DEMOCRATIZING NANOTECHNOLOGY: INTERSECTING THE PHILOSOPHY OF SCIENCE AND SCIENCE POLICY

by

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April 2007

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Abstract: This thesis is an examination of the relevance of the philosophy of science to science policy, focused around an analysis of philosopher Philip Kitcher's 2001 *Science, Truth and Democracy*. I consider and draw upon Kitcher's work from a variety of perspectives. Initially, I use Kitcher's work to argue for a more democratic approach to nanotechnology policy. The thesis then examines the philosophical changes in position that seemingly enabled Kitcher to embrace a democratic position on science policy. Kitcher's work is placed in context of the history of the philosophy of science, where I suggest that Otto Neurath might serve as a better model for engagement with science policy. I evaluate Kitcher in terms of criticisms from science policy scholars, and indicate that there are other current philosophers whose work could be more fruitful. My thesis argues that Kitcher should not become the model for philosophers of science to engage in science policy. Kitcher's justification of democratized science and his ideal of well-ordered science represent a worthwhile and general approach for interfacing philosophy of science and science policy, but the value of further work here is not clear. More particular roles of engagement need to be developed for philosophers of science in science policy.

Outline of the Thesis:

Prologue: Democratizing Nanotechnology

This thesis project began as an attempt to study the history of the 21st Century Nanotechnology Research and Development Act and its specific provisions mandating research on the societal and ethical implications of nanotechnology (SEIN). The SEIN legislation indirectly led to the creation of the Center for Nanotechnology in Society (CNS) at Arizona State University, which has sponsored my work on this project and provided me a conceptual framework to better understand questions of science policy. From the work of Ira Bennett and Daniel Sarewitz (2006), I began to realize that the SEIN provisions in the act arose independently of the academic science policy to which I had been exposed at CNS-ASU. My aspirations to write a historical analysis of SEIN's origins diminished as I worked on my 2006 article, "Nanotechnology: Constructing a Proactive Science Policy Toward Democracy," where I became more interested in the social and philosophical importance of democratizing science policy.

As an aspiring philosopher of science, my focus for this thesis began to shift alongside my growing interest in democratizing science. I soon realized that the larger "movement" to democratize science was often motivated by concerns that were unrelated to philosophical studies of science. The democratization of science movement today emerges from academic programs begun decades ago by sociologists of science within what has become the discipline of science and technology studies. Among their central insights is the idea that values influence human decisions about technology and that the resulting technological systems influence human lives, for better or for worse. In turn, an imperative for including more voices in the scientific and technological enterprise has been established. This is one of many possible motivations for democratizing science, and the idea has its critics. Even at CNS-ASU, there is no consensus about the desirability of democratized science, but my 2006 article argued that CNS-ASU's social scientific approach offered some valuable tools, regardless of any disagreement about the ultimate goal.

As will become clear, I have been deeply impressed by philosopher Philip Kitcher's 2001 *Science, Truth and Democracy*. It was my first exposure to philosophy of science that had an agenda for social change, which resonated with me personally. I originally became drawn to philosophy coursework as an engineering major because of my desire to think at the level of ideas and to debate about them. Philosophy of science also allowed to me to continue a natural passion that I have for the sciences. Originally, I likely overvalued Kitcher's work, carried away by my interest in an intellectual advocacy for social change. Part of why Kitcher's work is important is because it hopes to serve as a discussion point, through the ideal of well-ordered science, for exploring the democratic guidance of science and the ethical aspects of science policy. However, given the larger STS democratization movement, it is clear that Kitcher offers just one of several possible justifications for the democratization of science; there are also alternative philosophical ways to motivate and implement democratic changes to science policy. Given this variety of options, I am deeply interested in the value of philosophy in these issues of social change. In what sense is it pragmatically effective to use the philosophy of science to advocate the democratization of science? Are there particular benefits for philosophers of science to be involved in the debate, or for science policymakers to have added philosophical insight?

Introduction:

Science has played an important role in shaping the course of human history. But what, exactly, is the role of science in society? And what should it be? There have been some recent calls to develop a philosophical approach to fruitfully addressing these questions (Mitcham and Frodeman 2004). Philosophy is not a monolithic entity; instead there is an incredible diversity in the form and the content of the many philosophical traditions. The term "analytic philosophy" is often meant to demarcate a technical approach to philosophy which, in the eyes of many, seemingly detaches philosophy from traditional questions about humanity (for example, the meaning of life, the nature of existence). On the subject of science, the divide between analytic and continental philosophy of science has been explored in some detail[1]. While the distinction between analytic and continental philosophy is often a conceptual over-simplification, the term analytic philosophy of science serves to highlight a specific academic community of professional philosophers[2]. However, when presented with common questions about

[1] Many cite Michael Friedman's 2001 *A Parting of the Ways* for a discussion of one origin of the divide between analytic and continental philosophy. Friedman examines the break between Heidegger and Carnap, with particular focus on Carnap's article "Overcoming Metaphysics". Carnap focused on Heidegger's claim, "the Nothing Noths," and held the claim to be a logical violation of nothingness as Heidegger defines it. For Heidegger, the focus on logical rules is misplaced as he is attempting to redirect attention towards a more fundamental concern for the finitude of being. Carnap's influence on the subsequent development of American philosophy generally led to a more technical and rigid analytical approach that was often connected to science. Consequently, Heidegger's approach to science and technology (1977), which focuses on exploring the essence of technology and ensuring human liberation from techne, could seem to be more politically inclined and suited to answering questions about the relationship of science and society. There is other literature that focuses on developing a continental philosophy of science and technology [Gutting 2005, Mitcham 1994]. My discussion of Reisch (2005) in FN 1 calls into question this stereotypical understanding of Carnap. I owe my understanding of Friedman's argument to the 2005 ASU 'A Parting of the Ways' Reading group.

[2] According to Brian Leiter of the influential philosophicalgourmet.com [http://www.philosophicalgourmet.com/analytic.asp], analytic philosophy is a 'style' that

what role science has in policy and in society (much less what role it *should* have), the applicability of the philosophy of science is not at all clear. Some might even go so far as to say it is nonexistent[3].

While philosophers have largely avoided the question, the academic field of science and technology studies (STS) has made both descriptive and normative evaluations of the role of science in society. From the community of STS, a movement has emerged that is aimed towards the democratization of science[4]. According to David Guston,

> What democratization does mean, in science as elsewhere, is creating institutions and practices that fully incorporate principles of accessibility, transparency, and accountability. It means considering the societal outcomes of research at least as attentively as the scientific and technological outputs. It means insisting that in addition to being rigorous, science be popular, relevant, and participatory.[5]

Throughout this project, the desirability of the democratization thesis will be implicitly advocated. Framing this discussion are recent efforts to put theory into practice, albeit in

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predominates in nearly all major philosophy departments within the United States, and is loosely noted for clarity and logical precision. Analytic philosophy doesn't exist as a coherent set of research programs or beliefs. More specifically, analytic philosophers of science might be identified by way of membership in the Philosophy of Science Association.

[3] For harsh criticisms of the philosophy of science, see Mirowski (2003). Mitcham and Frodeman 2004 discuss many of the limitations of current philosophy of science.

[4] For those who want a better description of how democratized science is defined in this thesis, see Appendix One.

[5] Guston (2004). By "democratization of science" movement, I am implying literature relevant to Guston (2004). That is, I am referring to Science and Technology Studies literature that affirms Guston's point, as well as the political theory literature that tries to focus on the same point. However one describes this literature, it's fairly clear that the analytic philosophy of science has been largely separate from this movement, particularly before Kitcher. A noted focus for the democratization of science movement is the LOKA institute, see www.loka.org. See Brown (2005) footnote 4 for a list of literature on the democratization of science. Democratization as articulated in this thesis engages with all aspects of Guston's definition save for the term "popular."

a limited capacity. At Arizona State University (ASU), the creation of a Center for Nanotechnology in Society (CNS) is a new experiment designed to test principles from the democratization of science literature. CNS-ASU aims to employ a form of real-time technology assessment, which will work to analyze scientific endeavors early on, and to provide the early information and reflection that is critical for democratic deliberation about science[6]. As will be discussed, there has been no substantial connection between philosophers of science and advocates of democratized science. But does the movement stand to gain anything from a hypothetical influx of philosophers of science who seek to engage science policy?

This thesis is an exploration of the ways in which the analytic philosophy of science can become valuable in work related to science policy. I focus on the democratization of science policy as an overarching theme representing large scale changes to science policy that can direct the scientific enterprise toward the production of better societal outcomes, but my analysis need not be limited to democratization alone. As a field, science policy can certainly proceed independently of academic philosophers of science. To some extent, this is a desirable division of labor in that it allows science policymakers to handle many of the global, practical and political problems faced by society[7]. But, as I will argue in the subsequent pages, some genuinely philosophical

[6] Guston and Sarewitz (2001). Pirtle (2006) tries to link the deliberative capacity of RTTA to the democratization of science movement. This paper is included as part of the present thesis.

[7] Philosopher of science Rudolf Carnap is noted for his description of the "division of labor" between philosophers and political advocates (Reisch 2005). He is often villainized for believing that philosophy is a soulless engagement with symbolic logic, but this caricature ignores his proposed division of labor. Carnap argued for a tripartite conception of philosophy including semantics, syntax and pragmatics. His admittedly abstract work in semantics and syntax is separate from larger questions about normative

understandings of science can have a tremendous impact on how one approaches questions of science policy. Science policy must often deal with expansive questions about why one research project should be funded above another (or above some sort of social program). Such debates can benefit from philosophical framing and analysis.

Columbia University philosopher of science Philip Kitcher's book *Science, Truth and Democracy* is perhaps the most prominent of recent attempts to try to frame and intersect the philosophy of science with science policy issues. His book contains a discussion of traditional issues within the philosophy of science that are structured to justify a more democratic approach to science policy. The second half of the book proposes an ideal for science policy, well-ordered science (WOS), that tries to articulate what science policy should look like. Kitcher was the first philosopher that I read with an interest in societal issues, but there are others whose work might be deserving of an equal level of prominence. I was originally quite taken with Kitcher's work, and saw his approach to philosophy as a powerful way to push for change. The understanding of Kitcher's work that is expressed below allowed me to appreciate his argument within a proper context, and the criticisms I surveyed made me realize that other approaches to philosophical engagement need to be developed.

My recent conclusion is that philosophy can play an important role at helping to enact social change, but by no means an authoritative one. As will become clear, I now think that Otto Neurath exemplifies a better attempt than Kitcher to make the philosophy

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ethics and the broader understanding of science. Carnap, however, does not assign any less importance to the pragmatics than to the logical questions that he examined. They are all equally important, and Carnap simply worked where his talents were greatest. Carnap showed his care for pragmatic concerns in his personal life; he was active politically on of science useful in a societal context. His Unity of Science movement – far from being a totalitarian program aimed at trying to reduce all of science down to physics (as critics allege) – was an effort to establish collaboration in the orientation of science towards the public good. Further, I believe that Rudolf Carnap's Principle of Tolerance (in conjunction with some arguments from Neurath) can be used to justify democratized science as effectively as – or more effectively than – Kitcher's *Science, Truth and Democracy* argument. Kitcher's position can be helpful in swaying epistemologically conservative thinkers to a more democratic perspective, but his eventual conclusion was largely visible in the workings of the 1930s logical empiricists (and likely others before them).

Other recent philosophers, such as Helen Longino and Miriam Solomon, have addressed these issues, and their work must be part of a fuller discussion of the intersection of philosophy of science and science policy. These other philosophers might not be subject to some of the criticisms addressed in this project, but I do not believe this undermines my choice to focus on Kitcher. Even accepting the criticisms of Kitcher, I think that one can still use his work, as I do here with nanotechnology, to argue for better science policies, and I believe that his ideal of well-ordered science can lay out important ethical landmarks that need to be considered more in the practice of science policy[8]. As I

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behalf of socialist causes, and was highly supportive of others efforts to enact political change (Reisch 2005, p 382).

[8] Partially because of Kitcher's command of the philosophy of science, the book has a key merit that it places the impetus for democracy within science itself, which critic Mark Brown acknowledges (2004). Kitcher's approach is also characterized as that of an ideal, or model, to be aspired to. This global model has severe limitations because it is hard to apply to specific circumstances, but Kitcher's work offers nuances that might be utilized in more practical capacities, such as his emphases on avoiding "brainwashing" of the

say later, such general work should not become the model for numerous philosophers to follow in engaging science policy; I suspect that philosophers now need to find more particular and practical roles than Kitcher plays. For myself, the search for such a role will continue beyond this project.

Summary of the Parts:

This analysis will begin with my first work related to Kitcher, a paper that I wrote for The Triple Helix, an Arizona State University undergraduate journal focused on science, society and law (Pirtle 2006). I discuss and highlight recent research on the societal and ethical implications of nanotechnology (SEIN) taking place at Arizona State University's Center for Nanotechnology in Society. By giving a short history of nanotechnology and its possible applications, I place current nanotechnology policy within the historical context of United States science policy. Given the insufficiencies of scientifically elite approaches to policymaking, I argue for the desirability of democratizing science by relying on Kitcher's *Science, Truth and Democracy* (*STD*). I argued that the research at CNS-ASU should be viewed as developing a social science complement for Kitcher's philosophical ideal.

The second and third parts of the thesis are a study of the way in which Kitcher uses traditional positions in the philosophy of science to establish a foundation for democratized science. I focus on his change of position from his earlier book, *The Advancement of Science*, which argued that the sciences might converge on an ultimate cache of explanations, serving to make some scientific research epistemically significant independent of any social or political influences. In *STD*, Kitcher shows that social

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public, to consider the global benefits and harms resulting from science, and to influences affect all research, and the importance of avoiding prejudiced outcomes from science serves as the moral imperative behind his democratic ideal of well-ordered science.

The fourth part of this thesis examines a snapshot from the history of the philosophy of science by focusing on George Reisch's book *How the Cold War Transformed Philosophy of Science*. An early split around the work of Thomas Kuhn led to much of the divide that exists between science and technology studies (STS, from which the democratization of science movement can be said to have emerged) and philosophy of science (which rarely, if ever, engaged democratrization until recently). While many within STS favor a more sociological approach to studying science, the perceived divide between STS and philosophy of science is not as great as was originally presumed. The oft maligned logical empiricists are more sophisticated than their STS critics (and even most of their own descendants) presume, and they had their own program for making the philosophy of science relevant to politics. The model for political engagement of the logical empiricists is the greatest ancestor of Kitcher's recent societal engagement; their actions and ideas can help to contextualize Kitcher's *STD* as well as provide insights for further developing Kitcher's ideal.

The fifth part of the thesis focuses on particular criticisms of Kitcher and of the philosophy of science more generally. Two particular STS criticisms, by political theorist Mark Brown and by economist Philip Mirowski, attack both the content of Kitcher's approach as well as the practical use of *STD* in interdisciplinary contexts. Kitcher himself criticizes some of the academic science policy literature, which he contends lacks the

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acknowledge changes in significance over time.

philosophical ideal that he is trying to elaborate. Understanding these mutual criticisms is a first step to generating a framework for possible future epistemic and practical collaborations between philosophers and advocates of democratization.

An appendix is also included to briefly establish how I perceive democratized science and why it might be valuable.

Part One: Nanotechnology: Constructing a Proactive Science Policy For Democracy

The following is a slightly adapted version of my 2006 article in The Triple Helix at Arizona State University, an undergraduate journal focused on science, society and law. This article examines current research on the societal and ethical implications of nanotechnology and argues, using the work of philosopher Philip Kitcher, that the research should be used to push for a more democratic approach to science.

Already crowned "the next industrial revolution," the emerging field of nanotechnology has the potential to remake society anew. Nanotechnology, if we so choose, also offers a second potential: to remake the scientific enterprise as we know it, by incorporating it within a new, democratic framework for science policy.

Given that science is a human good, how does society decide what outcomes science should achieve (Kitcher 2003)? No individual should be able to guide science toward their subjective 'good', and while scientists may know the best direction for their own individual research, the only mechanism worthy of directing the scientific enterprise as a whole is a democratic one. Developments within science can have a profound impact on society, and research on the societal implications of science can uncover many substantial interconnections. Changing the structure of science policies and funding can significantly impact the outcomes science achieves. To better allow for democracy to decide what good science should achieve, a serious and proactive assessment of the possible implications of scientific work is needed in order to make informed, democratic decisions (Guston 2004).

Democratizing science is about a systematic reform in science policy to incorporate public values in the science policy-making process, not about instituting a popular vote on what scientists should maintain. Recent opportunities have paved the way for a practical complement to the ideal of democratized science. New legislation to support nanotechnology mandates research into the societal and ethical implications of nanotechnology (SEIN). One very promising approach to SEIN is “Real-Time Technology Assessment” (RTTA) (Guston and Sarewitz 2001). If undertaken properly, RTTA-based research could be a limited but important first step toward the democratization of science because it seeks to cultivate an early assessment of scientific projects, and to apply a rigorous social science assessment in conjunction with practicing nanoscale scientists and engineers. Conventional science policy may yield good outcomes, but this is almost accidental, not intentional. By enabling a more democratized approach to science policy, we can aspire to better outcomes from science for society.

Nanotechnology: Science, Technology, and Policy-making:

Because it is new and less bound in tradition, nanotechnology is ripe for implementing a new approach to science policy and, through RTTA-driven SEIN, the drive to create the tools for a more democratic governance of science is underway. The creation of a new approach to science and science policy begins with a solid understanding of the science itself, and the basic science behind nanotechnology is fascinating. Nanotechnology is science and engineering work at the level of 10^{-9} meters, or 1/80,000th the width of a human hair, where the basic properties of materials are defined. Out of the total US public research and development budget of 135 billion dollars, the 2006 US nanotechnology research budget stands at a relatively impressive one billion dollars (AAAS 2006, NNI 2006). The research that is described by the word nanotechnology is broad in scope. Some focus on bionanotechnology, where there are remarkable new possibilities in the traditional fields of biology and medicine, such as the creation of nanoscale drug delivery modules that can be used to image and treat disease infections (Ferrari 2005). Materials nanotechnology groups have been working on the creation of new materials, some with ultra-high strength and low-weight properties that result from new nanoscale structuring (David 2004). Overall, a variety of nanoscale, cutting edge research programs from a mix of traditional disciplines such as materials science, microbiology, physics, semiconductor science and mechanical engineering are being relabeled as nanotechnology.

Like other "revolutions," nanotechnology has its own creation mythology, where the nano-dream began with famed physicist Richard Feynman. In his 1959 talk, "There's Plenty of Room at the Bottom", Feynman described a world where the entire Encyclopedia Britannica could be written on the head of a pin (Feynman 1959). The first popular use of the word nanotechnology came with Eric Drexler's 1986 book Engines of Creation. There, Drexler focused on the possibility for molecular nanotechnology, and detailed a vision for creating robotic control at the nano-level. His account did more than merely foretell a type of engineering; he speculated about potential societal outcomes of molecular nanotechnology. The most infamous of these scenarios is Drexler's depiction of self-replicating nanobots that could consume the world, made infamous by Michael Crichton's characterization of out-of-control nanobots in his sci-fi bestseller Prey (2002).

In Washington D.C, the political creation of nanotechnology's funding is also mythologized. Key policymakers like Mikhail Roco, and his colleague, William Bainbridge have lobbied for nanotechnology funding since the mid-1990's as part of a federal working group (McCray 2005). Their efforts reached their greatest success with the 2000 creation of the US National Nanotechnology Initiative, which was the first of several federal acts sponsoring nanotechnology. Henceforth, there would be a continuing ‘nano’ presence on the national scene, and Roco was named director of the National Nanotechnology Initiative. Political opposition to nanotechnology became organized during this time as well. The 2000 *Wired* magazine article “Does the Future Really Need Us?” by Bill Joy highlights the potential dual-use of nanotechnology, particularly regarding fears about the powers of molecular nanotechnology, as Drexler and Crichton have envisioned. Given the potentially catastrophic dangers of nanotechnology, Joy argued that the science might be too dangerous for human use, and that the research should be abandoned. Scientists affiliated with the National Nanotechnology Initiative battled to convince the public of the impossibility of molecular nanotechnology and other potentially dangerous nanotechnology developments. Despite all this, the debate largely glossed over Joy’s essential logic: if an emerging science could lead to harm to society, then it should be avoided (Bennett and Sarewitz 2007).

Many of these questions directly relate to a choice about what good science should achieve. The current state of nanotechnology science policy may be oriented toward the values of a few individuals. This is because one key mission of the US nanotechnology research and development program has been to establish the convergence of nanotechnology, biotechnology, information technology, and cognitive science (NBIC) as a central focus of science and technology (Roco and Bainbridge 2001). In a speech reflecting his personal views, nanotechnology policy entrepreneur William Bainbridge described this “convergionist approach” for nanotechnology, and described his two goals of personality enhancement and “personality capture,” where “information about a person’s mental and emotional functioning [is captured] into a computer…system” to create a simulation of a human (James Martin Institute 2006).

Although many might not associate nanotechnology with things such as human enhancement, significant National Science Foundation-sponsored nanotechnology research is working toward these NBIC goals (*14*). Many describe this pursuit of exceeding humanity's physical and social limitations as raising serious questions about the nature of human beings. Transhumanism, or the attempt to enhance human abilities and to potentially escape mortality, may be waiting in the wings behind particular developments in nanotechnology.

While transhumanism is seemingly welcomed and supported by science policy entrepreneurs like William Bainbridge and Mikhail Roco, the American public has yet to recognize that US science policy has been partially oriented toward establishing a nanotechnology enabled transhuman future (Roco and Bainbridge 2001). When one-third of all US science and technology research is funded by the government, often setting the precedent for private investment, the potential to concentrate public science policy-making in the hands of a few is alarming (Sarewitz 2003). Bainbridge speaks as though transhuman outcomes are inherently good, but the radical potential of a transhuman future demands that this conclusion be examined more seriously. This deliberation is part of the goal for a democratized science.

Defining the Societal and Ethical Implications of Nanotechnology:

Nanotechnology has been supported by public funding since its inception, and many of the questions surrounding nanotechnology are directly relevant for a society that seeks to guide itself. To better connect science and democracy, there must be a way for society to guide the developments of science. SEIN research is one potential tool that can be used in the democratization of science, but the conception of SEIN must be structured properly.

The definition of the social and ethical implications of nanotechnology, or of any science, must be mindful of the societal context. This broader context is often overshadowed by a focus on tangible environmental, health and safety impacts of potential nanotechnologies. Researching these possibilities is important for ensuring safety, and most policymakers agree that the government should ensure the safety of emerging nanotechnologies. However, many scientists exclusively identify environmental and toxicological effects as the only possible social implications. This identification is unfortunately narrow and in some ways naïve. Science impacts broad societal concerns, such as inequity and civil liberty. Insightful analyses have shown a complex and interdependent relationship between technology and society, with each having a substantial influence on the other (Braudel 1992, White 1962). One can clearly see how the threat of nuclear weapons could have broad social implications, but others have convincingly argued that even mundane technologies, such as basic architectural structures, can likewise have a profound effect on an individual's sense of political identity (Winner 1985). The conception of SEIN needs to pay as much attention to the subtle influences of technology as it does to the tangible safety risks.

For nanotechnology, what could the social implications of Bainbridge's NBIC-enabled goals of personality capture and human enhancement be? Some object to the alteration of human traits by potential NBIC technologies as removing humanity from an essential mortal core. Personality capture could potentially eliminate the traditional notions about life and death by enabling a simulated personality to live on forever. There are also broad social issues to consider: What happens if access to these abilities is restricted to the richest members of developed nations? Would social classes of rich and poor be further separated by transhuman capabilities? And if NBIC personality capture is possible, what would the existence of digitized transhumans do to our conceptions of individual rights? Potential social conflicts loom in the background of a potential NBIC convergence, and the ability for society to impact the direction of this research seems to decrease once technologies are on the market. Awareness of such SEIN-related issues should be used to foster informed debate about why NBIC research is done, especially when public funding is supporting it.

The democratizing potential of SEIN research can be seen in the terms of the 2003 21st Century Nanotechnology Research and Development Act (Nano Act), which authorizes much of the SEIN research[9]. The act calls for "ensuring that ethical, legal,

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[9] It was Section 10 of the Nano Act that contained the language that called for a dramatic new societal implications program:

> (10) ensuring that ethical, legal, environmental, and other appropriate societal concerns, including the potential use of nanotechnology in enhancing human intelligence and in developing artificial intelligence which exceeds human capacity, are considered during the development of nanotechnology by--
> (A) establishing a research program to identify ethical, legal, environmental, and other appropriate societal concerns related to nanotechnology , and ensuring that the results of such research are widely disseminated;
> (B) requiring that interdisciplinary nanotechnology research centers established under paragraph (4) include activities that address societal, ethical, and environmental concerns;
> (C) insofar as possible, integrating research on societal, ethical, and environmental concerns with nanotechnology research and development, and ensuring that advances in nanotechnology bring about improvements in quality of life for all Americans; and

environmental, and other appropriate societal concerns, including the potential use of nanotechnology in enhancing human intelligence and in developing artificial intelligence which exceeds human capacity, are considered during the development of nanotechnology" (United States Congress 2003). By the legislation, SEIN is intended to have a broad scope in investigating a range of issues before the underlying technology comes to market. Furthermore, it calls to "integrat[e] research on societal, ethical, and environmental concerns with nanotechnology research and development, and ensur[e] that advances in nanotechnology bring about improvements in quality of life for all Americans". This is a broad mandate, but the importance of integration is to become aware of significant issues early enough such that meaningful decisions can be made about how to handle the direction of scientific research. The NBIC example shows how tangible technologies may be analyzed prior to their full development, and can be used to imagine complex results from numerous areas of science. This forethought and understanding can be used as the basis for enabling better democratic decision making about the science well before the technologies reach the market.

As NBIC is only one of many research paths within the nanotechnology umbrella, the concept of SEIN will be very diverse. Surveillance might be one issue within nanotechnology development. Research being done at Berkeley is typical of other efforts

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(D) providing, through the National Nanotechnology Coordination Office established in section 3, for public input and outreach to be integrated into the Program by the convening of regular and ongoing public discussions, through mechanisms such as citizens' panels, consensus conferences, and educational events, as appropriate to create undetectable nano-sized surveillance devices[10]. Such “nanodust” could alter the character of privacy in public spaces, as surveillance can become undetectable over large areas. Other nanotechnologies seem poised to drastically alter social structure. Nanomaterials research is enabling the creation of fundamentally new macro-scale properties by way of manipulating materials at the atomic level. This could enable a transformation of industry as it is known today. Often ignored or hidden, military nanotechnologies could revolutionize war. Alarmingly, much of the nanotechnology budget goes to the military, for projects ranging from new, powerful weapons to super-soldier technologies[11]. Other questions surrounding the societal context of nanotechnology exist. Many worry that the benefits of nanotechnology will be used to exclusively benefit developed nations, or that nanotechnology will cause the acceleration of industrialized economies to a speed that the developing world cannot catch up to (Invernizzi and Foladori 2005).

The societal and ethical implications of nanotechnology are both deep and broad, but the utility and worth of SEIN research is not always clear. Some SEIN research might have immediate practical uses on its own, but it is best actualized by a guiding framework designed to encourage democratic deliberation. By including meaningful reflection on SEIN early in nanotechnology’s development, SEIN research can be used to highlight unforeseen opportunities to guide the science toward socially beneficial outcomes, and it can be used to help prevent and mitigate inequitable outcomes and disasters.

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[10] See Warneke, B., Liebowitz, B., Pister, K. Smart dust: communicating with a cubic-millimeter computer. January 2001 IEEE Computer pp.44-51
[11] Altmann, J. , Gubrud, M. Anticipating Military Nanotechnology. Winter 2004 IEEE Technology and Society Magazine pp. 33-40.

The directors of Arizona State University's National Science Foundation-funded Center for Nanotechnology in Society (CNS-ASU) have termed such a framework "Real-Time Technology Assessment" (RTTA) (Guston and Sarewitz 2001). Through empirical, conceptual, and historical studies as well as public engagement exercises, the goals of the methodology are: to assess possible societal impacts and outcomes; develop deliberative processes to identify potential impacts and chart paths to enhance desirable impacts and mitigate undesirable ones; and evaluate how the research agenda evolves. By integrating these processes within a proven social science approach, CNS-ASU's analysis is structured to aid in the process of making choices about technology. "The only novelty of this process… is rendering explicit and self-aware the currently implicit and unconscious process of co-production" between science and society (Guston and Sarewitz 2001). Further founding their assessment approach, CNS-ASU has the ability to work from a partnership with ASU's Biodesign Institute, which has strong resources in nanoscale science and technology.

Real-time technology assessment is one of many paths that have been indirectly established by the Nano Act. While SEIN research is mandated for all major nanocenters, not all such research embodies a proactive, real-time focus. Many individual nanotechnology centers have a SEIN committee, but their research agendas are independent and they are not networked together in any systematic way. Beyond the Nanotechnology in Society Network, which includes CNS-ASU and another CNS at University of California at Santa Barbra, SEIN research is decentralized and unguided. Perhaps SEIN will become a lost opportunity, but the spirit of the Nano Act, as seen by its text and legislative history, provides a foundation to enable an ambitious view of

SEIN. To understand why many may want SEIN to be limited in its scope, one must confront SEIN's framing experience.

SEIN's Shadow: The Human Genome Project and Technodeterminism:

From the inception of the National Science Foundation, there is a long history of scientists attempting to protect the scientific enterprise from the realm of democratic control (Brooks 1995). One hope for a change away from this history ended up partially reinforcing the insular practice of science. The Human Genome Project's Ethical, Legal, and Social Issues program (ELSI) has been seen as the direct ancestor of SEIN. The Human Genome Project was a concerted attempt to research and sequence the entire human genetic code, and ELSI is the only other major research effort dedicated to societal implications. Unlike SEIN research, ELSI benefited from having a centralized organization, but the program was without a forward-looking direction analogous to real-time technology assessment. ELSI chronicler Robert Cook-Deegan has stated that the intent of ELSI was not to influence policy in a democratic framing, but to react to understand the changes after they happen (Cook-Deegan 1995). ELSI produced some valuable contributions to understanding the effects of the Human Genome Project, but overall it was too reactive and not sufficiently proactive.

If the ELSI program was intended to merely pave the way for the progress of science, it would be an intellectual fit for much of US science policy. The founding US science policy document, Vannevar Bush's 1945 Science: The Endless Frontier, argued that research organizations should be controlled exclusively by scientists. Bush's justification for funding relies on the premise that all science, basic or applied, inevitably leads to public good. For Bush, the progress of science requires guidance by experts from within science, entailing that public input should be avoided (Bush 1945). While the ELSI program offers a forum for the social issues surrounding science to be addressed, it does not seek to change Bush's scientist-exclusive decision-making process. No matter the concerns raised by ELSI, the research would go on, and society would have to adjust.

Many scholars of science and technology have labeled this attitude "technodeterminism"[12]. There is an entire academic literature dedicated to showing that the simple answers given by technodeterminism are not sufficient for explaining how science actually works[13]. The Manhattan and Apollo projects are instances where social direction of the scientific enterprise was explicit from the beginning, and where political guidance of science was effective. There are other examples where public participation early on in scientific projects led to appropriate outcomes (Guston and Sarewitz 2003, Wynne 1989). Following the conclusions of this literature would have led ELSI along the deliberative RTTA-based approach that has been sketched. That ELSI didn't follow this more proactive and democratic path is regrettable, but understandable, given the general technodeterminist leanings of science policy.

[12] It should be noted that my discussion of technodeterminism includes two separate perspectives. First, as advocated by Michael Polanyi (1962) in his "Republic of Science," some consider that the current workings of the scientific enterprise are as good as society could potentially expect, and that any attempted control of science will harm the success of the enterprise. The other perspective, that science and technology are autonomous entities that proceed independently of human concerns, might hold that the outcomes from science are undesirable, but merely unavoidable. In both perspectives, SEIN research for the potential democratization of science is undesirable, either because a) it will harm the enterprise, or b) it will have no effect and the money should be spent elsewhere.

[13] See M. Smith, L. Marx, Does Technology Drive History? The Dilemma of Technological Determinism. (MIT Press, Cambridge, 1995). Also, M. Gibbons. Science's new social contract with society. 1999 Nature 402

ELSI has been severely criticized, in part due to an increased understanding of the failings of technodeterminism. In a 2001 editorial, *Nature Genetics* called the ELSI program a practice of 'ethics as usual' that too often supported scientific practices instead of advocating for societal concerns. This dissatisfaction is an incremental and limited push for a potentially democratized science. The desire for a mature and democratically oriented science policy was visibly present at the congressional hearings for the 2003 Nano Act. In testimony to the House Science committee surrounding the passage of the nanotechnology act, science and technology scholar Langdon Winner said "technological change is never foreordained, the future never foreclosed. Real choices need to be identified, studied, and acted upon despite recurring efforts to say, 'Sorry, you're too late. Your participation won't be needed, thanks'" (Winner 2003)[14]. In Winner's sense, ELSI was too friendly to the scientific enterprise, and failed to generate critical, meaningful, proactive deliberation. Many of ELSI's advocates supported SEIN in order to get more of the same[15].

Winner's influence on the SEIN legislation provides hope that SEIN will avoid a technodeterminist position, but the vision for SEIN research expressed by the Nano Act

[14] Winner (2003) elaborates at length: "In writings on nanotechnology, there seems little willingness to ask: What are society's basic needs at present? What basic goals define our sense of wellbeing going forward? What we find instead is a kind of opportunistic means-to-ends logic. Researchers and institutions interested in doing molecular and atomic scale engineering scan the horizon to see what opportunities might be identified as justifications for public funding and private investment"'

[15] Potentially reflecting the Congress's sincerity for supporting SEIN, there was an amendment in the House to include a provision for a 5% set-aside of funding to SEIN that was voted down. One congressional staffer (whom I had an informal conversation with in December of 2005) cautions against viewing the amendment's failure as indicative of the Congress' commitment to SEIN. It should be noted that more SEIN is not necessarily better and Congress generally does not like to include set-asides, and the

has the potential to go either way[16]. The technodeterminist approach to nanotechnology has clear influences on the Nano Act, but an ambitious and expanded vision for SEIN does as well. In a 2006 analysis by Erik Fisher and Roop Mahajan, both potentially conflicting themes are seen to be written into the act. The majority of the Nano Act's provisions are focused on a perceived global research race for the supremacy of nanotechnology. The goal is to develop as fast as possible, and thus the majority of the text is oriented toward an outcome that technodeterminists would find highly agreeable. On the other hand, Fisher and Mahajan identify a "heightened awareness of the role public concerns and perceptions can play in the adoption of new technologies… [and] extraordinary legislative language requiring research on societal concerns to be integrated into nanotechnology research and development" (Fisher and Mahajan 2006). Despite potentially technodeterminist leanings, the Nano Act has done more to open the possibility for the integration of social implications research with basic research than any prior legislation, and this can be interpreted in the democratically deliberative fashion of ASU's real-time technology assessment. This proactive interpretation of the legislation should be embraced for its apt understanding of the moral good of science and because it can serve as the beginning of a more democratized science.

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set-aside provision for SEIN is against the spirit of the act, which is to unite SEIN research with the standard nanotechnology research process.

[16] It should be noted, regrettably, that the lack of a policy influence in the HGP ELSI is likely similarly present in SEIN. Sarewitz and Bennett (2006) are skeptical of the ability for the SEIN approach to impact policy. They believe that the mechanism for policy influence here is no better than the HGP – ELSI but that the potential for an integration between societal implications and basic research could potentially move SEIN research "upstream and into the lab," helping to make research decisions more in conjunction with the considered societal outcomes. Further, Sarewitz and Bennett argue that the SEIN legislation came about due to no major insights from the science policy academic

Democratizing Science: The Moral Imperative for an Expanded Vision of SEIN:

Given the implicit conflict within the Nano Act over the role of SEIN, it is imperative to argue for the expanded vision for SEIN. When examined under close scrutiny, the technodeterminist position that scientists alone have the authority to guide the organization of science falls apart. In Science, Truth and Democracy, philosopher Philip Kitcher explores the notion that the scientific pursuit of truth should be preserved against moral and ethical concerns external to science (Kitcher 2001). Within basic science, there are situations where the pursuit of some truths might be questioned, either when there may be a moral objection to a particular research project or a dispute as to whether the funding for science should be spent elsewhere. One example of a moral objection could be research on genetic differences between ethnic groups that could confirm the beliefs of racists (pp. 93-108). Even though this research could be used for persecution, technodeterminists argue that understanding racial genetic differences is of greater importance, i.e. that the truths being pursued are more significant than the social risks. No one argues that all possible truths are significant such that they can override societal considerations: for instance, no one would sacrifice human life in order to learn the number of pebbles of sand on a beach. But technodeterminists may hold that certain truths are *objectively* significant, thus outweighing any one group's objections. The science is said to be significant enough that research must be carried out. But what justification is there for relying on an objective notion of epistemic significance?

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community; it was rather the common sense of the staffers on the House Science

As Kitcher shows, the idea that scientists are pursuing some objectively important truths is questionable (pp. 55-83). Despite many attempts, the idea of a universal conception of scientific merit is unattainable, and Kitcher shows that scientific significance is constructed from a social origin. There can be no reliance on an overriding conception of the pursuit of truth to justify all scientific research, and research should be examined in terms of *both* its potential practical and epistemic benefits. Beyond the question of whether particular research is moral, the cost of pursuing truth must be evaluated against opportunities lost: should society fund basic science, or pursue more direct social goods, or both? Why fund research on a multimillion dollar superconducting supercollider when millions of people live without potable water? Both basic and applied scientific research can generate good outcomes for society, but the risks of failure or of harmful repercussions should be evaluated by a democratic process. Because the epistemic values of scientists stem from the same social origin as other concerns, there is no ground for advocating the supremacy of scientific authority. Thus, even within basic research, the moral value of scientific work is an important human good, but it is a good on equal standing to all aspects of society.

How does society decide what good science should achieve? For Kitcher, the mechanism least likely to engage in prejudice is a democratized science (pp. 117-135). In an ideal democracy, each member is committed to the process, and will not disenfranchise individual groups. Instead of trying to force the values of scientists onto the public, representatives would instead have their natural preferences tutored in the relevant science. Most importantly, each member would be tutored in the epistemic and

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Committee that led to the legislation.

practical significance values of the entire body, thus allowing for a sharing of social context. If all deliberators were sincere, this shared information would allow an open debate for and assembly of a list of objectives for science that well reflects society. Kitcher calls this ideal ‘well-ordered science’, and this deliberation is used to decide what research should be funded, what the most efficient yet moral research path is, and how to use the results of research.

Many scientists recoil at the idea of an ‘uneducated’ public having an input on what research is done. Moreover, a drastic societal shift to the ideal of well-ordered science would be impractical, and if mandated all at once it would impose democracy onto science in a way that would be disastrous. However, Kitcher has no interest in ‘vulgar democracy’, whereby an underinformed majority makes decisions on the basis of gut-feelings. Further, Kitcher recognizes the dramatic practical difficulties of public involvement in well-ordered science. So it is important to consider well-ordered science as a philosophical ideal to be aspired to, to help clarify the intent of and justification for a more practical approach towards introducing democracy within science policy. Following the ideal sense, it becomes clear that a democratized science shouldn’t involve subjecting science to the supremacy of an uninformed public. It should instead allow for the embedded values in all parts of science to become clearer, allowing for scientific research programs to better reflect the ends society wants science to achieve. A society fully oriented toward well-ordered science would have the publicly funded scientific community well in tune with the needs of humanity, directing its work to societal needs.

That Kitcher’s ideal is philosophical in character does not mean that it should not be used to advocate for a more democratized science policy (pp. 181-197). Kitcher’s ideal is designed to help justify and guide a more developed political approach towards improving science policy incrementally. In the case of CNS-ASU's real-time technology assessment, the ability to generate information about possible societal outcomes early enough to enable public and policymakers to make decisions is a step toward democratization (Guston and Sarewitz 2003). In particular, CNS-ASU's attempts at scenario-building are designed to create realistic visions of nanotechnological developments – and their implications – that can be used early enough to foster some of the ideal deliberation to which Kitcher aspires. The methodology has also been supported by social science literature and past practices, and the investigation is strengthened by a strong relationship with nanoscience resources at ASU (Guston and Sarewitz 2003*)*. There is a variety of other ways in which democratic science policy changes can be practically implemented, all of which have the potential to achieve some of the societal, moral, and even philosophical benefits that Kitcher describes (Guston 2004).

Since science is a human good, democracy has the potential to most fairly determine what good to hope for, and SEIN might help achieve the goal to order science more justly and more effectively. However impractical the philosophical ideal of a fully democratized science, RTTA-driven SEIN offers a way to create a realistic, political method for democratizing science policy (at least in this nano-cosmic case, if you will, but likely with more general applicability). Through real-time technology assessment, SEIN can be proactive in a way that ELSI never was, and as such SEIN can be viewed as an experiment to test the potential worth of democratized science. The claim that proactive democratic deliberation using SEIN could lead to better social outcomes needs to be proven in experience. If successful, work on SEIN and real-time technology assessment could become the practical complement to Kitcher's philosophical ideal, and should then be emulated throughout science policy. With an aim to revolutionize science policy, research on SEIN should be used to enable a proactive approach to science policy that can allow for a more democratized science.

Part Two: Politically Relevant Philosophical Positions

This section will try to more generally contextualize the relationship between philosophy of science and science policy. The next section will extend this, to analyze in detail the inner workings of Kitcher's justification of democratized science which was just used to argue for specific directions for research on the societal implications of nanotechnology research.

Any possible assessment of philosophical support for democratization must first be contextualized by an assessment of how philosophy can impact science policy generally[17]. This section uses Philip Kitcher's discussion of traditional issues in the philosophy of science and tries to draw connections to science policy relevant issues. Kitcher's 2001 *Science, Truth and Democracy* (*STD*) is the most prominent book on democracy and science amongst philosophers, and is an attempt at expositing traditional debates internal to the philosophy of science and using those conclusions to argue for a more democratized form of science. Kitcher uses four key philosophical positions in order to carve out an ideological space for the justification of the democratization of science, which will be the focus here.

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[17] Mitcham and Frodeman (2004) mention an important distinction for discussing science policy. 'Science policy is commonly divided into "policy for science" and "science for policy"' (p. 1). Science policy is thus framed for policies designed to support scientific research, or a descriptive for approaches on how to use science in everyday policy relevant decision processes. Much of Kitcher's analysis is focused on policy for science, but his ideas clearly cover both. For a criticism of WOS as being incapable of dealing with tight time tables for research for decisionmaking (as experienced in science for policy), see Biddle (2006).

STD is largely framed by Kitcher's earlier work. In particular, Kitcher's 1993 book, *The Advancement of Science*, which was an attempt to lay out a comprehensive philosophy of science, is used to justify many of the points within *STD*. However, Kitcher revises some of the integral positions that he made in the earlier book, which thus provides interesting focal points in the attempt to understand his overall philosophy of science policy. Perhaps it can even be said that his change in philosophical position helped to motivate his foray into more societally engaged philosophy. As Kitcher frames it, the attempt to carve out a space for the democratization of science requires one to steer through many false dilemmas, and he tries to avoid the polarization between those who believe in the scientific ideal and those who attempt to demonize science. In *STD*, Chapters 2 and 3 serve to critique the ideal scientific practice as an ultimate examination of the world, and chapters 4 and 5 serve as a resistance of extreme critics of science.

Kitcher surveys several issues in the philosophy of science. Four of them are emphasized here because they are emblematic of how philosophical positions frame science policy decisions.

> 1: *Realism*. Scientific claims about the world can accurately represent reality, but there is/can be multiple, equally correct, representations of the same phenomena.
>
> 2: *Convergence*. While science continues to make progress, there is no convincing reason to expect that science will converge on an ultimate description of reality.
>
> 3: *Epistemic and Practical Significance*: Importance of scientific achievements can be described in terms of both epistemic and practical

significance. The distinction between epistemic and practical significance is not absolute and can become vague in some cases.

4: *Role of Values*: Social influences play an important role in the creation of the particular framework of significance, for science as with all of society.

I will here make a quick global sketch with the intent of highlighting the interconnections between each position, as well as their larger science policy relevance. Kitcher certainly uses his first two positions to create space for arguing for the third and fourth positions, and these positions become critical for justifying a democratic approach towards the guidance of science. All of these questions have relevance for science policy, broadly construed[18].

The realism debate is important on two separate levels. First, science makes many claims about the actual world; the importance of achieving those claims is largely a function of how accurate and powerful those statements can be within our interactions with the world. The classical debates about realism touch on whether our immediate body of knowledge is 'true' of the world. For many, the idea that scientific claims are true seems commonplace, but a more serious analysis tends to push towards the treatment of

[18]Daniel Sarewitz has noticed a common preference for the construal of science policy as budgetary policy (see his 2003 "Does Science Policy Exist and, If So, Does it Matter?" Sarewitz would prefer for science policy to be much broader and to include ethical dimensions). Even within a narrow framing of what science policy is, debates about value influences on scientific practice and on the importance of epistemic pursuits are relevant for analyzing how much effort we want to invest into the scientific enterprise. Within a budgetary sense, most would hold the realism debates to have *some* relevance; the lower level of the realism debate certainly reflects on what the immediate products of research are; the higher level of the realism debate (on eventual convergence of theory into the ultimate description), helps to evaluate the importance of funding ambitious but abstract scientific projects.

scientific claims (even the most ingrained "laws" of physics or "principles" of biology) as being wholly fallible, and replaceable by alternative claims about the world. Kitcher defines his position as that of "modest realism", which holds that claims can be accurate about the world, but that there can be alternative and multiple ways in which to correctly describe a portion of reality, and that a scientific theory often is fallible and incomplete. Thus, the current state of scientific theory is not definitive of reality, but it does have significance due to its (partial) accurate representation; such a philosophical position offers a humble perspective on what the output of scientific research is, which is a fundamental contribution for science policy analysis.

The second level of the realism debate expands beyond current knowledge to the eventual trajectory of the sciences. Will science, perfected over countless ages of human effort, eventually reach the ultimate description of reality? This secondary debate hinges on questions of pragmatism and the unity of science, and can have direct science policy relevance. A believer in the convergence of knowledge will likely hold that the present course of science will attain the ultimate prize of knowledge, and thus is to be supported regardless of the cost. Kitcher's discussion in *STD* is a reversal from his earlier approach in *AoS*, where he argued for a form of special convergence of scientific knowledge.

One key question here is: *Had Kitcher not changed his mind about the overarching convergence of science, would his central premises regarding well-ordered science have been substantially weakened or reversed?* This is the first of several instances where a position within the philosophy of science is policy relevant . This position on convergence serves as the foundation for the third and fourth positions described below, which have an even more striking relevance for science policy. Also, alternative views on the realism debate seem to yield different conclusions about science policy outcomes. For example, Longino (2004)[19] holds a much more anti-realist position about the claims of science than Kitcher, and she draws different conclusions about what the democratized form of science should look like.

Kitcher touches on philosophical discussions about the role of values within science, and in part unifies it within his conception of modest realism. His third key position is to establish the epistemic products of science as an important human good, but not an ultimate good. Kitcher begins his analysis of the significance of scientific claims by introducing a distinction between epistemic and practical significance. Epistemic significance is closely related to the traditional conception of how science supposedly functions: it is the analysis of the importance of particular knowledge claims for their own sake. Kitcher establishes that the goods of science have both practical and epistemic significance values. Further, *because the knowledge of science is assumed not to converge into a True conception of reality*, the value of epistemic significance is inherently limited. Because of this limitation on epistemic significance, the value of knowledge cannot necessarily trump other practical values. As a result of this, Kitcher indeed affirms that science is a human good, but that it is a good on par with other human goods.

[19] Helen Longino's discussion (2002) of *STD* discusses her continued dissatisfaction with the realist positions that Kitcher holds, but she seemingly has little criticism of the way in which he embraces democracy. She applauds his approach for its ability to sway over epistemic conservatives. Her criticisms are that a) Kitcher's emphasis on ensuring public return on investment is outdated now that science is often based within private research; b) that the ideal of well-ordered science may compel individual action but that the suggestion to leave immoral research projects presumes an equity of work availability that doesn't exist in private industry; c) that well-ordered science doesn't cover dilemmas that may arise when different societies have conflicting goals.

The fourth philosophical position is Kitcher's elaboration of the ways in which epistemic significance values can change over time. This sort of philosophical analysis, which most likely requires a strong interdisciplinary complement, can allow for an examination of the underlying foundations of how a science functions. This examination of the justification for doing research is important; research becomes significant for a reason, and a policymaker should try to evaluate this context. By examining this process, Kitcher lays the groundwork for a better science policy approach toward examining the influences on science by society, as well as the influences on society by science. In his later moral analysis, Kitcher draws upon the idea of significance graphs[20] as a tool to assist in analyzing the moral inputs/outputs of the scientific enterprise.

The formalization and clarification of these distinctions can become useful for science policymaking decisions because it offers a conceptual framework to evaluate the prospective benefits of science in one form or another. In many ways, the perspectives here can be applied to any debate over the governance of science. Each point can have important implications for a larger debate over the value and limits of science, and the subsequent sections will examine the specific ways in which shifts and positions held at these philosophical levels may have an effect on science policy decisionmaking.

Part Two: Kitcher, the Unity of Science, and Epistemic Significance

Context on the Unity of Science:

The rejection of the Unity of Science is a popular position within current analytic philosophy of science. Perhaps the most novel aspect of Kitcher's *Science, Truth and*

[20] Discussed in *STD* pp 78-81.

Democracy is the position that he takes with respect to the Unity of Science thesis, and its subsequent impact on the significance of science. Kitcher establishes his *STD* position by attacking an extreme version of the Unity of Science thesis. It is important to understand the historical context surrounding the Unity of Science argument because the views Kitcher attacks are prominent but rarely held by professional philosophers of science. Indeed, Kitcher mischaracterizes much of what previous advocates for the Unity of Science project would have believed in. His argument against context-independent epistemic significance is critically important, however, as Kitcher is able to embrace realist philosophical positions, establish the power of the sciences, yet still call for a more democratic science policy.

Many aspects of what is called the Unity of Science thesis (UoS) are quite ancient in origin, but the term most commonly refers to a particular philosophical movement in the 20th century, logical empiricism. One common caricature depicts UoS as the belief that the sciences are structured in a hierarchy flowing downwards from the fundamental laws of physics. This pyramid conception of science generally holds that connections between all levels of science wait only to be discovered and that a unity of scientific theories will be discovered by finding ways to unite the disciplines.

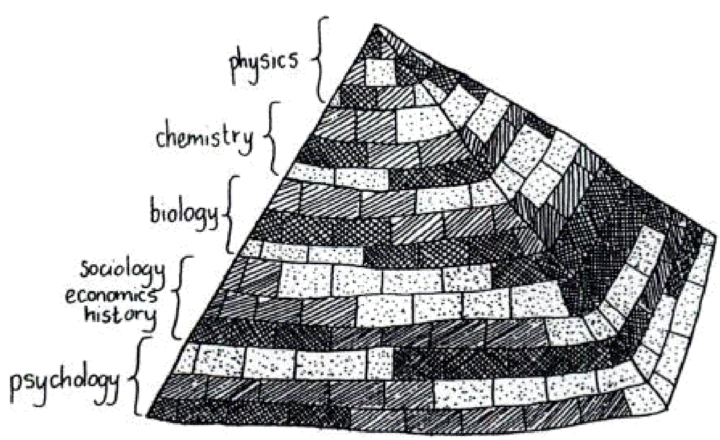

Figure 1: From Cartwright's Dappled World (1999, p. 8)

Some people associate the Unity of Science thesis with a second, radical idea: that the sciences, once unified, would have attained an ultimate conception of the workings of the world[21]. This belief in a long-term convergence upon the "real ontology" is certainly lurking in the shadows surrounding many UoS discussions. By characterizing UoS in terms of scientific hierarchy and long-term convergence, is this creating an unfair caricature of the UoS movement?

Although variations of the UoS position have existed for centuries, the most recognized form of the 20th century is associated with Rudolf Carnap and the logical empiricists[22]. In Carnap's 1938 "The Logical Foundations of the Unity of Science", he lays out the central tenets of the UoS thesis, which in many ways rebuts the traditional caricature of the UoS movement. By examining science in terms of languages, Carnap lays out a variety of levels in which the logical connections of the sciences can be articulated. His first connection is drawn at the level of the language of things, where each scientific branch can describe the same phenomenal objects using different terminology. Because of the relative ease in comparing semantic conceptions of things, this level of logical unity is uncontroversial and readily realized. The larger connection Carnap explores is about the potential to take the individual laws within particular

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[21] This view of Carnap is clearly expressed in E.O. Wilson's 1998 *Consilience: The Unity of Knowledge.* pp. 60-65

[22] My understanding of Carnap comes primarily from studies with Dr. Richard Creath. Creath (1995) has a solid discussion of the unity of science thesis from the perspectives of Carnap, Neurath and Peter Galison (who is a strong critic of the unity of science thesis). Creath argues that the traditional conceptions of the logical empiricists are misplaced and that Galison's views are roughly commensurate with Carnap's. Neurath and Carnap were interested in the unity of science thesis for its ability to keep language

branches of science and to translate the essential meaning and usage of each law into the language of other branches. For Carnap, this ease of translation is readily realized within physics and parts of chemistry, but has yet to be realized between physics and biology, or the social sciences. Carnap is certainly optimistic that the logical translation of laws between disciplines is possible, but he is not dogmatic in his insistence, instead preferring to let the evidence decide. Indeed, the concluding sentence of the paper seems to argue primarily for the UoS based upon its practical necessity:

> If now the terms of different branches had no logical connection between one another, such as is supplied by the homogeneous reduction basis, but were of fundamentally different character, as some philosophers believe, then it would not be possible to connect singular statements and laws of different fields in such a way as to derive predictions from them. Therefore, the unity of the language of science is the basis for the practical application of theoretical knowledge[23].

Carnap's position on the Unity of Science strongly correlates with the vision of the sciences as an hierarchy that can be unified logically. He held then, as he likely would today, that the then-current state of the sciences was disunified in this stronger sense. His belief in the possibility of such a unity seems strongly motivated by pragmatic reasons of explaining the effectiveness of interdisciplinary work. This is to say, if it were impossible to communicate with a shared language across disciplines, then it would seemingly be impossible to explain the success of interdisciplinary work that occurs between different branches of science. However, Carnap avoids the second stereotype about the belief that the sciences will converge on a particular truth. Although he is commonly associated with the view that the sciences can become a mechanical truth-generating machine, key

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publicly testable. Later, I try to make the connection that the such an emphasis on keeping language publicly testable is relevant to democratized science.

[23] Reduction here is not meant in the context of, say, reducing biology to physics. Rather, Carnap allows for logical reductions to definitions that are not completely defined.

portions of Carnap's published literature focus on the perhaps insurmountable difficulties involved in making "external", or context-independent, decisions about science [Carnap 1937 p. 318, 1953]. If Carnap is not responsible for the second caricature of UoS, who is?

A likelier inspiration is Hilary Putnam and Paul Oppenheim's "The Unity of Science as a Working Hypothesis" (Opphenheim and Putnam 1958). In the piece, the authors acknowledge the possibility that the sciences cannot be brought to converge in a seamless hierarchy, but they point to the benefits of using the notion of the convergence of the sciences as a guide for future research. There is still no dogmatic assertion that science will reach a level of ontological unification, or a unity of theory. Ernest Nagel, in his *Structure of Science* (1961), seems to have offered a more stereotypical characterization of the unity-of-science thesis, and this is indeed what Kitcher cites in his later arguments against the Unity of Science (Kitcher 2001, p. 207).

As will be discussed later, recent scholarship seeks to change the historical legacy of the logical empiricists. George Reisch (2005) sharply distinguishes between the Unity of Science *thesis* and the Unity of Science *project*. Otto Neurath led the logical empiricists in establishing a movement to create more unified sciences, but at all times this unity was considered a practical matter. Neurath believed that the unification of the sciences at the level of theory was impossible. Regardless, the prevailing view of the logical empiricists has in some ways been affected by political criticisms of the movement, such as that by Horace Kallen, which accused the group of attempting to implement a totalitarian control of society by science (Reisch 2005). Politically, the group was painted as extreme, and this perception of the original Unity of Science carried over to the assumption that they believed in the philosophically extreme versions of the

UoS thesis as well. This point is mentioned here as a preliminary, so as to establish that the image depicted in Figure 1 is in many ways a caricature.

The stereotype that advocacy of the unity of science must entail a convergence on the ultimate description (or a unification of theories or ontology in science) primarily stems from non-philosophers' misinterpretations of the logical empiricists. Famed sociobiologist E.O.Wilson's influential account of *Consilience* paints a heroic image of the scientist who is boldly exploring for the ultimate description of reality[24]. Wilson wrongly finds common sympathy with Carnap and the logical empiricists, whom he considers to have been seeking an ultimate description of the world through logic. Wilson believes that the failings of the movement can be surmounted by embracing developments in neuroscience that will allow human reasoning to transcend its current limitations.

The unity of science thesis, even in a weak form, is unpopular amongst professional philosophers, and there may be few professional philosophers who ever believed in the strong form of the thesis. However, it is politically critical to realize that the thesis is still alive in both strong and weak forms within the self-conceptions of many scientists and in the view of the public.

Kitcher's Version of the Unity of Science Thesis:

Philip Kitcher's 1993 book *The Advancement of Science* (*AoS*) was his attempt to present a comprehensive philosophy of science. Much of the book is influenced by his teachers, Carl Hempel and Thomas Kuhn, and the book serves as an attempt to continue their research into the ways in which scientific movements progress over time. Kitcher

[24] Wilson (1998). Chapter 4. Particularly 61-64.

describes both his *AoS* and *STD* as attempts to steer between two misconceptions of science: the demonic conception of science, and Legend, the idealized conception of science that is associated with the logical empiricists[25].

In Chapter 4 of *AoS,* Kitcher utilizes a version of the UoS thesis in order to argue for the context-independence of epistemically oriented scientific inquiry. The notion of epistemic inquiry is Kitcher's way of distinguishing 'pure' knowledge-driven research from research oriented toward practical goals. All practical pursuits are inevitably bound to human interests and research. It is seemingly easy for a sociologist to critique the social motivations for actions that have implications in the everyday world. In order to resist the demonic conception of science and its view that all of science is socially constructed "all the way down", Kitcher's *AoS* approach is to establish that epistemic inquiry in science can be objectively significant in an epistemic sense that is independent of human desires.

Kitcher's view of epistemic significance is distinct from Carnap's and Putnam and Oppenheim's because it is based upon a logical analysis of the historical development of the explanatory power of scientific theories. Kitcher highlights ways in which the sciences have continued to develop in terms of ever-increasing powers of explanation amidst a unified purpose:

> Significant experiments help us to resolve significant questions. Methodological improvements help us to learn better how to learn, to improve our evaluation of significant statements. Thus, to sum up the general approach, significance is derivative from the background project

[25] Much of the scholarship reconsidering the work of the logical empiricists would seemingly pave away the need to avoid the follies of Legend. If the follies of the logical empiricists are not so grave as once thought, then Kitcher's model can no longer be viewed as charting a course between extremes.

> of ordering nature, a project that is articulated in our attempts to conceptualize and to explain. (95)

Science has a basic purpose to explain and organize our understanding of the world. Kitcher is not like E.O. Wilson, who holds that there is a general structure to nature that we can discover (indeed, this is part of the Legend that Kitcher aims to resist). Instead, he seems to hold that the sciences uniquely tend to converge on our best explanations, and that this explanatory understanding is cumulative. Significance generally flows from the background project of explaining nature, but Kitcher tries to be specific in defining what is and is not an accumulation in explanatory ability. In examining biology from the time of Darwin, Kitcher examines the critical differences between the views of Darwin and his contemporaries. For even among one of Darwin's critics, "at a more fundamental level, Owen share[d] [Darwin's] common vision. Like them, he contend[ed] that biology should explain the diversity of living things and trace the patterns of that diversity." (94). Kitcher postulates that such historical connections could be drawn all the way back to Aristotle, or at the very least to the beginnings of 'modern science.'

From this historical claim about a shared purpose within the sciences, Kitcher tries to establish a context-independent notion of significance. He seeks to conclude that the sciences have been driven by "the impersonal epistemic aim of fathoming the structure of the world. In less aggressively realist language, what they have wanted to do (as a community) is to organize our experience of the world" (94). Kitcher sharply distinguishes the possibility of an impersonal epistemic aim from historical conceptions of a deistic support of the sciences. This point is critical: if God created an underlying structure for the world that was within our grasp, then the sciences would be objectively significant. Kitcher wants to craft a notion of objective significance that deals only with an internal understanding of our epistemic goals.

Kitcher tries to justify this conclusion by offering an axiomatic account of explanatory progress (*AoS*, 111-112)[26]. Progress can be made by identifying more inclusive sets of entities and properties in the world, or by identifying more correct claims about the workings in the world. Kitcher holds that the historical continuity that he illustrates in biology is based upon this type of progress. In a passage that does much to illustrate his divergence from the traditional caricatures of the Unity of Science, Kitcher argues that

> A suggestive (but not entirely adequate analogy) is to think of the work of children engaged on a large and complex jigsaw puzzle. Subregions of the puzzle correspond to the structure of dependencies among a particular class of phenomena. Identifying correct schemata is analogous to fitting a few pieces together, the correction of schemata corresponds to scrapping faulty efforts at fitting pieces, the completion and extension of schemata consist in putting the pieces already fitted into larger chunks of the puzzle. The ultimate aim, of course, is to complete the picture. (Here, perhaps, the analogy breaks down, for there may be no complete – or completable – picture). (114)

Kitcher thus indicates that the conception of attaining a true and convergent description of the world may be impossible, and thus clearly distinguishes his position from one of the common caricatures of the UoS thesis. In a footnote, Kitcher adds that his view "plainly has some affinity with the ideas of the logical empiricists about the unity of science. However, there are important differences – stemming from my rejection of the demand that there be accumulation *at the level of details"*(112).

[26] Kitcher makes a number of preemptive remarks in defense of his realist language. His general claim is that you can utilize his account of objectivity while denying the existence of the real world if you conceive of the overarching project as a matter of organizing experience.

Perhaps one interpretation of Kitcher's approach here could follow from the claim that individual sciences continue to make progress. Then, even though the details might not converge, the conception of science as hierarchical pyramid can be salvaged so long as one has progress that supersedes any potential gaps between the levels. Of greater relevance is his point that explanatory power is always cumulative. If one accepts Kitcher's argument about the cumulative development of explanatory ability, Kitcher can argue for an ultimate storehouse of scientific explanations for all science. Even though the sciences will not converge on an ultimate ontology of the world, our collection of scientific explanations will accumulate.

The goal for having this explanatory storehouse is clear: to avoid the full criticisms of those who would demonize science, "We need a specification of impersonal goals for science, goals that can ultimately be defended as worthy of universal endorsement" (94). If one can refer to an ultimate storehouse of explanations, this goal can be pursued independently of any social or political matters in the world. Obviously, research programs change over time, often due to social pressures. But, in *AoS*, Kitcher ascribes temporal changes in research emphasis (for example, the changes seen from comparing a 1950 copy of *Nature* magazine with a 2006 copy)[27], as being a result of the difference in *derivative* questioning. Kitcher claims that the *primary* questions that drive research today are the same as they were in the 1950's, and that these questions are important because they are part of the pursuit of the ultimate storehouse of explanation.

[27] This is one example used in *AoS*, Chapter 4.

Kitcher's Change on the Unity of Science and Explanation:

Scientists often explain data in terms of a larger body of scientific theory. As with many other aspects of the philosophy science, current thought about explanation is framed by dissatisfaction from philosophers with the workings of the logical empiricists. The logical empiricist model of explanation, the deductive-nomological (DN) model, held that events are explained if their occurrence could have been logically derived from a set of fundamental scientific laws[28]. As explained by Hempel and Oppenheim (1948), this explanatory model would hold that the only permissible explanations are based upon true and universal scientific laws. This theory drew great scrutiny, however, for many reasons. First, due to the ability to logically derive a great deal of nonsense from these universal principles, the DN model would allow excessive amounts of derivations to count as explanations. Second, the DN model failed to describe numerous cases where it is recognized that explanations do occur; for example, in many sciences (such as biology) it is claimed that there are no universal laws. Third, even in the select cases of physics where is may seem possible to derive predictions from fundamental laws, there is no guarantee that the law being used to explain is true, and not merely an accidental generalization.

Kitcher's discussion of the unity of science in *Science, Truth and Democracy* implicitly relates to his positions about explanatory unification. The two concepts are inextricably linked for Kitcher, as he views explanation in the context of unity. For Kitcher, explanation is a series of derivations based upon the total set of laws and theories that we have at a given time (Kitcher 81). The significance of an individual explanation is derived from the value that it gives to the overarching picture of science.

For Kitcher, an explanation thus involves an addition to the overall store of scientific knowledge. This addition must come by way of unifying various pieces of theory together. Kitcher justifies this approach by citing examples in the history of science that follow by the explanatory pattern that he describes. Kitcher discusses the Unity of Science thesis in *STD* (Chapter 6), but does not mention his own program.

Kitcher's changes of position on the issue of explanation since 1981 have been recorded in Bechtel and Hamilton (2006). The *AoS* position he took was a moderated version of a 1981 paper that attempted to argue that all explanations throughout science could be unified within a massive program. In large part, the debates Kitcher engaged in with social constructivists, such as Steve Shapin, led to his backing away from context-independent notions of epistemic significance[29]. Kitcher's position continued to change: in 1999 he further moved away from his position about the context independence of epistemic significance (Kitcher 1999). The next section will analyze this change in Kitcher's philosophy as represented in *STD*.

The Rationale As Expressed in *STD*:

The first sentence of *STD* says the book stems from Kitcher's recent study of the social effects of science, and he seeks to utilize positions within the philosophy of science to argue in favor of a change in how society thinks of science. As mentioned, Kitcher followed much of the structure of *The Advancement of Science*, including the framing to steer between extreme views about science. As will be described later, Kitcher makes key changes that play a critical role in the construction of a philosophy of science policy, but

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[28] My discussion here follows Klee (1996).
[29] In the acknowledgements to *AoS*, Kitcher explicitly mentions Shapin.

the exact motivation for Kitcher's change on explanatory significance will first be sketched.

It is at first difficult to trace what Kitcher's change of position is, exactly. Kitcher does not discuss his prior account of explanatory progress, but he instead focuses on general criticisms of the UoS movement. Some of the *STD* criticisms of the UoS movement seem to be criticisms of the caricatures that Kitcher himself did not embody in *AoS*. For example, on page 59 of *STD*, Kitcher says that he is skeptical that the sciences will converge on an overarching aim, and in a footnote says that his *AoS* account did hold such a convergence to be possible. However, as noted in the *AoS* quotation above (*AoS* p23), Kitcher in *AoS* did not believe that the sciences would converge, but merely held that the background project of explaining/organizing experience was sufficient to provide an impersonal motivation for the practice of science. Kitcher focuses his discussion on the idea that some theoretically possible convergence of theories could justify epistemic significance. In *STD* (p.73), Kitcher says "the Unity-of-Science view made it look as though there was a fundamental set of maps from which any map we might care to use could be constructed, and so gave content to the conception of the ideal atlas. Once we abandon that view, it looks as though all that may remain is a collection of charts that may proliferate indefinitely with our changing interests".

While some of the *STD* UoS discussion is irrelevant to his change in position, a few key arguments serve as direct rejections of objective epistemic significance. The major argument that Kitcher raises is that there must be a way to distinguish significant epistemic truths from insignificant ones. Whether one utilizes Kitcher's prior account of explanatory progress or the UoS caricature about the structure of the world, both accounts hold that there will be what Kitcher calls a “relevance relation…that holds between the topic and the objective complete answer” (73). But no account of relevance seems to be able to distinguish between a significant or insignificant truth; “it looks as though any truth, however banal, will occur somewhere in the explanatory store, unless we are offered a filter that lets just the “pervasive” truths enter the class of the epistemically significant” (74). The only way to effectively distinguish between significant and insignificant relations is to rely upon human interests and desires.

There are also other justifications for context-independent significance that Kitcher rejects in *STD*. Most important are his rejection of the first portion of the unity of science thesis, that the sciences can converge in the hierarchy of a pyramid; as well as his rejection of the idea that causal explanations can serve to provide objective explanations. In terms of Kitcher’s change in positions, these two arguments are not as important as the significance/insignificance criterion, but they are important positions that respond to some substantial philosophical literature.

In the end, Kitcher does not reject his prior account of the explanatory progress of the sciences. He instead argues that it is insufficient to establish the context-independence of epistemic inquiry, and thus holds that the epistemic pursuits of science are framed by human curiosity and other social phenomena. Why is this important?

How Kitcher Relies on the Argument Against Objective Epistemic Significance: The Mythic Defense of Science:

Kitcher’s argument in the first half of *STD* concludes with his embrace of human context as the only coherent source of epistemic significance. Kitcher begins the second half of *STD* using his position to reestablish the proper orientation of the sciences. In Chapter 7 Kitcher tackles the “myth of purity,” which is “the claim that gesturing at the absence of any practical intent is enough to isolate a branch of inquiry from moral, social or political critique" (91). Kitcher elaborates the defense of the myth of purity – which is, really, the defense of much of scientific practice – as follows:

> The sciences seek to establish truths about nature. How the resultant knowledge is used is a matter for moral, social and political debate, but it is intrinsically valuable for us to gain knowledge. If the circumstances in which knowledge is applied are likely to generate harmful consequences, then that is a sign of defects in the social mileu that surrounds the sciences, and, ideally, we should try to gain the knowledge and remove the defects. (85)

Kitcher shows some sympathy to the defense of science, and reiterates that his positions hold that science achieves some truth and objectivity. However, Kitcher then rejects the defense of the myth of purity, saying that it "depends on a view of the aims of the sciences we ought to abandon" (85). Kitcher begins to clarify:

> All kinds of considerations, including moral, social, and political ideals, figure in judgments about scientific significance...Inquiries that appeal to us today, and that we characterize as epistemically significant, sometimes do so because of the practical projects our predecessors pursued in the past. With our eyes focused on the present, it's easy to deny that these inquiries are in any way connected with broader values (86).

Thus, Kitcher is relying on social influences on scientific significance as the backbone of his attack against the myth of purity.

What philosophical commitments would Kitcher have had before his *STD* conversion? The question of the social context of scientific practice is broached primarily at the end of *AoS*: "Given a clear view of the epistemic achievements and prospects of science, how should we modify the institution so as to enhance human well-being? Reflective understanding and constructive critique should, I believe, replace both sleepy complacency and Luddite rage. The philosophers have ignored the social context of science. The point, however, is to change it." (391). Clearly, the desire for a more dedicated approach to the social problems surrounding science existed in a nascent form within *AoS*. As has been briefly explored, there are key differences between the philosophical positions taken within *AoS* and *STD*. Had the first half of *STD* been an exact restatement of *AoS*, how would the second half of *STD* have been different?

At first glance, the overall tone of moderation that Kitcher takes within *AoS* makes it unlikely that he would have embraced the positions of the scientific ideal that he argued against within *STD*. His intention through *AoS* was to defend only the best aspects of Legend. However, Kitcher's *STD* seems to be based upon a fundamental reversal on one position: he denies arguments he made in *AoS* that it is possible to create a notion of objective explanation for the sciences, and thus it becomes impossible to have any criterion for epistemic significance exclusive of human context.

Given the emphasis of this chapter, perhaps the primary question should be whether or not Kitcher would have been forced to embrace the myth of purity under the *AoS* positions. Perhaps Kitcher would have been forced to defend "unpopular scientific research," and to argue that "moral, social and political concerns…should not be invoked in the appraisal of investigations", at least to some extent (85). There could be any number of alterations in Kitcher's other positions that could enable him to avoid becoming a defender of purity. For example, one could embrace objective significance, yet still be extremely concerned about the ways in which science becomes used in practical endeavors, and thus encourage democratic scrutiny. This would likely be a step forward from other science policy practices, but there is surely a loss here. The notion of epistemic significance as socially contingent is a powerful political tool, which is generally able to make the defense of science impractical, as well as to make untenable the distinction between pure science and technology.

What would *STD* have been like without this change of position? The moderate tone of *AoS* would likely have enabled Kitcher to write a strong but different second half of *STD*. But many of the essential insights of *STD* all depend upon the conception of human context-dependent epistemic significance. Perhaps the rhetorical power to argue for good social changes should be an additional reason for favoring Kitcher's mature position on objective significance.

Part Four: Logical Empiricism and Political Philosophy of Science

While there were some philosophers of science in America prior to World War II, it is fair to say that modern philosophy of science began with the emigration of Rudolf Carnap and the logical empiricists away from the rising Nazi empire[30]. Composed of a variety of physicists-turned-philosophers and scientifically-minded philosophers, the logical empiricists considered themselves to be revolutionaries who sought to move beyond tiresome, old debates in philosophy and to study the revolutionary potential of the sciences. Some, such as Reichenbach and Carnap, were highly respected by the scientific community for their early embrace of the importance of the Einsteinian revolution and their work on other aspects of physics[31]. Through Carnap and Neurath's publication of *The Encyclopedia of Unified Science*, the logical empiricists had an outlet for scholarship

[30] As is noted in Reisch (2005), various philosophers, largely associated with American pragmatism, did write about science prior to World War II. John Dewey wrote at length on science and his views have been cited by Mirowski (2003) as a neglected yet critically important philosophy. Following from the training of the logical empiricists, more and more analytically inclined philosophers of science developed, thus coming to characterize the discipline as it stands today.

aimed at uniting the languages of the sciences. Such work could potentially orient research toward bridging gaps between disciplines and motivating the sciences by searching for ways to solve problems that existed outside of disciplinary bounds[32].

In many contemporary accounts, the logical empiricists mistakenly practiced a highly technical philosophy that supported a value-free science[33]. The folly of this endeavor was exposed by historian Thomas Kuhn in his contribution to the Encyclopedia, *The Structure of Scientific Revolutions,* where he is said to have forcefully showed the important role history and politics play in the sciences, thus proving that "science is social," value-laden and cannot be captured in purely logical treatments. Once free of the confining grasp of the logical empiricists, new studies of science from sociology and philosophy opened the door to understanding the role of social values in science. It became imperative to abandon the old logical empiricist conceptions of rationality

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[31] See Friedman (2001), p. 30

[32] For a way to understand the classical canon of the logical empiricist research program, see Hempel's *Philosophy of Natural Science.* This piece comes after his 1952 rejection of Carnap's analytic/synthetic distinction, which some argue is the movement's most philosophically important thesis given the limitations of today's post-Quinean philosophical environment. Michael Friedman (2001) argues that Carnap's position on the analytic/synthetic distinction can be transformed into a new relativized conception of Kant's a priori knowledge, which can serve as a viable basis for a multi-tiered epistemology (which stands in contrast to Quine's one tiered epistemology). This point is relevant here, as the movement (discussed below) to reconsider the political history of logical empiricism is intricately bound with the philosophical program that Friedman is advocating. Friedman's relativized a priori is coupled with a point of view from the continental philosopher Jurgen Habermas, whose concept of communicative rationality is used by Friedman. This philosophical program might become another way in which the criticisms of science as guided primarily by political values can be brought back within a conception of science that is still fundamentally rational. The success of the political reconsideration of logical empiricism may well affect the philosophical reconsideration of logical empiricism, and vice versa.

[33] This is the perspective offered in Helen Longino's *Science as Social Knowledge*. In her caricature, the logical empiricists tried to remove any discussion of values in the

because such a conception could be used to enforce harmful biases. Science had to be watched so as to prevent the abuse and subjugation of society's oppressed who are seemingly persuaded into self-sacrifice by the value-free reputation that science is wrongly said to possess. The modern discipline of science and technology studies (STS) takes its cue from the sociological interpretation of Kuhn. While STS is highly varied and contains a number of conservative thinkers of its own, many of the most vocal critics of science come from within it and, as is described later, many of the advocates of democratized science come from STS as well.

Most philosophers of science avoided the strong sociological program towards science, which developed a critical distance between philosophy and the STS community. Some philosophers, such as Ronald Giere in his *Science Without Laws* (1999), conceive of philosophy as being one strand within the multiplicity of STS, and envision a role for a moderate sociological perspective in philosophical investigation. This moderation is not uncommon: many philosophers, including Kuhn himself, have fought at length to downplay the relativistic tendencies that the STS discipline eventually latched onto within Kuhn's book. As an alternative, these philosophers followed the position sketched in Frederick Suppe's *The Structure of Scientific Theory* (1977), which suggested a way forward by treating theories semantically. In Giere's subsequent approach from within the semantic tradition, theoretical models are applied to the world through a socially constructed similarity relationship, and the humble role of the philosopher is to analyze the logical characteristics of those theories.

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discussion of science. Philosophy needs to move beyond such a simplistic impression, but Longino attacks a straw man.

Regardless of the different positions held by STS and philosophy of science about the social influences of science, most of the intellectual descendents from Kuhn share the same naïve view of the logical empiricists. Recent studies of the history of the philosophy of science have challenged the politically disengaged and value-free conception of logical empiricism. The history of the logical empiricist movement's early years in America has been captured by George Reisch in his book, *How the Cold War Transformed Philosophy of Science*. In brief, the logical empiricists, while in Europe, were extremely politically motivated, and hoped to use the analytical insights of a philosophically inclined mind in order to help clarify political issues, educate the public, and make it more difficult for psuedorational political ideologies to control public opinion. The Unity of Science was primarily a political *movement* aimed at creating better interdisciplinary relationships amongst the sciences. The stereotypical view of logical empiricism cited above came about for a reason, however: the logical empiricists squelched their political motivations and talk about values in response to the intense scrutiny that they achieved in the wake of the Cold War. It was in the wake of this transformation that the movement appeared like the caricature that Kuhn supposedly attacked.

Logical empiricism is often described as having a ideological "Left," including Carnap and Neurath, and "Right," including Moritz Schlick. While generally united on their respect for the sciences, on the importance of a logical approach to understanding science, and on the existence of some form of a verifiability criterion, enormous differences existed. The Left of the Circle generally leaned toward a more coherentist approach to knowledge – i.e. that knowledge claims must be tested against the collective body of prior beliefs – while the Right tended to be more foundationalist. While this dichotomy of Left and Right is important, it is largely irrelevant in terms of the eventual impact of logical empiricism in America. Given Schlick's death and the lack of a prominent leader from the Right in America, Carnap and Neurath exerted the greatest influence on logical empiricism after the American emigration. The differences between these two, however, are substantial themselves and help to found one of the most historically important differences in the logical empiricist history, which eventually contributed to the philosophy of science becoming more technical and detached from society.

As described in detail by Reisch (2005), Carnap favored a more technical approach to the practice of philosophy, preferring to keep his values separate from his philosophic work. However, in his personal life, Carnap actively engaged in civic causes, with interests in racial inequality, peace in the Cold War, and academic freedom of speech[34]. Unlike some of his colleagues (some of whom expressed very little interest in society), Carnap thus took an active interest in social issues. For philosophers similarly interested in social issues, Carnap could represent one way for philosophers of science to be active in societal issues: by developing a political identity separate from their role as philosophers. As Reisch shows, the pressures of the Cold War led to Carnap's position being emulated by other philosophers of science who took a similarly technical and professional approach to their work. However, these philosophers, unlike Carnap, did not keep an active interest in social issues, which largely explains the detached orientation of philosophy of science that makes Kitcher's book somewhat unique in recent decades[35].

[34] Reisch (2005). Chapter 17.
[35] As has been hinted at previously, there are other current philosophers who try to engage society. Helen Longino is an important thinker who wrote some of the earliest

Unlike Carnap, Neurath seemed to envision philosophy as having a more direct relevance to politics and society. Neurath believed that the Unity of Science movement could serve as a focal point between the philosophy of science and society[36]. Neurath was drawn against metaphysics by principle: he belived that it could be used to justify and support totalitarian regimes. He believed that socialism could work, which involved some advocacy of central planning. He believed in a deeply pluralistic world, where multiple conceptions of reality could apply to available evidence equivalently. He was much more extroverted and charismatic than Carnap, and was deeply suspicious of metaphysics.

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work advocating for consideration of values in science (1990). Hugh Lacey (1999) and Miriam Solomon (2001) are two other philosophers who have pursued similar studies. Regardless, Kitcher's position is still uncommon, and the prominence of his book makes a powerful argument that can sway epistemic conservatives toward taking a more social approach toward understanding science. I also argue that Kitcher's approach to WOS offers some insight into the ethical landmarks that should be taken account of in practicing science policy which is worthwhile task, but one that must be complemented by other, more specific, work.

[36] Given that recent works have advocated for the Disunity of Science (Cartwright 1999, Galison and Stump 1995), there could be a tendency to applaud Neurath's democratic inclinations but to deplore his advocacy of the unity of science. Reisch strongly disagrees about the conclusiveness of any so-called proof of the disunity of science. "Far from a refutation of "unity of science," however, [Rosenberg, a recent critic of the unity of science] constructively engages the topic as Neurath and Carnap framed it…[L]ogical empiricists would generally have welcomed Rosenberg's result that considerations internal to science cast strong doubt on one kind of unity…"unity of law"…but [Carnap] would not accept that the result refutes the unity of science" (374-375). Reisch emphasizes that other kinds of unity, like Carnap's unity of concepts or other as yet undiscovered forms of unity, can still be attained. Reisch seems to say that because alternative concepts of unity will always exist, then Neurath's emphasis on unity was not foolish and was a plausible vehicle for his social agenda. However, this does not show why the UoS title is important. In constrast to the unity of laws (which could have important practical benefits), many of the alternative ways to attain unity, such as the unity of language, are much less interesting and not practically worthwhile. At the point where the Unity of Science thesis becomes unimportant, Neurath's project is interesting only for its collaborative and democratic dimensions – it could be retitled away from such a loaded title. As Creath (1995) argues, however, the emphasis on Unity of Science as a thesis underlies a concern about the public testability of language. Perhaps avoiding elite

As described above, the Cold War helped push all of philosophy of science away from societal engagement. Carnap's technical approach became emulated throughout the discipline, but today most do not even have the social concerns that Carnap espoused in his personal life[37]. As indicated by their 1929 manifesto, Carnap and Neurath both conceived of their philosophy as being relevant for social change. Neurath's death in 1945 and the close scrutiny put on Carnap during the McCarthy era buried this agenda. I want to argue that Neurath's Unity of Science movement can help to inspire a more democratic approach to science. I think that Carnap can provide much of the same philosophical justification for democratized science using his Principle of Tolerance. The combined strength of Carnap's technical rigor and Neurath's collaborative agenda could have created a powerful way for the philosophy of science to intersect with science policy.

I contend that Neurath's approach, including these above principles, is more democratic than was recognized at the time. This can be seen in several ways. First, the *Encyclopedia of Unified Science* was intended to be malleable over time, allowing itself to reflect the changing concerns of society and the scientific community[38]. Neurath

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languages can support more democratic approaches to science policy due to the public criterion.

[37] One could characterize Carnap's political involvement as being wholly separate from his philosophical work. Insofar as he engaged in politics, it was not as a philosopher, but as a citizen. Under this characterization, there would be no intersection between philosophers of science (working as philosophers) and science policy. I do not believe Carnap subscribed to this characterization: I believe he held his own philosophical work to be connected to the "pragmatics" of philosophy that are cited above in footnote 1. While his work would not have immediate connections to policy, they would still be connected, however indirectly.

[38] Even if Neurath embraced democratic approaches to governance, does he still buy into some form of elitism by restricting this democratic collaboration to scientists and other specialists? Likely, his UoS movement would have had that effect at first. But Neurath

emphasized collaboration, and insisted that he would not force language upon any community (Reisch 2005, p. 175). He would only implement what is mutually agreed upon, and insisted that the value of the Unity of Science movement lies primarily in this collaboration. Also, Neurath didn't believe that science had the answers to all situations: There is an "unpredictability in principle" that clouds much of the possible success that work can have[39]. Thus, no purely technocratic approach could succeed at predicting in full the occurrence of events. Perhaps Neurath thought that the democratic collaboration that he advocated was a strong response to dealing with this complexity in the world.

A Carnapian Justification for the Democratization of Science [40]:

The Principle of Tolerance is described in Carnap's *The Logical Syntax of Language* (1934):

"In logic, there are no morals. Everyone is at liberty to build up his own…form of logic, as he wishes" (52). "It is not our business to set up prohibitions, but to arrive at conventions." (51)

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seemed to take pains to try to keep the movement's intellectual trajectory in synch with the general public, such that it eventually would be able to incorporate a real democratic approach. His physicalism, after all, was oriented to create a language that was popularly accessible.

[39] Reisch (2005) describes Neurath's democratic tendencies. In particular, the ninth chapter addresses Horace Kallen's charge of totalitarianism and Neurath's defense against it. Against the charge that Unity of Science would require one uniform language for science (assumedly dictatorially imposed), Neurath responded that his pluralism stands in contrast to uniformity and that "more than one unified science is to be discussed" (183). Further, Neurath emphasized that the logical empiricists "rejected the plan of forming anything like a programme, and we stressed the point that actual cooperation in fruitful discussion would demonstrate how much unity of action can result, without any kind of authoritative integration" (175). Reisch (2001) describes Neurath on unpredictability in principle.

[40] I thought of this while reading Reisch (2005, p. 171), but I don't think he goes this far.

The Principle of Tolerance is a call for complete freedom in choosing logical systems. It is a recognition of the inefficiency that results from trying to mandate prohibitions in behavior. It abandons questions of uniqueness, or of a correct language (correctness is now conceived as always being internal to a language). With this newfound liberty, all concepts and ideas are free to be experimented with toward pragmatic goals. He stipulates that there are some theories that are observationally equivalent, and that the only difference between them is a matter of pragmatic effect. The choice between theories is a matter of convention, with the decision between them based always upon external, or pragmatic, considerations (Carnap 1953).

Carnap (though he does not say it in Carnap 1953), in practice seemed to include social values in these lists of external choices (Reisch 382-384). If Carnap was willing to tolerate anti-realists and nominalists, surely he would tolerate a social perspective. Given that there is no unique language frame, each scientific framework is chosen based upon some external values. Science is thus made significant through a social process much in the same way as Kitcher describes. Hence, while Kitcher was shrugging off some of his previously extreme philosophical positioning, the outlines of his democratic conclusion could have been attained using some oft misinterpreted philosophy. A Carnapian/Neurathian argument, using merely the Principle of Tolerance combined with some Neurathian flair, could serve to justify the democratization of science just as well.

Reisch advocates on behalf of Neurath's anti-metaphysical platform. Neurath rejected the notion that philosophy is the study of uniquely true ideas that are timeless and universal. If such ideas existed, then philosophy has a timeless purpose, that may be diverted temporarily by political influences but will always be headed toward an objectively significant view (in Kitcher's terminology). Referring to a view of theory as an "enduring, timeless object," Reisch continues the point by saying that "if philosophy of science is devoted to the study of anything like such an ontological domain of metaphysical objects… --- truth, explanation, confirmation… and so on" then "political forces could cause, at most, a temporary diversion in philosophy's historical development" (7). Reisch thus describes Neurath's attitude toward particular types of metaphysical philosophy: in the public sphere, the rejection of such a position is held to be emancipating for the individual. As should be clear, this Neurathian argument, which rejects platonic conceptions of truth, is politically used in a fashion that is similar to Kitcher's argument against a context-independent epistemic significance. Kitcher's attack against a context-independent source of epistemic significance should sit well in line with Neurath's overall position.

Neurath himself likely would consider Kitcher to be a conservative for being so late in rejecting a position of context-independent significance. At the very least, this is grounds to question the uniqueness of Kitcher's *STD* position based upon the richness of the Neurathian argument.

Reisch's Schema for Political Engagement:

Critically for the political purpose at hand, Reisch offers a schematic for how philosophy of science could become more politically relevant. In Reisch's conception,

> Philosophy of science must be conceived as a set of practices, values, goals and jargons that are chosen, utilized, and (hopefully) improved by individuals for their intellectual pursuits. These practices are taught to others and modified by debate as well as by often undetected historical or sociological processes. All these processes and the agents that sustain them exist in the same earthly plane, right alongside culture, society and politics (8).

Recognizing the ways in which philosophy as a discipline can be affected by society (just as science is affected by society) opens the door for greater political engagement by philosophers. At the 2006 Philosophy of Science Association meeting in Vancouver, Canada, a panel was held discussing Reisch's book. At the discussion, Reisch outlined two ways for philosophy of science to become more politically relevant.

> *Type One: "Political engagement consists in promoting policy on the basis of philosophical expertise in research"*
> *Type Two: "Political engagement consists in establishing collaborations amongst intellectual institutions [broadly construed] so as to better equip philosophers and their collaborators to engage in Type one engagements and to better equip citizens to critically evaluate political arguments (whether they be related to the Philosophy of science or not)"*

Type One engagement could consist of philosophers who engage directly in political issues and argue for particular conclusions on the basis of philosophical expertise. It was a matter of contention during discussion as to whether Type One engagement could involve advocacy for particular issues. Some philosophers, perhaps including Carnap, would say that philosophy cannot inform what the right course of action is, but other philosophers like Neurath might be more willing to use philosophy for normative purposes. Reisch seemingly believed that strong involvement on the part of philosophers on partisan issues should be encouraged. Not all Type One advocacy would need to be activist based: one panelist, Heather Douglas, suggested that philosophers of science might appear before federal advisory commissions in order to testify on standards of evidence. This type of interaction can utilize the philosophy of science as expertise even if one believes that philosophical analysis must always be value-free.

Type Two engagement was considered to be the more novel of the two concepts that Reisch was proposing. Reisch's conception of intellectual institutions was framed broadly so as to encourage interpreting it as language, university, culture, or government. If one assumes Reisch's stance that philosophers can use philosophy in an activist capacity, Type Two engagement is a general strategy for establishing connections to enable the philosophy of science to make a strong discipline. Reisch's fascination with Otto Neurath can help to illuminate his goals for a Type Two engagement. This new account of logical empiricism offers an important counter-perspective to an unquestioning embrace of Kitcher's *STD* because it contextualizes previous attempts to make the philosophy of science relevant. Is Kitcher less important if philosophy of science has been "political" for a long time? Certainly, Kitcher is not leading some revolutionary new charge of philosophy into the social realm, but he may be giving new, critical inspiration for philosophers and other academics to become more societally engaged. Instead of diminishing Kitcher, perhaps it's wiser to say, if philosophy of science has been primed to be used for political purposes (such as democratization) for a long time, then *STD* is a recent but valuable inspiration for further work.

In Reisch's terms, Kitcher's model can be seen as an attempt both at Type One and Type Two engagement. His arguments against particular philosophical positions such as context-independent significance and consequently for democratized science would be seen as Type One engagement. His general framing of the ideal of well-ordered science sets out a guideline in which one can structure institutions in order to enable more direct interventions, but would itself still be a Type Two engagement.

Perhaps the distinction between Type One and Type Two philosophy could have some relevance in considering the ways in which general philosophers of science and substantive philosophers of science work toward social goals (this distinction is described below). Substantive philosophers of science are more likely to be engaged in the specific details of a discipline, and subsequently to have expertise in areas of controversy. General philosophers of science might be better inclined to engage in a Kitcherian approach toward constructing a philosophical ideal that can interface with other disciplines.

Part Five: Interfacing Criticisms from STS

As has been described, the first half of *Science, Truth and Democracy* (*STD*) focuses on a variety of issues from the philosophy of science including realism, the Unity of Science and epistemic significance. The second part of the book goes beyond the traditional boundaries of the philosophy of science to construct an ideal for science policy, well-ordered science (WOS). The strengths and weaknesses of the two sections should be evaluated separately. This part of the thesis will elaborate more on the second half of *STD*, and will focus on arguing that WOS is Kitcher's attempt to create an interface between the philosophy of science and science policy academics[41]. The criticisms of Mark Brown and Philip Mirowski both attack *STD* by focusing on the utility of WOS, but they leave the general utility of Kitcher's philosophy of science positions from the first half of the book underutilized. Brown and Mirowski both look to John Dewey as an alternative source of inspiration for a philosophy of science policy; they see such a position as having philosophers get more engaged with specific issues and using the logical clarity that philosophers acquire to assess potential science policy frameworks.

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[41] "I hope the ideal will serve as a first shot at the kind of standard we need, and will provoke others to refine (or replace) it and to do the empirical work of connecting it with the concrete decisions that now confront us" (Kitcher 2001, p. 146). As noted below, Kitcher 2002 refers to well-ordered science as a common idiom for discussing the value influences of science.

Their approach, which might more closely relate to cultivation of Type One engagements for philosophers, is valuable, but it should be seen as a complement to the more general approach that Kitcher employs. Brown and others have given specific criticisms of WOS, but these criticisms can be embraced within an alternative version of WOS that can still have significant worth.

On Kitcher's Proposed Interface of Well-Ordered Science:

Kitcher's ideal is called well-ordered science (WOS), wherein a democratic process amongst a set of ideal representatives determines what good society wants science to achieve. In his bibliographic essay supporting WOS, Kitcher states that that his ideal owes an obvious debt to philosopher John Rawls[42]. For Rawls, justice is defined by an ideal deliberation. Within Rawls's ideal, members of society debate and decide upon just systems of government and moral actions. The idealized character of Rawls's approach comes from his assumption that the deliberators operate under a "Veil of Ignorance." The Veil of Ignorance holds that that each deliberator is separated from their prior values and commitments. The group then debates to establish a mutually satisfactory system of rules. Because each deliberator recognizes that they have an equal chance to endure the maximum amount of suffering in society, the deliberators will vote to minimize the worst possible outcome that they might survive under. Amidst possible social harms, the Veil of Ignorance would result in deliberation leading to the least harmful system for the least well off, which would then define what a just system would

[42] Kitcher (2001. p 210). For discussion by Mark Brown about the ways in which Kitcher's ideal should be considered to be Rawlsian in nature, see Brown (2004 pp. 83-86). Brown was a student of David Guston, and he does much to emphasize some of the motivations of the democratization of science movement. For example, Brown describes

be for all members of society. By reflecting on what a group of such ideal deliberators would choose as just, the embrace of a Rawlsian ideal can guide political discussions towards more just outcomes.

Kitcher's ideal of well-ordered science has some differences from the Rawlsian ideal, but it does have great similarities. There is no Veil of Ignorance in WOS. Rather, WOS's decision-making members are idealized in that they are assumed to be absolutely committed to continuing the democratic process to a conclusion, they are trained to respect the moral perspectives of others, and they are assumed to be able to make informed decisions about science. Kitcher indicates that his philosophical ideal could not be fully implemented in all science policy endeavours.

> There's no thought that well-ordered science must *actually institute* the complicated discussions I've envisaged. The thought is that, however inquiry proceeds, we want it to match the outcomes those complex procedures would achieve at the points I've indicated. Quite probably, setting up a vast populationwide discussion that mimicked the ideal procedure would be an extraordinarily bad idea, precisely because transactions among nonideal agents are both imperfect and costly. So the challenge is to find institutions that generate roughly the right results. (Kitcher 2001 p. 123)

Even though well-ordered science offers a sequence describing how a decision-making process could be accomplished, Kitcher's language here reflects a reluctance to consider utilizing actual democratic procedures. In *STD*, Kitcher uses this balance to effectively assuage concerns by scientists that he might impose a "vulgar democracy," which would involve public voting on scientific practice.

Political theorist Mark Brown focuses on the above passage as part of his critical 2004 review. Brown has two major criticisms of Kitcher. First, Brown rejects Kitcher's

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benefits of democratic participation, which includes the positive effects on citizens that reliance on an *ideal* as being abstract, and claims that philosophical ideals are anti-democratic when they are used to justify political action. Because *STD* uses WOS as the ultimate standard for determining the goals policy should achieve, Brown sees Kitcher as putting his philosophy "first," and politics "easily becomes restricted to questions of means" (88, 91). Brown argues that such reliance on ideals has throughout history been generally unsuccessful at actually generating reform, and that reliance on ideals can often lead to support of outcomes that are anti-democratic[43]. Brown argues that Kitcher is wrong to claim that there are no existing social theories of science that could facilitate democratized science, and includes a lengthy footnote listing literature on how to create democratic deliberation (78, FN 4, 84). Because Kitcher does not have a grasp of existing empirical knowledge about how participation works, the ideal of WOS is so disconnected from existing social science that it becomes very difficult to use WOS to enact positive social change. Strongly put, Kitcher's disinterest in the political process by default entails his disinterest in making his philosophy useful to other disciplines.

Brown's second criticism of Kitcher is that Kitcher does not actually support democracy or public participation. WOS is too focused on results, not enough on what people want (p. 83). Brown cites Kitcher's discussion of participation, but argues that the emphasis is still placed on goals, and not on having a genuinely democratic process. As hinted at in the above quote, Kitcher seemingly assumes a 'right' outcome, determined by an ideal, and thus lessens any commitment to the public being represented. Ideal models

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result from participation (87).

[43] Brown (2004 p. 89) dicusses the futility of applying ideals of communism, anarchism and other movements to politics. Further, "when public officials adopt ideals elaborated by philosophers, they risk setting political goals that are neither appropriate nor welcomed by their constituents" (90).

tend to push action on the level of individuals as opposed to collective bodies (and democracy), which further questions the utility of Kitcher's idealized approach for enacting social change (92).

Brown admits that he may be wrong in his interpretation that Kitcher does not support the democratization of science (88). I believe this to be the case. In several places in *STD*, Kitcher holds that his ideal should be a guide for implementing democratic processes[44]. Further, even within a WOS thought experiment, one must still determine what individuals in society would desire, and in order to use WOS as a guide in public policymaking one would need to find some avenues for attaining public input. Can one really determine what the democratically valuable outcomes would be without implementing some democratic procedures? Helen Longino (2002) interprets WOS as an invitation to create democratic processes within "small scale" policies that would mimic the societal deliberation that WOS entails[45]. This interpretation of Kitcher seems more apt. Kitcher's message likely is focused to allow the social science to guide the way in implementing procedures[46].

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[44] Kitcher (2001, p. 201) concludes by saying that he is hoping to attain more democratic policies. In a book with Democracy in the title, it's hard to imagine this not being the case. It does seem that it is because of Kitcher's desire to avoid the stereotypes associated with vulgar democracy that he emphasizes the notion of an ideal which Brown considers to be antidemocratic.

[45] (Longino 2002, p. 566, Note 3.) Brown quotes Longino's point in his review, but emphasizes that Kitcher does not make the point himself (p. 83, note 8). I think he's being uncharitable for reasons described in the previous note.

[46] Kitcher himself, in a 2004 reply to Helen Longino, states that WOS is an ideal, and that it is not meant to be implemented (569). "I *think* that Longino recognizes an important distinction between an *ideal* at which the sciences aim and a *procedure* for working towards that ideal; but since other readers confuse the two, it's worth making the point explicit. Well-ordered science is intended as an ideal, and, though my 2001 book poses the problem of how we might work towards this ideal, I believe that solving this problem (giving a substantial account of the governance of science) requires a significant body of

Should WOS inform the generation of policy and as such be used to help democratize science? Yes, some of the mechanisms within WOS are sufficiently detailed that they can provide rough guidelines for use in some policy situations. It is true that the more that WOS is applied on a large scale, the more impossible it is to generate actual democratic engagement as the discussion often breaks down. But even in small scale deliberations, WOS can indicate important ways to identify the role of social values in science, seemingly providing additional motivation for democratizing science. Further, because Kitcher casts WOS as an iterative process, it helps to emphasize that both pragmatic and epistemic values can change over time due to social influences. Given this temporal change it becomes clear that the goal WOS will aim for will change, it seems increasingly likely that any aspiration to WOS will require democratic policies that are sensitive to such social change. This likewise can inform policy by indicating how policies should adapt and try to garner new information as science develops.

Brown's Alternative: Dewey, Substantive Philosophy of Science:

On the last page of his essay, Brown sketches out his more preferred philosophical approach, which would operate in the spirit of John Dewey. Brown suggests joining "Dewey's search for a 'philosophy which will intervene between attachment to rule of thumb meddling and devotion to a systematized subordination of intelligence to preexisting ends…' Rather than offering an ideal standard to work for, a

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empirical knowledge (which I lack). Thus I provide evidence to show how scientific research currently seems to be organized in ways that lead it to diverge from my ideal, and invite a collaboration between philosophy of science and the social sciences to investigate how we might make up for the deficiencies" (569-570). Brown uses this quote to buttress his argument that *STD* doesn't actually support democratization, but I think that Longino's point and my attempt at a charitable interpretation give a better explanation.

political philosophy of science policy could explore other ideas and ideals that people could or do work with.” (94). The Deweyan approach would have philosophers get more actively engaged in applying philosophic concepts to tangible science policy questions. A Deweyan approach “might explicate the logic and implications of political practice; analyze the concepts and categories of science policy; and formulate hypotheses for research” (94). Brown makes no argument as to why such an approach would be valuable, although he implies that such an approach would better enable philosophers to help resolve “tensions between science and democracy” (94). Given the outline of Dewey that Brown provides, I believe that such an approach could very well be fruitful, and I would be eager for more clarification and elaboration.

As it is not clear what Brown’s alternative is, I argue that it seemingly would (or more likely should) embrace what I will here call substantive philosophy of science. Within philosophy of science, work done on general issues such as confirmation, explanation or unification touch on classical studies within philosophy that can be applied across the scientific disciplines. Such work exists within ‘the general philosophy of science.’ By contrast, philosophers who focus on specific issues within the sciences, such as the conceptual adequacy of concepts such as species in biology or on the robustness of disease categories in medicine, practice ‘substantive philosophy of sciences.’ Perhaps Brown’s Deweyan philosophy would have more of an emphasis on focusing on specific issues within science (as he says, to explore ideas that scientists and policymakers actually work with), which could strongly encourage substantive philosophy of science to become involved in the democratic dialogue that Brown encourages.

This identification of the Deweyan approach with a "substantive" approach to philosophy seems apt given the above passage from Dewey on which Brown focuses (94). Many public debates about science (such as those about stem-cells, cloning, genetically modified food, or potentially toxic nanoparticles) involve specific questions about science that can be fruitfully examined by philosophers who specialize in specific sciences[47]. Ergo, a philosopher of biology who has an understanding of embryonic development can serve as a moderator in actual democratic deliberation; his or her focus would be to parse apart ethical framings and logic employed in the debate and to use their philosophical knowledge to better frame scientific issues[48]. Substantive philosophers of science can get actively involved in explicating the logical concepts that are at play in actual democratic deliberation, which may be what Brown is advocating for when he wants a philosophy to "explicate the logic and implications of political practice". The facilitation of public discussions about science by substantive philosophers of science would create a role for philosophy to assist in the democratic process in a way that Brown advocates for. The potential benefits of this kind of Deweyan approach are striking in light of the potential facilitation of democratic deliberation.

[47] It seems to me that most public debates related to science don't generally involve questions related to general philosophy of science. This would seemingly emphasize the connection between substantive philosophy of science and Brown's Deweyan approach, which could resolve democratic tensions surrounding science and touch on ideas that people actually use. Possible exceptions, where general philosophical questions are controversial, could involve situations of justifying the epistemic significance of particular research.

[48] This type of philosophical work seems to relate to the approach taken by Dr. Jason Robert in his attempts to develop anticipatory democratic deliberation about scientific issues. This certainly deals with ideas people work with during existing debates about science.

I thus am arguing that the Deweyan approach should critically involve a role for substantive philosophy of science. In contrast to this substantive approach, Kitcher's conception of WOS can be fairly characterized as strongly relating to general philosophy of science. Many of the arguments that Kitcher (who himself is an outstanding philosopher of biology) uses to underpin WOS include arguments from the general philosophy of science, such as arguments about explanation and the Unity of Science thesis. As was discussed above, Kitcher's argument against the context independence of epistemic significance stems from the argument that only human context can serve to make epistemic discoveries significant.

Brown ignores much of the potential utility of Kitcher's arguments from the general philosophy of science. He doesn't find a problem with any of Kitcher's arguments from the philosophy of science, and indeed he applauds Kitcher for his arguments against common defenses of "pure" research. Thus, a Deweyan approach to philosophy of science could still utilize all of Kitcher's philosophy of science, such as his distinction of epistemic and practical significance values, his description of how those values are socially constructed, and his denial of an objective (and assumedly overriding) epistemic significance justifying science. Further, Kitcher's perspective about the ontological limitations of science offer, as was discussed in Part One, important ways to frame decisions about science policy. The entire first half of *Science, Truth and Democracy* thus contains useful arguments from within the philosophy of science that can guide science policy decision making in ways Brown would value.

Brown's emphasis on critiquing Kitcher's proposed interface of WOS rightly focuses attention on ways to orient philosophy of science to interact with other science studies disciplines. Addressing Brown's criticisms that WOS is too idealized to be relevant for policy is critical if WOS is to assist in Reisch's conception of Type Two engagement. As all of Kitcher's philosophical positions can fruitfully be used to advocate for democratic goals, the importance of having a common 'idiom,' well-ordered science, with which to make other science policy academics aware of their utility becomes clear (Kitcher 2002, p. 571). Addressing such interfacing concerns can help to better connect the philosophy of science institutionally with other disciplines, and is a critical part of an adequate political philosophy of science policy.

Mirowski:

Brown's criticism focuses on political theory in order to criticize Kitcher's proposed interface. Another influential attack was leveled by Notre Dame Professor of Economics and History and Philosophy of Science, Philip Mirowski, in a 2003 article. Mirowski likewise argues that an ideal (he terms WOS to be a "game-theoretic model") is ineffective, but he makes two further criticisms. He holds that philosophers of science have historically been blind to social manipulation of their own discipline and that Kitcher's ideal for WOS is irrelevant because of its ignorance of economic changes affecting science.

Mirowski argues that philosophers of science are overly self-congratulatory about their recent attention to the social influences on science. He discusses how philosophers have been thinking about science and democracy for a long time, and argues that pre-World War II US science was highly corporatized. He cites Dewey as being an early, intelligent (and ignored) philosopher on issues of science and democracy; Dewey accurately measured the corporate influence on science of his time, but he underestimated the difficulty of implementing democratic processes and his philosophy lost relevance when science became military-focused rather than corporate focused. It is interesting that Mirowski cites Dewey in a context that is significantly different from Brown. He focuses on Dewey's philosophy of science, whereas Brown only focused on utilizing Dewey's general approach to ethics as a guide for a philosophy of science policy. This will not be examined here but Dewey could become a more important influence on discussions about how to make use of philosophy in politically relevant contexts[49]. (It is interesting to note that Kitcher is now the John Dewey Professor of Philosophy at Columbia, and has stated that he is writing a book on "pragmatism that will elaborate a general Deweyan approach to philosophy"[50]).

Mirowski begins to establish his criticism of the societal insularity of philosophy of science by looking at one of its greatest influences, logical empiricism. Mirowski paints the empiricists in a very dark light by focusing on Hans Reichenbach, another logical empiricist. Following the standard arguments against logical empiricism, he negatively describes how Reichenbach worked to make philosophy of science a much more mathematical and technical endeavor, and offers a variety of arguments about how logical empiricists attempt to keep values external to science (such as with the distinction between the context of justification and the context of discovery). He cites some of the new Reisch literature about the social ambitions of the logical empiricists, and admits that

[49] I talked to Brown in February, 2007 at the American Association for the Advancement of Sciences meeting in San Francisco. He says he indeed means to include both Dewey's ethics and his philosophy of science, just as Mirowski does, and that his dissertation has a section on this topic. I will look forward to pursuing this angle in the future, as I learn more about Dewey and other pragmatists.
[50] Barndt (2006).

he is presenting a caricature of logical empiricism[51]. Mirowski's key point in terms of his economic-ignorance argument is that scientific research became military focused during this time, with much work being spent on Operations Research where the military, through the RAND Institute, tried to have experts on "general scientific method" exposit on any variety of issues. Philosophers like Reichenbach and Carnap were hired by RAND, and the spectre of Operations Research helped to orient much of the logical empiricist program, of which current philosophers are ignorant.

In examining recent philosophy of science, Mirowski notes the commonplace that science today is considered to be much more corporatized, but thinks philosophers are critically negligent in how they approach this change. In his view, science went from being corporate in Dewey's day, to being guided by the military industrial complex in Reichenbach's, and today is back again to being corporate and tech-transfer driven. Most philosophers of science have ignored the economic changes that science has undergone, and as such have embraced the notion of science as separate from society. He focuses on Kitcher to criticize much of the recent philosophy of science. Generally, he says that Kitcher says he supports democracy, but in effect he does little more than reinforce the idea of science as an isolated enterprise. He says WOS is so abstract as to be meaningless in today's world of privatized science and patent abuse, political work by scientists, among other things. He bashes WOS as a game-theoretic model that, because it isn't meant to represent how people actually work, becomes as detached from society as Reichenbach supposedly was.

[51] Uebel (2004) criticizes Mirowski's depiction of logical empiricism as attacking a straw man. Reisch (2005) has his own criticisms of Reichenbach. He might likely agree with

If Mirowski is right about the importance of economic changes surrounding science, any proposed political interface for philosophy of science should be able to address the issue[52]. There can be ways that WOS could be augmented to better apply to the governance of private research by including within its possible outcomes recommended restrictions on private research or far more plausible programs to provide a combination of positive and negative incentives to private industry so as to direct them away from undesirable research. Further, publicly funded science still plays a key role in establishing the long term research agenda for private science (Sarewitz 2003). Even if Kitcher's WOS genuinely fails to address the problem, however, this is not so great a failure as Mirowski seems to imply as no one, in philosophy or in STS, has a plausible approach for effectively dealing with the corporate world in science today. Further, as was argued in the response to Brown, many of Kitcher's general philosophical positions can still be used to examine many of the social influences on science and societal outcomes in 'private' science.

Conclusion of the Thesis:

Democratization of Science as a Goal:

Philosophy of science has recently been disconnected from engagement with larger societal issues. As argued in Part Two, broad philosophical framings always have some bearing (be it small or large) on how one approaches science policy, but one certainly need not be a philosopher to make reasoned decisions. Philip Kitcher's *Science,*

Mirowski's negative view of Reichenbach, but distance it from logical empiricism generally.

[52] Longino (2002) discusses the difficulty of applying WOS to the private sphere. See my footnote 13 (from part 1) for more information. Kitcher (2002) responds by briefly discussing ways in which WOS can be applied to the private sector.

Truth and Democracy is an important attempt to engage with science policy in a democratic way. Kitcher changed his philosophical positions over the course of the past few decades, and philosophical arguments about the unity of science had a major role in pushing him toward a democratic position. Criticisms of the book help to indicate the challenges of using philosophy to justify social change: it is difficult to compel action using abstract philosophical debates, and effective action requires a much more grounded social scientific knowledge than most philosophers can possess. Kitcher's argument against context-independent epistemic significance can be an important way to argue for the role of values in framing science, and thus in establishing more democratic approaches to science. A philosophical text like *Science, Truth and Democracy* can be read by people throughout the world, and has the capacity to create change in ways that simple political activism cannot.

As touched upon in Appendix One, I believe there are significant advantages to embracing a democratic approach toward science that can offer an approach for political engagement. Some philosophers might want to avoid Reisch's conception of Type One political engagement – which advocates for policy on the basis of philosophical expertise – because they don't believe that philosophers have relevant expertise on policy, and such political engagement would be based purely upon subjective personal values[53]. This point could be disputed, but I think that a different form of political engagement is more worthwhile. Reisch's conception of Type Two political engagement – where

[53] This concern might map onto discussions of the "Value-Free Ideal" that is argued against by Heather Douglas. If philosophers believe their work to be value-free, then political engagement should be avoided except on purely technical matters. My point here is that, even if one accepts the value-free ideal, democratization of science is still

philosophers would work in collaboration with others to generate more reflexive institutions – could be focused upon the democratization of science. As argued in the appendix, democratization of science seems to be objectively valuable, requiring no subjective ethical commitments beyond a commitment to democracy itself. There are risks in democracy as the public will can sometimes lead to harm, but democracy is the mechanism for guiding society least likely to engage in prejudice. Democratized science as a collaborative goal can allow for greater sensitivity to societal values and a greater emphasis on working to ensure that scientific research can attain desired philosophical outcomes.

Philosophical Paths for the Engagement of Science Policy:

Philosophers can play role in such a collaboration regarding the democratization of science. Even if one disagrees with the democratization thesis, my work here tries to envision philosophical engagement with science policy across the board. As argued above, nanotechnology, because of its relative youth, offers a fresh beginning for a new approach to science policy. Kitcher positions his ideal of well-ordered science as an interface to collaborate with the social sciences. While I do not believe that social science research should become the complement to Kitcher's well-ordered science, I believe that Kitcher's work can help to motivate and justify efforts aimed at transforming nanotechnology policy toward potentially desirable ends. Kitcher's work has value for this purpose.

Should Kitcher, then, become the model for similarly interested philosophers of science to engage in science policy? I argue no. I want to contextualize Kitcher's two

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desirable as per the benefits described by the social science literature mentioned in the contributions – his justification of the democratic scrutiny of science and his ideal of well-ordered science – as being part of a *general* approach toward engaging science policy. Kitcher's arguments have value, but my alternative argument to justify democracy of science using the writings of Carnap and Neurath has changed my conception of the role of philosophy in political engagement. The Principle of Tolerance and Neurath's collaborative and anti-totalitarian attitude could be simple and effective ways for advocating for democratized science, perhaps moreso than Kitcher's approach. Given that the democratization of science could be justified through numerous arguments, I do not see how the creation of further justifications for democratizing science is vitally important. Further, Kitcher's other general contribution, well-ordered science, can likewise be useful to identify important ethical issues and to inspire change, but it is drastically disconnected from specific and local issues regarding science policy. The value of involving numerous philosophers at this general level is not clear.

For political engagement by philosophers to be worthwhile, there needs to be more specific, practical and worthwhile roles for philosophers to engage in. It is difficult to sketch out a complete alternative. For myself, I have become interested in philosophical literature about modeling, I recently presented a paper highlighting some of the limits that global climate model predictions can have, and argued that these limitations establish the importance of incorporating recent social science on how to adapt to climate change and make decisions. Discussion of models, both within science and as they are used for policy, can offer a role for a philosopher to collaborate with

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appendix.

others to work for conceptual clarification on issues that are of great social relevance.

This is not a grand role, but is likely to be my future for the next few years.

Appendix One: What is the Democratization of Science and Why is it Valuable?

My discussion of democratizing science is focused on questions of accountability, direction of research, and the method by which research should be done. As Kitcher rightly maligns in *Science, Truth and Democracy*, a simple up or down vote on scientific issues across the board would not be effective and could likely be disastrous. I will briefly illustrate current connections between democracy and science and indicate more tangible ways in which democratic approaches to science can be undertaken. I will also try to indicate how democratization of science can become a way of dealing with a complex world. Democratized science policy will thus include better approaches to dealing with the funding of science research as well as how to incorporate public and scientific input in larger policy problems.

Science policy as it exists today has democratic inputs as the US Congress authorizes and appropriates funds for scientific and technological research and development. At the congressional level, total budgets are assigned broadly, as funding is apportioned generally to entire agencies, such as the National Institutes of Health or the National Science Foundation[54]. There is no detailed selection of individual projects at the congressional level. Such deliberation occurs within individual government agencies which solicit research proposals from the scientific community and utilize peer review to select the research. Given that Congress is the democratic body representing the people, the agency level peer review selection process introduces a critical distance between democracy and science. This is not necessarily bad: it might be impossible, given the

[54] For a token summary of how funding is apportioned, see [http://www.nsf.gov/statistics/infbrief/nsf06330/]

time limitations and level of science education in the US Congress, to select in detail individual research projects at the congressional level. Further, the individual scientific agencies make efforts to include the desires of the public within the peer review process. For example, the National Science Foundation includes a Second Criterion on all of its solicitations, requiring scientists to detail the proposed public benefit of the research. However, there are some criticisms of how the agencies select research to fund (as well as strong criticisms of the NSF Second Criterion specifically)[55].

I advocate that democratized science involves the introduction of more detailed scientific scrutiny at the congressional level as well as efforts to better incorporate democratic input within the funding agencies. The beginnings of such an approach are present at the federal level with the recent calls for a new science of science policy. At a recent conference[56], President Bush's science advisor, John Marburger, proclaimed his favorite science policy article to be Daniel Sarewitz's 2003 paper, "Does Science Policy Exist, and If So, Does it Matter?" Marburger cited Sarewitz's insight that the R&D budget has remained a constant fraction of the overall federal budget, which entailed that

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[55] For example, see NAPA (2001).

[56] March 31st 2007, Science and Technology in Society: An Interdisciplinary Graduate Student Conference, in Washington, D.C. I had the opportunity to ask Marburger a question; I asked him to justify his claim that science policy academics impact science policy decisions, and if I was correct in interpreting his new science for science policy as including research on the social and ethical aspects of science. He answered the first question by discussing how he and others have to respond to analysis that is developed by science policy academics, and that it has influenced him. He did not answer the second question, which I think indicates that I misinterpreted him. My strong suspicion is that he views his new science for science policy as a purely economic tool which ignores many of the important ethical dimensions in science policy that Kitcher's well-ordered science highlights. It will be interesting to see how the research plays out, but this is certainly a good beginning.

there is always competition for limited scientific resources[57]. In order to resolve this competition of research, the government needs to have strong analytical tools that can better identify research with tangible public benefits. To fulfill this demand, Marburger looks to his new multi-million dollar funding solicitations for a science for science policy. This research would develop tools to evaluate the economic benefit of proposed research, which could help introduce accountability for scientists to show that their work has public value.

A science for science policy could become a tool to allow for more democratic control of science. Congress could better assess research and have the tools to attempt to direct it towards more democratically desired goals[58]. A science for science policy might also help to indicate better ways to incorporate public perspective internally within R&D agencies.

Democratized science should also be applied to larger policy decisions throughout the government. In a variety of areas external to the mere funding of research, society needs to be able to reflexively adapt to technological change (Guston and Sarewitz 2003). There is a large literature assessing the difficulty of making decisions in a world whose socio-technical systems are becoming so complex that they can no longer be modeled in simplistic predictive fashion (Allenby 2005, Sarewitz et. al. 2001, Schwarz and

[57] On listening to Marburger's talk, it seems clear to me that Sarewitz has had a very strong influence on his thinking and on his new call for a science for science policy. My coursework with Sarewitz is what initially influenced me to take an interest in democratized science.

[58] Even if Marburger narrowly construes the notion of a science for science policy as a tool to assess the economic benefits of particular research programs, such an analysis will still enable Congress to better direct science to democratically desired *economic* goals. Other efforts on the societal implications of scientific research might help to guide future work in the science for science policy toward more expansive programs.

Thompson 1990, Scott 1990). Even if accurate predictive ability becomes nonexistent, technological and environmental change will still greatly affect (and be affected by) our political, economic and cultural existence. Societies that can implicitly learn to adapt to societal change will be able to simulate some aspect autonomous control over technology, such that they can better orient technology with societally desired outcomes. Democratized science could seek to bring out the variety of perspectives that are necessary to begin to understand changes as they occur, allowing for a chance to adapt to rapid change. Democratic deliberation about science could distribute significant amounts of decision-making to local groups and individuals (as opposed to a loose centralization of science policy focused around funding agencies, with little attempt to understand the scope of research programs), which would have a chance to try to direct technological change at a large number of levels (for an example of success, see Lach et al 2005). Programs like ASU's real-time technology assessment can help generate some reflexive understanding of emerging technologies that could foster more democratized science and could provide the preemptive predictions and deliberations necessary to make meaningful decisions in a chaotic world.

I conclude this section by stating that philosophers who conceive of science as value-free could and should still find more democratic approaches to science desirable. So long as one is committed to democratic forms of government generally, there is no specific and direct link between advocacy of democratized science and particular ethical points of view. Democratized science could well produce outcomes that one thinks are disagreeable, but such is a necessary risk if one is wary of more totalitarian forms of government. Given that advocating democratized science asserts no one individual’s ethical perspective, and given the potential societal benefits of democratized science indicated by the social science literature cited above, democratized science could well serve as the locus for the Type Two engagement that George Reisch advocates (as described in Part Four of the thesis).

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Acknowledgements:

I am very grateful to my entire committee for an engaging defense and insightful commentary. I would like to acknowledge Dr. David Guston for his gracious support through the Center for Nanotechnology in Society. I'd also like to thank Dr. Heather Douglas for traveling here from the University of Tennessee – Knoxville for my defense and for her insights on engaging science policy as a philosopher. I'm grateful to Barrett, the honors college for the chance to bring her here.

I attribute much of my interest in the philosophy of science to my coursework with Dr. Richard Creath, and I attribute much of my interest in science policy to my coursework with Dr. Daniel Sarewitz. Dr. Jason Robert has been a good mentor given my dual interests in the philosophy of science and science policy. I owe all three greatly. My time spent in Dr. Robert's laboratory and working in the Consortium for Science, Policy and Outcomes has given me a welcome and engaging intellectual environment as well as great friendships.