M., & Cartwright, N. (forthcoming). Theories: Tools versus models. *Studies in the history and philosophy of modern physics*.

Uebel, T. (2005). Political philosophy of science in logical empiricism: the left Vienna Circle. *Studies in the history and philosophy of science A*, *36*(4), 754-73.

Welchman, J. (1995). *Dewey's ethical thought.* Cornell University Press.

West, C. (1989). *The American evasion of philosophy.* University of Wisconsin Press.