Examining the Experts: Science, Values, and Democracy

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Dissertation Advisor: Professor Nancy Rosenblum Zeynep Pamuk

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Abstract

This dissertation examines the role of experts in democracies, with a focus on issues that involve the translation and use of science in political decisions. Conventional accounts of the relationship between expertise and politics have assumed the validity of a Weberian division of labor, in which experts provide a neutral assessment of the facts, while citizens and their representatives supply the values necessary for political judgment. I challenge this model on the grounds that it presupposes an outdated view of scientific inquiry as a value-free process, and develop a new normative theory for the use of expertise in politics that builds on a wide range of recent work in the philosophy of science that shows how the values, assumptions and purposes of experts shape the production of scientific knowledge. My main argument is that the role of values in science makes it problematic to leave the determination of the science entirely to scientists in policy decisions. I argue on both epistemic and democratic grounds that scientific claims must be submitted to critical democratic scrutiny to prevent democratic policy from being guided by the unexamined judgments of experts on scientific issues such as climate change, biotechnology, obesity, nuclear weapons and environmental safety.

The basic argument is simple, but demonstrating its plausibility requires addressing three important challenges, which correspond to the three parts of the project. The first challenge is to trace the theoretical link from a particular philosophy of scientific knowledge and uncertainty to the necessity of particular forms of

democratic (rather than scientific) contestation, and to provide an account of what democratic scrutiny could accomplish on scientific issues. The second challenge is to show how democratic debate on science should be structured in order to realize the epistemic and democratic goals outlined in the first part. I develop an institutional proposal for publicly-monitored, adversarial science courts with citizen juries, designed to overcome the difficulties of deliberation between those who have asymmetric knowledge and authority, and the difficulties of democratic participation and accountability on complex issues in a public sphere that is often highly distortive. The first two parts take the science as given and focus on its use in political decision-making. But the agenda for political debate on science is largely determined by earlier decisions about which research should be pursued and how, typically made at the funding stage. In the third part, I turn to institutions of public funding for science, and develop a theory of the proper forms of democratic input into science funding to enhance the possibilities for the democratic use of expertise at the decision stage.

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To my parents

“It would not only be foolish, *but downright irresponsible* to accept the judgment of scientists and physicians without further examination.”

Paul Feyerabend, *Science in a Free Society*

“The rational layman will recognize that, in matters about which there is good reason to believe that there is expert opinion, he ought (methodologically) not make up his own mind.”

John Hardwig, “Epistemic Dependence”

Introduction

On October 22, 2012, in the small Italian town of L'Aquila, seven earthquake experts were convicted of manslaughter and sentenced to six years in prison.[1] The prosecutor claimed that they had failed to adequately assess and communicate seismic risks to the city ahead of the earthquake in 2009, which resulted in the death of 309 residents. In the three months preceding the earthquake, the city had experienced about two or three low-level tremors daily – an event that experts call a seismic swarm.[2] An additional 57 tremors took place in the five days before.[3] Residents were unnerved and looked to experts for guidance on whether these tremors signaled a major earthquake, and if so, whether they should evacuate the city. Their worries were exacerbated when a local lab technician named Giampaolo Giuliani began to predict a major earthquake on the basis of his measurement of radon gas levels.[4] The scientific community had repeatedly rejected the reliability of radon measurements for short-term predictions of earthquakes, and Giuliani had been denied funding for his research several times because his work was insufficiently scientific.[5] This did not stop him from setting up a website to post daily radon readings and sharing his predictions with the locals. A few days before the earthquake hit, the mayor issued a gagging order on Giuliani for fear that his website would provoke panic in the residents.

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[1] Elisabetta Polovedo and Henry Fountain, "Italy Orders Jail Terms for 7 Who Did Not Warn

[2] Stephen S. Hall, "Scientists on Trial: At Fault?" *Nature*, 14 September 2011, Web.

[3] Ibid.

[4] Ibid.

[5] John Dollar, "The Man Who Predicted an Earthquake," *The Guardian,* 5 April 2010.

It was in this context that the Italian Civil Protection Department and local officials decided to hold a meeting with seven seismologists to assess the probability that the seismic swarm in L'Aquila might precede a major earthquake. The current scientific wisdom is that this is quite rare. A 1988 paper found that seismic swarms precede a major earthquake only about 2% of the time.[6] According to the meeting minutes, Enzo Boschi, one of the scientists participating in the meeting, said, "It is unlikely that an earthquake like the one in 1703 could occur in the short term, but the possibility cannot be totally excluded."[7] The meeting was very short and was followed by a press conference in which Bernardo De Bernardinis, vice-director of the Department of Civil Protection, announced that the situation was "certainly normal" and added, "The scientific community tells me there is no danger because there is an ongoing discharge of energy."[8]

This press conference was the ground for the charges that led to the conviction of the experts. The charge was not a failure to predict the earthquake, which the prosecutor recognized was not possible given the current state of scientific knowledge, but rather the misleading assurance by a group of experts that there was no danger. He claimed that this message had led residents – and especially the younger and more educated ones – to change their plans and stay in L'Aquila, with disastrous consequences.[9]

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[6] Guiseppe Grandori et al., "Alarm Systems Based on a Pair of Short-Term Earthquake Precursors," *Bulletin of the Seismological Society of America,* 78 (4), 1988.

[7] Hall, "Scientists on Trial."

[8] Nicola Nosengo, "Italian Court Finds Seismologists Guilty of Manslaughter," *Nature,* 22 October 2012, Web.

[9] Hall, "Scientists on Trial."

This small but dramatic episode illustrates some of the key features of the use and misuse of expert advice.[10] On the one hand, it shows the dependence of citizens and public officials on scientific expertise on a matter literally of life and death. The residents of L'Aquila turned to science for an explanation of the frightening and unknown natural event they were experiencing and expected an assessment of the risks of a potential disaster. The science was absolutely everything on this issue. Attempting to express the problem as a conflict over values, such as whether the residents were the sorts of people who would leave their city when faced with an existential threat, would be to miss the point completely. The questions that mattered here were factual: What was the likelihood of an earthquake, and if an earthquake hit the city, what was the risk of harm to the residents of a city with so many old buildings?

On the other hand, the incident also shows the potential limitations of scientific knowledge. Like many other areas of science, though more so than most, earthquake science is uncertain and inexact. Scientists have become increasingly capable of predicting the likelihood that an earthquake will strike a given area within a given time period, but there is still no accepted scientific method for reliable short-term prediction.[11] The seismologists who were consulted had some data on the likelihood of a major earthquake in the days following a seismic swarm, but these findings were far from conclusive.

After the highly publicized trial, scientists and scientific associations around the world protested the conviction on the grounds that it penalized scientists for

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[10] See Melissa Lane, "When the Experts are Uncertain: Scientific Knowledge and the Ethics of Democratic Judgment," *Episteme,* 11 (1), 2014 for a detailed discussion of the role of values and uncertainty in this case.

[11] Polovedo and Fountain, "Italy Orders Jail Terms for 7 Who Did Not Warn of Earthquake."

making a prediction that turned out to be incorrect. The President of the American Association for the Advancement of Science wrote a letter to the President of Italy, arguing that this kind of treatment would have a chilling effect and discourage scientists from public engagement. While the scapegoating of scientists through the criminal system was clearly not an appropriate response to what had taken place, this reaction was a symptom of the mishandling of expert advice before the earthquake. The officials had denied the public a chance to understand the content and uncertainty of the science, instead delivering an authoritative judgment with an appeal to the views of "the scientific community." This had created a false sense of security that arguably led to a higher death toll from the earthquake. It had also deprived citizens of the ability to evaluate the information for themselves and to reach an informed decision about the level of risk they would be willing to accept.

Thinking about the mistakes made in L'Aquila involves asking three interrelated questions: 1) What kind and level of public engagement with the content of the science was appropriate; 2) How the answer to this depended on the epistemic status of earthquake science – its certainty, reliability and applicability to that particular time and place; and 3) How the answer to these two questions depended on the circulation of pseudoscientific predictions that spread panic among citizens. This dissertation addresses these questions in the broader context of the use of scientific expertise in politics.

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The L'Aquila case is a particularly dramatic example of a community's dependence on expert advice and the potential consequences of its misuse, but it is hardly unique. Our ability to act on some of the biggest problems of our times, such as climate change, biotechnology, nuclear weapons or environmental safety, requires relying on the knowledge provided by scientists and other experts. The modern state has struck an unprecedented partnership with science as its designated source of knowledge and as a means for bringing about better outcomes. New scientific research determines what we take to be our problems, as well as the range of options we have for solving them.

Meanwhile, contemporary political life is increasingly characterized by pathological treatments of expertise, with denials of science and distrust of scientists on the one hand, and appeals to the authority of experts and complaints about the ignorance of the citizenry on the other. These attitudes are often intensified in reaction to one another: frustration with denial and pseudoscience leads to increased appeals to the authority of scientists, which in turn generates resentment – and more denial. It is a vicious cycle. In the L'Aquila case, the local officials' appeal to the authority of science to reassure the public was an ill-conceived attempt to respond to the panic caused by Giuliani's scientifically dubious alarmism. The officials' appeal, in turn, precipitated a public backlash against scientists after the earthquake. Climate change provides a similar story: for a long time, the widespread response to the denial of climate change in the United States was to appeal to the consensus among 97 percent of scientists, which created the impression that the public was being asked to take the agreement of experts as a substitute for scientific evidence.

The partnership between democracy and expertise is intrinsically unstable. Democracy holds out the promise that the people will rule themselves by shaping the laws that will be binding upon them, either directly or through elected representatives. Expert knowledge threatens to alter or limit the possibilities for democratic action. It presents a rival source of authority in the public sphere, one that is based on truth, rather than agreement. This creates the constant danger that the authority of experts will be used to circumvent democracy and that claims to scientific knowledge will crowd out the space for deliberation over how to shape a collective existence.

At the same time, expertise has no direct access to political power; it is a source of authority that lacks the means to coerce. Even though the truth of scientific claims does not depend on the number of people who believe in them, their uptake in politics inevitably goes through persuasion. In the realm of politics, scientists must convince people who do not share the scientific community's practices for settling the truth of competing claims. Citizens and their representatives always retain the right to reject scientific knowledge – a right that they exercise quite often.

Efforts to eliminate this inherent tension would be problematic for both science and politics. Determining scientific truth democratically can have disastrous consequences, while justifying democratic decisions by appeal to standards of scientific correctness risks jeopardizing the legitimacy of democratic decision-making, especially when it falls clearly short of the relevant standard. The challenge is to devise ways for expertise and democracy to coexist productively. Expert knowledge could be used to expand the power of politics, or it could lead to the alienation of citizens from a politics that seems to defy their control. The success of the relationship between democracy and expertise depends on whether democracies can find ways to use expertise to further their own ends and produce good outcomes. Recent failures in the use of science for political decisions on issues such as climate change, vaccines, GMOs or earthquake warnings suggest that it is necessary to rethink how the relationship between science and politics should be structured. These are not just failures of political practice; they are also failures of political theory.

The tension between expertise and democracy is by no means a new problem, but the alliance between the state and the scientific enterprise, cemented with the provision of large amounts of public funds for science, is an unprecedented mid-twentieth-century development. Earlier thinkers such as Mill and Dewey, who were concerned with problem of expertise in politics, wrote with a very different model of scientific inquiry in mind – one that consisted of the private activities of curious individuals. The idea of a professional scientist was a novelty, as many scientists lacked formal training. The term "scientist" was coined by Whewell in 1833.[12] This earlier picture of science as an amateur project stands in stark contrast with the sophisticated, highly professionalized and expensive scientific enterprise that was established in the latter part of the twentieth century. The emergence of an insulated and self-regulating scientific community with shared credentials, norms and standards, whose findings have direct influence over the policymaking process, has given the old problem of experts an entirely new aspect. This calls for new answers.

It has also become increasingly obvious that a widely accepted model for the relationship between expertise and politics has serious limitations. The twentieth-century solution to the problem of expertise, developed mostly in the context of social science and especially economics, was to maintain a division of labor between experts and laypeople, modeled after the Weberian account of the relationship between bureaucracy and political leadership. On this view, experts would provide a neutral assessment of the facts, while citizens and their representatives would supply the values necessary for political judgment. Although Weber was pessimistic about the ability of bureaucracies to be truly neutral, he held this up as the ideal to strive for. Isaiah Berlin gave a clear expression of this same view in the opening lines of his famous 1958 essay "Two Concepts of Liberty": "Where ends are agreed, the only questions left are those of means, and these are not political but technical, that is to

[12] Laura Snyder, *Reforming Philosophy: A Victorian Debate on Science and Society*. Chicago, IL: University of Chicago Press, 2006.

say, capable of being settled by experts or machines, like arguments between engineers or doctors."[13]

Even scholars such as Habermas, who were deeply concerned with the encroachment of technical expertise into the political sphere, nonetheless accepted the validity of this division of labor. In *Toward a Rational Society*, Habermas deplored the fact that the exigencies of new technologies were increasingly supplanting the decision-making power of political leaders and that value judgments were being displaced by objective necessity.[14] He was concerned that this "rationalization" of politics would result in science and technology usurping the realm of ends, such that political power would become an empty fiction and all practical matters would be formulated as problems that experts could solve. But he did not question the assumption that the experts could be trusted to settle problems about the means in a purely technical and rational way.

The problem with Habermas's argument was that it never examined the limitations of science as a tool of instrumental rationality. Habermas was not interested in the epistemic status of scientific claims, nor in the uncertainty and fallibility of science. He took for granted that the natural sciences were successful at providing accurate predictions for rational control over nature and he was not concerned with the inner workings of science beyond this general point. His one brief mention of uncertainty reveals this attitude: he claimed that the reduction of all practical decisions to "choice under uncertainty" would be the very culmination of

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[13] Isaiah Berlin, "Two Concepts of Liberty." In Isaiah Berlin, *Four Essays on Liberty,* Oxford: Oxford University Press, 1969.

[14] Jürgen Habermas, *Toward a Rational Society.* Trans. Jeremy Shapiro. Cambridge: Polity Press, 1987. Also see Habermas, *Knowledge and Human Interests.* Trans. Jeremy Shapiro, Cambridge: Polity Press, 1987.

rationalization. This ignores the fact that choice under uncertainty necessarily requires assumptions about morality: should one maximize expected utility, avoid the worst consequences or something else? The answer depends on the moral theory accepted by the decision-maker, as well as on normative assumptions about rationality and orientations toward risk. Furthermore, such decision calculations will be possible only if the uncertainty is determinate and calculable. Habermas did not even consider the possibility of radical uncertainty of the sort that cannot be expressed in probabilistic terms and that defies scientific attempts at calculation – the kind of uncertainty involved in the science of climate change, earthquakes, floods and hurricanes, for instance.

Habermas's "pragmatistic" solution to the problem of technocracy was to orient scientific developments toward the needs, interests and value orientations of the lifeworld. Scientists must be attentive to the practical implications of their work, and politicians must consult with scientists in accordance with practical needs. This required more communication between scientists and the public. The main departure of the pragmatistic solution from the classic division of labor was in conceptualizing the relationship between science and politics as a dialectic in which social interests and needs would be reevaluated in light of technical possibilities and the development of science and technology would be shaped according to public needs. This thoroughly Deweyan proposal emphasized the importance of a communicating public in shaping science and technology and being shaped by it in turn.

This was a democratic reimagining of the Weberian model, but one that nonetheless fully accepted the premise that experts could be trusted to determine the technical means for value-beliefs supplied by non-experts, whether citizens or elites. Although Habermas acknowledged that science was not value-free, he shared many of the assumptions of a positivist view of science as the source of objective, value-free knowledge. His conviction of science's capacity for prediction and technological control played a far more important role in his theory than thorny questions about the relationship between the practice of science and its epistemic standing raised by his own pragmatist conception of truth.

Habermas is one of the most influential thinkers to address the relationship between science, technology and politics, but he is by no means alone in assuming the continued validity of this division of labor. Recent scholarship in democratic theory implicitly or explicitly falls back on the same assumption in addressing the question of how ordinary citizens of a democracy should evaluate expert advice. Recent work on this problem has taken one of two main approaches: The first is to argue that laypeople cannot evaluate the content of expert claims and that the focus must be on the possibility of second-order judgments based on the trustworthiness, sincerity or credentials of the expert. The second is to argue that first-order evaluations by laypeople are possible and to think about how this could be done. Both sides, however, have operated with a naïve view of expert knowledge.

O'Neill and Anderson have recently defended the first position.[15] They argue that the problem of expertise in politics should be addressed by focusing on how laypeople can judge the credibility and character of the speaker, rather than the content of the speaker's claims. "I simply wouldn't know how to appraise the evidence even if you gave me all the detail. I want to know not if the evidence supports this or that conclusion, but whether I have good reason to trust those who

[15] John O'Neill, "The Rhetoric of Deliberation: Some Problems in Kantian Theories of Deliberative Democracy," *Res Publica* 8 (3), 2002; and Elizabeth Anderson, "Democracy, Public Policy, and Lay Assessments of Scientific Testimony," *Episteme* 8 (2), 2011.

offer it,"[16] O'Neill writes. Anderson similarly argues that judgments of credibility can be taken as a reliable substitute for judgments of content: "The solution to our problem is therefore to show that laypeople have the second-order capacity to judge trustworthiness and consensus, and access to the information needed to make such judgments."[17]

Those who argue that laypeople can assess experts only based on credibility judgments face the challenge of developing criteria for assessing credibility that can track knowledge and competence,[18] but credibility assessments are notoriously difficult. The attribution of credibility to a speaker depends upon many social and cultural factors, which may or may not track competence. Demeanor, age, gender, appearance or clothing can play a role in determining whether a person is regarded as credible. Anderson proposes a set of criteria that includes honesty, responsiveness to criticism, openness to peer review and the presence of conflicts of interest. Since each of these can be assessed based on information accessible and comprehensible to ordinary citizens, she concludes that citizens can distinguish between competing experts without understanding the substance of their claims.

O'Neill takes a different route, focusing on the role that rhetoric can play in generating trust between experts and laypeople. He argues that credibility is not only found in observable qualities of the expert, but is also constructed through the dialectical relationship between expert and layperson. He rejects the Platonic view of

[16] O'Neill, "The Rhetoric of Deliberation."

[17] Anderson, "Democracy, Public Policy, and Lay Assessments of Scientific Testimony."

[18] Alvin Goldman, "Experts: Which Ones Should You Trust?" *Philosophy and Phenomenological Research,* 63 (1), 2001 is the classic article on this question.

rhetoric as necessarily manipulative and incompatible with reason,[19] and defends the Aristotelian view, which does not dispense with reasoned arguments but gives them a different role. On this view, evidence and arguments cited by the expert are not meant to persuade the listener directly of the validity of a conclusion (since the listener cannot evaluate these) but are used to demonstrate the speaker's credibility.[20] Reasoned argument becomes a testimony to the speaker's good character. The work of persuasion is done by demonstrated trustworthiness, rather than by the content of what is said.

Trust is a central component of expert-layperson relationships,[21] but it is dangerous to reduce the role of experts in politics to a matter of trust cultivation. Neither the demonstrated ability to make reasoned arguments, nor honesty and responsiveness to criticism is a perfect tracker of scientific knowledge. Second-order assessments will be helpful in distinguishing between experts and frauds, but they will not be enough to judge the views of genuine, trustworthy experts who disagree. [22] Moreover, focusing on second-order assessments limits citizens' power vis-à-vis experts. This approach assumes that the task of laypeople is to identify a trustworthy

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[19] O'Neill joins Garsten and others who have called for a revival of rhetoric. See Bryan Garsten, *Saving Persuasion: A Defense of Rhetoric and Judgment.* Cambridge, MA: Harvard University Press, 2006; Garsten, "The Rhetoric Revival in Political Theory," *Annual Review of Political Science,* 14, 2011; Danielle Allen, *Talking to Strangers: Anxieties of Citizenship Since* Brown v. Board of Education. Chicago, IL: University of Chicago Press, 2004; Arash Abizadeh, "The Passions of the Wise: Phronesis, Rhetoric and Aristotle's Passionate Practical Deliberation," *Review of Metaphysics,* 56, 2002; Bernard Yack, "Rhetoric and Public Reasoning: An Aristotelian Understanding of Political Deliberation," *Political Theory,* 34, 2006; and O'Neill, "The Rhetoric of Deliberation."

[20] Allen makes the same point in *Talking to Strangers.*

[21] Mill puts it well: "But when all is done, there still remains something which they must always and inevitably take upon trust: and this is, that the arguments really are as conclusive as they appear; that there exist no considerations relevant to the subject which have been kept back from them; that every objection which can suggest itself has been duly examined by competent judges, and found immaterial." (*The Spirit of the Age, Part II.* In *The Collected Works of John Stuart Mill*, J.M Robson ed. London: Routledge and Kegan Paul, Vol. XXII).

[22] See Lane, "When the Experts are Uncertain."

expert and defer to her claims. The framing of the problem as one of whether and how laypeople can identify the correct experts allows laypeople only a narrow and passive role. The possibility that they might have something to contribute to knowledge or that they might question the claims of a "correct" expert is precluded.

It might be possible to carve out a more active role for citizens from within the second-order framework trust and credibility. O'Neill's analysis of rhetoric and trust could be extended to go both ways. Laypeople could perhaps be taught how to make better arguments that demonstrate their credibility. Although conceiving of experts and laypeople as both speaker and listener in turn would be an improvement, ultimately the point that reasoned argument is a way of demonstrating the speaker's credibility favors expert credibility over the credibility that laypeople could claim in expert-layperson interactions. Not only do experts have a natural advantage in this form of trust-building, but any attempt at making the rhetorical performance of laypeople more like that of experts might distort what they would like to communicate and how. A more equal two-way interaction would require revising both the scope and the content of how rhetoric can play a role in generating trust. In its current form, the framework is not well-suited to enabling citizen participation in debates about knowledge.

The second view argues that it is both possible and necessary for laypeople to evaluate first-order claims. Lane, Kitcher and Keohane et al. have argued that citizens and their representatives must be able to understand and evaluate at least some of the content of scientific and technical knowledge since they are ultimately responsible for making the political decisions that rest on scientific findings.[23] Lane focuses on the

[23] Lane, "When the Experts are Uncertain;" Philip Kitcher, *Science, Truth, and Democracy*. Oxford: Oxford University Press, 2001; Robert Keohane et al. "The Ethics of Scientific Communication Under Uncertainty," *Politics, Philosophy & Economics,* 13 (4), 2014.

importance of uncertainty in the relationship between science and politics and argues that laypeople can exercise good political judgment on scientific issues only if they have a good grasp of the nature of scientific uncertainty. Kitcher focuses more generally on how scientific findings can be translated for laypeople to allow for informed decisions. His ideal involves scientists and citizens deliberating under idealized conditions of mutual engagement to make decisions that reflect the moral commitments and priorities of a democratic public. All of these scholars are concerned with making science comprehensible to ordinary citizens and politicians so that they can participate meaningfully in the decision-making. They have focused especially on how experts can communicate their findings in language that is accessible to ordinary people[24] and how they can be more transparent about their methods, data and interpretation.

But much like Habermas, O'Neill and Anderson, these theorists assume that experts can be trusted with the content of science. Their goal is to ensure that political decision-making in light of scientific issues can proceed with full information. Their main concerns are external to the science; and they take the science as a given once it is ready for use in politics. It is telling that Kitcher uses the word "tutoring" to describe how laypeople should be taken behind the scenes by experts to be given in-depth information; his view of democratic engagement over science is primarily as an educational process.[25] He wants decision-making to be more inclusive and participatory, but only once the facts are provided by the experts.

Deliberative experiments such as citizen juries, deliberative polls and consensus conferences have taken a similar approach. Their implicit expectation is

[24] Keohane et al. "The Ethics of Scientific Communication Under Uncertainty."

[25] Kitcher, *Science, Truth, and Democracy*.

that citizens will accept the expert views more or less at face value and deliberate on the basis of these facts, rather than examining the facts themselves. This educational orientation is reflected in the institutional design of existing models. Their expert panels typically consists of one expert from each relevant field, which makes it difficult for citizens to see the weaknesses of expert views or to get a sense of possible alternatives. Furthermore, the effectiveness of these institutions are measured through before-and-after surveys that use the degree of citizen uptake of information provided by experts as an indicator of success.[26]

This is not meant as a critique of the methodology of these deliberative experiments. Their purpose is to assess whether and when the communication of expertise is successful and their design is appropriate for this purpose.[27] It is important to know what kinds of techniques allow laypeople to process technical information better and make more sophisticated decisions. My point is that most treatments of the role of experts in democracies in recent political theory have been stuck in a rather narrow framework that has not given enough thought to the possibility – even the necessity – of laypeople questioning expertise. Despite their marked differences, both those who focus on second-order credibility assessments and those who advocate democratic engagement with the content of science have effectively assumed that laypeople can accept the facts provided by a "correct" expert.

I have criticized both of these positions for putting citizens in the passive role of taking science on authority from experts, but it may not be obvious why this is a

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[26] Robert Goodin and John Dryzek, "Deliberative Impacts: The Macro-Political Uptake of Mini-Publics," *Politics and Society,* 34 (2), 2006.

[27] Experiments conducted so far show that these kinds of efforts at improved communication do produce good results, at least in small deliberative settings See e.g. Goodin and Dryzek, "Deliberative Impacts;" Archon Fung, "Survey Article: Recipes for Public Spheres: Eight Institutional Design Choices and Their Consequences," *The Journal of Political Philosophy,* 11 (3), 2003; Mark B. Brown, "Citizen Panels and the Concept of Representation," *The Journal of Political Philosophy,* 14 (2), 2006.

problem. Since experts possess more knowledge in their area of expertise, it might seem both unnecessary and epistemically arbitrary for laypeople to examine the content of expert claims. This view is implicit in both approaches discussed above, and it assumes the validity of the division of labor between experts and laypeople mentioned earlier, which suggests that technical or factual matters could safely be left to experts.[28]

The continued reliance on this division of labor between experts and politics is puzzling because the view of science it presupposes has been widely criticized in decades of research in the philosophy and sociology of science. After the explosive impact of Kuhn's *Structure of Scientific Revolutions* and the early debates between Feyerabend, Lakatos, Toulmin and Popper, it became difficult to maintain that values played no part in the production of science.[29] Scholarship in the philosophy and sociology of science in the decades after Kuhn focused on *how* values shaped scientific findings and what this meant for the general reliability of scientific claims. It is rare to find scholars today who subscribe to a version of the naïve positivist view of science. Yet there hasn't been a revision of the political division of labor model that takes seriously these developments in theories of science. This is problematic because how we answer the question of the proper use of expertise in politics depends on the role that social and moral values play in the production of science. The validity of the division of labor model presupposes an outdated view of science: Leaving the means to experts is acceptable because the determination of the means to attain

[28] Fabienne Peter, "The Epistemic Circumstances of Democracy." In *The Epistemic Life of Groups,* Michael S. Brady and Miranda Fricker eds. Oxford: Oxford University Press, 2016.

[29] See Thomas Kuhn, *The Structure of Scientific Revolutions.* Chicago, IL: University of Chicago Press, 1962; Paul Feyerabend, *Against Method: Outline of an Anarchistic Theory of Knowledge.* London: Verso, 1975 and a collection of responses to Kuhn in Imre Lakatos and Alan Musgrave eds. *Criticism and the Growth of Knowledge.* London: Cambridge University Press, 1970.

independently decided ends is assumed to be a purely technical task – one that could be settled by experts or machines. If there is reason to think that the particular perspective, priorities and social context of the experts making the knowledge claims shape the knowledge that is produced, then we must reevaluate how such knowledge should be used for political purposes. This is what I aim to do in this dissertation.

Although I am critical of many aspects of the postwar structure of the relationship between science and politics in the United States, I am indebted to it in one crucial respect. One of its remarkable features was how a set of philosophical views about scientific progress became the foundation for a comprehensive structure for the proper relationship between science and politics that had implications for the use of science in policy, the responsibilities of scientists in the public sphere, and a long-term strategy for science funding. Abstract philosophical debates about scientific progress were elegantly linked first to a political theory for the role of science in a modern democracy and secondly to a blueprint for long-term science and technology policy. This dissertation borrows this structure of following a theoretical thread that runs from the philosophy of science to democratic theory to institutions for funding, but fills it in with new arguments.

I start from a philosophy of science that takes into account the role of values, backgrounds assumptions and uncertainty in scientific claims – that is, one that takes seriously the developments in the philosophy of science in recent decades. I then develop the implications of this philosophy for how science should be used in political decision-making. Finally, I trace the implications of these arguments for funding strategies for science both in the short and long term. My main argument is that the role of values in science makes it problematic to leave the determination of science entirely to scientists in policy decisions. I argue on both epistemic and democratic grounds that the content of expert claims must be submitted to critical democratic scrutiny and that citizens must have a chance to question the evidence supporting various claims, their level of uncertainty, and possible background assumptions made by scientists.

The basic argument is simple, but demonstrating its plausibility requires addressing three important challenges. The first is to show at a theoretical level what exactly the purpose of democratic participation would be and what it could accomplish. This requires tracing the link from a particular view of science to the necessity of particular forms of democratic input. The second challenge is to show how democratic debate on scientific issues should be organized in order to realize the epistemic and democratic goals outlined in the first part. This involves developing institutional structures designed to overcome the obstacles to deliberation on complex issues, focusing especially on the asymmetries of knowledge and authority between experts and non-experts.

The first two parts of the argument focus on how science should be used in political decision-making. The problem with focusing only on decision-making is that the possibilities for politics at the decision stage are largely determined by earlier decisions about which research areas should be pursued and how they should pursued. These decisions, typically at the funding stage, shape the agenda for politics and determine which choices will be available for decision-makers. Citizens can question the science, examine its assumptions and decide whether to accept uncertain claims, but they cannot procure a different kind of science; nor can they wish away findings once they are public.

The third challenge is therefore to revisit the structure of public funding for science in light of the argument of the first two parts and develop a theory for science funding that complements the democratic deliberation and decision-making further downstream. Scholars studying the relationship between science and politics often draw a distinction between science for policy and policy for science.[30] The former describes the science that informs policy decisions, while the latter focuses on the rules and regulations designed to oversee the conduct of science. This thesis treats the two as interdependent and argues for democratic input at both stages.

There are two important concerns about this project that are worth dispelling from the start. The first is that raising questions about the objectivity of scientific findings will devolve into radical skepticism about the ability of science to deliver reliable answers. This will blur the distinction between science and politics and encourage disregarding expertise and replacing it with common sense, anecdotal evidence or wishful thinking. It will become clear in the following chapters that this is not my argument. The starting point of this project is that expert knowledge is indispensable in a modern democracy and experts have superior knowledge and understanding on many crucial questions of fact. The question of how we should respond to climate change, for instance, cannot be settled by our experience of the weather, nor can it be resolved by deliberating about how much we care about nature or future generations. The answer requires knowing how much the earth will warm and what the impact will be on different regions. We depend on scientists for these answers.

The point of thinking about the implication of the role of values in science is not to delegitimize scientific claims, but to be clear about why and how citizens must examine expert claims and what room there is for citizen input on scientific issues.

[30] Homer Neal et al. *Beyond Sputnik: U.S. Science Policy in the Twentieth Century*, Ann Arbor, MI: University of Michigan Press, 2008; Heather Douglas, *Science, Policy and the Value-Free Ideal*. Pittsburgh, PA: The University of Pittsburgh Press, 2009.

We don't need to believe that science is infallible to make productive use of it. My claims is that what we know about the ways in which it is fallible should influence the appropriate attitude to take toward knowledge claims and the correct institutional structures for handling them. To quote Clifford Geertz, "I have never been impressed by the argument that, as complete objectivity is impossible in these matters (as, of course, it is), one might as well let one's sentiments run loose. As Robert Solow has remarked, that is like saying that as a perfectly aseptic environment is impossible, one might as well conduct surgery in a sewer."[31] This project considers how to change the way we do surgery once we realize that the environment is less aseptic than originally believed.

The second concern is that even if this theory is careful about the status of scientific claims and the proper balance between scientific evidence and democratic decision-making, it might nonetheless have the unintended consequence of increasing mistrust of scientists and disregard for evidence. The argument for democratizing the use of expertise inevitably involves drawing science and scientists onto the political stage and exposing their weaknesses. Given the widespread denial and mistrust of science today, this might embolden those who disregard or discredit scientific evidence. Would it not be more appropriate for theorists today to think of ways to shield expertise from politics rather than exposing it to further scrutiny?

This is a serious challenge, especially since I argue in Chapter 5 that researchers must bear some responsibility for the unintended but foreseeable consequences of their research and that democracies may withdraw support from scientific research if it appears likely to cause certain kinds of grave harm. Still, I

[31] Clifford Geertz, "Thick Description: Toward an Interpretive Theory of Culture." In *Readings in the Philosophy of Social Science,* Michael Martin and Lee McIntyre eds. Cambridge, MA: MIT Press, 1994.

think it is dangerous to respond to pessimism about the current state of democracy and worries about the ignorance of citizens by retreating from democratic principles and removing more and more issues from public input. This response avoids dealing with the root causes of the problem and might lead to a backlash against expertise, as the L'Aquila case demonstrates.

People often feel anxious and fearful about scientific or technological developments because they cannot reconcile new truth claims with their deeply held values and cultural commitments. Scientific claims do not intrinsically favor one worldview or set of values over another, but those who control how science is used in public life wield significant power in determining which worldviews or values will appear compatible with the knowledge claims of experts. If decisions about findings are accepted as true for political purposes and what new knowledge becomes available for use are removed from democratic influence, citizens might find themselves reduced to a choice between between deference to the judgments of others or a rejection of the authority of science altogether. This disempowers the public and encourages unaccountable and irresponsible policy-making. Expanding the possibilities for democratic engagement over science is a way to avoid this stark choice and open up more flexible options for reconciling science with politics. This, in turn, can only be done by reinvigorating existing democratic institutions and imagining new ones.

Scope of the Argument

A few clarifications about the scope of the argument are in order. This project focuses on expertise in the natural sciences and draws the line at the social sciences. The distinction is admittedly arbitrary since the theory of natural science developed in

Chapter 1 challenges the conventional distinction between natural and social sciences as value-free and value-laden respectively. The philosophical views of science that I draw on treat the natural science and social sciences as continuous, rather than different in kind. Still, I have two mainly practical reasons for drawing this line. First, this is a widespread distinction both in theory and in practice. Philosophers of science typically focus on one or the other or compare the two with the assumption that they are distinct enough, both in subject matter and in methodological challenges. Political institutions such as legislative committees, executive agencies and funding institutions also treat these two areas separately.

Secondly, even if the natural and social sciences lie on a spectrum, it is difficult to deny that the social sciences lie at the end of the spectrum where predictions are less reliable; there are fewer well-established findings, and greater difficulties with concept formation and measurement. In addition, there are well-known methodological challenges specific to explaining and predicting human behavior. On the one hand, these factors might make the social sciences a more fruitful, less controversial and overall easier target for an argument that starts from the epistemic status of knowledge claims to argue for democratic scrutiny of science. On the other hand, the same reasons make the social sciences a less challenging and rewarding subject for this project because I suspect that few would disagree with the argument. Moreover, if this argument succeeds in the case of the natural sciences, then a fortiori, it should also apply to the social sciences.

Another clarification concerns the applicability of the argument within the natural sciences. Is it meant to apply to all natural sciences or only to some? Do we want democratic participation on all issues or can we leave some safely to experts? These questions are more difficult to answer because they depend importantly on which scientific issues become politicized and how. The easy part of the answer is that the argument applies to science that has some relevance to policy. It is not concerned with science in the lab that acquires no relevance for public affairs, except for the discussion of funding for basic research in Chapter 4.

Within the politically relevant sciences, I think the argument will be most salient on issues that are highly uncertain, with many unknown variables and relatively scant evidence, and especially in cases where the political stakes are high. Although we could try to classify sciences according to their level of certainty – earthquake science and climate change being far less certain than physics or chemistry for instance – it would be a mistake to try to be more specific about which particular scientific areas are likely to fall in this category. I do not meant to suggest that every technical issue should be politicized – if a bridge needs to be built, we could safely leave it to engineers – but rather that the question of which issues should be or will be politicized is not one that can be specified in theory.

The point about uncertain and high-stakes science suggests another reason why this project is timely: The big scientific question of our time – climate change – has been marked from the beginning by a high degree of uncertainty and disagreement among scientists, as well as high political stakes. That there is anthropogenic climate change may not be in dispute among scientists anymore, but the key policy-relevant details about how much warming there will be and how it will affect different regions remain unclear. Different climate models prioritize different epistemic values and make different background assumptions about the historical record, future human behavior and the importance of certain risks over others. These features make it particularly clear why the democratic engagement over climate change must be over the content of the science and involve some scrutiny of competing models, rather than a debate about moral values that could be addressed independently from the facts. This has not always been the character of the scientific issues that have commanded national-level attention. The most important scientific issues on the political agenda after the Second World War – the bomb, the space program – were cases where the science was not in dispute. The dilemmas they raised were moral ones about the responsible use of the science. If the division of labor model seemed appropriate for the scientific problems of those times, the more thoroughly democratic model proposed in this project will be more appropriate for ours.

Plan of the Work

The dissertation is organized as follows. Chapter 1 makes the philosophical case for why the role of values in science requires democratic scrutiny of expert claims. Drawing on case studies from evolutionary biology, climate change, AIDS and nuclear waste, as well as philosophical work by Kuhn, Feyerabend and feminist philosophers of science such as Helen Longino and Miriam Solomon, I show that 1) the link between evidence and hypothesis always rests on background assumptions about the world; 2) the choice between equally well-supported theories requires trade-offs between epistemic values, which, like ethical values, are subjective and cannot be settled by evidence; and 3) the move from experiment to real life requires judgments about the sufficiency and significance of the similarity, and both sufficiency and significance are relative to a purpose.

I argue that scientific claims require democratic scrutiny on three grounds: 1) that a democratic public has the right to understand claims that rest on values they may not share and whose consequences will affect them 2) that assumptions and values that are widely shared among scientists may be difficult to detect and challenge within the scientific community and 3) that laypeople may possess valuable knowledge that can improve findings.

Chapter 2 develops an account of the political treatment of scientific uncertainty through a conceptual distinction between belief and acceptance. While belief aims at the truth, responds only to epistemic factors and is context independent, acceptance – defined as the attitude of taking something for granted in the background of one's deliberation and planning – responds both to practical and epistemic factors and depends on the context, the stakes and the purposes of the agent. Science is primarily concerned with acceptance rather than belief, and evidence alone cannot determine the acceptance of a hypothesis. What is required is the judgment that the evidence at hand is sufficient, and what constitutes sufficient evidence depends on the stakes and the context. Scientists' professional goals determine what they take to be the appropriate threshold for scientific acceptance. These goals include the discovery of the truth, but also the fruitfulness of the hypothesis for future research, a desire to minimize error and a priority for long-term success over short-term considerations.

Politics has a different set of considerations, which means that what constitutes sufficient evidence for the acceptance of science in politics will be different than scientific acceptance. Political acceptance depends on the stakes, on whose interests will be affected, on the urgency of the problem and on the opportunities for action. Since acceptance requires deliberation on the practical considerations relevant to the political context, stakes and so on, laypeople have both the competence and the right to participate in the decision about the acceptance of scientific findings for political purposes. This chapter also suggests that political theory would benefit from more attention to this distinction, since in many cases what we care about is not what individual citizens believe, but what they will accept for a specific political purpose and context.

Chapter 3 develops an institutional proposal to facilitate democratic engagement over science. I identify two challenges to interactions between experts and non-experts in the public sphere: how to make it possible for non-experts to examine competing expert claims and how to overcome the difficulties of mutual deliberation under conditions of asymmetric knowledge and authority. To address these challenges, I propose a publicly monitored science court with adversarial proceedings where experts are brought in to make the case for different views on a scientific question. A citizen jury then interrogates the experts and delivers a decision, which is fed directly into the policymaking process in an advisory role. The adversary structure of the proposal is designed to expose the background assumptions behind factual claims and to reveal their level of uncertainty. The separation of scientist-advocates from citizen-jurors avoids the difficulties of mutual deliberation under conditions of unequal authority, while allowing citizens to be active participants despite their lack of expertise. I also suggest that theories of democracy would benefit from paying more attention to the role of questioning as an empowering mode of communication that can facilitate participation in situations of asymmetric knowledge and power. I conclude by responding to two possible objections: that this institution puts scientific truth to a popular vote and that it overestimates citizen competence.

Chapter 4 focuses on funding committees as the site for longer-term and more foundational democratic input into science. The question that motivates the chapter is whether there should be political input on decisions about the distribution of funds among scientific projects, and if so, on what grounds. I frame the issue of public funding for science as lying at the intersection of two normative issues that political theorists have neglected: the provision of public goods and political significance of knowledge creation. Following a distinction made by Rawls, I first argue that public funding for science must be thought of as a discretionary public good, rather than one required by justice. Justifications for public support of science must therefore appeal to the needs and interests of the public. I show that this has indeed historically been the case.

I then trace how postwar structures for funding basic research have justified giving a high degree of autonomy to scientists on the basis of Polanyi's view of scientific progress as a linear and cumulative process, in which the greatest public benefits will be realized through the trickle-down effects of scientists pursuing their curiosity. I show that Kuhn's account of scientific revolutions seriously challenged this view and argue that extrapolating Kuhn's views to a system of science that depends on funding suggests that a closed community of scientists sharing a paradigm may end up extinguishing truly radical views if they also control the allocation of funding. Outside intervention with the scientific community may thus be necessary to ensure support for dissenting views and the continuation of the most innovative scientific discoveries.

The chapter then turns to the question of continued public funding for scientific issues that have already become politicized. Since the stakes and potential consequences of these issues are more certain than in the case of basic research, I argue for more political input into these decisions. I outline three different reasons for political intervention with funding decisions: the importance of agenda setting for research on the basis of democratically determined priorities; the need to support dissent and diversify funding in order to ensure that a robust democratic debate on scientific issues will be possible; and the possibility that not having certain kinds of new knowledge can be empowering in cases where the new knowledge is likely to threaten a democracy's capacity to take action.

Chapter 5 explores the question of whether a democracy may restrict certain lines of inquiry altogether by withdrawing funding on the basis that they pose a risk of harm to society. It is widely accepted today that research projects may be restricted if they pose harm to human subjects participating in the research process. Far more controversial is the suggestion that a project may be restricted on the grounds that the findings pose a risk of harm to society, even if the research is ethically conducted and the findings are true. Two claims justify drawing a moral distinction between harm to subjects during the research and harm to society from the findings. The first is that knowledge is never intrinsically harmful and that harm only results when knowledge is used or abused by people with bad intentions. The second is that it is not permissible to restrict the activities of scientists for harms that other people inflict using their findings. We can hold scientists responsible for the harms that they intentionally inflict on participating subjects, but nothing more.

I argue that even if we grant the first claim, we should reject the second. In the first section, I defend a more robust understanding of responsibility that is sensitive to the context in which scientific research takes place and that involves assigning scientists some responsibility for the foreseeable consequences of their research, even if they themselves neither inflict nor intend harm. I argue that in cases where scientific research is likely to lead to significant harm to a large of number of people, a democratic society would be justified in preventing the research from going forward. In the second section, I shift the focus from the magnitude of the harm to the specific people affected. I argue that in cases where a line of research is likely to disproportionately harm marginalized groups and reinforce existing inequality and discrimination, a democracy's commitment to ensuring the equal standing of citizens would justify limiting scientific inquiry. I make the case through a discussion of research in biology involving race and gender.

Chapter 1: The Role of Values in Science

Political theorists have not been attentive to recent developments in the philosophy and sociology of science, but scholars in these disciplines now widely accept that knowledge is value-laden and influenced by the social context in which it is produced. Exactly what role values play in science and the quality and objectivity of the resulting findings are more contentious. The claim that science involves values takes different forms. One way the argument has been made is to claim that the personal interests or beliefs of scientists cloud their judgments, sometimes unintentionally. Charges of financial conflicts of interest in medical research and of ideological bias in studies of IQ differences between races and genders fall into this category. These are cases where the validity of the results and even their standing as science may rightly be called into question because the science is distorted by the researcher's personal prejudices and interests. This is a problem that, at least in theory, could be corrected by exposing and checking biases and conflicts of interest. Although this may be difficult to achieve in practice, the charge is theoretically less serious because it shows only that individual researchers allow their biases to play an objectionable role in their research, not that science itself is always value-laden. If only researchers were more careful or vigilant – if only human error could be corrected – science would remain free of social values.

A second argument, made by sociologists of science, focuses on the role that social relationships, hierarchies and power dynamics play in the research process.[32]

[32] Barry Barnes and David Bloor, "Relativism, Rationalism and the Sociology of Knowledge," in *Rationality and Relativism,* Martin Hollis and Steven Lukes eds. Oxford: Blackwell, 1982; Bruno Latour and Steven Woolgar, *Laboratory Life: The Construction of Scientific Facts.* Princeton, NJ: Princeton University Press, 1986; Harry Collins and Trevor Pinch, *The Golem: What Everyone Should Know About Science.* Cambridge: Cambridge

This argument sees scientific knowledge as the product of power struggles, clashes of interest, negotiation and compromise. What counts as scientific knowledge in a community is explained with reference to interests and power, rather than epistemic criteria such as evidence and logic. Science is politics by other means. Latour's analysis of how scientific acceptance depends on the success of scientists in enlisting a network of allies, patrons and supporters is a classic example of this kind of argument.[33]

This line of research has contributed a lot to our understanding of how science works, but it goes too far in repudiating the possibility of a normative theory of scientific knowledge. Concepts such as evidence, logic, truth or reasons all but lose their justificatory significance except insofar as they can be used to explain the discourse and self-understanding of scientists. This is difficult to accept: If justification were entirely socially constructed, it would be impossible to account for why some knowledge generating processes are more successful than others in explaining natural phenomena. Furthermore, this view provides no guidance for deciding how to structure these processes when there is a choice between alternatives.

A third way of making the argument is to start with a normative account of scientific justification and shows how it necessarily incorporates value judgments at different stages. This approach rejects the possibility of a value-free method and argues that the logic of scientific inference is necessarily context-dependent. While the first view retains the ideal of value-free science and the second pushes toward collapsing the distinction between science and power, the third view revises the ideal without renouncing it. In doing so, it opens up the conceptual space for productive

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University Press, 1993; Karin Knorr-Cetina and Michael Mulkay eds. *Science Observed: Perspectives on the Social Study of Science.* London: Sage, 1993 among many others.

[33] Bruno Latour, *Science in Action.* Cambridge, MA: Harvard University Press, 1987.

debates about which values and whose values should appropriately shape scientific knowledge, rather than urging the chimerical task of removing all values from science or giving up the possibility of useful scientific knowledge altogether. The account I will develop in this chapter falls in this third category.

The scientific research process can be represented schematically as involving the following stages: the selection of research questions, the generation of theories, the design of experiments, the collection and analysis of data, and the judgment that the evidence supports a theory. The literature commonly distinguishes between external stages such as the selection of the research question and the application of findings, and internal stages, such as theory construction, experimental design and inference. I say more in Chapter 4 about the politically crucial question of selecting research areas, mostly in the context of public funding for science, but I will bracket that here and focus on the the role that values play at the other stages of scientific research.

The chapter is organized in four sections. The first three sections develop an account of the role of background assumptions and values in scientific discovery and justification, each focusing on one of the key stages of the research process. The fourth and last section argues that the role of values in science requires submitting expert claims to democratic scrutiny before they are used in political decisions in order to prevent the values of experts from encroaching on popular sovereignty. I conclude with a discussion of the possible contributions that democratic participation could make on scientific issues.

I. Evidence and Theory

It is fairly straightforward to say that scientific hypotheses should be accepted or rejected on the basis of evidence, but whether the evidence supports a hypothesis is not a self-evident matter: evidence does not say what it is evidence for. A set of background beliefs, also called auxiliary hypotheses, which are held by the person making the inference, are necessary to determine whether a state of affairs supports a hypothesis. The same evidence can be taken to support quite different hypotheses depending on one's background beliefs. There is thus always a gap between observation and hypothesis; evidential relations are more properly thought of as three-way relationships between evidence, hypothesis and background assumptions.[34]

Background assumptions can sometimes simply be rules of syntax or logic. The move from evidence sentences to hypothesis sentences in these cases will be a task for philosophy alone. Theories in physics that relate a given number of variables to one another in fixed proportions, such as Boyle's Law and Pascal's Law, are some examples. Logical positivists maintained that all scientific inference would be like this. It soon became clear, however, that this would limit science to a small area of physics resting on generalizations from a finite set of observed relationships between variables.[35]

In most cases of scientific research, background assumptions will involve far more than rules of logic: they will necessarily bring in other beliefs about the world. Some of these will be previously accepted theories, in which case the judgment that

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[34] This is sometimes called the Quine-Duhem thesis. Duhem was a physicist and noted this only about physics. Quine made the more general point that this difficulty applied to all forms of knowledge, including pure mathematics and logic and not just scientific theories. See W. V. O. Quine, "Main Trends in Recent Philosophy: Two Dogmas of Empiricism," *The Philosophical Review,* 1951.

[35] Helen Longino, *Science as Social Knowledge: Values and Objectivity in Scientific Inquiry.* Princeton, NJ: Princeton University Press, 1990.

these previously accepted theories are evidentially supported will involve further background assumptions. Some, however, will be assumed without evidence, or even without awareness that they are assumptions because they will seem to the scientist to be just the way things are. The claim that something is evidence for something else always implicitly introduces a judgment about what other things are taken to be true about the world and relevant to a particular evidentiary relationship. Two people can agree on a description of the evidence, but disagree about what it is evidence for.

This implies that theories cannot be falsified by evidence alone: where the evidence does not appear to support a hypothesis, a scientist can do one of three things: reject the evidence, for instance as noisy or poorly measured; reject the hypothesis; or change her background assumptions. If a scientist observes that two objects falling from the same height do not hit the ground at the same time, she can either decide that the measurement was faulty, she can reject Galileo's theory of free fall or she can question one of her assumptions, for instance the assumption that gravity is the only force acting on the objects. Whether to take the hypothesis or one of the background assumptions to be false is not a decision than can be made on the basis of evidence alone.

Thomas Kuhn argued that mature sciences have a shared network of background beliefs, theories and assumptions – a paradigm – in light of which scientists judge whether new evidence supports or contradicts a hypothesis.[36] The paradigm ensures agreement over what constitutes evidence for what among practitioners in the same area. Scientific results can rapidly accumulate when scientists stop questioning assumptions and concentrate on solving puzzles. Kuhn called these periods of stable growth normal science. When a paradigm begins to fail

[36] Kuhn, *The Structure of Scientific Revolutions.*

in solving important problems, another paradigm can replace it, but only after a radical revision of the fundamental beliefs, assumptions and standards of the field: a scientific revolution. Under the new paradigm, the same set of evidence will be seen as supporting different theories because background assumptions have changed.

One of Kuhn's classic examples was of the discovery of oxygen: although Priestley and Lavoisier ran the same experiment of heating red oxide of mercury in a test tube, they reached different conclusions about the hypothesis it supported because they had different background assumptions about the behavior of air. Priestley took the lively combustion he observed to be a sign that the tube was depleted of its usual amount of phlogiston, whereas Lavoisier claimed that it was evidence of the production of a new gas – oxygen. Lavoisier reached this conclusion because he assumed that specific proportions of air and metal could combine to produce a new substance. This was conceptually impossible under the phlogiston theory, which Lavoisier took for granted.

There are many other examples. In the seventeenth century, Tyco Brahe argued that the failure to observe stellar parallax (the visible shift of a distant star against background objects) was evidence that Copernican heliocentrism could not be true. He concluded this because he assumed that the stars could not be as far away as would be implied if heliocentrism were true. Others who assumed heliocentrism to be true took the same observation about stellar parallax as evidence that the stars were very far away indeed.[37]

In Kuhn's examples, the gap between evidence and hypothesis is invariably filled by scientific theories. What interests me here is the possibility that what fills the gap are social and contextual assumptions that are not backed by evidence, that are

[37] Example from Elizabeth Anderson, "Knowledge, Human Interests, and Objectivity in Feminist Epistemology," *Philosophical Topics,* 1995.

simply taken for granted by the scientist. Feminist philosophers of science building on Kuhn, but pushing his argument in more radical directions, have shown how unexamined assumptions about gender norms shape scientific findings. Longino demonstrates how a male-centered paradigm dominated research in human evolutionary biology on the origins of anatomical and behavioral development.[38] The paradigm's central assumption was that the figure of man-the-hunter is key to explaining the trajectory of the species. Researchers interpreted new evidence about the timing of the emergence of tool use, changes in canines and the increase of brain size in light of this background assumption. The timing of tool use was taken to support the story that male hunters developed tools to hunt more effectively. The use of tools freed them from having to show aggression by baring their teeth, which relieved the selective pressure on big canines. Finally, the increase in brain size was thought to be due to the intelligence and cooperation required in hunting.

Although the man-the-hunter story – like Ptolemaic astronomy or the phlogiston theory – did a good job explaining available fossil records, Longino points out that female scientists started questioning why male hunting and aggression – the epitome of masculinity by twentieth century standards – should be central to the evolutionary story. They proposed instead a female-the-gatherer hypothesis, which assumed that changes in female behavior played the central role in evolution. The same set of evidence supported quite different hypotheses once scientists made this assumption: the evidence about the timing of tool use supported the hypothesis that the female's need to cope with the difficulty of gathering food for herself and her infants under changing ecological conditions provided the impetus. The increase in brain size was due to the female's need to show resourcefulness when faced with this

[38] Longino, *Science as Social Knowledge.*

survival challenge, and the smaller canines in males were selected because females preferred mates who displayed more cooperation and less aggression.

That evidentiary relationships depend on background assumptions is not in itself a problem for science. It only becomes problematic if the possibility of questioning and revising assumptions is foreclosed. Idiosyncratic individual assumptions pose the least difficulty for science because they can easily be spotted and challenged by the many other scientists who do not share them. Assumptions that are widely shared among a community of scientists are more troublesome. These can reach the status of incontrovertible truths and become difficult to notice, let alone to challenge. Criticism from outside the community may be necessary to expose these assumptions.

Kuhn argued that it might not be possible to settle debates about theory choice rationally across rival paradigms: paradigms determine scientists' worldview to such an extent that different ways of seeing the evidence become impossible. This has the ironic result of once again closing the gap between evidence and hypothesis. Scientists cannot agree on the merits of rival paradigms because their observations, standards of evaluation and even conceptual vocabularies are paradigm-dependent and different paradigms are incommensurable. Justifications and assessments that make sense in one paradigm have no meaning or purchase for scientists in a rival paradigm and scientists trying to communicate across paradigms speak past one another. Kuhn rather controversially concluded that paradigm shifts would result from arational acts of conversion rather than rational persuasion.

We can admit the difficulty of communication and agreement across paradigms without following Kuhn to incommensurability. Kuhn's critics have pointed out that neither historic experience nor linguistic analysis supports the claim that different paradigms are incommensurable.[39] Critical evaluation of paradigms is possible and can be essential to scientific progress. Theories of incommensurability face the charge of relativism unless they admit the possibility of comparison. Relativism maintains that justification is always relative to shared and unacknowledged standards and that these standards cannot be justified independently. This charge can be avoided by accepting the possibility that shared assumptions may be exposed and either revised or justified.

Feyerabend criticized Kuhn for ignoring the role of critical discussion among alternative views in the advancement of science. He proposed combining Kuhn's argument about the tenacity of scientists working in a paradigm with Popperian (or Millian) discussion and criticism.[40] The problem with Kuhn's argument was that it took for granted the continued emergence of alternative views capable of precipitating a revolution, but provided no reason to believe this would be true, especially since normal science is structured precisely to extinguish this possibility. Feyerabend argued that the dogmatic tendencies of normal science had to be countered actively by encouraging the emergence of new and imaginative alternatives in order to ensure the continuation of innovative discoveries. This stood in tension with Kuhn's claim that there could only be one dominant paradigm in an area at any time, but Feyerabend pointed out that this was neither an accurate description, nor a healthy prescription.[41]

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[39] Karl Popper, "Normal Science and Its Dangers." In *Criticism and the Growth of Knowledge,* Imre Lakatos and Alan Musgrave eds. London: Cambridge University Press, 1970; Longino, *Science as Social Knowledge*; Mary Hesse, *Revolutions and Reconstructions in the Philosophy of Science*. Bloomington, IN: Indiana University Press, 1980.

[40] Paul Feyerabend, "Consolations for the Specialist," In *Criticism and the Growth of Knowledge,* Imre Lakatos and Alan Musgrave eds. London: Cambridge University Press, 1970.

[41] Lakatos also made this point. See Imre Lakatos, "Falsification and the Methodology of Scientific Research Programmes." In *Criticism and the Growth of Knowledge,* Imre Lakatos and Alan Musgrave eds. London: Cambridge University Press, 1970. See also Peter Godfrey-

A critical exchange between rival paradigms was the best way to expose the problems of existing views and move science forward. This required ensuring the proliferation of views.

Feyerabend's critique of Kuhn has important implications – not only scientific, but political. In Kuhn's account of science, the mundane and narrow activity of specialists turns out to lead to the most innovative and radical discoveries. The structure bears some resemblance to Adam Smith's invisible hand. Just as the invisible hand argument is taken to support laissez-faire economic policies, Kuhn's picture of science implies that scientists should be left alone to pursue their seemingly unimportant puzzles without questioning the assumptions of their paradigm because this will lead to the greatest discoveries. Shared and unquestioned assumptions are necessary for scientific progress, rather than a potential obstacle.

Feyerabend rejects this and argues that there isn't enough reason to believe that normal science can lead to good outcomes without interference. If assumptions are shared, internalized and immune to rational debate as Kuhn claims, it will be difficult for the scientific community to generate paradigm changes from within. It is necessary to take active steps to ensure that there is enough criticism to expose the assumptions of the dominant paradigm. This critique takes added political significance in light of the work of feminist philosophers of science mentioned above, which shows that these background assumptions can be socially determined, rather than strictly scientific. If scientists working in the same area share the same assumptions, criticism from outside the community will be necessary to expose them.

Smith, *Theory and Reality.* Chicago, IL: University of Chicago Press, 2003 for an overview of this debate.

II. Underdetermination and Epistemic Values

A second point at which values play a role is in the choice between two or more scientific theories that are equally well supported. This is called the underdetermination of theory by evidence. Underdetermination can be permanent or transient. Permanent underdetermination means that no possible evidence could settle the debate between two alternative theories. Philosophers have generated imaginary cases that logically fit this description, but these are often far-fetched.[42] It is difficult to find real life examples, especially because of our ignorance of what kinds of evidence may become available in the future. Many scientific problems that appeared to have reached a permanent impasse at one point have later been resolved with the emergence of new evidence or of different ways of thinking.[43]

The transient form is more modest: it states that the evidence available now does not allow scientists to rule in favor of one theory or another, although evidence to be discovered in the future may well change this. Unlike permanent underdetermination, this is a fairly pervasive state in science and does not particularly worry scientists: even if the evidence available now cannot settle the matter, they can count on the choice being resolved in the long run once more evidence becomes available.[44] This appeals to the Peircean idea that investigation carried far enough will yield truth, correct mistakes and settle open debates. Although this long-term view is

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[42] André Kukla, "Does Every Theory Have Empirically Equivalent Rivals?" *Erkenntnis,* 44 (2), 1996. Quine was also preoccupied with the question of whether there could be two bodies of science that implied the same set of observation-conditionals and if so, how we could choose between them. This, however, is hardly the actual problem facing a scientist in theory choice. See Kyle Stanford, "Refusing The Devil's Bargain: What Kind of Underdetermination Should We Take Seriously?" *Philosophy of Science,* 68 (3), 2001; and Justin Biddle, "State of the Field: Transient Underdetermination and Values in Science," *Studies in History and Philosophy of Science,* 44, 2013.

[43] Kitcher, *Science, Truth, and Democracy.*

[44] Kitcher dismisses temporary underdetermination as unthreatening and focuses only on refuting the permanent version.

useful as a regulative ideal for science, it hardly eliminates the problem that the underdetermination of theories by evidence poses in the present. Even if future evidence could settle scientific debates in the long run, this does not obviate the need to make theory choices today. The right evidence that will settle the matter may not arrive for centuries. Meanwhile, science has to move forward and a decision between rival theories has to be made based on something or another.

In cases where the evidence cannot settle the matter, the decision has to be made based on other features that are valuable in a theory besides how well it is supported by the evidence. One could decide to withhold all judgment until further evidence becomes available, but that, too, involves a value judgment, namely that it is better to wait and that we can afford to do so. This is not feasible in many cases. Accepting a theory is crucial for continuing research in an area and is often necessary for discovering those new pieces of evidence that could confirm or contradict the theory. It is unusual for scientists to pause a research field because two theories seem equally well supported by the evidence at hand. More typical is to accept a theory and carry on, even while knowing it could later turn out to be false.

The question, then, is how these choices should be made in cases where the evidence does not settle theory choice. Philosophers of science, including Kuhn, have argued that it is acceptable to use so-called epistemic values to make this choice, but not social or political ones.[45] Kuhn listed the values of simplicity, scope, theoretical elegance and fruitfulness as examples of the sorts of values that could play a

[45] Making such a distinction between epistemic and non-epistemic values is fairly widespread. Hilary Putnam makes a similar point in Putnam, *The Collapse of the Fact/Value Dichotomy*. Cambridge, MA: Harvard University Press, 2002.

legitimate role in filling the gap left by evidence.[46] The typical justification for separating epistemic and non-epistemic values and allowing only the former a legitimate role in scientific research is that only epistemic values can be conducive to the generation of knowledge. Selecting theories based on how well they conform to moral or political values would be to assume that nature must reflect our moral sensibilities, and there is no reason to think this is true. How we want the world to be is not a reliable guide for understanding how the world is.

This argument is plausible, but it makes more of the distinction between epistemic and non-epistemic values than can be justified. Epistemic values are still values and they will run into the some of the same problems as moral values. All of the arguments about contextualism and relativism with respect to ethical values could be repeated in connection with epistemic values. The choice among different epistemic values cannot be determined by physical evidence, nor can their content be established by precise rules. Disagreement in epistemic values is just as likely and difficult to resolve as disagreement in moral values. To see why, we should submit epistemic values and their relationship to the knowledge-generating properties of science to closer scrutiny.

Many different values are classified as epistemic, but there are important differences in how they contribute to achieving epistemic goals, as well as in what is meant by epistemic in each context. Let's take Kuhn's proposed list of epistemic values. Some of these, such as accuracy and scope (defined as a theory's ability to extend beyond the particular observations it was designed to explain), are values that depend directly on empirical success in the world. These are fairly uncontroversial

[46] Thomas Kuhn, "Objectivity, Value Judgment and Theory Choice" In Thomas Kuhn, *The Essential Tension: Selected Studies in Scientific Tradition and Change.* Chicago, IL: University of Chicago Press, 1977.

values that scientific theories should realize. Others, such as consistency with earlier findings and fruitfulness for future research, are values specific to the internal purposes of the scientific community. It is important for the scientific enterprise to preserve important findings that support others and to open up avenues for future research by raising interesting new puzzles or connections, but these are hardly epistemic values in the sense of directly enhancing a particular theory's ability to explain things in the world.

Values such as the elegance or beauty of a theory are purely aesthetic, while simplicity could either be thought of as aesthetic or as serving the purposes scientific community, for instance because simple theories are easier to work with than complex ones. More interestingly, it could also reflect a metaphysical assumption about how the world is actually structured. Assuming that the basic principles of the universe could be expressed by a set of relatively simple fundamental laws or regularities was fairly standard among scientists, at least until recently. Many of the epistemic values that are regarded as acceptable in determining theory choice are values derived from the practical purposes of scientists, aesthetic preferences or metaphysical assumptions not supported by the evidence. These values are epistemic only in the sense that they are related broadly to the purpose of knowledge acquisition, not because they have a bearing on the level of evidential support of the theory being tested.

Different values can favor different theories and trade-offs may be necessary if improvement in one value is costly with respect to others. The theory that has the best predictions, for instance, might be unwieldy and difficult to work with. There may be disagreement over how trade-offs should be made in these cases, even if everyone agrees about the relative merits of competing theories, but agreement on the merits of theories cannot be taken for granted, either: What counts as a fruitful or elegant theory is a subjective matter. Epistemic values exhibit the same problems as kindness and goodness, from an epistemological point of view.[47]

To give an example, epistemic values play an important role in the study of climate change, where scientists have to make these kinds of choices among a wide range of climate models that enjoy similar degrees of support by the evidence. The complexity of the Earth's climate system, coupled with the difficulty of collecting the kind of evidence that can clearly confirm or disconfirm a climate model make it difficult for scientists to speak of having confidence in the likely truth of one model over the others. The choice requires appealing to epistemic values that are seen as more desirable. The trade-offs in the climate case include choosing between simple models that are easier to work with and more accurate models that use hundreds of thousands of lines of computer code; between predictive accuracy and explanatory power, between system-level accuracy and process-level accuracy, and accuracy at different time scales.

These judgments about epistemic values cannot be made except with reference to practical considerations. Scientists can make these choices based on their professional purposes, for instance about which theories will be more productive for future research or easier for them to work with. Alternatively, they can make these decisions based on what they think is more important from a social perspective. The important point is that working with the goals, values and priorities of different people will yield different choices under the same evidentiary circumstances. Scientific communities with different moral or political values – for instance, scientists in different countries – may also disagree on epistemic values.

[47] Hilary Putnam, *Realism with a Human Face,* James Conant ed. Cambridge, MA: Harvard University Press, 1990.

The particular menu of values is also open to debate. Feminist philosophers of science have challenged the widely accepted list above and suggested alternatives inspired by a feminist outlook. Longino proposes novelty over consistency, ontological heterogeneity over simplicity, and complexity of interactions between causes over reductionism.[48] Accessibility and instrumental efficacy could be added to the list. Like Kuhn's epistemic values, these are derived from aesthetic preferences, practical purposes and background metaphysical assumptions – only different ones.

It is possible to try resolving these value disputes by arguing that a set of values – the values that are currently popular, for instance – have been selected over time for their conduciveness to truth.[49] This must be true to an extent: comparing Kuhn's list to some other epistemic values held in the past, such as compatibility with a religious text or consistency with traditional beliefs, supports the view that the values in use today are more successful than some earlier ones. But it is difficult to conclude that current values are superior to all past ones, or that their success must have been the reason for their selection. This generalization requires a confidence in the success of current theories that cannot be justified under a moderate degree of fallibilism. There is also the possibility that different values that have not been tested or even imagined will outperform the values currently in use. The point of the feminist critique is precisely to challenge default assumption that theories exhibiting simplicity and homogeneity are more successful at representing the world than theories that exhibit different values.

I have argued that the significance of the fact that values play a role in theory choice should not be diminished by drawing a line between epistemic values and

[48] Longino, *Science as Social Knowledge.*

[49] Aviezer Tucker, "The Epistemic Significance of Consensus," *Inquiry,* 46 (4), 2003.

moral or political ones. Acknowledging the role of values in science makes possible two kinds of discussions. The first is a more abstract normative debate about which values should play a role and how trade-offs among competing values should be made. The feminist argument that scientists should opt for novel and complex theories over simple ones because the quest for simplicity reflects a physics-driven and patriarchal worldview is an example of the kind of argument that might be made in such a debate. Marxist philosophers of science have made similar arguments in the past about the need for dialectical relationships in scientific theories. Such arguments can be made on the grounds that theories displaying certain values are metaphysically superior, that they are more suited to the purposes of science, that the purposes of science so far have been guided by the wrong values and so on. The second-order arguments for and against first-order epistemic values can involve social and political considerations, as well as competing views about the purposes of science.

These debates will be difficult to settle at a general and theoretical level, much like debates about moral values. It is naïve to think that we possess enough knowledge about the success and truth of theories with different epistemic values to determine which ones scientists should prefer when the evidence does not settle the matter. A general, context-independent answer may not even be possible. More important are case-by-case discussions that expose how scientists use these values to settle theory choice in particular cases so that others can challenge them and suggest alternatives. This is a more modest approach, but it can prove more effective in practice for questioning and revising the choice of values.

The arguments in this section apply to cases where the evidence supports two or more theories equally, so it is natural to think that the debate about epistemic and non-epistemic values will not be significant in practice. First, no matter how scientists make this temporary choice, it won't matter in the long run because future evidence will lead them to converge on the empirically better supported theories. Kitcher makes this argument and concludes that scientists should not worry about transient underdetermination. Secondly, it might be rare for the evidence to actually support two theories equally, which means that it might never be necessary to appeal to these values as tiebreakers.

The first point may be true, but its truth would only be reassuring from the perspective of the scientific enterprise in the long run. That the truth may be discovered in an unspecified future is not helpful from the perspective of practical affairs today. Only in the more abstract sciences, which are removed from any practical or political applications can this be enough; for sciences that have some implication for our lives today, what matters most are these temporary theory choices in conditions of uncertainty. Value trade-offs will be of great importance in such cases.

The second point is an empirical one: how often do scientists come across cases where the evidence does not settle the choice between two or more theories? It is difficult to give a precise answer, but Stanford argues that the historical record suggests that recurrent transient underdetermination is very much our epistemic predicament, rather than a speculative possibility.[50] He points out that the examples of Aristotelian, Cartesian, Newtonian and modern mechanics; phlogiston and oxygen; Ptolemaic and Galilean astronomy; corpuscular theories of light, wave theories of light and quantum theories; germ-plasm theory, Mendelian genetics and molecular genetics all demonstrate that at one point two or more theories enjoyed equal support by the available evidence. Of course, these examples also show that future evidence

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[50] Kyle Stanford, *Exceeding Our Grasp: Science, History and the Problem of Unconceived Alternatives.* Oxford: Oxford University Press, 2006.

will settle the choice, but it is enough for my argument here that there can be long periods, sometimes centuries, in which the evidence cannot determine the choice between two or more theories. If evidence is scarce, it is more likely that several theories will be equally supported and that this situation will persist for a long time. The prevalence of underdetermination may vary across the different sciences, but Stanford's examples show that underdetermination is found in many fields.

In sum, theory choice cannot be explained only with reference to the evidence. The background assumptions of scientists determine what they will take evidence to be evidence for, and the weights they give to different values will determine how they adjudicate between equally well-supported theories. Both of these highlight an important feature of science: that it is always possible for scientists to accept theories that are empirically adequate without being true. An important implication of this is that the knowledge produced will depend on the imagination of those who construct theories. If social beliefs and other background assumptions play a role in determining our theories, and there could always be other theories that do as well or better, it becomes important to make sure that these beliefs, assumptions and values are exposed and questioned. This is especially true if the particular piece of knowledge is not of purely abstract scientific interest, but has significant social and practical implications. It is possible that one day new evidence will lead climate scientists to converge on the empirically best supported climate model, but this is hardly reassuring from the perspective of decision makers today. Especially in cases of high uncertainty and high stakes, such as climate change or the Zika virus, what we care about from a political perspective is precisely how these value trade-offs are made between rival theories that are equally supported by the evidence.

III. From Lab to Life

So far I have focused on the role of background assumptions in determining the fit between evidence and theory, and the role of values in determining theory choice. Another point in scientific research where the values, assumptions and practical judgments of the researcher play a role is in the decision that an experiment is an appropriate "stand-in" for the real life phenomenon it is designed to explain. All scientific inference from one state of affairs to another relies on the validity of such standing-in relationships – or metonymic relationships, as Shapin has aptly called them.[51] What the researcher cares about is not just the local and specific result obtained under particular conditions, but the generalization that can be drawn from it. This requires a judgment that the experiment is sufficiently similar to the object of interest and that the similarity is in the relevant dimension. But of course both sufficiency and relevance are relative to an assumed purpose.

When Pascal sent his brother-in-law up the Puy-de-Dome with a barometer on September 19, 1648 and asked him to measure the drop in the level of mercury, he was counting on the validity of at least three different standing-in relationships: first, that the behavior of the mercury in the glass would stand-in for the weight of the atmosphere; second, that this particular change in weight would stand-in for what would happen if one were to go up even higher in the atmosphere; and third, that what happened to that particular barometer on September 19, 1648 would stand-in for what would happen to other barometers at other times and places. Similarly, when Robert Boyle performed his famous air pump experiment by placing a barometer in an air pump and vacuuming the air, he was taking this to be a stand-in for what would

[51] Steven Shapin, "Cordelia's Love: Credibility and the Social Studies of Science," *Perspectives on Science,* 3 (3), 1995.

happen to a barometer were it possible to take it to the top of the atmosphere.[52] These relationships are crucial for science not only because they make the move from particular to general possible, but also because they allow scientists to draw conclusions about phenomena they cannot experience from observations that are readily available to them. Scientists who find innovative ways of making experimental conditions stand for real life thereby push the boundaries of attainable knowledge. At the same time, their findings are valid only insofar as the underlying metonymic relationships are legitimate.

The difficulty is that the link between the particular event and the general conclusion, or between the experimental contraption and the real life phenomenon cannot be established deductively. It must always be supported inductively, with reference to other beliefs, assumptions and findings. The inference from an experimental object to a similar but distinct non-experimental object requires analogical reasoning: that two things share some relevant features is the basis for the inference that what is true of one will be true of the other.[53] The assumption that what happens in an air pump represents what would happen at the top of the atmosphere, or that the behavior of laboratory rats in medical experiments represents the behavior of humans involve this kind of reasoning.[54] Inferences from analogy are necessarily

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[52] Both examples from Shapin "Cordelia's Love." See also Steven Shapin and Simon Schaffer, *Leviathan and the Air Pump*. Princeton, NJ: Princeton University Press, 1985.

[53] Mill was famously interested in analogical inference. See Chapter XX of *A System of Logic, Ratiocinative and Inductive.* In *The Collected Works of John Stuart Mill*, J.M Robson ed. London: Routledge and Kegan Paul, Vol. VII.

[54] This is one form of the more general problem of induction, first formulated by Hume. Other forms include generalization from an individual sample to the entire population or from a finite number of observations to a universal hypothesis; and predictive inference from one sample to another not overlapping with the first. There are many interesting things to be said about these as well, especially on the appropriateness of different methods of statistical inference relative to the purpose of the scientific research. I bracket this because it is a complicated issue that would require much more space for proper treatment.

incomplete: as long as two things are not identical, what is true for one may turn out not to be true for the other. Relying on analogies is therefore a second-best option, useful when it is not possible to conduct experiments directly on the object of interest, for instance because of the impossibility of carrying a barometer to the top of the atmosphere or the ethical impermissibility of testing risky drugs on human subjects.

Analogical inference requires two kinds of judgment. The first is about what constitutes a sufficient degree of similarity between two objects. This depends on the purpose and the stakes of the study. The intrinsic properties of the objects alone cannot determine whether they are sufficiently similar. During the Cold War, nuclear missile tests in the U.S. were conducted on an east-west trajectory. No missile had ever flown the north-south polar trajectory that would have to be flown in case of an actual attack on the Soviet Union. Would an east-west test be an adequate stand-in for a north-south one?[55] Given the consequences of being wrong, it seems unlikely that the answer would be yes. Judgments about sufficiency rest on practical and contextual factors, rather than epistemic ones.

A second kind of judgment concerns whether two things *are* in fact similar and whether the similarity is relevant to what is tested. In some cases, this will depend only on background scientific knowledge. The determination of whether a vacuumed air pump is similar to the top of the atmosphere is a question that can be settled with reference to widely accepted facts from physics about conditions at the upper levels of the atmosphere and the principles of vacuums.

In other cases, these judgments rest on socially determined beliefs and assumptions. Lloyd provides an example of this from studies in evolutionary biology

[55] Donald MacKenzie, "From Kwajalein to Armageddon: Testing and the Social Construction of Missile Accuracy." In *The Uses of Experiment: Studies in the Natural Science,* David Gooding et al. eds. Cambridge: Cambridge University Press, 1989.

on the relationship between female sexuality and reproductive capacity. For a long time, researchers mistakenly concluded that female sexuality must be tied to the reproductive cycle, with peak sexual desire occurring at the time of peak fertility, because they assumed that the sexual behavior of female mice and dogs, whose hormone levels fully determine sexuality, was analogous to that of human females.[56] Years later, clinical trials with human females revealed that only about 6 to 10% of women in fact experience peak desire around the time of ovulation.[57]

On one level, the mistake in the earlier studies was due to the researchers' inadequate knowledge of female sexuality. They did not have sufficient evidence to assume that female human sexuality was analogous to the sexuality of female mice and dogs. They could also be blamed for negligence: if the researchers had asked some women about their levels of sexual desire over the course of their menstrual cycles, it might have become clear that the analogy was not reliable. If one of the scientists had been a woman, that might have made a difference as well. But Lloyd makes a further and more interesting point: She argues that the researchers weren't simply carelessly assuming an analogy based on no knowledge at all. That might simply have been dismissed as bad science. They were rather operating with the traditional assumption that female sexuality could be fully understood by its function of childbearing – an assumption that was entrenched and widespread in society at the time. Against this shared social background, it was not careless to assume that studies in female mice and dogs could be extended to humans.

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[56] Elisabeth Lloyd, "Pre-Theoretical Assumptions in Evolutionary Explanations of Female Sexuality," *Philosophical Studies,* 69 (2), 1993.

[57] Lloyd cites Irving Singer and Josephine Singer, "Periodicity of Sexual Desire in Relation to Time of Ovulation in Women," *Journal of Biological Science,* 4 (4), 1972 and a few others.

Lloyd's case study illustrates how social assumptions can influence science and shows that judgments about similarity depend on the researcher's beliefs and assumptions.[58] It also points out how non-experts can challenge the social assumptions of scientists. Uncoupling women's identity and sexuality from their reproductive capacity was a major battleground for second wave feminists.[59] Women's struggle to establish the distinction between sexuality and reproduction could have changed approaches to the scientific study of the subject if biologists had taken seriously the possibility that this social critique had *scientific* implications.[60]

The failure of analogies can also cause problems at the stage of applying findings from controlled laboratory environments to real life settings. Scientists conducting the experiment may not know enough about the local conditions in which their findings will be used, and might make the wrong assumptions about the similarities between experimental and actual conditions. Wynne's study of Cumbrian sheep farmers illustrates this problem.[61] In 1986, after the Chernobyl accident, upland areas of Britain suffered from deposits of radioactive cesium isotopes. Scientists and political officials immediately dismissed these deposits as negligible because scientific models predicted that the cesium would be absorbed by the soil and immobilized before it could pass into the vegetation. They also predicted that this

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[58] See Michel Foucault, *The Order of Things: An Archaeology of the Human Sciences*. London: Routledge, 2002 on the relationship between representation, resemblances and knowledge.

[59] Lloyd, "“Pre-Theoretical Assumptions in Evolutionary Explanations of Female Sexuality."

[60] Many assumptions about the similarities and differences across races pose similar problems. Ian Whitmarsh and David S. Jones eds. *What's The Use of Race: Modern Governance and the Biology of Difference.* Cambridge, MA: MIT Press, 2010, which asks whether race can be a meaningful category of analysis in science, law and medicine without reinforcing discrimination.

[61] Brian Wynne, "Misunderstood Misunderstanding: Social Identities and Public Uptake of Science," *Public Understanding of Science,* 1 (3), 1992.

process would take about three weeks, so a three-week ban was placed on the movement, slaughter and sale of sheep in these areas. The radiation levels did not go down after three weeks, however, nor did they change throughout that summer. The ban was extended indefinitely, with the sheep facing starvation and the farmers the loss of their livelihood.

It turned out much later that the prediction about the chemical absorption of cesium isotopes was made based on a model that assumed alkaline clay soils, whereas the upland areas had acid peaty soil, which allows cesium to move around and become absorbed by plant roots.[62] Wynne argues that the Cumbrian farmers had valuable knowledge about the local environment that could have assisted scientists in better understanding upland conditions, even if the farmers couldn't put their knowledge in the technical language of the experts. This source of knowledge was untapped because the farmers were not included in decision-making over the issue. The water crisis in Flint, MI involved a similar, though probably more culpable disregard of evidence provided by local residents, who insisted to the authorities that their water was contaminated, even as scientists continued to assure them that it was safe.

A final example is from randomized control trials, where scientists' judgment about the appropriateness of different stand-in relationships was contested by lay activists in highly publicized cases such as cancer and AIDS drug trials. The controversy was over the appropriate composition of trial populations. Some scientists insisted that the composition of a trial group could be decided on purely scientific grounds, much like the question of how to construct an experimental contraption that

[62] Any kind of modeling requires these kinds of assumptions. There is far more to be said about problems with scientific modeling e.g. climate change models. See Kirsten Hastrup and Martin Skrydstrup eds. *The Social Life of Climate Change Models.* London: Routledge, 2013.

would adequately represent the top of the atmosphere. In the AIDS case, the accepted scientific wisdom was that homogenous populations, excluding blacks, women and intravenous drug users, and composed of patients taking no other medications, would form the ideal trial group.[63] AIDS activists rejected this and argued that what constituted a good study population had to be defined relative to the practical purposes of the study. Homogenous populations and subjects who had no other medical problems would not be an appropriate stand-in for real patient populations if the purpose were to treat people, rather than to answer purely theoretical questions.

Epstein documents how the AIDS movement succeeded in changing the scientific attitude towards RCTs. Scientists began to recognize two scientifically valid approaches to the design of trials.[64] The first is the "pragmatic" approach, which prioritizes the practical purpose of solving problems that may arise in clinical practice.[65] Homogenous populations with patients taking only the drug under study are not the ideal population for these studies because these groups are not suitably analogous to the target patient population. Clinical practice is messy: Patients will have different conditions and proclivities and will be taking many medications at the same time to increase their chances of survival. A drug tested on a pure trial population may therefore not produce the expected effect in real patients. The second, "fastidious" approach is the more traditional. Here the purpose of the trial is to understand as clearly as possible what effects the drug produces in the human body. This requires an artificially controlled, homogenous trial group and yields less

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[63] Steven Epstein, *Impure Science: AIDS, Activism and the Politics of Knowledge*. Berkeley: University of California Press, 1996.

[64] Ibid.

[65] See Alvin Feinstein, "An Additional Basis for Clinical Medicine II: The Limitations of Randomized Trials," *Annlas of Internal Medicine,* 99, 1983; and Robert J. Levine, *Ethics and Regulation of Clinical Research.* Baltimore: Urban & Schwarzenberg, 1986.

ambiguous, more secure findings. The weakness of this approach is that the tested drug may not produce the same results in real clinical subjects.

All three examples demonstrate how the similarities assumed in scientific studies may turn out to be inadequate or wrong. This may result in false findings or in a mismatch between laboratory findings and their practical applications. Of course, mistakes are possible at many other points in the scientific process. But mistaken assumptions about analogies are different than, say, errors in measurement because the former involve background knowledge and beliefs, and therefore cannot be corrected simply by repeating the experiment. When assumptions are widely shared among scientists, people with different backgrounds will be better placed to challenge them than those with more scientific knowledge.

IV. The Role for Politics

I showed in the last three sections how background assumptions and values play a role in scientific research. The recognition that science involves values does not make expert claims hopelessly relative, but it does have important consequences for the political use of expert knowledge. In some cases, such as the Cumbrian sheep farmers or AIDS drug trials, the failure to examine background assumptions can result directly in harms to people. In others, such as the studies on female reproduction or on the evolutionary origins of anatomical and behavioral development, the values embedded in scientific findings can lend support to views about social relations that reflect a partial social perspective disguised as an objective scientific one. Moreover, examples such as Ptolemaic astronomy or the man-the-hunter theory of evolution show that even scientific theories that are empirically successful may turn out to rest on questionable or false assumptions.

Since the logic of scientific discovery necessarily involves values, it is unrealistic to expect scientists to remove their values from science altogether. It is more productive to understand how values and unexamined assumptions shape science and then consider *which* values and *whose* values should properly influence findings. This requires exposing the contingency of knowledge claims by submitting them to the critical scrutiny of others, especially of others with different views and assumptions. The argument here is Millian: individuals are fallible, but critical discussion between competing viewpoints can help correct errors, piece together partial truths and facilitate the collective discovery of truth. [66] I will argue that this critical scrutiny must be democratic and that the public and its representatives must participate in examining expert claims before they are used in policy decisions. But first I want to consider an alternative.

Philosophers of science such as Longino and Solomon have argued that a critical discussion among different viewpoints is necessary to reconciling the value-ladenness of science with its claims to objectivity, but they have limited participation to the scientific community. This may be adequate for philosophers of science

[66] See John Stuart Mill, *On Liberty*, in *The Collected Works of John Stuart Mill*, J.M. Robson ed. London: Routledge and Kegan Paul, 1963-1991, Vol. XVIII. The question of whether Mill meant to include the natural sciences within the scope of his argument in *On Liberty* comes up frequently in the literature on Mill. It seems clear that he was more interested in moral and religious questions, although there are a few passages where he uses examples from the natural sciences. Alan Ryan argues that Mill did not offer this as an empirical model for intellectual progress, but as a normative model for distinguishing between received beliefs and beliefs subjected to open discussion. (Alan Ryan, "Mill in a Liberal Landscape," In *The Cambridge Companion to Mill,* John Skorupski ed. Cambridge: Cambridge University Press 1998, p. 509.) Whatever Mill's intentions were, if his argument is extended to the natural sciences, it stands in tension with his scientific method in *A System of Logic.* The inductivist method he develops there leaves no room for criticism; a careful individual following the method should be able to reach true scientific beliefs. Feyerabend argues that Mill simply changed his mind between *Logic* and *Liberty,* (Paul Feyerabend, "Imre Lakatos," *The British Journal for the Philosophy of Science,* 26 (1), 1975.) but this seems unlikely. Moreover, even if we take Mill to have developed a proto-Popperian view of the role of criticism in the production of scientific knowledge in *On Liberty,* there would still be no grounds for the view that non-experts should participate in this kind of critical exchange.

primarily concerned with the implications of the role of values for the possibility of truth, objectivity and progress in science in the long run. But for those concerned with the moral and political implications of using scientific findings as the basis of public policy, leaving out citizens and their representatives from a critical evaluation of the assumptions and values that shape knowledge claims cannot be acceptable.[67] I will give three arguments for this claim. The first follows directly from my analysis of scientific knowledge. The second is rooted in normative democratic theory and the third is about the knowledge of laypeople.

First, democratic participation can play an important role in exposing background assumptions and values. Democratic engagement on scientific issues should not be thought of only as having non-experts identify the "correct" expert and then deferring to her claims. It is more importantly about allowing non-experts to assess the normative judgments behind expert claims and determine which ones are compatible with their values.

Even a demographically diverse community of scientists may fail to generate sufficient diversity in viewpoints to adequately question and challenge each other's claims. For one thing, the scientific community today is not particularly diverse, but even if it became more diverse demographically, selecting on demographics is a blunt instrument for ensuring a diversity of viewpoints. Non-scientists, especially those engaged with social and political issues, may be more sensitive to hidden social assumptions than even a diverse body of scientists. Scientists are not trained to question or recognize social assumptions; the critical abilities required to spot assumptions about gender, for instance, are different than the training required for

[67] Douglas also criticizes them on this point. See Douglas, *Science, Policy and the Value-Free Ideal*.

scientific research. Feminists activist who challenged the biological study of female sexuality were able to do so because of their different background assumptions.

Ordinary citizens also bring a different perspective – different purposes, interests, goals and priorities – to these issues, which cannot be replicated by any amount of demographic diversity within a professional community of scientists. And this difference in perspective may well be the most crucial kind of difference. Even a diverse body of scientists will be alike in their professional goals, interests and commitments. The professional goals of scientists play a role in the choice of theories, the weighing of competing epistemic values and the design of experiments. The discovery of truth may be the central goal of science, but scientists also have an interest in continued scientific progress, the productivity of their paradigms and the expansion of future research areas. These purposes affect the scientific choices they make, especially where the evidence cannot determine the choice between rival theories, and trade-offs between competing epistemic values are necessary. Non-scientists will often favor prioritizing different things and therefore making different choices under the same evidentiary circumstances because of their different commitments. The AIDS case I described earlier provides an example of this.

My second argument for democratic participation is rooted in a normative theory of democracy. A democratic public has the right to understand and question claims that rest on values and assumptions that they may not share, but whose consequences will affect them. Scientific findings have a direct political impact when they are used as the basis of policy decisions, and they have an indirect impact in shaping what citizens and politicians take to be the facts about the world they live in. Scientists' decisions about evidence, and their choices between competing theories determine the beliefs of non-scientists about the feasibility, efficacy or correctness of different courses of action. Failure to detect and question the assumptions and values embedded in knowledge claims will result in democratic policy being guided by the unexamined judgments and purposes of the experts making knowledge claims. If in the end citizens or politicians do take expert views wholesale, it is important that they do so with the assurance of having examined the view in a democratic process, instead of taking it completely on authority.

The third and final argument in favor of democratic participation is that laypeople may actually possess knowledge that could lead to the revision of assumptions made by scientists. Although some areas of science, such as certain kinds of physics, have moved far beyond the everyday experience of citizens, others, such as ecology, biomedicine and psychology still rest on observations that are within the realm of lay experience.[68] Laypeople can provide information about local conditions, their environment or their own bodies that are inaccessible to scientists. Brian Wynne's study of Cumbrian sheep farmers provides one example of this possibility.[69]

Of course laypeople will not be able to produce their own research, and will ultimately depend on experts for new knowledge. Their participation will be in the form of a critical and reactive audience to public exchanges between different scientists. Laypeople can question the claims and counterclaims of competing scientists, with the goal of exposing background assumptions and value choices. Even those who do not possess knowledge can still play a valuable role by exposing problems with other people's claims. Mill observed that most people are suited for

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[68] Deborah Coen argues that in the past, earthquake science relied heavily on data provided by local oberservers. This changed in the twentieth century as a result of what she calls the construction of incommensurability between lay experience and scientific data, but she suggests that the twenty first century may see another reversal, given increased uncertainty. See Coen, *The Earthquake Observers: Disaster Science from Lisbon to Richter.* Chicago, IL: Chicago University Press, 2013.

[69] Wynne, "Misunderstood Misunderstanding."

this form of discussion because it requires "less study and less real knowledge" of its participants than other forms of inquiry.[70] Even those who do not possess knowledge can serve a valuable purpose by exposing the indefensibility of other people's claims or by supplying partial truths that can be pieced together to reach the whole truth. An important underlying principle of a critical exchange is that the truth is unknown and that individuals are fallible, but that others might be able to expose one's errors – even others who possess less knowledge than the claim-making person.

Discussions between scientists and laypeople can focus on three kinds of criticism, which map onto the three stages discussed earlier where the assumptions and values of scientists influence knowledge claims. The first would aim to expose the background assumptions that fill the gap between evidence and theory. What assumptions are scientists making in concluding that a piece of evidence can be taken to support a theory? Are these assumptions backed by other scientific theories, or are they social or metaphysical assumptions? Are they defensible, and if so based on which arguments or evidence? What are some plausible alternatives?

The second category would focus on the values that determine the choice between two or more equally supported theories. On what grounds have scientists decided to select one theory over the others? Which epistemic values or purposes have played a role in the decisions? How are these values related to epistemic, scientific or social goals? Do scientist agree on the trade-offs between values or are there unresolved value conflicts within the scientific community that cannot be resolved by the evidence?

Finally, the third category would expose the assumptions that determine that a scientific experiment is adequate to the phenomenon it is designed to explain. This

[70] John Stuart Mill, *The Spirit of the Age, Part I.* In *The Collected Works of John Stuart Mill*, J.M Robson ed. London: Routledge and Kegan Paul, Vol. XXII, p. 234.

requires questioning the assumed similarities between the experiment and local conditions. Are the similarities relevant to the purpose of the experiment and if so, are the similarities sufficient? What did scientists take to be the purpose of the experiment and does the experiment serve this purpose? Are scientists aware of the particularities of the local conditions? What assumptions have they made to simplify the real conditions and are these reasonable? These questions can guide the examination of competing expert claims by non-experts.

Before concluding, I want to discuss the practical implications of these arguments. To be clear, my basic claim is not that lay citizens should enter the science lab or contribute to peer review, but that there should be better discussion about science in the democratic public sphere. This will be both more likely and more important on scientific issues that have some bearing on policy. The examination of expert claims that I have argued for applies at the stage where scientific research is required for political decision-making. Exchanges between scientists and non-scientists can take place in a wide range of venues including publicly monitored debates between scientists, scientific advisory boards, congressional hearings, local town-hall meetings, protests and rallies – different venues will be appropriate for different issues.

Of course, such venues already exist for the discussion of science, so it is important to clarify exactly what my argument would change. The argument in this chapter has three main practical implications for how scientific issues should be addressed in the democratic public sphere. The first is that it is important for non-experts to participate in debates about the use of science in politics. This implies that non-experts should be included in the relevant venues that already exist, and also that there would be benefit in increasing the institutional spaces where such interactions can take place.

The second point is about the role non-experts should play when they participate. While other scholars have also called for more democratic participation on technical issues, my argument differs importantly from others in the literature in the way I theorize the possible contribution of democratic input: while other scholars have assumed that democratic processes will supply value judgments about how to act on the basis of knowledge provided by the experts, I argue that a democratic process should be extended to questioning the value choices and purposes driving the knowledge claims themselves.

The third implication is about the content of the arguments that will be appropriate in debates over scientific claims in the public sphere. I have argued that it would be a mistake to think that evidence alone is relevant to discussions about what facts or theories to accept as the basis of political decisions. Arguments about values and purposes have a role in shaping not just what to do in light of the facts, but also what to accept as the factual background of political decisions, alongside the evidence. This of course implicitly provides support for the second point just mentioned – that discussions about the content of expert claims must take place in the political realm.

These three points in turn will have implications for institutional design. If we believe science must be examined democratically, we need to consider what kinds of institutional arrangements and rules can facilitate the participation of non-experts as equal and active participants rather than a passive audience to be educated by experts. I turn to this question in Chapter 3.

Conclusion

Political theorists and philosophers of science have stopped short of extending the Millian ideal of free and public discussion to technical or scientific subjects on which there are qualified experts. Political theorists have limited the scope of democratic deliberation to moral and political beliefs, while philosophers of science have limited participation in critical exchange over scientific claims to the scientific community. Both have assumed that discussions of natural science with a lay audience would not serve its primary epistemic purpose of enabling the discovery of truth, though it might have educative benefits. The view of science I developed in this chapter challenges the accepted wisdom that scientific facts are best left to experts and that the non-expert public can take facts as provided by the correct expert. I have argued that it is necessary for scientific claims to be subjected to critical democratic scrutiny because of the role of unexamined assumptions and values in science.

Chapter 2: Belief, Acceptance, and Evidentiary Standards

The conventional view of practical reasoning is that we seek to realize our intentions and satisfy our desires in light of our beliefs.[71] On this view, the cognitive background of our planning and actions can be explained simply by our beliefs, which are generally taken to be responsive only to the evidence for their truth.[72] This framework falls short in accounting for some complexities: It cannot explain cases where we act on things that we do not believe or believe things that we would not act upon. Nor can it account for how practical pressures, such as the context, the stakes and the agent's goals, shape the cognitive background of planning and deliberation.

This chapter has two main goals. The first is to sketch a conceptual distinction between the cognitive attitudes of belief and acceptance – specifically, acceptance in a context or acceptance for a purpose – and argue that using acceptance resolves some of the problems with the belief-action framework by accounting for the role of practical factors in the background of deliberation toward a decision or action. The second and more important purpose is to argue that this philosophical distinction, developed in the context of individual practical reasoning, can illuminate parallel complexities about the factual background of *collective* deliberation and decision-making in the sphere of politics, focusing especially on taking scientific knowledge from experts for use in policy decisions.

The belief-action framework assumes that the factual background of decision-making appeals only to epistemic factors. This provides support for the view that it is

[71] Michael E. Bratman, "Practical Reasoning and Acceptance in a Context," *Mind,* 101 (401), 1992.

[72] See Jeremy Fantl and Matthew McGrath, "Evidence, Pragmatics and Justification," *The Philosophical Review,* 11, 2002 for a rare paper that disagrees on this point.

unproblematic for a democratic public to take scientific knowledge on authority from experts and then deliberate about what to do in light of it, as long as scientists do not make practical judgments about how to act. The arguments for the distinction between belief and acceptance show that such a neat line between the epistemic and the practical is difficult to maintain. Evidence can provide support for the truth of a proposition, but evidence alone cannot motivate acceptance. What motivates acceptance is the judgment that the evidence is sufficient, and the judgment of sufficiency is always relative to a purpose. Since the practical pressures on acceptance are different in the scientific and political realms, different evidentiary standards for acceptance will be appropriate in each. What scientists accept may not be accepted for political purposes, and vice versa.

The chapter has four sections: The first section focuses on the philosophical grounds for the belief-acceptance distinction, drawing on work by Michael Bratman. The second provides an account of the distinct and central role of acceptance in scientific research and the role of practical factors in scientists' acceptance of hypotheses. The third focuses on the practical pressures on acceptance in the political realm and argues that they justify using different evidentiary standards for political purposes than for scientific purposes. The last section argues that decisions about the appropriate evidentiary standards for politics must be made democratically, rather than having scientists set their evidentiary standards based on their own assessment of the social and political consequences.

I. Belief and Acceptance

Michael Bratman defines acceptance as the attitude of taking something for granted in the background of one's deliberation and planning.[73] It is an active commitment to including a proposition or rule among one's premises in deciding what to do or think in a particular context and given a particular purpose. When we accept a proposition, we adopt a policy to reason, plan and act in that context *as if* the proposition is true. This might sound like supposition or hypothesis, but we do not act upon things that we merely suppose; the distinctive feature of acceptance is precisely that we act upon things we accept. Supposing the world will end tomorrow is very different than accepting that it will.

The best way to clarify the properties of acceptance is by contrasting it with belief. I'll start with some widely accepted properties of belief and show how the properties of acceptance differ from these.

1. Belief is responsive to epistemic factors, but not to practical ones. It is justified only by the evidence in favor of its truth. Although individuals can have valid reasons for believing or wanting to believe things that are not true, these reasons would not justify the belief itself. They may justify the individual's believing, but not the content of the belief.
2. Belief is not subject to direct voluntary control. We cannot believe or disbelieve things at will. We may be able to exert indirect control over what we believe, for instance by exposing ourselves to certain kinds of evidence or intentionally seeking out or omitting certain sources, but our control invariably

[73] See Bratman, "Practical Reasoning and Acceptance in a Context;" Bratman, *Shared Agency: A Planning Theory of Acting Together*. New York, NY: Oxford University Press, 2013. Jonathan Cohen, *An Essay on Belief and Acceptance.* Oxford: Clarendon Press, 1992 and Facundo Alonso, "Reasons for Reliance," *Ethics,* 126, 2016 also use a similar concept.

goes through the evidence in favor of belief. We cannot simply choose our beliefs without regard for their truth.[74]

3. Belief is context-independent. At any one time, an individual reasonably either believes something or does not believe it (or has some degree of confidence in it). Her belief does not change across contexts. Moreover, beliefs tend to exhibit inertia across time: once a belief is formed, it remains unchanged unless or until it is disturbed by new evidence. Contrast this with, for example, emotions, which may dissipate simply with the passing of time.

These criteria suggest that belief is a relatively passive mental state. An agent can control the epistemic activities and processes that lead to belief, but not the eventual outcome of belief formation. Acceptance differs from belief on all of these points. It is responsive to both practical and evidentiary factors; it is subject to voluntary control; it involves active decision or commitment; and it can vary with the context. I will demonstrate these points with some examples. First, an example from Bratman:

"Suppose I have a chair and a two-storey ladder. In each case I think it equally and highly likely that it is in good condition. Indeed, if you offered me a monetary bet about whether the chair/ladder was in good condition I would accept exactly the same odds for each object. But when I think about using the chair/ladder things change. When I consider using the chair I simply take it for granted that it is in working order; but when I am about to use the ladder I do not take this for granted."[75]

The example captures how belief and acceptance come apart. I have the same degree of belief that the chair and the ladder are in good condition and nothing about the evidence changes at the moment I contemplate using them. But practical considerations – that is, the asymmetric cost of being wrong – make me prepared to

[74] Bernard Williams pointed out that if we could choose beliefs without regard to their truth, this attitude would no longer be recognizable as a belief. See Williams, "Deciding to Believe," in his *Problems of the Self.* Cambridge: Cambridge University Press, 1973.

[75] Bratman, "Practical Reasoning and Acceptance in a Context."

act on this belief in one case, and not in the other. I accept that the chair is safe, but not the ladder, even while my degree of belief is the same and remains unchanged in both cases. The example also suggests that different tests are appropriate for belief and acceptance. An individual's belief or degree of belief can be discovered by asking whether she would accept certain bets. The test for acceptance, on the other hand, is whether she would act or plan around the proposition. Willingness to bet on the truth of something does not imply being prepared to act on it – that is, being prepared to accept it – and vice versa.

Bratman's example is a case of believing something without being prepared to accept it. Here's an example that illustrates the reverse, where I accept something that I do not believe[76]:

> Suppose I find myself by a river and the nearest village is on the other side. The current is fast and dangerous, and I'm not a good swimmer, so trying to swim across is not option. But I need to get across. It is very cold and I will freeze if I don't reach the village on the other side before the night falls. There is a cable ahead that I could possibly use to haul myself across, but I don't know whether it will carry my weight. I inspect the cable and I don't think there is sufficient evidence to believe that it will in fact carry my weight. I wouldn't bet money on it, if somebody offered me the chance. But I can't say for sure that it won't either. I just can't tell. Given the fact that I know that I will freeze if I don't get across, I decide to accept that the cable will carry me and proceed to try hauling myself across.

In this case, I do not have sufficient evidence for belief, but I do not believe that the cable is unsafe either. If I believed it to be unsafe, I wouldn't, perhaps even couldn't act on it. Despite the fact that I don't believe it to be safe, I still accept it in my decision-making because acceptance is clearly the superior strategy given the practical considerations. Note the interplay of evidentiary and practical factors in determining acceptance: First, I check to see whether the proposition that the cable will hold my weight has plausibility. Then, I evaluate the practical factors and

[76] I have adapted this example from Alonso, "Reasons for Reliance."

determine what I will accept. Both kinds of considerations affect the decision, but evidentiary considerations have lexical priority. If the evidence settled the matter one way or the other, then practical considerations would not play any role in my deliberation.

Both of these cases could be redescribed in terms of an expected utility calculus based on the agent's subjective degrees of belief, although Bratman never mentions this. In the case of the chair and the ladder, we could say that what I really believe is that there is, say, an 80% chance in each case that the object is in good condition. The 20% chance that the ladder is not in good condition multiplied by the high negative payoff of falling off the ladder yields a negative utility, which suggests that I should not act on this degree of belief. In the case of the cable, even a relatively low degree of belief in the cable's safety can be the grounds for acceptance because of the extremely high cost of not accepting. This observation raises the worry that the concept of acceptance may turn out to be redundant after all.

One argument against the charge of redundancy is that acceptance provides a more accurate way of describing a person's mental attitude in cases of partial belief than expressing it in term of subjective probabilities. Someone who wants to reduce everything to degrees of belief – say a radical Bayesian – may explain my behavior ex post by my having assigned a certain probability to the chair and ladder being in good condition and having made expected payoff calculations on its basis. But it would be reasonable for me to object that I do not recognize this as the correct description of my mental state at the time; I really believed both the chair and the ladder were in good condition. Beliefs may not be as fine-grained as the Bayesian assumes them to be.[77]

An example provided by Robert Nozick supports this point. Nozick writes:

"I believe my new junior colleague is not a child molester. (Asked to list the people in the philosophy building who are not child molesters, I unhesitatingly place his name on the list.) Now the context changes; I need someone to watch my young child for two weeks. A mistake here would be serious—the stakes have escalated. Now I think more carefully. It is not that I did not believe in my colleague's innocence before. In that context, for those purposes, I did believe it; I did not consider, or assign a probability to, the possibility of his being a child molester. In this context, with high stakes, I consider what probability that might have."[78]

To insist in this case that I must only have partially believed that my colleague is not a molester from the beginning would strike me as a mischaracterization of my attitude. Nozick's own proposal for resolving this duality is to contextualize belief: In one context I believe the proposition; in another I have a partial degree of belief in it. But this, too, is rather odd and runs afoul of some of the widely accepted properties of belief listed above: that belief is not context-dependent, that it exhibits some inertia across time and that it responds to evidence, but not to practical factors.

The belief-acceptance distinction can make sense of precisely these kinds of situations where I may be reluctant to act on propositions I genuinely would say I believe, and it does so without going against widely accepted properties of belief. I did not and still do not believe that my colleague is a molester, but given the stakes, I think it is best for me to act *as if* he might be.

These two examples show how evidentiary standards for acceptance can vary with the stakes in cases of uncertainty. The concept is also helpful in making sense of

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[77] Thomas Kelly also makes this point with a vivid imaginary contrast between Alpha Centaurians, who have degrees of beliefs to the seventh decimal point, and humans. See Kelly, "Evidence Can be Permissive." In *Contemporary Debates in Epistemology,* Matthias Steup et al. eds. Chichester: Wiley-Blackwell, 2014.

[78] Robert Nozick, *The Nature of Rationality.* Princeton, NJ: Princeton University Press, 1993.

cases where we may accept things that we know to be false if doing so is justified by practical reasons, such as special commitments to loved ones or the pressure to avoid very bad consequences. An example from Peter Railton's defense of consequentialism illustrates the latter case.

"Imagine an all-knowing demon who controls the fate of the world and who visits unspeakable punishment upon man to the extent that he does not employ a Kantian morality. (Obviously, the demon is not himself a Kantian.) If such a demon existed, sophisticated consequentialists would have reason to convert to Kantianism, perhaps even to make whatever provisions could be made to erase consequentialism from the human memory and prevent any resurgence of it."[79]

In explaining the example, Railton draws on the distinction between belief and acceptance: "Does this possibility show that objective consequentialism is self-defeating? On the contrary, it shows that objective consequentialism has the virtue of not blurring the distinction between the *truth-conditions* of an ethical theory and its *acceptance-conditions* in particular contexts."[80]

The truth conditions of a theory determine what we should believe; our goals in a particular context determine what we should accept. This maps onto the distinction between p being the rational thing to believe and accepting p being the rational thing to do. The practical goal of the consequentialists in Railton's example is to avoid unspeakable punishment at all cost. Even if this requires accepting an ethical theory they believe to be false, it would be rational to accept it. This does not mean that the threat persuades them of the truth of Kantianism; only that it has made acceptance rational for them in this particular circumstance.

Acceptance here and in general should not be confused with wishful thinking (or believing). Although both allow practical considerations to determine a cognitive

[79] Peter Railton, "Alienation, Consequentialism and the Demands of Morality," *Philosophy & Public Affairs,* 13 (2), 1984.

[80] Ibid.

attitude, their logic is quite different. Wishful thinking is the formation of beliefs based on wishes, or on whatever is likely to give pleasure, satisfaction or relief. The reasoning in wishful thinking takes the form "I wish p to be true, therefore p is true." Beliefs formed in this way would be unreliable and would make it difficult for the person to attain her goals in life, assuming she has other goals besides immediate psychological satisfaction. Wishful thinking, then, is likely to be an irrational practice. Acceptance is the opposite. When we accept things based on non-evidentiary considerations, we do so precisely because doing so would be the best means to achieving our ends. Acceptance aims at practical rationality, and the cases where it involves accepting false propositions are cases where cognitive and practical rationality come apart.

Acceptance has more in common with Pascal's wager than it does with wishful thinking.[81] Pascal famously argued that it would be rational to wager that God exists, regardless of the available evidence, since the expected utility of believing in God is infinite. As long as there is a positive chance that God exists, however small, the infinite utility from salvation would trump any finite amount of disutility on the other side of the balance. Even if one cannot will oneself to believe, a rational person should take the necessary steps to cultivate belief in God. The difference between Pascal's wager and the concept of acceptance is that religion would presumably require real belief; accepting that God exists and making it a premise in one's deliberation and action would not be enough. At best, it might be a starting point for trying to cultivate the belief. Pascal suggested that the person should start by acting as if they believed, for instance by taking the holy water, attending mass etc. with the

[81] Blaise Pascal, *Pensées.* W.F. Trotter ed. London: Dent, 1910.

hope that these may eventually lead to genuine belief. Acceptance, however, cannot constitute a reason for belief because it would be the wrong kind of reason.[82]

Despite the conceptual differences between belief and acceptance, in practice they will be closely connected. One might wonder whether it would be psychologically possible for a person to accept something she does not believe. This may be easy in cases of uncertainty, where I may not believe that what I accept is true, but do not believe that it is false, either. It may be more difficult to accept something that directly contradicts my beliefs. In some cases, the only way to deliberate and act as if something is true may well be to find a way to believe it.

I have illustrated the key features of acceptance and clarified how it differs from belief. The examples in this section are not meant to be exhaustive of the concept's possible applications. The next two sections will illustrate further applications and demonstrate the concept's usefulness in contexts beyond individual decision-making. First, I will take up the distinct and important role of acceptance in the context of scientific research.

II. Scientific Acceptance

We typically care less about what scientists believe and more about what they accept. There are a couple of reasons for this. The first is that the concept of belief has been defined and developed mostly for the purposes of epistemology and does not travel well to the domain of science. The standard practice in epistemology is to identify believing p with believing p is true. Although this works for factual

[82] Andrew Reisner, "The Possibility of Pragmatic Reasons for Belief and the Wrong Kind of Reasons Problem," *Philosophical Studies*, 145, 2009, and Pamela Hieronymi, "The Wrong Kind of Reason," *Journal of Philosophy*, 102, 2005.

propositions such as “the sky is blue,” it does not work so well for scientific theories, which we rarely know to be true. In fact, the history of science suggests that most of the scientific theories we currently accept will turn out to be false. This may not make them any less useful or adequate for practical purposes, but it does pose a problem for belief in the usual sense. For those sensitive to the historical record, the rational attitude to take toward a theory we believe is to say, “I believe this theory, but I know it is most likely false.”

This may be an awkward attitude, but it does not preclude belief. Indeed, it is common for scientists and laypersons alike to believe current, well-supported theories, even while acknowledging that they may turn out to be false. The point is not that scientists do not or should not believe, but rather that acceptance is often the more accurate and informative concept for describing the appropriate attitude to take toward scientific theories. This is especially true of theories we already know to be false or partially false or false in some domains. The belief-acceptance distinction makes sense of the fact that there are nonetheless good reasons to accept and rely on these theories. Strictly speaking, it would be wrong to believe Newtonian mechanics, but it is perfectly sound to accept it in some domains, such as for the purposes of engineering. Acceptance may also be the appropriate attitude to take toward theories we do not (yet) know to be false, but that we already recognize as inconsistent or as making inaccurate simplifying assumptions and therefore do not believe.[83]

Another reason acceptance is more central to science than belief is that the professional goals of science require a commitment to shared evidentiary standards for acceptance, which may or may not match the individual scientist’s standards for

[83] Bohr’s theory of the atom was famously inconsistent, but still accepted. See Newton da Costa and Steven French, *Science and Partial Truth: A Unitary Approach to Models and Scientific Reasoning.* New York, NY: Oxford University Press, 2003.

belief. Scientists have to make a decision about whether to accept or reject the hypothesis being tested, but this decision cannot be made based on the evidence alone. What is required is the judgment that the evidence is sufficient, and what constitutes sufficient evidence is always relative to the context and the purpose: *Sufficient for what?*

In an alternative world, scientists could accept hypotheses based on whether they found the evidence personally persuasive. This would close the gap between belief and acceptance: sufficient for acceptance would mean sufficient for the scientist to believe. This is not the practice in science today because the social nature of the scientific enterprise and the dependence of scientists on each other's findings render it necessary to have shared standards for sufficiency. The exact threshold is determined by the particular goals of science. For instance, in cases where evidential support can be quantified and expressed probabilistically, it is common to look for 95 percent statistical significance before accepting scientific hypotheses.[84] The specific level varies with the field, but findings within a field are typically expected to conform to the same level.

The practice of looking for the relatively high standard of 95% statistical significance reflects an interest in avoiding mistakes. Since new scientific research builds upon earlier results, scientists have a strong preference for not accepting false results. Science has a long time horizon and can afford to wait a long time for positive results. At the same time, an even higher threshold would be counterproductive

[84] Focusing only on statistical significance is a reductionist way of representing the process of scientific acceptance, but I think doing so is helpful for explaining the argument. Acceptance within the scientific community depends not only on the statistical significance in one study, but also on the extent to which other researchers can replicate the findings, with the ultimate goal of a consensus. I am more interested here in the stages before there is consensus, and I use statistical significance as a simplified way of explaining what it would mean for evidentiary standards to vary. Of course, evidence is not always quantifiable or probabilistic, but the argument for adjusting evidentiary standards applies even where it is neither.

because of the need to accept a reasonable number of hypotheses in order to allow research to move forward, even if some of these results later turn out to be false. Of course, drawing the line at 95 percent, rather than at 94 or 96 is arbitrary, just as any other evidentiary threshold would inevitably be, but the decision to draw the line in that general region, rather than at, say, 80 percent or 99.9 percent, can be explained with reference to the goals of science and scientists.

The belief of individual scientists is separate from this professional process of acceptance, just as the belief of an individual juror is separate from her verdict as a juror. The individual scientist who believes the truth of a theory can be compared to the juror who is convinced that the defendant is guilty. Both may nonetheless conclude that the evidence does not meet the required standard for acceptance. Belief and acceptance come apart in these cases, though of course they are not fully independent. Scientists' belief in their theories can be very important in motivating research, especially when the belief precedes the accumulation of evidence sufficient to meet professional standards of acceptance. Both Planck and Einstein passionately believed the theory of special relativity, far before the evidence was sufficient to gain acceptance in the community.[85] William James anticipated this when he noted that individual scientists who passionately believe their theories and desire to get their beliefs confirmed can play a crucial role in the progress of science.[86]

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[85] Hilary Putnam, "A Reconsideration of Deweyan Democracy," *Southern California Law Review,* 63, 1989.

[86] William James, "The Will to Believe." In William James, *The Will to Believe and Other Essays in Popular Philosophy.* New York, NY: Dover Publications, 1956. James is often accused (or celebrated) for having argued for something similar to Pascal's wager, both about religious belief and about belief in general. While it is clear that James did not take the extreme view that people should believe whatever maximizes their utility, regardless of the evidence, he argued that the psychological satisfaction afforded from a proposition being true could justify believing one proposition over another, where the evidence does not settle the matter for that person.

The reverse can also happen: scientists can accept theories in order to explore their implications without believing them to be true. The meaning of acceptance here is different than the more technical use of scientific acceptance above. Here, the point is that scientists can treat their hypothesis *as if* true and carry out their research on that assumption in order to trace the implications of the theory and see where it might lead, even if they don't think it is the most likely or believable. In practice, this kind of exploratory acceptance may be more fruitful if accompanied by some degree of belief. After all, if scientists believe that a new avenue is unlikely to be true, they might find it difficult to be motivated to pursue it – being the devil's advocate may not be the preferred role for any individual scientist seeking success, even though the discipline might need benefit from some people playing the role.[87] Scientific progress may well require individuals who genuinely believe outré ideas and pursue them with dedication, as Feyerabend famously argued.[88]

III. Science and Politics

Decisions about what to do or how to act in the sphere of politics require a shared background of facts. In order to determine what is morally right or what best realizes a set of values and commitments, individuals participating in a decision need to reach an agreement about what is effective, what is possible and what consequences they can expect from different alternatives. Individual beliefs will inevitably diverge on these points, both among laypersons and experts. This

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[87] Peter Railton makes this point. See Railton, "Scientific Objectivity and the Aims of Belief." In *Believing and Accepting,* Pascal Engel ed. Dordrecht: Kluwer Academic Publishers, 2000.

[88] Feyerabend, *Against Method.*

disagreement over the facts must first be resolved before a group can reach a decision about how to act.

What is accepted in a context depends on the beliefs of the agents participating in the decision, but it cannot be reduced to a simple function of those beliefs, for instance as an aggregation of or agreement in individual beliefs since different factors play a role in determining belief and acceptance. The determination of the factual background of political decisions therefore requires something more than appeals to the evidence. I will describe four kinds of practical pressures that can influence decisions about the acceptance of scientific findings in the public sphere. The first three are analogous to the cases discussed in the first section in the context of an individual agent, while the fourth is unique to collective decision-making.

First, there are considerations about the stakes in a particular decision context. When we are deliberating about what to do collectively, what matters is not whether we would take certain bets on the truth of something, but rather whether we would be willing to make plans and take actions based on it. The situation is similar to that of the professor trying to determine if his colleague is a child molester, given that he has to decide whether to leave his child with the colleague. The facts that a group would be willing to accept as the basis of a policy decision in high stakes cases could be different than what most members of the group believe.

Recall the example from Chapter 1 about Cold War nuclear missile tests. Nuclear missile tests in the U.S. were conducted on an east-west trajectory. No missile had ever flown the north-south polar trajectory that would have to be flown in the case of an actual attack on the Soviet Union. Would the east-west tests provide sufficient evidence to accept that the north-south test would work and use this as an assumption as the basis of future political decisions?[89] Given the consequences of being wrong, it seems likely that the answer would be no, even though this is compatible with most people individually believing that the north-south tests would work, as many physicists did at the time. This is a case where the evidentiary levels for acceptance may be different than that of belief, based on the high cost of error.

Special commitments provide a second reason for varying evidentiary levels for acceptance. If my mother is in a coma and the doctor says that it is very likely that she will never wake up, I might not consider this sufficient certainty to accept the claim and base my actions on it, for example by allowing her to be removed from life support. The doctor, however, might think "very likely" is good enough and consider the matter settled. While the small chance that my mother may not be dead means a lot to me, the doctor is motivated by different practical considerations. There is nothing irrational in my refusing to accept this until there is a bit more evidence – even while the doctor's conclusion that she is dead is also justified.

Bratman gives the similar example of a friend who is charged with a terrible crime.[90] The evidence against her is strong, but my friend insists that she is innocent. In such a case, my friendship gives me a strong reason to accept my friend's word and assume she is innocent. At the very least, I might require more evidence before I accept her guilt. If there is smoking gun evidence, it might be difficult for me to carry on with the assumption that she is innocent, but until then, I can take her innocence for granted. At a societal level, we may take a similar approach about scientific findings that appear likely to disproportionately harm marginalized groups and reinforce existing inequalities. For instance, a democracy's moral commitment to

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[89] MacKenzie, "From Kwajalein to Armageddon."

[90] Bratman, "Practical Reasoning and Acceptance in a Context."

ensuring the equal standing of citizens and protecting the vulnerable can justify raising the standard of evidence required for accepting the results of research in sociobiology on IQ differences between races and genders, if it looks likely that accepting these results will have harmful consequences for certain groups of citizens. I pursue this thought further in Chapter 5.

Thirdly, acceptance of an uncertain theory may be the precondition for any action at all. A Kantian example will illustrate this point: I may not believe I have free will, but I must take for granted that I do to be able to engage in practical reasoning at all.[91] There might be analogous political situations where acceptance of an uncertain theory may be the precondition for any collective action at all, for instance in potentially catastrophic scenarios, such as nuclear destruction or climate change. There might be scientific evidence that shows that an apocalyptic scenario is quite likely, but our ability to engage in productive political action may rest nonetheless on our assuming the possibility of survival because apocalyptic scenarios would lead to fatalism and paralysis. I make this suggestion tentatively since others might argue the opposite: namely, that fear-based politics could be an effective spur to action on these types of issues.[92] The important point here is that this kind of *political* discussion is relevant and necessary in decisions about the kinds of scientific theories accepted for political purposes, alongside the evidence for rival theories.

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[91] Samuel Scheffler's argument that many of our projects and actions in life presuppose a belief that humanity will continue to exist after our death exhibits a similar logic, although he does not use the terminology of acceptance. See Scheffler, *Death and the Afterlife*, Niko Kolodny ed, New York, NY: Oxford University Press, 2013. Note that these examples are structurally similar also to Railton's demon; we can simply substitute political paralysis for the demon's "unspeakable punishment."

[92] A cautious version of this is Alison McQueen's position in McQueen, "Salutary Fear: Hans Morgenthau and the Politics of Existential Crisis," *American Political Thought,* 6 (1), 2017.

A final practical pressure on acceptance is unique to the case of collective decision-making: this is the necessity of overcoming disagreement and achieving cooperation. When individual beliefs about scientific facts diverge and there is no possibility of acquiring more evidence to persuade people of the truth of one view, the recognition that we need a determinate commitment can make it reasonable to simply agree to accept something. This does not mean the agreement should be arbitrary; the decision should be made as carefully as possible, taking into consideration all the available evidence and so on. The point is that the need for agreement can be a good reason to lower evidentiary standards and accept something, even though the accepted proposition may not meet most people's standards for belief.

As I hope has become clear by now, the broader conclusion I would like to draw from the theoretical groundwork laid out so far is that the appropriate level of evidence for scientific acceptance need not be appropriate for accepting scientific findings as the basis of political decisions. I have showed that acceptance depends on the context, the stakes and the purpose of the agent. Science and politics are activities with different stakes and different purposes. Although both have an interest in finding out the truth, which justifies giving priority to the evidence in determining what to accept, both have other goals, which exert different practical pressures on the level of evidence sufficient for acceptance.

The discovery of truth is the central goal of science, but science also has an interest in continued progress and the expansion of future research areas. If the only goal of science were to accept true hypotheses and true hypotheses alone, it could reach its goal simply by accepting fewer hypotheses. The need to ensure the continuation of scientific progress is a reason to lower evidentiary standards so that there are a reasonable number of accepted findings that other scientists can build upon, some of which will turn out to be mistakes. The scientific enterprise can (very schematically) be seen as aiming to strike a balance between accuracy and fruitfulness.

Politics, on the other hand, is not primarily aimed at the discovery of truth – certainly not of scientific truth – although truth is necessary for anchoring politics to reality and instrumental for attaining goals. Politics aims at other things: realizing the common good, achieving justice, ensuring peace and stability, bringing about good consequences, protecting the vulnerable, solving coordination problems among many others. Although both science and politics share an interest in the truth, their other aims diverge. These divergent aims exert different kinds of pressures on the standards of evidence appropriate in the two realms. If the stakes are too high and the consequences of accepting something are too costly for a society, it would be justifiable to require higher standards of evidence in politics than scientists do. Alternatively, if there is urgent need for action and the cost of waiting is too high, it would be justifiable accept something that scientists are reluctant to accept because their professional standards of evidence have not been met. In cases where the stakes are extremely high, a society may not be willing to accept *any* amount of evidence as sufficient, short of conclusive or irrefutable evidence. Most cases will not fall on this extreme point, and reasonable individuals will accept some level of evidence as sufficient.

This argument presupposes uncertainty about the truth. Where there is no uncertainty, the stakes should not change what we accept; we should accept (and believe) what we know to be true. For instance, if a village needs a bridge and a group of engineers know exactly how a bridge should be built, the stakes should not change whether the villagers accept this bridge-building knowledge. If, on the other hand, they need a nuclear reactor and the engineers are uncertain about the physics, the stakes should rightly determine what the villagers decide to do.

Uncertainty also plays a key role in cases such as the mother in a coma, the friend charged with a terrible crime and Pascal's wager, although its role in these cases is different. These are cases where the agent's primary aim is not to get things right; her aim is to avoid terrible consequences or fulfill special obligations. Doing so may or may not be compatible with believing something that seems supported by the evidence; the force of the examples is that it looks likely that it will not be compatible, though we do not know for sure. The plausibility of Pascal's wager rests on the assumption that we do not know whether God exists. If we knew that God did not exist, then there would be no point in the calculus. The role of uncertainty in the mother and friend cases are mostly psychological: if there were no doubt in my mind that my mother was dead or that my friend had committed a terrible crime, it may not be possible for me to deliberate and act as if these were not true.

The comparison with Pascal's wager raises the question of why citizens should not simply go all the way with Pascal and *believe* whatever serves their interests in the realm of politics, regardless of what scientists say about evidence. One way to answer this would be to appeal to the intrinsic value of believing the truth: we want to believe the truth because it is the truth, regardless of whether it serves any useful purpose, affords us psychological satisfaction or maximizes our utility. While this may be right, the intrinsic value of believing the truth could be weighed along with whatever other costs and benefits come from belief before determining what to believe. If the truth is so valuable for a person that it overwhelms all other considerations in every single case, then believing the truth and only the truth will be the right thing for that person to do. Other people can behave differently depending on the intrinsic value they attach to the truth compared to other goals they have.

A better response would appeal to the instrumental value of belief. Virtually everyone values true beliefs at least in part because they are much more likely to enable us to achieve other things we value in life than false beliefs or no beliefs at all. Even if believing a single falsehood might not be harmful in the short run, it is likely to lead to problems in the long run and make us worse off in ways that we may not be able to predict. Falsehoods will have ripple effects and result in contradictions, inconsistencies and further false beliefs. After a certain point, we will find it difficult to piece together a correct and coherent understanding of the world, form new true beliefs or make good predictions on matters that are important to us. We might thus eventually lose our anchoring in the world.

This does not provide an answer for why we should never believe what is more useful or convenient in every imaginable instance, but I'm not sure such a blanket argument is possible. The flip side of justifying principles instrumentally is that there will always be exceptional cases where the rule will clearly not apply. Especially in hypothetical scenarios in which believing a falsehood is absolutely certain to serve us better, both in the short term and in the long term, and not lead to any ripple effects, cause incoherence in our future beliefs and so on, I think it is difficult to say why we should never believe a falsehood.[93] Individual deviations from the rule are perfectly compatible with making it a general principle in life to believe p when p is true and avoid believing it when p is false. It is because of the all-things-told advantages of this principle over others in helping us achieve our goals in life and

[93] Railton's demon is the perfect example for this: He artificially stipulates the certainty of the terrible consequences (as hypotheticals often do). In real life, it is hard to imagine being absolutely certain that believing a falsehood will serve us better all things told than believing the truth.

obtain the things we value that I think we should work with this principle and reject more radical views that counsel some version of believing what is useful. This doesn't require adherence to the rule in every single instance, as long as our guiding epistemic principle is to evaluate propositions based on the evidence.

Acceptance avoids these dilemmas about ripple effects or the loss of coherence and consistency in our cognitive background because it is limited to a particular context and purpose, and the agent who accepts a proposition need not lose sight of this context-dependence. Her beliefs present a coherent background against which she decides what to accept in the particular circumstances of a practical decision.

It may be difficult to envision how individuals can bracket their beliefs for a particular practical purpose, but there are already important institutions that function on the assumption that this is possible: The jury trial is one example. Jurors today are expected not to bring their private beliefs about the defendant's guilt or innocence – beliefs based on media reports and their private or local knowledge – to the trial.[94] They are asked instead to be impartial and accept that the defendant is innocent until proven guilty. They deliberate based only on the information provided at the trial. This is an example of how belief and acceptance are expected to come apart for the purpose of a particular practice: What matters in the context of the trial is not what any individual juror believes, but what she and eleven others agree to accept. The verdict is a statement of acceptance, not of belief.

The challenge at the jury selection stage is to negotiate the possibility that jurors will in practice be able to bracket any beliefs they may have formed and accept

[94] This is a recent development. In the past, the local or private knowledge that jurors would bring to the trial was an integral part of the justification for jury trials. See Jeffrey Abramson, *We, The Jury: The Jury System and the Ideal of Democracy.* New York, NY: Basic Books, 1994 for the history of this shift.

innocence until guilt is proved. This can be difficult if they have been exposed to public and private information and reports before the trial. This creates precisely the dilemma mentioned above: Can a person who has a formed belief accept something that goes against it for a particular purpose? The answer implicit in the rules of jury selection is that it is possible, but not always. Many potential jurors are eliminated as hopelessly biased by the kind of information they have been exposed to. Others, who may have some degree of belief, are allowed to bracket their beliefs and serve on the jury.

IV. Who Decides on Evidentiary Standards?

The role of practical pressures on acceptance raises the question of who should decide on the appropriate evidentiary standards for the acceptance of scientific findings in politics. Some philosophers of science have used the incompleteness of scientific inference at the point of acceptance to argue that scientists should determine the appropriate evidentiary standards in light of the political stakes and context, rather than based on purely professional or scientific ones. This sort of direct relationship between science and politics may not be possible in many cases because of the difficulty of foreseeing the practical application of most scientific research, but where it is clear that the science is immediately relevant to policy decisions, for instance in cases such as the Manhattan Project or climate change science, scientists could use standards of acceptance with particular practical purposes in mind.

This might seem unrealistic today, given the insularity of academic scientists from the policy sphere, but in the 1940s and 50s, before science had acquired a high degree of autonomy from the political sphere, physicists such as C.W. Churchman and Richard Rudner defended an even stronger version of this argument: they argued that scientists had a moral responsibility to consider the practical purposes for which their findings would be used and adjust their evidentiary standards according to the stakes and the possible consequences. [95] Since statistical inference would always be incomplete, it should be completed with reference to social and ethical values.[96] Of course the political issue looming in the background of these arguments was the development of the atomic bomb.

Other scientists took Churchman and Rudner's suggestion to be a threat to science's newly developing aspiration to being value-free. They argued that there could be no place for ethical and social judgments in a science that aspired to objectivity.[97] At the same time, they had to concede that the judgment about sufficiency for acceptance inevitably depended on practical considerations. To resolve this dilemma, Isaac Levi argued that scientists should only allow practical considerations about the goals of the scientific community, leaving out broader social and ethical considerations.[98] He proposed that the decision about when to accept a hypothesis should be made based on epistemic values, such as fruitfulness for future research, simplicity and the need for new findings in the given field. These factors could be distilled to a more or less uniform standard within each field, obviating the

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[95] C. West Churchman, "Statistics, Pragmatics, Induction." *Philosophy of Science,* 15, 1948; Churchman, "Science and Decision Making," *Philosophy of Science,* 22, 1956; and Richard Rudner, "The Scientist Qua Scientist Makes Value Judgments," *Philosophy of Science,* 20, 1953.

[96] At the background of these debates was the bomb: Rudner was aware that his argument implied that no amount of evidence should make a scientist accept that an uncontrollable pervasive chain reaction would not occur when the first atom bomb was detonated.

[97] Richard Jeffrey, "Valuation and Acceptance of Scientific Hypotheses." *Philosophy of Science*, 22, 1956.

[98] Isaac Levi, "Must the Scientist Make Value Judgments?" *The Journal of Philosophy,* 57 (11), 1960.

need for subjective decisions by individual scientists and providing science the cover of having allowed only epistemic values in its determinations.[99]

This solution turned out to be very successful and shaped the way scientists handled the incompleteness of statistical inference in the following decades. It also facilitated the increasing autonomy and insularity of science from social concerns by removing a serious philosophical obstacle to its realization. Heather Douglas has recently criticized this turn in the relationship between science and society and has suggested bringing back the ideal of the socially responsible scientist.[100] She has revived the Churchman-Rudner line and argued that scientists should consider the consequences of mistakes in deciding how much evidence to consider sufficient to accept a claim.

But scientists making personal evaluations of the social, political and moral considerations for acceptance would be problematic for two reasons. First, it is valuable to preserve the distinction between scientific acceptance, guided by considerations relevant to the long term professional goals of science, and acceptance for political purposes, guided by practical considerations relevant to the society at a particular time. Just as scientific standards of evidence cannot be transposed unproblematically to the political realm, it would not be appropriate for social and political considerations to determine standards of evidence for organized science. The

[99] Kuhn's *Structure* played an important but complicated role in all of this. On the one hand, *Structure* was path breaking because it showed how social, historical and arbitrary features are essential to scientific revolutions. This inspired an entire field dedicated to criticizing the idea of value-free science. On the other hand, Kuhn's own insistence on a separation of epistemic from political and ethical values legitimized this strict separation of the two for decades, reinforcing the idea that only the former were acceptable in science. Kuhn spent a lot of time countering some of the radical implications others wanted to draw from his work. See Kuhn, *The Road Since Structure: Philosophical Essays 1970-1993.* Chicago, IL: University of Chicago Press, 2000.

[100] Douglas, *Science, Policy, and the Value-Free Ideal*; and Douglas, "The Moral Responsibilities of Scientists (Tensions Between Autonomy and Responsibility)," *American Philosophical Quarterly,* 40 (1), 2003.

underlying principle is that the point and purpose of a practice should determine the rules and standards that apply to it.

The second and more important problem with this proposal is a worry about representation. The kind of judgment required for the determination of evidentiary standards is a political judgment, not an epistemic one. It requires weighing the importance and urgency of the matter, balancing the different stakes for different people, and evaluating the consequences of acceptance. Scientists have neither a claim to superior knowledge on these kinds of matters, nor a claim to being authorized representatives of the public with a right to make decisions in its name. These are political matters that must be settled through the regular channels of democratic decision-making and representation. The judgment about evidentiary standards must therefore involve participation by ordinary citizens and their representatives.

Let me put the argument in a slightly different way: If practical considerations play a role in the determination of evidentiary standards for acceptance, the reasons and justifications relevant to debates about whether to accept scientific theories as the basis of policy will go beyond the evidence for different theories. They will involve deliberations about the stakes, the possible consequences, the urgency of action and competing moral commitments. Since these are political considerations by all accounts, ordinary citizens of a democracy have both the right and the competence to participate in these debates.

One possible objection is that this approach effectively puts scientific truth to a vote. This objection misunderstands the aim of acceptance for a purpose. The decision about what facts to accept for the purposes of politics is not a decision about what is true; it is a decision about what can be reliably assumed in order to attain a particular purpose, given that we do not know what the truth is. The consequences of different alternatives can justify varying the required levels of evidence. This does not mean that truth is relative to the context, only that the acceptance of a theory depends on something more than evidence for its truth. This is a core point about the use of science in democracy. Of course, it is possible that what is accepted in a context will also turn out to be true, but this may not always be the case. Political acceptance is not a method for discovering the truth – certainly not of scientific truth.

Another objection is that neither belief not acceptance matters for politics; what we care about is action. This objection assumes a strict separation between cognitive and practical reasoning that I have been arguing against in this chapter. Even if what we ultimately care about is an action or decision, reasoning about how to act always refers to a cognitive background. The set of options for actions available to us is constrained by what we believe or accept. The argument here gives a central role to decision or action, but questions the adequacy of referring only to context-independent beliefs in deliberation about how to act, or in explaining an agent's actions ex post.

Of course, much of the scientific knowledge relevant to the decision will be best understood by scientists, and will have to be taken on authority by laypeople. Political decisions about acceptance can be meaningful only if scientists clearly communicate the strength of the evidence and its degree of uncertainty.[101] Still, there is an important difference between taking the conclusion of scientists about what to accept as an exclusionary reason for accepting the same and deciding whether to accept the conclusion based on a combination of evidentiary and non-evidentiary

[101] Keohane et al. "The Ethics of Scientific Communication Under Uncertainty." *Politics, Philosophy & Economics,* 2014; Michael Lamb and Melissa Lane, "Aristotle on the Ethics of Communicating Climate Change." In *Climate Justice in a Non-Ideal World,* Clare Heyward and Dominic Rose eds. Oxford: Oxford University Press, 2016.

considerations of our own. While the former would affirm Hardwig's statement that "the rational layman will recognize that, in matters about which there is good reason to believe that there is expert opinion, he ought (methodologically) not make up his own mind,"[102] the second model defended here suggests that different interests and purposes, as well as considerations of moral psychology would justify laypeople making up their own mind, without meriting the charge of irrationality or rule by ignorance.

One of the fundamental points of this chapter is that the decision to accept a theory, whether for scientific or political purposes, is precisely that – a decision. Although this decision will be related to underlying individual beliefs and the evidence in support of them, it cannot be reduced to them. A final takeaway from this is that theorists of democracy should be wary of giving too central a role to individual beliefs in politics, and would do well to shift the focus to the concept of acceptance instead.

[102] John Hardwig, "Epistemic Dependence," *The Journal of Philosophy*, 82 (7), 1985.

Chapter 3: A Proposal for a Science Court with a Citizen Jury

The last two chapters have argued that expert claims must be examined democratically to ensure that policies based on expert advice incorporate values and reasons that are acceptable to the public. I listed four main goals for the democratic scrutiny of science: 1) exposing and evaluating the normative judgments behind expert claims; 2) judging the sufficiency of evidence for accepting uncertain scientific claims as the basis of policy; 3) incorporating local knowledge; and 4) legitimating expertise. In this chapter, I propose institutional arrangements for the public scrutiny of science designed to realize these goals.

The role of experts and expertise has been a weak spot for theories for democracy. Scholars have consistently argued for the need to bring experts and lay citizens together in deliberative venues such as consensus conferences, deliberative polls and planning cells, but they have avoided dealing with any of the challenges specific to expert-layperson interactions. This chapter also fills this gap by analyzing potential obstacles to expert-citizen interactions and developing institutional arrangements to overcome them.

My most significant departure from existing treatments of expert-citizen interactions in participatory settings lies in replacing a conventional division of labor, where experts inform citizens on the facts and citizens deliberate about their values, with an orientation toward the democratic scrutiny and contestation of expert claims themselves. Two serious challenges arise from this shift in emphasis: how to make it possible for non-experts to examine competing expert claims and how to overcome the difficulties of mutual deliberation under conditions of asymmetric knowledge and authority. To address these challenges, I propose a science court with adversarial proceedings where experts are brought in to make the case for opposing sides of a scientific question. A citizen jury then interrogates the experts and delivers a decision, which serves an advisory role in policymaking processes. The adversary structure of the proposal is designed to expose the background assumptions behind factual claims and to reveal their level of uncertainty. The separation of scientist-advocates from citizen-jurors avoids the difficulties of mutual deliberation under conditions of unequal authority, while allowing citizens to be active participants despite their lack of expertise. I also suggest that theories of democracy would benefit from paying more attention to the role of questioning as an empowering mode of communication that can facilitate participation in situations of asymmetric knowledge and power.

The chapter is organized in five sections. The first section discusses the challenges specific to lay-expert interactions, focusing especially on the difficulty of deliberation between those who have unequal knowledge. The second section recovers a historical proposal for a science court developed in the 1970s by the physicist Arthur Kantrowitz. The third section makes the case for reviving the science court, but with the significant departure of replacing the earlier model's scientist-judges with a citizen jury. The fourth section responds to two possible objections: that this institution puts scientific truth to a popular vote and that it overestimates citizen competence. The last section concludes with a discussion of how this proposal improves upon the treatment of expertise in recent scholarship on citizen juries in the democratic theory literature.

I. On the Perils of Deliberation Between Experts and Non-Experts

Theories of deliberative democracy have been criticized for setting highly idealized criteria of equality and reciprocity among participants as a precondition to deliberation.[103] Critics have pointed out that background inequalities make it extremely difficult for deliberation to be guided by "the unforced force of the better argument"[104] and have argued that the results of deliberation under conditions of inequality are likely to be shaped by existing differences in power among the participants. Structural inequalities affect who is seen as persuasive in deliberative settings, as well as determining a person's capacity – and willingness – to express herself in the reasoned, logical arguments that are the gold standard of deliberation.

In response to these charges, scholars of deliberative democracy have proposed institutional mechanisms for structuring deliberation in ways that could offset the known effects of background inequalities among participants.[105] They have also maintained against their critics that properly conducted deliberation could be instrumental for mitigating existing inequalities: discussion and argumentation could neutralize the effects of power by exposing it as resting on illegitimate reasons.[106]

Although these debates have claimed to address the effects of inequality broadly understood, they have typically focused on inequalities in wealth, class,

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[103] Lynn Sanders, "Against Deliberation," *Political Theory,* 25 (3), 1997; Jack Knight and James Johnson, "What Sort of Equality Does Deliberative Democracy Require?" In *Deliberative Democracy: Essays on Reason and Politics,* James Bohman and William Rehg eds. Cambridge, MA: MIT Press, 1997; Iris Marion Young, "Activist Challenges to Deliberative Democracy," *Political Theory,* 29 (5), 2001.

[104] The expression is from Jürgen Habermas, *Between Facts and Norms.* Trans. William Rehg. Cambridge, MA: MIT Press, 1996. For variations of this fundamental idea see Joshua Cohen, "Deliberation and Democratic Legitimacy." In *The Good Polity: Normative Analysis of the State,* Alan Hamlin and Philip Pettit eds. Oxford: Blackwell, 1989; Amy Gutmann and Dennis Thompson, *Why Deliberative Democracy?* Princeton, NJ: Princeton University Press, 2004.

[105] Archon Fung, *Empowered Participation: Reinventing Urban Democracy.* Princeton, NJ: Princeton University Press, 2009; Jane Mansbridge et al. "The Place of Self-Interest and the Role of Power in Deliberative Democracy," *The Journal of Political Philosophy,* 18 (1), 2010.

[106] Simone Chambers, "Rhetoric and the Public Sphere: Has Deliberative Democracy Abandoned Mass Democracy?" *Political Theory,* 37 (3), 2009; Samuel Bagg, "Can Deliberation Neutralise Power?" *European Journal of Political Theory,* 2015.

gender and race, and have paid little attention to the specific difficulties caused by inequalities in knowledge and expertise. The neglect in the literature is understandable since it is not clear at first whether asymmetry in knowledge among deliberators is a bad thing at all. It is not obvious whether this is similar to inequalities of wealth or gender in that it should be irrelevant to the outcome of deliberation, or whether it is more similar to, and perhaps correlated with, the quality of arguments – that is, precisely what the outcome of deliberation is meant to track.

To determine whether inequalities in knowledge pose a problem for the deliberative ideal, it is important to be clear about what kind of equality is required for deliberation. Dworkin's distinction between equality of impact and equality of influence is helpful for clarifying the conception of equality required in this context.[107] Dworkin defines equality of impact as the difference a person can make on his own, by voting for or choosing a decision. The principle of "one person, one vote" is emblematic of this conception. Equality of influence is the difference a person can make by inducing others to believe or vote as he does. Citizens who have equal impact may have vastly different levels of influence due to differences in wealth, charisma, reputation, skill, knowledge or intelligence. Equality of influence is the appropriate conception of equality for evaluating deliberative settings since they are primarily about persuasion and mutual justification, rather than a binding vote.

As Knight and Johnson have pointed out, deliberation cannot aspire for equality of influence over the outcome; that would defeat the purpose.[108] Deliberation aims to discriminate between competing ideas based on their quality and justifiability

[107] Ronald Dworkin, *Sovereign Virtue: The Theory and Practice of Equality.* Cambridge, MA: Harvard University Press, 2002, pp. 184-203.

[108] Knight and Johnson, "What Sort of Equality Does Deliberative Democracy Require?"

with the goal of producing better arguments and conclusions. Good and bad ideas cannot be treated alike, which means that some people will – and should – have more influence than others. The relevant conception of equality must therefore focus on procedure, rather than outcome. What we care about is equality of opportunity to influence the outcome. This requires an equal chance for participants to speak – what Habermas describes as "a symmetrical distribution of the opportunities for all possible participants to choose and perform speech acts."[109] It also requires that differences in influence over the final outcome of deliberation be insensitive to inequalities in factors such as resources, power, gender or race, which should be irrelevant from the perspective of the better argument. Inequalities in influence should be due purely to the differences in the quality of the reasons and arguments.[110]

At first, this definition may seem to affirm the view that inequalities in expertise are of the acceptable sort, and should be reflected in the outcome of deliberation, assuming that experts will be better at providing good reasons and arguments for their positions. But the requirement that participants must have *equal opportunity* to influence the outcome should give us pause. In deliberation over an issue requiring expertise, those who do not possess expertise will not have a meaningful opportunity to influence the outcome. By definition, they will not have the knowledge required to counter expert claims, and they will not be able discuss the merits of an expert's evidence using expert vocabulary. Even if they attempted to contribute to the discussion, it is highly unlikely that they would be able to out-argue

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[109] Jürgen Habermas, *On the Pragmatics of Social Interaction.* Trans. Barbara Fultner. Cambridge, MA: MIT Press, 2000.

[110] Note the parallel with the ideal of equality of opportunity in distributive justice, which states that access to desirable offices should be distributed on the basis of merit rather than on any of these other characteristics. See e.g. John Rawls, *A Theory of Justice,* Cambridge, MA: Belknap Press of Harvard University Press, 1999;
or Thomas Scanlon, "Equality of Opportunity: A Normative Anatomy," Annual Uehiro Lectures, Oxford University, 2013.

experts in their area of expertise. Although it is difficult to be precise about what equal opportunity would require – equal knowledge? equal capacity to formulate arguments on the subject? equal education levels? – laypeople will fall short vis-à-vis experts on any reasonable formulation of the concept.

The asymmetry in knowledge can also create an asymmetry in epistemic authority between experts and laypeople. This is different than saying experts will inevitably be more persuasive. There is nothing wrong with differences in the ability to change other people's beliefs by giving good reasons; that is the aim of deliberation. Authority differs from persuasion in being a content-independent and preemptive ability to change someone's beliefs, to put it in Raz's well-known terminology.[111] Content independence means that others believe a claim because the authority has said so, rather than because of the content of what they have said. Preemptiveness means that the authority's claims override a person's own deliberation on the matter. An expert possesses epistemic authority if non-experts take the advice of the expert to override their own conclusions, even where they could weigh the reasons for and against a claim.

This definition reveals why asymmetries in epistemic authority are problematic for deliberation. Deliberation requires that individuals weigh reasons and arguments, whereas epistemic authority preempts such weighing. Deference to the authority of experts short-circuits deliberation. This may not always be a problem. In certain contexts, the expert's role may simply be to educate non-experts so that they can go on to deliberate about other things on the basis of what they have learned. But I have argued that such educational interactions will generally not be enough in cases where expert advice must be used for policy purposes; non-experts must scrutinize

[111] Joseph Raz, *The Morality of Freedom.* Oxford: Clarendon Press, 1986.

expert claims before they are used in policy decisions to make sure that the values of experts are not driving policy.

Of course, experts may not always enjoy epistemic authority in the public sphere. Epistemic authority is as much about credibility and trust as it is about competence.[112] A person with superior knowledge may not be authoritative if people mistrust her, and a trusted fraud can be authoritative without possessing knowledge. If laypeople do not regard an expert to be credible or trustworthy, they will not defer to her claims; they will reject her as an authority. Those who do not want to accept an expert's claims, but cannot refute them can always take the route of denying the expert's authority. Since epistemic authority is almost always advisory rather than binding, others will have a choice about whether to accept or reject. But neither deference to nor rejection of authority is salutary from the perspective of deliberation since both of these involve content-independent reactions to knowledge claims. Reasons and mutual justification play no part in either case.

One way to resolve the problem posed for deliberation by differences in expertise and authority would be to demand that experts give reasons for their claims, rather than simply making appeals to their authority. This solution would not eliminate the fact of unequal authority, but it would provide a way for experts to minimize its effect by opening up their claims to challenge. It would also be an attitudinal signal that the experts treat non-experts as equal co-deliberators rather than as pupils to be educated. Knight and Johnson have recommended this solution, and the idea is in keeping with the spirit of many deliberative theories.[113] They conclude

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[112] Miranda Fricker, "Rational Authority and Social Power: Towards a Truly Social Epistemology," *Proceedings of the Aristotelian Society*, January 1998.

[113] Knight and Johnson, "What Sort of Equality Does Deliberative Democracy Require?"

that authority relations can be acceptable in deliberation as long as authority is subject to challenge.

But subjecting authority to challenge may prove more difficult. Although it would be good for deliberation if experts gave reasons for their claims – they should do this in any case – this might not be enough to meaningfully open their authority to challenge. For one thing, this solution leaves too much to the good intentions of the experts. But even if we assume good intentions on the part of experts, the effective difference between reason-giving and appeals to authority may not be much if the audience does not have the knowledge to challenge the speaker on the reasons given.

Of course there remains an important expressive difference between giving reasons for one's claims and asking to be taken on authority. The audience would appreciate the speaker's demonstrated willingness to give reasons and might interpret this as a sign of respect for their autonomy.[114] But these more symbolic effects are ancillary to the main purpose of good deliberation among equals, aimed at reaching mutually justifiable conclusions.

The difficulty of submitting expert claims to lay scrutiny has led some theorists to give up on the possibility of meaningful interaction between experts and laypeople on the substance of technical matters.[115] O'Neill has argued that experts giving reasons should be seen as a rhetorical device to signal credibility and trustworthiness. The evidence and arguments cited by the expert should not be interpreted as aiming to persuade the listener of the validity of a conclusion (since the listener could not evaluate this) but as a demonstration of the speaker's intention to share power with the audience.

[114] Allen, *Talking to Strangers.*

[115] O'Neill, "The Rhetoric of Deliberation."

The problem with this is that neither the demonstrated ability to make reasoned arguments, nor openness, trustworthiness or willingness to share power is a great tracker of correct scientific knowledge. Moreover, focusing on second-order assessments limits citizens' power vis-à-vis experts in deliberative settings. This is designed to enable experts to gain the trust of citizens, rather than helping citizens to deliberate about expert claims. It also raises the worry that experts might suppress information,[116] or present it selectively in order to make their case seem more persuasive than it is. They might do it for self-interested motives, which would be outright manipulation, or they might do it for what they believe to be the benefit of the audience, in which case we would call it paternalism. Experts might worry that providing full information may mislead laypeople to see the findings as less certain than they are, which might lead them to make bad decisions – that is, decisions that are bad for themselves. This problem has been studied particularly in the context of the physician-patient relationship.[117]

There is, of course, no neutral way of communicating facts. The selection and presentation of statistics, the particular ordering and emphasis of information, the omission or inclusion of details will always give more support for some conclusions and interpretations over other. This is how communication works; this is not a distinct feature of expert contexts. But what makes the problem more intractable in expert contexts is that non-experts may have no way of discovering the suppressed information or contesting the selective emphasis. In deliberation among epistemic equals, if one person suppresses reasons or presents the facts with a certain emphasis,

[116] Bernard Manin, "Democratic Deliberation: Why We Should Promote Debate Rather than Discussion," Paper delivered at the Program in Ethics and Public Affairs Seminar, Princeton University, Vol. 13, 2005.

[117] Lucille Ong et al. "Doctor-Patient Communication: A Review of the Literature," *Social Science & Medicine,* 40 (7), 1995.

there is a good chance that others who disagree will challenge her. In expert cases, only other experts may be in a position to bring up this kind of information.

There is no easy solution to the difficulties of deliberation between experts and laypeople under circumstances of unequal knowledge and authority. Nonetheless, I think we should at least think about institutional arrangements that would be better suited to realizing this goal, rather than ignoring the problem entirely or tackling the issue only in terms of second-order trust cultivation in deliberative contexts.[118] In the rest of this chapter, I will propose a new democratic institution for dealing with issues requiring scientific expertise and give reasons why it is likely to be more effective than a deliberative set-up in addressing the difficulties described above. I will do so by first describing a historical institution proposed for a similar purpose and then offering some modifications to revive it for present purposes.

II. The Science Court

In a series of articles written in the 1960s and1970s, the physicist Arthur Kantrowitz developed a proposal for a new institution for dealing with controversial scientific issues in policymaking.[119] It was designed to address the problem of expert disagreement on scientific issues that required political decisions, ranging from nuclear power and disturbances to the ozone layer to food additives and fluoridation. Kantrowitz lamented the state of public debate over scientific controversies. He complained that competing experts made many contradictory technical claims in the

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[118] For a defense of the latter, see Michael Fuerstein, "Epistemic Trust and Liberal Justification," *The Journal of Political Philosophy,* 21 (2), 2013.

[119] Arthur Kantrowitz, "Proposal for an Institution of Scientific Judgment," *Science*, 156 (3776), 1967; Kantrowitz, "The Test: Meeting the Challenge of New Technology," *Bulletin of the Atomic Scientists,* 25 (9), 1969; Kantrowitz, "Controlling Technology Democratically," *American Scientist,* 63 (5), 1975; Kantrowitz,"The Science Court Experiment." *Jurimetrics Journal,* 17 (4), 1977.

public sphere, and none of these got challenged or refuted directly. This left the public in an enormous confusion about the state of current scientific knowledge, weakened the scientific basis of public policy and heightened mistrust of experts.

His solution was to create an adversarial institution in which rival experts would defend their case and then cross-examine each other in front of a panel of impartial scientist-judges. The judges would then reach a verdict on the disputed scientific points and highlight points of agreement between the two sides. The proceedings would be open to the public and the decision would serve an advisory role for Congress and the President. Kantrowitz initially called his proposal an "Institution for Scientific Judgment," but the media coined the more pithy term "Science Court," which stuck.[120]

The proposal had three key features. First, it was supposed to separate the facts and the values involved in scientific controversies and evaluate only the facts. Kantrowitz complained about scientific advisory committees encroaching upon the moral and political aspects of scientific issues in their advice, which resulted in science becoming politicized and losing its claim to impartial authority. Good policy depended on hard facts and hard facts became obscured if scientists putting them forward took political positions based on their personal views.

The second key feature was the separation of advocate and judge and the use of adversary proceedings. Kantrowitz argued that scientists working in area inevitably became attached to certain theories or developed financial conflicts of interest, which made it unrealistic to expect them to give unbiased assessments of the merits of competing approaches. But scientists themselves were particularly well suited to the role of advocate because they understood the evidence best and had a natural

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[120] Andrew Jurs, "Science Court: Past Proposals, Current Considerations, and a Suggested Structure," *Virginia Journal of Law and Technology,* 15 (1), 2010.

commitment to defending their findings. For the same reason, they were best placed to challenge and examine each other.

The third rule was that the judge or judges had to be scientists, although they were not supposed to be specialists on the question being judged. Scientist-judges would have the competence to assess the claims on their scientific merits, without the potential biases of a scientist working the area. Kantrowitz also believed that their decisions would carry the presumptive legitimacy of expertise because of their perceived impartiality and competence. These three features together were meant to ensure the objectivity and accuracy of science advice to Congress and the President.

By 1975, the science court proposal had acquired great popularity and was also backed by the White House.[121] President Ford created a task force of academics and government officials within his Advisory Group on Anticipated Advances in Science and Technology to explore the feasibility of the proposal.[122] Kantrowitz was appointed chairman. The task force decided to run a series of preliminary experiments to test the science court to better understand its benefits and drawbacks.[123] Two hundred and fifty scientists and legal scholars participated in a public debate organized to discuss the proposal, and despite some criticism, many of the points in the proposal met with approval.[124] Twenty-eight prominent scientific organizations,

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[121] Jurs, "Science Court."

[122] Ibid.

[123] Phillip Boffey, "Experiment Planned to Test Feasibility of a 'Science Court,'" *Science,* 193, 1976; John N. Wilford, "Science Considers Its Own 'Court'" *The New York Times*, Feb. 29, 1976.

[124] Philip Boffey, "Science Court: High Officials Back Test of Controversial Concept." *Science,* 194 (4261), 1976; Wil Lepkowski, "USA: Science Court on Guard," *Nature,* 263, 1976.

including the American Association for the Advancement of Science, offered their support.[125]

An opportunity for testing the science court came from Minnesota in 1976.[126] There was a controversy over the construction of a high-voltage power line that would cut across 172 miles of farmland. The farmers were deeply upset, not only because their lands would be appropriated but also because they believed that the selected path would be particularly harmful for irrigation patterns and other farming practices. They also had concerns about health and safety issues and the potential environmental damage that would be caused by the power line. The utility companies, however, denied that the farmers' claims had any scientific basis. The Governor of Minnesota stepped in and proposed to resolve the scientific aspects of this dispute in a science court. He took on the responsibility of organizing it and tried to persuade the farmers and the utility companies to participate.

But the farmers refused to participate in a science court under the rules proposed by the governor, which were basically the rules of the Kantrowitz proposal. They saw the court's separation of the factual and political aspects of the problem as a cover for delegating an essentially political decision-making power to technical experts. They proposed instead a modified court where the scientific and political parts would be argued together. Of course, it the facts and values were to be addressed together, it no longer made sense for the judges to be scientists, so the farmers asked the governor himself to act as judge. Their two other demands were for funding to

[125] John N. Wilford, "Leaders Endorse Science Court Test," *The New York Times,* Jan. 2, 1977.

[126] The account of this experiment is from Barry Casper and Paul Wellstone, "The Science Court on Trial in Minnesota," *Hastings Center Report,* 1978. Also see Arthur Kantrowitz, "In Defense of the Science Court," *The Hastings Center Report,* 1978 for Kantrowitz's reply to Casper and Wellstone.

develop their case and to bring in their own experts, and for the hearings to be directed at the public.

In the end, the governor rejected the farmers' demands, and the science court never took place. All of this took place during a presidential election year. Although both candidates, Ford and Carter, had publicly endorsed the science court in their campaigns – thus proving and increasing the popularity of the idea – Carter completely abandoned the project when he was elected president. The idea silently disappeared. In the next section I will make a case for reviving the science court, but in a form closer to the one proposed by the farmers.

III. A Proposal for a New Science Court

The main weakness of the Kantrowitz proposal was its naïve assumption that the facts of a scientific dispute could be separated from the values involved. Some critics attacked it on this point when it was first proposed, but they either focused on the difficulty of drawing a line between the science and its practical applications, or they argued that most public controversies arose from value disagreements about what to do in light of the science, rather than the facts themselves.[127] The latter rang particularly true in the 1970s since the most prominent scientific controversy of the day – nuclear power – was less about the science than about moral and political disagreements over its use.

Although these early criticisms remain valid, I have shown in the last two chapters that facts and values are intertwined in scientific claims in even more radical and thoroughgoing ways. I pointed to the role of background assumptions and values

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[127] Dorothy Nelkin, "Thoughts on the Proposed Science Court," *Newsletter on Science, Technology, and Human Values,*1977; Barry Casper, "Technology Policy and Democracy," *Science*, 194 (4260), 1976; Allan Mazur, "Science Courts," *Minerva,* 15 (1), 1977.

in the judgment that the evidence supports a theory, in theory choice, in experimental design and in the determination of evidentiary standards for acceptance. These show that the attempt to separate facts and values is difficult not only because of a fuzzy boundary between science and its applications, as the early critics maintained, but more importantly because science itself can incorporate questionable assumptions and values.

Kantrowitz's hope for an objective and uncontroversial resolution to questions of fact therefore appears unrealistic. This does not render the institution useless, but it shows that its key feature of separating facts and values is misguided. This, in turn, makes it difficult to justify scientist-judges. As Kantrowitz pointed out, scientist-judges would be desirable because of scientists' superior ability to assess the scientific evidence presented, and the presumptive authority that this competence would lend to the decisions.[128] If the matter were a purely scientific one, these would be the main considerations. But given the difficulty of separating the facts and values, the authority of the judge cannot be justified purely on the grounds of competence. The inseparability of facts and values suggests that a science court should be treated as a political institution, which in turn means that the authority of the judge must be justified on grounds of democratic, rather than scientific authority. An institution for the democratic use of science in policy cannot have experts decide on the facts and the facts alone.

Although I depart from Kantrowitz by rejecting the possibility of separating the facts and values for the purposes of a science court, I share his broader goal of bringing science and technology under democratic control and removing expert values from political decisions on science. I therefore think a properly modified science

[128] Kantrowitz, "Controlling Technology Democratically."

court, designed to overcome the difficulties of expert-layperson deliberation laid out in the first section, would be a valuable institutional innovation for democracies today.

My proposal is for a new science court, which would address a scientific policy question in the form in which it would face policy-makers. The court could be initiated either by citizens or by elected officials. Its decisions would advise ordinary policy-making processes, but the court would not possess the power to enact policies without legislative approval. The proceedings would involve competing experts making the case for different sides of a scientific question, followed by a jury interrogating the experts about the evidence and then delivering a decision. The jury would be made up of ordinary citizens selected through random sampling from the relevant national or local national jurisdiction, depending on the question. The facts and the values involved in the issue would be addressed together.

I have already explained why facts and the values must be addressed together. Let me now say more about each of the other features in turn.

A. Adversarial Proceedings

Adversary hearings are designed to examine competing claims over an issue and to reveal the weaknesses of each through confrontation with opposing views and arguments. The aim is to expose questionable assumptions and to identify the errors of all sides. The underlying principle is that the validity of truth claims can best be tested if they are tried out against the strongest arguments that can be made in opposition.

Of course, ordinary discussion can also bring opposing views into conflict and produce the same results. Mill held this up as one of the chief benefits of free discussion in *On Liberty.* He praised diversity of opinion precisely because he thought it would lead to the kind of critical exchange that he believed was crucial for the discovery of truth and the examination of the grounds of widely held beliefs. However, it cannot be assumed that freedom of discussion alone would be sufficient to ensure that opposing views would emerge and that a critical examination of opinions would take place. Nor could the creation of deliberative bodies ensure this. Critical exchange is not precluded in deliberation, but it is not guaranteed.

The crucial difference between adversarial and non-adversarial arrangements is that the former forces a confrontation between opposing views by design, while the latter does not. Adversarial institutions are therefore desirable in cases where there is reason to believe that opposing views may not be found or defended naturally, and also where it is crucial for them to be heard.[129] The practice of providing defense lawyers even to criminals who are caught red-handed, or the Catholic Church's practice of appointing a devil's advocate at the canonization of saints are both examples of cases where these two conditions make adversary procedures particularly appropriate.[130]

The two conditions – the importance of scrutinizing claims and the difficulty of it happening naturally – are also met in expert-lay person interactions. Expert claims are best tested against opposing expert claims. Laypeople would have difficulty challenging experts or examining the grounds of their knowledge claims without rival experts arguing for alternative scientific views. At the same time, it would be best for experts to be responsible for presenting their own findings and

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[129] Manin, "Democratic Deliberation."

[130] Manin rightly points out that Mill ignored the possibility that opposing views may not always spontaneously arise and may need deliberate encouragement. This point is especially true in the context of modern science, as I argue in the next chapter.

mounting challenges to opponents. Not only are experts themselves most qualified for the task because of their knowledge and experience on the topic, but they also have a natural incentive for defending their case in the strongest possible way– more so than defense lawyers or devil's advocates appointed to do defend positions often against their convictions.

The same factors, however, make it unlikely that experts will be the best judges. Even if we bracket personal conflicts of interest and assume that scientists are motivated solely by the pursuit of the truth, their commitment to and deep familiarity with their own research will inevitably make them biased in favor of it. This bias may also extend beyond the findings to the endorsements of practical implications and policy preferences. The advantage of adversary institutions is that they do not require the objectivity of participants; they are structured precisely on the assumption that no one can be expected to be so. What follows from this is the fairly intuitive thought that advocates should not be judges in their own case.

B. Citizen Jury

My proposal's most significant departure from Kantrowitz's science court is its replacement of scientist-judges with a lay jury. I defend this primarily on democratic grounds; the main point, to put it in Tocqueville's words, is that a jury in this context should be considered primarily a *political* institution, rather than a scientific (or judicial) one.[131] Arguments in favor of this proposal will be analogous to arguments for jury trials. I will provide the three main justifications for jury trials and show how each of these can be transposed to the context of a science court.

[131] Alexis de Tocqueville, *Democracy in America.* Trans. and ed. Harvey Mansfield and Delba Winthrop. Chicago, IL: University of Chicago Press, 2000, p. 258.

The first and most important justification for juries is that they are an expression of popular sovereignty – of the people's control over their agents in government.[132] Juries allow citizens to check the power of the state, of judges and (in civil trials) of corporations. While voting and elections empower citizens every few years, serving on a jury gives ordinary citizens direct decision-making power over an area of government on a daily basis. Their power is consequential and immediate. Along with voting, serving on a jury is one of the rare acts of direct self-government in modern representatives democracies.

The use of citizen juries in science courts would be justified on the same basis. Ordinary citizens would have the opportunity to check the claims of experts and participate in the shaping of policy on scientific issues. It might seem counterintuitive to require participation by ordinary citizens on issues that are the most difficult for them to understand, but it is precisely the difficulty citizens would ordinarily have in monitoring policy on technical issues that makes it desirable to create institutional settings where they might have a better chance of doing so. The complexity of the modern state has resulted in the removal of entire policy areas from the meaningful scrutiny of the democratic public on the pretext that the issues are technical or complex. Using citizen juries in science courts would be a step toward mitigating this problem. Justifications of jury trials in the law have historically followed the same logic. Bringing ordinary citizens into the esoteric domain of law was seen to be necessary to ensuring democratic scrutiny in a crucial but technical branch of government that would otherwise be left to the power of experts such as judges,

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[132] Akhil Amar, "Reinventing Juries: Ten Suggested Reforms," *U.C. Davis Law Review,* 28 (4), 1995; Amar, "The Central Meaning of Republican Government: Popular Sovereignty, Majority Rule and the Denominator Problem," *University of Colorado Law Review,* 65, 1993.

lawyers and prosecutors. That the law seems relatively more accessible today than science should not obscure this important fact.

A second justification for jury trials is that juries inject local knowledge and community values into legal procedures.[133] Scholars have pointed out that the meaning and content of this has changed dramatically over time.[134] Medieval juries were meant to have familiarity with the events and people of the case and to bring personal knowledge of the character of the defendant, the setting of the crime or the events that took place. The medieval juror was a peer and neighbor, effectively fulfilling the role of a modern day witness. Over the centuries, the expectation from jurors became quite the opposite: they were selected from among citizens with little knowledge of or opinion about the case to ensure that they would be impartial and make their judgments solely on the basis of evidence presented at the trial.[135] Jurors are now expected to bring "community norms and values" to the decision, rather than actual knowledge of the events or people. Community norms and values typically refer to interpretations about what counts as reasonable, credible, negligent or obscene, as well as beliefs and standards about values such as fairness, equity, justice or desert.

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[133] Abramson, *We, The Jury.*

[134] Abramson, *We, The Jury;* Thomas Andrew Green, *Verdict According to Conscience: Perspectives on the English Criminal Jury, 1200-188.* Chicago, IL: University of Chicago Press, 1985*;* Ellen Sward, "Values, Ideology, and the Evolution of the Adversary System*," Indiana Law Journal,* 64, 1999.

[135] Abramson argues that this understanding of impartiality has been problematic. As Mark Twain cynically pointed out: "We have a criminal jury system which is superior to any in the world; and its efficiency is only marred by the difficulty of finding twelve men every day who don't know anything and can't read." (Twain, "After-Dinner Speech: Meeting of Americans, London, July 4, 1873." In *Mark Twain Speaking*, Paul Fatout ed., Iowa City, IA: University of Iowa Press, 2006.)

This justification of jury trials can also be transferred to the science court. Both the medieval and the modern ideals would be applicable in the science court context; they would not be mutually exclusive. A citizen jury would bring both local knowledge and community norms and values to issues requiring scientific expertise. Local knowledge would be particularly relevant on area-specific or group-specific issues, for instance in environmental disputes, or medical research on specific patient populations. In these cases, citizens might possess experiential or observational knowledge that experts would have difficulty acquiring.

The argument for community values follows a different logic. The point is not that citizens would be expected to bring pre-defined community values that they would somehow be expected to *know* better than experts. They would rather be asked to examine the values and purposes driving scientific claims in light of their personal values, perspectives, interests and purposes. What would make citizen-jurors preferable to a panel of experts is that the former would be representative of public opinion in a descriptive sense because they would be selected from the population through random sampling. Ordinary citizens would bring different purposes and priorities than scientist-judges, who would be more likely to share the professional concerns of the testifying experts. Moreover, the jurors might show greater diversity in background and opinion, whereas experts tend to be a relatively homogenous group. These differences would be important in the evaluation of the purposes and assumptions driving scientific claims, as well as for the decision about whether the evidence is sufficient to accept a scientific claim as the basis of policy. The latter judgment is directly parallel to the legal decision about whether the evidence meets the burden of proof required in a particular kind of trial.

A third justification for the jury system is that it gives citizens the opportunity to understand the workings of the legal system and increases their assurance that it is functioning properly (if indeed it is). Firsthand experience with the system is more effective than secondhand accounts in allowing citizens to appreciate how and how well the system works. Making the system intelligible to citizens is a crucial step for allowing them to feel that the forces that determine their fate are not beyond their control. Jury trials ensure that the application of the law goes through the comprehension of ordinary citizens: If ordinary citizens cannot understand why an act is a crime, the defendant cannot be ruled guilty in a jury trial.

This point is crucial in the context of scientific issues – far more important than in the case of political disagreements driven by value conflicts. Intelligibility for citizens takes on greater importance in policy areas that are more opaque. The complexity of science and technology makes it particularly difficult for citizens to understand and control policy on these issues, which can lead to the feeling of alienation that is one of the central concerns about technocracy. An institution that allows citizens to get directly involved on issues requiring expertise, and to listen to firsthand accounts from the experts would serve an important demystifying purpose. This would make citizens feel less removed and more powerful in technical areas of policy.

C. Juror Questioning of Experts

Another key feature of my proposal is the questioning of expert witnesses by the jurors. There is currently some debate in the literature on jury trials over the advantages and disadvantages of allowing jurors to ask question during trials.[136] Proponents of the idea argue that jurors would become more engaged with the proceedings and would understand the arguments better if they were allowed to ask questions. The main argument against juror questions in the trial context is that jurors might ask questions that go against rules of evidence. This could bias members of the jury and jeopardize the requirement of impartiality. For instance, if a juror asks whether the defendant has a criminal record, the defense lawyer might want to object to the question for fear that an affirmative answer might prejudice the jury, while the jury might interpret the objection as an affirmative or be displeased by the refusal to answer. Jurors might also take over the role of advocate if they begin asking pointed questions, which would compromise their neutrality and interfere with the counsels' presentation of their case. Although juror questioning is left to the discretion of courts in almost all states, worries over jurors' unfamiliarity with trial rules have meant that most juries sit silently as the two sides examine each other.[137]

There are important differences between the legal setting and a science court oriented toward policy, however, and the relevant considerations for allowing juror questions are different in the two contexts. The most important difference concerns the role of impartiality. Justice requires impartial judges, who stand at an equal distance to both sides and judge solely based on the evidence. This has been interpreted in modern jury trials as requiring that jurors come to the trial with no knowledge or opinion on the case they are about to judge since familiarity with the events and people might give rise to prejudices and passions that could distort the

[136] Kristen DeBarba, "Maintaining the Adversarial System: The Practice of Allowing Jurors to Question Witnesses During Trial," *Vanderbilt Law Review,* 55, 2002; Eugene Lucci, "The Case for Allowing Jurors to Submit Written Questions," *Judicature*, 89, 2005-2006.

[137] Lucci, "The Case for Allowing Jurors to Submit Written Questions."

jurors' assessment of the evidence. Abramson argues that this interpretation of the requirement of impartiality exposes a tension between the justification of the jury as an instrument of justice, removed from the pressures of popular opinion, and its justification as a democratic institution that instills the values and commitments of the public into the justice system.

How this tension could be resolved within the justice system and whether the empty-minded-juror model is a reasonable interpretation of the requirement of impartiality are beyond the scope of this chapter. What matters here is that the strict constraints of impartiality could be relaxed in a science court. Since the science court is a political institution rather than a scientific one, it does not require jurors to bracket their values, perspectives and knowledge in order to participate. Citizen jurors are desirable precisely because they bring their values and knowledge to the process of decision-making over science. The court's goal of enabling citizens to scrutinize the claims of scientists as well as the grounds of their own opinions does not require starting the process from a blank state. To the contrary, the court is designed with the assumption that the confrontation between the partial and contradictory perspectives of competing experts and citizens could be instrumental in leading participants collectively to better views.[138]

Relaxing the requirement of impartiality would permit jurors to participate more actively in the courtroom drama instead of being a passive audience to a duel between experts. Jurors would have the opportunity to challenge the claims of experts in order to expose background assumptions and values, probe the implications of the findings, discover the priorities driving the research and determine the strength and certainty of the evidence presented. The questioning period would be essential for

[138] See Iris Marion Young's discussion of the ideal of impartiality in Young, *Justice and the Politics of Difference.* Princeton, NJ: Princeton University Press, 1990.

allowing citizens to contribute to the process by directly engaging with the content of expert claims.

Deliberative democrats have not focused enough on the role of questions in deliberation. They have prioritized reason-giving, justification and the presentation of evidence as the main speech acts driving rational discussion aimed at reaching mutually acceptable decisions. As critics have pointed out, this privileging of reasoned argumentation can be problematic if some people do not have the skills to make good arguments, and the distribution of these skills tracks objectionable background inequalities.[139] This problem is exacerbated on issues requiring expertise since laypeople by definition will not have the knowledge and evidence to be able to construct their own scientific arguments. This asymmetry makes it difficult for interactions between experts and laypersons to resemble a conversation among equals, which is held up as the ideal for deliberative democracy.

Shifting the focus from argumentation to question-and-answer could mitigate this problem. Questioning has several advantages for enabling citizen participation on issues requiring expertise. The person asking questions need not possess much background knowledge on the topic to ask good questions and can bring out the knowledge possessed by others by selecting the right questions. Once acquired, this skill can be carried over to many different contexts. Moreover, the questioner has the power to suggest or assert a particular interpretation of the facts through her choice of questions. Criminal trials revolve around the progressive unfolding of two competing narratives of the crime, and the attorney's selection of questions is crucial for the construction of a compelling and coherent story around the facts provided by witnesses. Although citizen participants in a science court need not have constructed

[139] See note 103 above.

narratives in advance, they would have the opportunity to develop and articulate their own interpretations in the questioning period.

There is already some research on the democratic potential of citizen questioning. In a study exploring why parents in low-income communities did not participate in decisions about public education, Rothstein and Santana found that the answer parents gave most often was that they did not know what questions to ask the authorities.[140] The problem persisted even after the researchers provided the parents with a list of issues to ask about; what the parents needed was not a list of issues, but the skills for developing and asking their own questions. Once the researchers focused on helping parents acquire these skills, parents were able to participate more effectively and also said they felt more empowered. The authors claim that it is easier to make people acquire good questioning skills than to teach them about the intricacies of the public education system. This suggests that quick strategies for helping citizens develop questioning skills may be a more important contribution to participatory democracy than having experts instruct them on the science. Focusing solely on instruction is unlikely to dissolve the comparative advantage of experts in deliberation, whereas attention to questioning skills could empower non-experts to interact as equals with experts, while actively seeking out the information they need.

This is partly because the question-answer format allows a meaningful debate over the substance of claims despite the asymmetry of knowledge between participants. Equally important, however, is the fact that such an arrangement artificially reverses the authority relationship by putting those who possess less knowledge in the more powerful position of asking questions. The power to ask questions gives the questioner the chance to control the direction of the conversation,

[140] Dan Rothstein and Luz Santana, *Make Just One Change: Teach Students to Ask Their Own Questions.* Cambridge, MA: Harvard Education Press, 2011.

and imposes limits on what the person being interrogated can talk about. The dominant role of the questioner is evident in the highly structured conversations between police interrogators and suspects, interviewers and interviewees or therapists and patients. In each of these examples, the party who knows less nonetheless has greater control over the interaction. Setting up expert-layperson interactions within a science court on this model would counterbalance the asymmetry in knowledge, and neutralize a potential source of inequality in influence over the outcome.

The last chapter highlighted the points at which values and assumptions are likely to play a role in scientific research. Juror question could be targeted toward these points and jurors could be instructed to devote special attention to these areas. Below is a rough categorization of the different types of questions that jurors could ask on a scientific issue during the questioning period.

1) Questions for clarification
2) Questions probing underlying assumptions
3) Questions about the values or purposes that drive the choice of theories or models
4) Questions about the strength of the evidence and the uncertainty of findings
5) Questions about alternative perspectives and opposing views
6) Questions about the implications of findings

A science court that gives jurors broad questioning powers would closely resemble another historic institution that has almost completely disappeared today: the coroner's jury. Coroner's juries were convened to conduct investigations into the cause of accidental, suspicious or violent deaths.[141] They typically involved an examination of the body by the coroner and a jury of ordinary citizens, followed by testimony from the finder of the body, other witnesses and medical experts. The distinguishing feature of the coroner's jury was that jurors were free to interject with questions to the witnesses and were encouraged to take an active role in the

[141] Ian Burney, *Bodies of Evidence: Medicine and the Politics of the English Inquisition 1830 – 1926.* Baltimore: Johns Hopkins University Press, 2000.

proceedings. When they were satisfied with their examination, they made a public declaration of the cause of death. They were also allowed to add riders to the verdict, expressing praise or blame, making recommendations for reform and even commenting on the operation of the inquest itself.[142]

Aside from juror questioning of witnesses, what made this institution particularly interesting for the relationship between science and politics was its precarious but well-maintained balance of power between medical experts and citizen jurors. Burney's historical study of the evolution of the institution in nineteenth- and early twentieth-century England reveals how the coroner's juries became sites of contestation between the rapidly increasing scientific authority of medical experts and citizens' political demands for information and participation.[143] Burney challenges the standard reformist narrative that saw the evolution of the institution in terms of the rise of the scientific expert at the cost of the gradual displacement of popular judgment. He argues instead that juries preserved their control over and interest in the inquest despite the injection of increasingly sophisticated and professionalized expertise into the process. The coroner's jury paradoxically succeeded in legitimating the status and influence of experts, while simultaneously remaining a "people's court" where citizens retained significant power to check the potential abuses of authority by legal and medical experts. This makes it a particularly relevant model for a new science court.

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[142] Burney, *Bodies of Evidence,* p. 5.

[143] Ibid., p. 3.

D. Admissibility of Expert Evidence

Relaxing the requirement of impartiality may allow more flexibility in court proceedings, but constraints will nonetheless be necessary to ensure that the trial stays on topic. Both expert arguments and juror questions must be relevant to the purpose of determining the truth and settling the policy question under consideration. Federal rules of evidence offer the following test for judging the relevance of a piece of evidence: “Evidence is relevant if it has any tendency to make a fact more or less probable than it would be without the evidence; and the fact is of consequence.” [144] Moreover, “the court may exclude relevant evidence if its probative value is substantially outweighed by a danger of one or more of the following: unfair prejudice, confusing the issues, misleading the jury, undue delay, wasting time, or needlessly presenting cumulative evidence.”[145] Except for the prejudice restriction, both of these rules are highly relevant for the science court as well. The institution would therefore benefit from a moderator responsible for judging the relevance of expert evidence and juror questions and making sure the court does not veer off course.

The application of legal rules of evidence in a science court also raises a question about the determination of admissibility standards for expert testimony. Should all purportedly scientific views be accepted in a science court, or should there be a filtering process to determine which views should be admitted? The standard for admissibility in the legal context changed dramatically after the Supreme Court’s decision in *Daubert v. Merrell Dow Pharmaceuticals, Inc.*[146] Previously, judges were

[144] Federal Rules of Evidence 401.

[145] Federal Rules of Evidence, 403.

[146] *Daubert v. Merrell Dow Pharmaceuticals, Inc.,* 509 U.S. 579, 1993.

asked to determine admissibility based on whether the scientific principles underlying the evidence were widely accepted in the scientific community.[147] The *Daubert* ruling gave judges broader gatekeeping powers by asking them to engage with the content of expert evidence. The decision specified four different dimensions on which judges could assess scientific views to determine its admissibility: 1) acceptance in the scientific community (the old standard) 2) peer review and publication 3) testability 4) rate of error. Legal scholars have been debating whether the *Daubert* standard serves the purposes of the law, whether the philosophy of science presupposed by the standard is coherent and whether judges have the competence to evaluate expert evidence.[148] My goal here is not to contribute to these debates, but to argue that applying a *Daubert*-like standard to filter the expert views to be admitted in a science court would undermine the purpose of the institution.

The underlying purpose of the *Daubert* standard is to shield jurors from pseudoscience and direct them toward evidence that is more likely to help them arrive at the truth on disputed matters of fact.[149] But the determination of what counts as pseudoscience is often one of the contested issues in a trial. The decision about whether to delegate scientific gatekeeping powers ahead of the trial is therefore a

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[147] *Frye v. United States*, 293 F. 1013 (D.C. Cir. 1923).

[148] Sheila Jasanoff, "Law's Knowledge: Science for Justice in Legal Settings," *American Journal of Public Health,* 95 (S1), 2005; David Caudill and Richard Redding, "Junk Philosophy of Science? The Paradox of Expertise and Interdisciplinarity in Federal Courts," *Washington and Lee Law Review,* 57 (3), 2000; Susan Haack, "Federal Philosophy of Science: A Deconstruction – And a Reconstruction," *NYU Journal of Law & Liberty*, 5, 2010; Susan Haack, "An Epistemologist in the Bramble-Bush: At the Supreme Court with Mr. Joiner," *Journal of Health Politics, Policy and Law,* 26 (2), 2001; Richard Friedman, "Squeezing Daubert Out of the Picture," *Seton Hall Law Rev*iew 33 (4), 2003; Frederick Schauer, "Can Bad Science Be Good Evidence? Neuroscience, Lie Detection, and Beyond," *Cornell Law Review,* 95, 2010; "Admitting Doubt: A New Standard for Scientific Evidence," *Harvard Law Review*, 123 (8), 2010.

[149] Neil Vidmar and Shari S. Diamond, "Juries and Expert Evidence," *Brooklyn Law Review,* 66 (4), 2001.

decision about who should have the authority to determine what counts as bad science in this context. To suggest that this should be anyone but the jury is to undermine the justification of the institution, which is precisely to give ordinary citizens the authority to examine available views as thoroughly as possible before determining which would be acceptable as the basis of a decision, whether political or judicial. A prior review for admissibility would take away a significant amount of the power of the jury and hand it to the person responsible for doing the shielding. In the legal context, scholars have justified this practice through empirically unsupported claims about the judge's superior ability to tell good science from bad,[150] but this undermines the justification of entrusting a jury with the decision on a technical matter in the first place.

Moreover, the practice would also be problematic on epistemic grounds. One of the arguments for a science court is that adversary proceedings are an effective way of examining alternative views and exposing the weaknesses of each. The adversary process is particularly well suited to exposing the defects of different views. It is part of the logic of this institutional arrangement that false views should also be admitted to the conversation. This ensures that the better, more scientific views are publicly tested against the strongest criticism that could be made against them, and also that the jury and the audience have a chance to examine and understand the grounds of good and bad science alike. The scientific community has its own filtering mechanisms to avoid dealing with pseudoscience at science at every turn, but since peer review is not a precondition for expressing purportedly scientific opinions in the public sphere, the only way to enable the acceptance of good science for political

[150] Vidmar and Diamond, "Juries and Expert Evidence."

purposes is by testing it against contrary views that have influential in the public sphere, even if they are based on pseudoscience.

E. Democratic Status and Legitimacy of a Science Court

Science courts could be initiated by concerned citizens or by elected representatives. Initiating a science court would give citizens the opportunity to contest the expertise behind controversial measures without possessing expertise of their own or having to hire their own experts, as interest groups and corporations might do. This would also increase the political agenda setting power of citizens and make it easier for them to hold politicians accountable in more complex policy areas. Elected officials could use the power to initiate a science court to share the political and cognitive burden of dealing with issues involving complex science that go beyond their own expertise. Alternatively, calling a science court could be a strategic move for officials to avoid the entrenched party line on a scientific question and shift the responsibility onto a jury of ordinary citizens. Officials would still retain the right to ignore the recommendations of the science court, but this could become a costly move if the science court acquired significant public authority. Initiating a science court would be a way of changing the political costs and benefits of decisions on scientific issues, as well as legitimating and improving the use of expertise on contested issues.

The science court would end result in a decision made by the jurors on the policy question under consideration. This decision would be advisory; the legislature, the president or the relevant local representatives would need to adopt its recommendation before it could be enacted as policy. The court would not replace processes of representative policy-making, but would complement and hopefully improve them. Still, various measures could be taken to increase the political influence of the court proceedings and to make it more difficult for officials to ignore its outcome. For instance, the conveners could seek an advance public commitment from policymakers to following the court's decision. The Minnesota Governor's commitment to the science court on the power line case was crucial in giving the project its public momentum, at least in the beginning. Alternatively, officials could be required to initiate a second science court on the same question if they rejected the recommendations of a first one. Only after a second court delivered a decision would they be allowed to disregard recommendations. The court could also acquire influence with policymakers by gathering significant public support. This is not difficult to imagine if it focused on a national level scientific question such as climate change and the proceedings were televised. The arguments and counter-arguments presented at the court might sway public opinion, or the court's final decision might simply acquire independent authority if the event were widely perceived to be fair, democratic and well organized.

Two important features about the status of the science court are worth clarifying: the source of its legitimacy and the scope of its influence. The procedural legitimacy of the science court would be straightforward: The official status of a science court would be similar to the status of a scientific advisory board. Since the science court would not possess the authority to make decisions that would be binding on other citizens, there would be no need to explain how an institution lacking an official mandate could possess such authority.[151]

The more interesting question concerns the substantive legitimacy that an organization authorized this way could come to enjoy based on the content of its

[151] On this point this proposal differs from recent arguments for replacing elected officials with citizen-representatives selected by lot. See e.g. Alexander Guerrero, "Against Elections: The Lottocratic Alternative, *Philosophy & Public Affairs,* 42 (2), 2014.

activities, rather than only on the basis of its authorization procedure. A standard account of advisory bodies authorized by representatives is that they are expected to act as agents of the representative by fulfilling its wishes and purposes.[152] However, this account falls short both in theory and in practice. The independence required for the proper functioning of some of these bodies, such as scientific advisory boards, central banks, bureaus of statistics or government auditors, belies the claim that they are expected simply to do as the representative wishes. Moreover, these bodies can end up acquiring a significant amount of autonomy and power such that representatives may have to defer to them even when they may have preferred not to do so.[153] When and how unelected bodies acquire this kind of power is an empirical question of course; the salient normative question here is whether and how the power of these unelected bodies could be justified, given that the purely procedural principle-agent account is not enough to explain their role and function.

Some scholars have argued that unelected bodies such as scientific advisory boards enjoy a different form of democratic legitimacy that is rooted in their expertise.[154] The logic of this argument is that the independence of these bodies allows them to serve the general public interest rather than partisan goals, and that their expertise allows them to do this well. These two features together are supposed to supply expert bodies with democratic credentials in their own right, beyond the credentials they have due to their mode of authorization. While it may be true that

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[152] Philip Pettit, "Representation, Responsive and Indicative," *Constellations,* 17 (3), 2010.

[153] Sheila Jasanoff, *The Fifth Branch: Science Advisers as Policymakers.* Cambridge, MA: Harvard University Press, 1990; Dan Carpenter, *The Forging of Bureaucratic Autonomy: Reputations, Networks, and Policy Innovation in Executive Agencies 1862-1928.* Princeton, NJ: Princeton University Press, 2001.

[154] Brown, "Citizen Panels and the Concept of Representation," *The Journal of Political Philosophy,* 14 (2), 2006; Pierre Rosanvallon, *Democratic Legitimacy: Impartiality, Reflexivity, Proximity.* Trans. Arthur Goldhammer. Princeton, NJ: Princeton University Press, 2010.

independent bodies that are effective in furthering the public interest acquire legitimacy in the sense of acceptance by the public, normatively this legitimacy is no different than the legitimacy that may be enjoyed by a benevolent dictatorship. There is nothing specifically democratic about this; this legitimacy rests on things done *for* the public rather than *by* the public.

This analysis reveals a paradox in the democratic legitimation of independent bodies of experts in government: on the one hand their democratic legitimacy is supposed to be derived from their authorization by and answerability to elected representatives; on the other hand their success and acceptance depends on the degree to which they can be independent and use their expertise to further the public interest as they conceive of it. Even when their policy recommendations ultimately go through the approval of the elected body that authorizes them, successful expert bodies can acquire significant leverage that representatives cannot overlook. And in cases where these bodies are weak and easy to ignore, they will have failed in fulfilling their function. In other words, these expert institutions are successful to the extent that they go beyond what legitimates them democratically.

This paradox could be resolved in a couple of different ways. One obvious way would be to argue against the creation of such bodies altogether, or to suggest that they should only be used in cases where a strict separation of facts and values is possible, such that the representative can be very clear in defining the public interest and these bodies could simply find technical ways to fulfill it. Both, however, are dissatisfactory: Eliminating independent bodies would be neither feasible nor desirable in a complex society that needs expertise and it would also reduce the possibilities of what a modern state can accomplish through a carefully constructed balance of democratic institutions. The second solution may work in some cases, but the difficulty of separating facts from values might mean that only a very narrow set of tasks would pass this test.

A more promising approach would be to devise ways to expose expertise to democratic contestation so that expert claims could be examined and expert advice legitimated democratically before being used in the policymaking process. This is where the science court comes in. An institutional setting where competing experts are scrutinized directly by a group of citizens, who then go on to reach a decision on the basis of the experts' views, would provide a way to instill democratic legitimacy into a process that would otherwise depend only on the so-called legitimacy of expertise. The science court would also go some way toward reducing the inherent tension of the role of experts in the policymaking process – namely, that policymaking would lose its democratic legitimacy if experts acquired a lot of power over representatives, and would lose the benefits of expertise if they had no influence at all. If a citizen jury scrutinized expertise, then it would be both desirable and legitimate for these bodies to acquire more authority to shape the representative's decisions.

In a political climate where experts enjoy high degrees of deference, the science court would act as a check on their activities, whereas in cases where there is a high degree of mistrust toward expertise, the democratic legitimation of citizen scrutiny might strengthen the social credibility and political influence of science, insofar as the science court manages to gain influence and publicity for itself. Either way, the result would be to strengthen the influence of citizens on technical issues, both over unelected expert bodies and elected representatives, thus contributing to the healthy functioning of representative government.

I have discussed the main institutional features of the science court, but have left some of the more micro-level design issues unaddressed. These include whether the jury selection process would allow peremptory challenge; whether jury deliberations would be open or secret; whether decisions would be made unanimously or by majority rule; and whether the verdict would only announce a decision or whether it would also involve an explanation or justifications. I mention these only to set them aside to be considered if and when this proposal is implemented.

IV. Objections

A. Scientific Truth Up for Vote?

One objection I anticipate to this proposal is that it amounts to putting scientific truth to a popular vote. A version of this objection was directed at the Kantrowitz proposal: Critics argued that adversary proceedings prioritize winning the agreement of the judges (or jurors) over the truth of their claims, whereas science does not depend on the assent of others for its truth.[155] The science court would therefore not be an appropriate setting for resolving scientific disputes.

The implied contrast between adversary and scientific methods of fact-finding is correct, but this does not constitute an objection to the science court because it misunderstands the purpose of the institution. The objection misses the distinction between the truth of a scientific theory and its acceptance in a science court discussed in Chapter 2. The decision about what facts to accept in this context is not a decision about what is true; it is a decision about what can be reliably assumed or accepted for the particular purposes of politics, given that the truth is unknown and there is disagreement about what it is. This is a core point about the use of a science court in

[155] Howard Markey, "A Forum for Technocracy? A Report on the Science Court Proposal," *Judicature,* 60 (8), 1977.

democracies. Scientific claims must gain the assent of citizens or their representatives to become the basis of democratic policy. Any institution designed for the incorporation of science into politics cannot but have this orientation toward persuasion.

This does not preclude the search for the truth, and I have argued that adversarial proceedings are well suited to enabling a thorough examination of competing truth claims. But the professional community of scientists will have different methods for settling controversies, given their shared methods and orientation toward the long-term discovery of truth. The science court does not claim settle controversies for the scientific community or to contribute to peer review. It is a political institution designed to open truth claims to citizen input and scrutiny on political questions requiring expertise.

B. Competence

Another serious objection is that this institution overestimates citizen competence. Would citizens be able to understand and question expert claims? Would not such an institution result in arbitrary decisions, divorced from scientific reality? There are two ways to respond to this. The first is to argue that this proposal would be an improvement over alternatives, given normative democratic constraints on which alternatives would be acceptable. The second is to give reasons to believe that citizens do in fact have the capacity to perform this task.

An important part of my argument is that it is necessary on democratic grounds for citizens and their representatives to determine the direction of political decisions on scientific issues, and that this cannot be realized by leaving the scientific part to experts. Once the option of leaving things entirely to experts is rejected on normative grounds, the challenge is to come up with institutions in which citizens could contribute most effectively. The institutional arrangements proposed here are designed precisely to overcome the obstacles posed for democratic debate over science by citizens' lack of expertise.

The obstacles are twofold: first, ordinary citizens will not have the specialist's knowledge to contest expert claims. Secondly, the rational ignorance thesis states that it will be irrational for citizens to invest time trying to acquire the knowledge that would allow them to better evaluate complex technical issues since the likelihood of influencing the outcome is slim and time is scarce.[156] The science court addresses the worry about rational ignorance by modifying two of the key variables that make information gathering irrational: time and the small likelihood of influencing the outcome. The proposal makes it mandatory for selected citizens to attend hearings and to listen to all presentations of evidence. This allows (or forces) participants to dedicate a serious amount of time to listening to experts; they effectively form a captive audience. Moreover, the requirement that the jury must reach a decision that will advise the policy-making process changes the stakes for participants: since this decision will have more influence over policy than the opinion of an ordinary citizen sitting at home, it is rational for jurors to invest mental energy to understanding the scientific components of the problem.

This responds to the worry about citizen *ignorance*. However, the competence objection consists of both a knowledge component and a capacity component. The worry about capacity is that citizens will simply not be able to understand expert claims, no matter how hard they try. A good way to test this claim is to look at

[156] Russell Hardin, "Street-Level Epistemology and Democratic Participation," *The Journal of Political Philosophy,* 10 (2), 2002; Ilya Somin, "Voter Ignorance and the Democratic Ideal," *Critical Review,* 1998.

evidence from jury trials, particularly civil cases, where juries have to evaluate complex scientific testimony from a wide range of experts before reaching a verdict. The charge that juries are confused by scientific testimony and reach arbitrary verdicts based on the emotional appeals of lawyers and expert witnesses is widespread,[157] but empirical research indicates that juries in fact perform their task very well: They evaluate experts based on the merits of their testimony rather than on their likeability or credentials, and understand the purpose and effects of the adversary process.[158]

Of course researchers who set out to assess the competence of jurors face the difficulty of positing some kind of external standard of correctness by which the evaluate the jurors. Since trial verdicts are never simply judgments about the truth or falsity of scientific claims, and since even the truth or falsity of competing scientific claims is often contested even among experts, finding such an evaluative standard poses a significant, perhaps insurmountable, methodological limit to these studies. Still, existing studies have used a variety of different methods, which together present a consistent picture of good performance. These methods include asking the presiding judge what verdict they would have given and comparing it to the jury's verdict, asking external and independent experts to evaluate competing expert testimony presented at the trial, interviewing jurors about their decision-making process to understand how much they engaged with the content of expert testimony, and observing actual or mock jury deliberations.

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[157] Stephen Daniels and Joanne Martin, *Civil Juries and the Politics of Reform.* Evanston, IL: Northwestern University Press, American Bar Foundation, 1996; Marcia Angell, *Science on Trial.* New York, NY: Norton, 1996 among many others.

[158] Neil Vidmar, "The Performance of the American Civil Jury," *Arizona Law Review*, 40, 1989; Larry Heuer and Steven Penrod, ""Trial Complexity, "*Law and Human Behavior,* 18 (1), 1994; Vidmar and Diamond, "Juries and Expert Evidence;" Sanja Ivkovic and Valerie Hans, "Jurors' Evaluations of Expert Testimony: Judging the Messenger and the Message," *Law and Social Inquiry,* 28 (2), 2003.

Let me briefly summarize the findings in the literature. Kalven and Zeisel's well-known study of jury trials in the 1950s found that in 80 percent of the 6000 cases studied, the presiding judges said they would have given the same verdict as the jury.[159] In the 20 percent where judges disagreed with juries, the disagreement was found to be unrelated to the complexity of the case. More recent studies, motivated by the possibility that a study from the 1950s would not capture the effects of increased complexity, followed the same methodology and found similar results both in criminal and civil cases, with complexity remaining irrelevant for explaining cases of disagreement.[160] Three studies asking physicians to examine jury decisions in cases of medical malpractice, and another study that closely examined thirteen "complex" cases found no evidence of jury irrationality.[161] One study that interviewed jurors in five cases involving scientific testimony found that a significant number of jurors could articulate the main scientific issues, and understood the basic points made by competing experts.[162] Another study interviewed 55 jurors who served on a range of cases including medical malpractice, workplace injury, product liability, asbestos or

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[159] Harry Kalven and Hans Zeisel, *The American Jury.* Boston, MA: Little Brown, 1966.

[160] Heuer and Penrod, "Trial Complexity;" Valerie Hans et al. "The Arizona Jury Reform: Permitting Civil Jury Trial Discussions: The Views of Trial Participants, Judges, and Jurors." *University of Michigan Journal of Law Reform,* 32 (2), 1999; Theodore Eisenberg et al., "Judge-Jury Agreement in Criminal Cases: A Partial Replication of Kalven and Zeisel's The American Jury," *Journal of Empirical Legal Studies,* 2 (1), 2005.

[161] Mark Taragin et al. "The Influence of Standard of Care and Severity of Injury on the Resolution of Medical Malpractice Claims," *Annals of Internal Medicine*, 117 (9), 1992; Frank Sloan et al. *Suing for Medical Malpractice*. Chicago, IL: University of Chicago Press, 1993; Henry Farber and Michelle White, "Medical Malpractice: An Empirical Examination of the Litigation Process," *Rand Journal of Economics*, 22 (2) 1991; Richard Lempert, "Civil Juries and Complex Cases: Taking Stock After Twelve Years." In *Verdict: Assessing the Civil Jury System*, Robert Litan ed. Washington, DC: Brookings Institution, 1993.

[162] Daniel Shuman et al. "An Empirical Examination of the Use of Expert Witnesses in the Courts – Part II: A Three City Study," *Jurimetrics,* 34 (2), 1994.

motor vehicles, and found that the majority of the jurors could critically evaluate the testimony of experts and gave nuanced responses to questions about the evidence.[163]

Particularly interesting for my purposes here are the findings from the Arizona Jury Project's videotaping of 50 jury trials, where jurors were allowed to question expert witnesses – a rare practice in most states due to concerns about compromising juror impartiality.[164] What is striking in this study is how detailed and probing some of the juror questions about science turn out to be. Here is a set of jury questions from a case involving a knee injury:

"Why no medical records beyond the two years prior to the accident? What tests or determination besides subjective patient's say so determined [your diagnosis of] a migraine? What exact symptoms did he have regarding a migraine? Why no other tests to rule out other neurological problems? Is there a measurement for the amount of serotonin in his brain? What causes serotonin not to work properly? Is surgery a last resort? What is indothomiacin? Can it cause problems if you have prostate problems?"[165]

Here are some questions from an automobile injury case:

"Not knowing how she was sitting or her weight how can you be sure she hit her knee? Would these factors change your estimate of 15 ft/sec travel speed? If a body in motion stays in motion, and she was continuing motion from prior to the impact, how did this motion begin and what do you base this on? How tall is the person who sat in your exemplar car to reconstruct the accident and how heavy was he? What is the error in your 10 mph estimate? Is the time of 50-70 milliseconds based on an estimate of the size of the dent? Do you conclude that the Olds was slowed and pushed to the left by the Lincoln and [if so] how would the plaintiff move to the right and forward?"[166]

Although these are cherry-picked examples, they are meant to illustrate the Arizona Jury Project's broader conclusion that jurors were able to engage closely with

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[163] Ivkovic and Hans, "Jurors' Evaluations of Expert Testimony."

[164] Shari Diamond et al., "Juror Discussion During Civil Trials: Studying an Arizona Innovation," *Arizona Law Review*, 45 (1), 2003.

[165] Neil Vidmar, "Listening to Jurors and Asking Them Questions," *Trial Briefs*, 36, 2002.

[166] Ibid.

the content of expert testimony. Overall, the empirical evidence refutes the claim that juries are incapable of understanding expert testimony and making sensible decisions in cases involving complex technical knowledge. The competence objection against the science court is not backed by long-standing evidence from the analogous institution of the civil jury trial.

One last thing I want to add on the question of competence is that it is important not to essentialize the concept by assuming that a person either has or does not have competence on an issue. Competence can be developed over time through practice and participation, or it can be acquired if and when an issue becomes particularly salient – for instance if one is called to jury duty on a scientific issue. Jurors can also be given training before the court proceedings on key scientific concepts, evidentiary standards and measures of uncertainty, as well as on the types of questions they could ask, as listed earlier. Furthermore, public comprehension and competence can be improved through more effective communication strategies and more intelligible presentations of complex information. The literature on jury competence not only evaluates how well juries perform, but also examines communicative strategies that improve the understanding of jurors. Research on the public communication of science can also prove helpful on this point.

V. The Science Court and Other Mini-Publics

Direct citizen involvement in political decision-making through small, organized, participatory settings is obviously not a new idea for democratic theory. There is a large and rapidly growing literature on small-scale participatory experiments such as citizen juries, deliberative polls, consensus conferences and planning cells.[167] These deliberative mini-publics are all meant to enhance citizen participation, increase the legitimacy of democratic decisions, allow citizens to hold decision-makers accountable, improve the quality and effectiveness of policies, bolster civic engagement and improve the education of the citizenry, although different institutional designs prioritize different goals. In this section, I will clarify how the proposal I have developed fits into this literature and improves upon existing models' treatment of expertise.

A serious shortcoming of the literature on deliberative democracy has been its neglect of the complexities of citizen deliberation on issues requiring expertise. This was perhaps inevitable for epistemic democrats, who maintain that large groups of ordinary citizens are superior to small groups of experts in arriving at certain kinds of truth. It would be difficult, if not entirely unreasonable, to maintain that randomly selected citizens would do better than a group of scientists in discovering *scientific* truths. The mechanisms by which the epistemic advantages of large groups are meant to be realized – cognitive diversity, dispersed private or local knowledge, deliberation, Condorcetian aggregation – could supplement expert knowledge, but they could not replace it. This is why Estlund defines the scope of his argument as issues of moral

[167] Ned Crosby, "Citizens' Juries: One Solution for Difficult Environmental Questions." In *Fairness and Competence in Citizen Participation: Evaluating Models for Environmental Discourse*, Ortwin Renn et al. eds. Dordrecht: Kluwer Academic Press,1995; James Fishkin, *Democracy and Deliberation.* New Haven, CT: Yale University Press, 1991; Fishkin, *The Voice of the People: Public Opinion and Democracy.* New Haven, CT: Yale University Press, 1997. See John Gastil and Peter Levine eds. *The Deliberative Democracy Handbook: Strategies for Effective Civic Engagement in the Twenty-first Century*. San Francisco: Jossey-Bass, 2005 for a full catalogue of mini-publics.

truth,[168] and Landemore defines it slightly more vaguely as political decisions, while conceding that it is likely not to include complex issues such as climate change.[169]

The neglect is more difficult to explain in other strands of deliberative democracy, and especially in the literature on mini-publics, which has given a central role to expert advice from its early beginnings.[170] The practice of bringing experts and citizens together to discuss policy questions about new technology can be traced back the Danish Board of Technology's first consensus conference, which was held in 1987.[171] Consensus conferences involve fifteen to twenty citizens who listen to expert testimony and then deliberate about the social and ethical issues raised by proposed technological innovations. The purpose is to make sure that the development of new technologies addresses the concerns of citizens. Citizen input is oriented toward agenda setting and information gathering, rather than decision-making about policy.

Fishkin's deliberative poll is another well-known example of a deliberative setting that brings together citizens and experts. Deliberative polls differ from consensus conferences in selecting a much larger number of citizens through random sampling. This ensures that the composition of participants is truly representative of public opinion. Although deliberative polls are not designed specifically for issues involving science or technology, the involvement of experts and briefing materials are key features of the institution. Kitcher has recently proposed a very similar model

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[168] David Estlund, *Democratic Authority: A Philosophical Framework.* Princeton, NJ: Princeton University Press, 2008.

[169] Hélène Landemore, *Democratic Reason: Politics, Collective Intelligence and the Rule of the Many.* Princeton, NJ: Princeton University Press, 2008.

[170] Fung, *Empowered Participation;* Philip Kitcher, *Science, Truth and Democracy*; Simon Joss and John Durant eds. *Public Participation in Science: The Role of Consensus Conferences in Europe.* London: Science Museum, 1995.

[171] Joss and Durant eds. *Public Participation in Science;* Gastil and Levine eds., *The Deliberative Democracy Handbook.*

designed exclusively for the discussion of scientific issues. His ideal of "well-ordered science" involves scientists tutoring selected citizen participants about the technical aspects of an issue so that they can go on to deliberate in a more informed fashion.[172] The aim is to transform citizen preferences by exposure to scientific facts, and to prevent "vulgar democracy," which Kitcher defines as the shaping of government policy on scientific issues by the "untutored" preferences of citizens, formed in ignorance of science.

The general problem with the scholarship on deliberative bodies with expert participants is that they conceptualize the relationship between experts and citizens as a primarily educative one. The information provided by the experts is meant to improve the quality of deliberation and help citizens develop "considered" or "tutored" opinions and preferences. The implicit expectation is that citizens will accept the expert views more or less at face value and deliberate on the basis of these facts, rather than examining the facts themselves.

This educational orientation is reflected in the institutional design of existing models. Their expert panels typically consists of one expert from each relevant field, which makes it difficult for citizens to see the weaknesses of expert views or to get a sense of possible alternatives. Fishkin's stipulation that "carefully balanced briefing materials" should be provided ignores the challenges of providing a fair representation of different expert views through such materials. Furthermore, the effectiveness of these institutions are measured through before-and-after surveys that use the degree of citizen uptake of information provided by experts as an indicator of success.[173]

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[172] Kitcher, *Science, Truth and Democracy.*

[173] Goodin and Dryzek, "Deliberative Impacts."

All of these features support the view that these mini-public are designed with the expectation that citizens should absorb information, rather than contest it. Although these proposals have the appearance of democratizing expertise by bringing experts and citizens together, the expert-layperson interaction seems designed to prioritize concerns about citizen ignorance. The roles assigned to experts and citizens ironically replicate the conventional division of labor in which experts supply the facts and laypeople debate about the values. Some commentators have even called the role of citizens "value consultants."[174] These proposals have paid virtually no attention to the potential limitations of expertise and the need to critically evaluate expert claims.

Fung offers a different model that lies between the educational model and epistemic democracy.[175] He argues that citizens often possess local knowledge and problem-solving ability on issues that defy experts and that citizens and experts deliberating together could generate better solutions to seemingly intractable technical problems that neither could address satisfactorily on their own. Although this does not see the experts' role as purely educational, it shares the educational model's primary orientation toward increased quality of decisions. The competence, knowledge or creativity that citizens can bring becomes the justification for their participation. This in turn implies that if citizens possessed no advantage over experts, the issue would not require their participation. Fung writes: "Some areas would benefit very little from

[174] Peter Dienel and Ortwin Renn, "Planning Cells: A Gate to 'Fractal Mediation.'" In *Fairness and Competence in Citizen Participation: Evaluating Models for Environmental Discourse,* Ortwin Renn et al. eds. Dordrecht: Kluwer Academic, 1995.

[175] Fung, *Empowered Participation;* Charles Sabel et al. *Beyond Backyard Environmentalism*. Boston, MA: Beacon Press, 2000.

deliberation because they require highly specialized kinds of knowledge or training or because citizens have no distinctive insight or information."[176]

Despite their democratic aims, both of these influential approaches serve to reinforce different varieties of a technocratic logic. While the first approach insists that public opinion must be educated before it influences policy, the second idealizes the managerial partnership of ordinary citizens and experts for "performance-based"[177] policy that can make up for the shortcomings of centralized expert policymaking, such as its ignorance of crucial local details. These models take scientific expertise at face value in assuming that inviting experts to participate in a forum is straightforwardly equivalent to instilling wisdom into the decision-making process. They do not take into account the obstacles caused by expert disagreement, the biases and unexamined assumptions involved in knowledge claims, or the difficulty of dealing with uncertain science for political purposes. Moreover, neither model pays attention to the difficulties of non-experts deliberating on technical issues with those who possess more knowledge and authority.

These models thus miss the opportunity to use mini-publics for the purpose of enabling more contestation of expert claims. They fail to take advantage of the full potential of these institutions for giving more direct power to citizens in areas where citizens are particularly far removed from their government. But even if we adopted their view that improving the education of citizens and the quality of decisions should be the priorities for mini-publics, the potential limitations of expert claims and the difficulties of deliberation between experts and non-experts raise doubts about whether the hoped-for improvements in quality could be realized under existing

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[176] Archon Fung, "Recipes for Public Spheres."

[177] Sabel et al., *Beyond Backyard Environmentalism*.

models. If the deliberation process is coopted by experts, the epistemic benefits that could be expected from the *interaction* of experts and laypeople would disappear since citizens would not have had a meaningful opportunity to shape the outcome of the debate in light of their own knowledge, values and diverse perspectives.

Empirical studies of mixed juries of professional judges and lay citizens provide support for this view: mixed tribunals are widely criticized for reducing laypersons to "nodders" with a tendency to follow the lead of the professional judges.[178] If the expert views that inform deliberation were problematic in some way, for instance because they make assumptions that are not shared by citizens, then the resulting deliberation would proceed on the basis of these problematic views. The legitimating role of citizen participation would effectively become a rubber stamp of the expert views that are made available. Under these circumstances, the mini-publics would fall short even on their own terms.

A major contribution of my proposal to this literature is that it develops institutional rules with an eye to overcoming these particular difficulties. I argue for an adversarial set-up to facilitate lay scrutiny over expert claims and a question-answer format designed to allow laypeople to examine the assumptions behind expert claims, rather than just to seek clarification or more information. This orientation toward the contestation of experts focuses on the potential value of these participatory institutions for opening up expertise to more meaningful democratic scrutiny. All scholars in this genre maintain that mini-publics are meant to enhance both quality and democratic accountability. But I raise doubts about the possibility of realizing either of these desired benefits without addressing the difficulties specific to expert-

[178] Valerie Hans, "Jury System Around the World," *Annual Review of Law and Social Science,* 4, 2008.

layperson interactions and changing the orientation of these institutional settings toward contestation rather than deliberation.

Concern with the quality of political decisions on technical issues is not a bad thing, of course, but it can become problematic if a trade-off must be made between quality and other democratic values. As Lafont has recently pointed out, the emphasis on quality may create a democratic legitimacy problem for the use of Fishkin-Kitcher style mini-publics to inform policy.[179] Although Lafont's criticism does not focus on expertise in particular, attention to the role of experts in these bodies brings her critique into sharper relief. Lafont notes (as I have) that Fishkin's mini-publics are meant to improve both the democratic legitimacy and quality of policy decisions. The former is achieved by the selection of participants by random sampling, which results in the descriptive representation of public opinion at a micro level. The deliberation process then improves the quality of this public opinion and produces the "considered" opinion of the citizenry.

But Lafont argues that precisely this transformation of the opinions of participants through the filter of deliberation means that the micro-public is no longer representative of the public as a whole in the descriptive sense crucial for Fishkin's conception of legitimacy. By becoming better informed and having reasoned about the issues at hand they have become more like experts on those issues than ordinary citizens.[180]

"Why should their judgments have any more recommending force that those of experts that may be substantively superior on their merits? Why should these judgments be particularly authoritative? The answer cannot be that these judgments

[179] Cristina Lafont, "Deliberation, Participation, and Democratic Legitimacy: Should Deliberative Mini-publics Shape Public Policy?" *The Journal of Political Philosophy,* 23 (1), 2014.

[180] John Parkinson, *Deliberating in the Real World.* Oxford: Oxford University Press, 2006 also makes this point.

are those of “the people,” because they no longer are. And since they are not, why should they have any recommending force for the citizenry at large? Precisely because they have not participated in the deliberative process of becoming informed, weighing the evidence, testing their arguments in light of counterarguments of opponents, and so on, non-participants have no specific reason or justification to endorse the recommendations of the deliberative poll. If this is correct, then on what basis would it be legitimate to ask them to blindly endorse these recommendations?”[181]

This point is most salient in cases where selected citizens are simply tutored by experts. If participating citizens wholly absorb the views of experts, then the democratic legitimacy that comes from the direct involvement of ordinary citizens in policy-making loses its force. It becomes unclear why policymakers should not simply defer to experts and ignore the mini-public altogether. But if the mini-public involves an exchange in which citizens critically evaluate expert claims rather than simply deferring to them, then the objection loses its force. The deference of policymakers and other citizens to the recommendations of the science court would be justified on the grounds that a group of ordinary citizens have spent a significant amount of time carefully examining different scientific views on an issue. Any representative institution, and especially one that considers complex issues, will involve a gap between the views of representatives and those of the represented.[182] The advantage of a science court is that this gap is likely to be smaller with citizen-jurors than with scientist-judges. The fact that jurors are descriptively representative of the general population allows non-participants to infer that they could have reached a similar conclusion if they had participated in a similar process.

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[181] Lafont, “Deliberation, Participation, and Democratic Legitimacy.”

[182] Dennis Thompson, “Who Should Govern Who Governs? The Role of Citizens in Reforming the Electoral System.” In *Designing Deliberative Democracy,* Mark Warren and Hilary Pearse eds. Cambridge: Cambridge University Press, 2008, pp. 46-49.

Chapter 4: Rethinking Public Funding for Science

"Was Duck Penis Study Appropriate Use of Taxpayer Money?" [183] ran a Fox News headline from 2013. The article attacked an NSF-funded animal behavior study on duck genitalia conducted at Yale University as a wasteful use of federal money. Two years before, another scientific study had been held up for ridicule: "Your Tax Dollars at Work: Shrimp on Treadmills."[184] Although the immediate target of the attack was different, the main goal was the same: To criticize the NSF for wasting hundreds of thousands of taxpayer dollars in support of scientific projects that were supposed to seem obviously trivial to a layperson. To make the point more vivid, the earlier story also included a rather fascinating video from the study, featuring – you guessed it – a shrimp exercising on a miniature treadmill.

Mocking randomly selected examples of "silly" science has become a standard rhetorical tool for Republicans who want to cut spending on science and complain about the federal government's wastefulness.[185] The scientists responsible for the studies on duck genitalia and shrimp on treadmills were the recent victims, but this popular strategy has been used by Republican politicians and media against many unsuspecting scientists over the years. Since these attacks on science have become both more frequent and more vocal, we must take seriously the theoretical questions they raise about the role of politics in determining funding for science. Should

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[183] "Was Duck Penis Study Appropriate Use of Taxpayer Money?" *FoxNews.com*, 25 March 2013, Web; See also Todd Starnes, "Austerity! Feds spend $400,000 to study duck genitals," *Human Events,* 20 March 2013, Web; and Gaiyathri Vaidyanathan, "Republicans Attempt to Use Mockery to Cut Sound Science," *Scientific American,* 12 May 2015, Web.

[184] "Your Tax Dollars at Work: Shrimp on Treadmills," *FoxNews.com*, 26 May 2011, Web.

[185] Nell Greenfieldboyce, "'Shrimp on a Treadmill': The Politics of 'Silly' Studies," *NPR.org,* 23 August 2011, Web.

Congress and politicians interfere with how scientists distribute public funds among scientific projects? This chapter addresses this question.

Science funding lies at the intersection of two areas political theorists have had surprisingly little to say about. The first is the provision of public goods. The vast literature on equality and distributive justice has focused mostly on principles and institutions governing the distribution of private goods to individuals.[186] State-produced goods such as social security, health and income support have been the focus of distributive justice only insofar as they are distributed to individuals. At first, it might seem to make sense that public goods are not the concern of theories of distribution: since no one can be excluded from benefiting once public goods are produced, there is nothing to be distributed. However, that people cannot be excluded from using public goods does not mean that all will use them equally, nor does it mean that all users will value them equally. This, coupled with the fact that the provision of public goods depends on funds collected coercively through taxation, raises questions about what justifies the provision of public goods, which goods can justifiably be supplied by the state, and what procedures must regulate the decision process. Scientific knowledge is a paradigm example of a public good: it is non-excludable – no one can be excluded from benefiting from knowledge once it is disseminated, and it is non-rivalrous – use by one person does not diminish the amount of knowledge left for others. It is therefore essential to consider public funding of science against the backdrop of broader normative concerns raised by the provision of public goods.

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[186] David Miller, "Justice, Democracy and Public Goods." In *Justice and Democracy: Essays for Brian Barry*, Keith Dowding et al. eds. Cambridge: Cambridge University Press, 2004.

The other area political theorists have neglected is the study of the relationship between democracy, justice and the creation of knowledge. While theorists readily admit that the production of material goods is of primary concern for theories of both justice and democracy, they have not paid enough attention to the concerns specific to decisions about the production and diffusion of knowledge. This, too, is a mistake since both the production and non-production of knowledge has implications for the possibilities for politics. The close connection between scientific inquiry and truth, and the special link between science and policy in the modern state create a set of additional concerns for the public funding of science that go beyond the considerations that apply to the public provision of roads, bridges, infrastructure, or even of art. It becomes essential to consider the conditions under which reliable scientific knowledge can be generated and how the knowledge produced will affect democratic decision-making.

To determine the proper role of political input over science funding decisions, we must therefore consider the following cluster of related normative questions about public goods and the creation of knowledge: 1) What justifies government spending on public goods broadly understood; 2) What justifies public support for science; 3) What is the relationship between an autonomous scientific community and scientific progress; and 4) What is the relationship between political decision-making on scientific issues and decisions on science funding. Each of these will be addressed in a section of this chapter. While the first three sections focus mainly on funding for basic research, the last will turn to funding for scientific issues that have already become politically salient. The distinction will not always be clear in practice, but it is theoretically useful for clarifying how the degree of uncertainty about the political stakes in funding decisions should affect the appropriate level of political input and the proper distributive principles guiding the decision.

I. Political Theory and Public Goods

In *A Theory of Justice,* Rawls divides expenditures by the state into two categories: those required by justice and those that are not.[187] The former are governed by his two principles of justice and apply to the background institutions in a society, including legal definitions of property rights and a scheme of taxation.[188] These are expenditures necessary for the sustenance of a just basic structure in which all resulting distributions of income and wealth would also be just. There can be different views on the particular expenditures required for just basic institutions, but Rawls suggests that fair equality of opportunity for education and training, basic healthcare and a decent income will be part of any liberal conception of justice.[189] These expenditures are not subject to a popular vote. What justifies their imposition on all citizens is the fact that they are a necessary cost of living in a just, mutually advantageous cooperative venture. This contentious claim gave rise to the famous debate between Rawls and Nozick on whether the benefits derived from a cooperative enterprise could ground an obligation to share its burdens.[190] I mention this foundational debate about the basis of political obligation only to bracket it; what is of interest here is Rawls' second category of public goods: those not required by justice.

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[187] Rawls, *A Theory of Justice,* p. 62.

[188] Ibid., p. 279.

[189] Ibid., p. 270.

[190] See Robert Nozick, *Anarchy, State and Utopia.* New York, NY: Harper and Row, 1974; and also George Klosko, "The Obligation to Contribute to Discretionary Public Goods," *Political Studies,* 38, 1990.

Rawls points out that the requirements of justice might not cover all public expenditures that citizens might wish to make. "If a sufficiently large number of them find the marginal benefits of public goods greater than that of goods available through the market, it is appropriate that ways should be found for government to provide them."[191] Since justice does not require the provision of these additional public goods, the principle regulating their provision will be solely that of benefit. Rawls then specifies how benefit should be understood in this context by appealing to Wicksell's unanimity principle. In his 1896 article "A New Principle of Just Taxation," Wicksell had argued that if a public good is an efficient use of social resources, there must be a distribution of tax burdens that would gain unanimous approval.[192] Decision-makers should consider proposals for public goods together with alternative schemes for the distribution of their tax burdens. Only those tax schemes that gained unanimous approval should be provided. This approach would ensure that those who would derive no benefit from the good would not be forced to pay, and the distribution of burdens across individuals would track the value of the good for each.

Rawls adopts this principle and proposes the creation of a separate branch of government – the exchange branch – to deal with the evaluation of interests and preferences necessary for the application of this principle to particular decisions.[193] Interestingly, the only specific class of goods he mentioned as an illustration for the functions of the exchange branch is public funds for the arts and sciences.[194]

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[191] Rawls, *A Theory of Justice,* p. 282.

[192] Knut Wicksell, "A New Principle of Just Taxation." In *Classics in the Theory of Public Finance*, Richard Musgrave and Alan Peacock eds. London: Palgrave Macmillan, 1958.

[193] Raws, *A Theory of Justice,* p. 283.

[194] Ibid., p. 331.

As others have pointed out, Wicksell's unanimity principle is problematic as a theory of public goods.[195] For one thing, it ignores the possibility of strategic behavior or bargaining by individuals to secure better deals for themselves. Under this system, individuals will have incentives to misrepresent their preferences in order to secure a lower tax rate for goods that they would like to have provided. Since everyone has this incentive, the unanimity principle will result in the under provision of public goods: this is a version of the classic free rider problem. Wicksell wrote with the assumption that individuals would simply state their true preferences in order to bring about a desired communal outcome; he did not consider the possibility of strategic behavior. This was not an unusual assumption for someone writing at that time, but it is puzzling that Rawls could endorse unanimity principle as a reasonable theory of public goods, given that he must have been aware of economists' critiques of its neglect of strategic behavior.

Even if we bracket the difficulties posed to the application of this principles by strategic actors, the Wicksell rule still allows for an extremely narrow scope for state provision of non-justice goods. Specifically, it only allows the provision of goods that represent a Pareto improvement. Under this rule, the state could not make anyone subsidize goods she would not benefit from, or pay more for a good than its value to her. Only those taxation packages under which no one would be a net loser would pass the unanimity requirement. The principle rests on a narrow understanding of economic efficiency; it simply institutes an alternative trading mechanism for cases where the market mechanism breaks down.

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[195] See Richard Tuck, *Free Riding*. Cambridge, MA: Harvard University Press, 2008, pp. 191-192 on Wicksell's failure to consider strategic questions and how this was not considered extraordinary at the time. See also Miller, "Justice, Democracy and Public Goods," for a discussion of the shortcomings of Wicksell's unanimity principle.

Rawls justifies his adoption of this principle on the grounds that it would prevent the state from imposing unwanted burdens on people by appealing to perfectionist justifications that they do not share.[196] While expenditures required by justice could be justified on the basis that just basic structures are necessary for all, this logic cannot be applied to discretionary goods, which are justified by appeal to particular conceptions of the good. "The principles of justice do not permit subsidizing universities and institutes, or opera and the theater on the grounds that these institutions are intrinsically valuable and those who engage in them are to be supported even at some expense to others who do not receive compensating benefits."[197] This, he argues, would be equivalent to forcing people to subsidize the private expenses of others.[198]

Is it possible to avoid the imposition of burdens on citizens on the basis of intrinsic-value justifications, while allowing the state to provide a wider range of public goods? An obvious alternative would be to replace the economic efficiency approach with political decision-making. Decisions about public goods are no different than other political situations where people with different and incompatible

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[196] Rawls, *A Theory of Justice,* p. 325.

[197] Ibid., p. 332.

[198] This became a fundamental tenet of liberal conceptions of state funding. In the 1990s, when conservatives were attacking government funding for the National Endowment of the Arts, there was a lively debate among liberal theorists on whether public support fort he arts could be justified on a liberal conception of the state, assuming that support for "high" art went against the preferences of the majority. Most theorists concluded that it was very difficult to do so. On this very interesting debate, see Harry Brighouse, "Neutrality, Publicity, and State Funding of the Arts," *Philosophy & Public Affairs,* 24 (1), 1995; Ronald Dworkin, *A Matter of Principle.* Cambridge, MA: Harvard University Press, 1985, p. 225; Joel Feinberg, "Not with My Tax Money: The Problem of Justifying Government Subsidies for the Arts," *Public Affairs Quarterly,* 8 (2), 1994; Noël Carroll, "Can Government Funding of the Arts be Justified Theoretically?" *Journal of Aesthetic Education,* 21 (1), 1987.

preferences must reach an agreement about what to do. The unanimity principle uses the logic of the market to settle the decision: The transaction will take place only if everyone agrees to it. The alternative method is to institute a fair decision procedure to select among competing preferences and values. This is what democratic decision-making is does: in cases where people hold competing conceptions of the good, the decision about what to do is reached through a fair procedure, such as majority rule, that is agreed upon in advance.

Of course, replacing unanimity with a fair decision procedure to settle decisions about the provision of public goods will mean that some people will be forced to subsidize goods that they do not want and will not benefit from. Nonetheless, the justification for this will not be that these goods are intrinsically valuable or beneficial. Those who want the good can justify their preference in terms of their own conception of the good and they can try to persuade others to support this with appeals to intrinsic value. Once the decision is made, however, what would justify the state's imposing burdens on the minority is not the intrinsic value of the good, but that it has been selected through a fair decision procedure.

What is attractive about this approach is that it allows everyone to have more of the public goods that they want, even if they may lose out on individual decisions and be forced to subsidize others. Most people who want some public goods therefore have reason to prefer this system to the Rawlsian one. If a society is divided between those who want to fund science and those who want to fund the arts, for instance, then forcing each to subsidize the projects of others will lead to a mutually beneficial outcome, even though each will have been coerced to pay for something they did not want. The system allows groups to cross subsidize each other, so that they take turns in having their preferred goods provided. If the system works well, most people might be net winners.

II. Justifying Public Funding for Basic Research

The most significant change to scientific research in the last century was the rise of an extensive system of public funding for science. The dramatic increase in public funds for science after World War II transformed the scale and power of science, while rendering most new scientific research dependent on the continuation of public support.[199] Since funding for science represents a non-trivial portion of the federal budget, and since this level of public support for science is unprecedented in history, it is worth analyzing the normative arguments that support it. The last section provided an argument for justifying the provision of public goods broadly speaking. This section will turn to the specific question of how public funding for science can be and has been justified. I will then examine the implications of these justifications for the distribution of funds among different scientific projects.

Following the framework set by Rawls, there could be two normative strategies for justifying science funding. The first would show that some level of science funding is a requirement of justice. Although Rawls mentions science and art subsidies as paradigm cases of public goods *not* required by justice, his discussion is meant to apply to a society with just background institutions and a just distribution of income and wealth. He does not discuss subsidies for science in a non-just society. If the assumption of background justice is relaxed, it becomes easier to make the case that subsidies for science can be necessary for justice, especially if their allocation is also guided by justice-enhancing principles. In fact, one possible reason why Rawls's

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[199] Daniel S. Greenberg, *Science, Money and Politics.* Chicago, IL: University of Chicago Press, 2001.

rather strange discussion of the provision of non-justice public goods through the exchange branch has been largely neglected might be the fact that a very broad scope of state expenditures could be justified simply by appealing to his principles of justice.

How might spending on scientific research be a requirement of justice? If we stay within the Rawlsian framework, the answer would link scientific research to the two principles of justice: securing equal liberties and promoting the long-term interests of the least advantaged. Scientific research can be shown to be related to the production and distribution of some of the primary goods necessary for free and equal citizenship. For instance, science can help strengthen national defense, fight disease, improve healthcare and ensure better environmental quality. Science can also stimulate downstream technological innovation and accelerate economic growth, thus creating more wealth for society as a whole. If these economic benefits can be coupled with a system of fair distribution, science could be an important long-term resource for improving the lot of the least advantaged. But we need not be committed to the Rawlsian theory of justice. As long as it is accepted that justice can require the provision of certain goods by the state, it should be possible to fill in the details of the link between science and these goods according to the requirements of different theories of justice, while keeping the same basic structure for the argument.

Although this line of argument is plausible, it runs into a serious obstacle: the problem of indeterminacy.[200] It is almost impossible to predict the social impact and benefits from particular lines of basic scientific research. The indeterminacy operates at multiple levels: We cannot tell which research projects will deliver a significant

[200] Dworkin relies heavily on the argument from the indeterminacy of benefits in his discussion of subsidies for art. I think the problem is even greater in the case of basic science. See Dworkin, *A Matter of Principle,* p. 226.

breakthrough; what the social implications and uses of new knowledge will be; whether the costs of a project will be worth the eventual benefits; or whether in the end there will be net benefits on issues that can be plausibly tied to the requirements of justice.[201] The ever-present hazard is that large investments will turn out to be a waste of resources; or worse, that the harm and injustice that results from the application of some new research will outweigh any potential improvements to justice.

Those in favor of large amounts of public support for science typically cite examples of scientific research that has led to advances beyond anything the researchers or the funders could possibly envision at the early stages. The history of the research that eventually led to the development of the Internet is a typical illustration of this happy scenario.[202] The Department of Defense's Department of Advanced Research Projects Agency generously funded cutting edge research into two budding theoretical concepts that were at the forefront of computing science at the time: "packet-switching" and "a network of networks." Although the hope was that this might lead to the design of a communications network for the military, nobody had any idea what form this would take. The research was carried out by a group of innovators separated from the ordinary operations of the military and removed from the pressures of delivering an end product at a specific time. Neither the funders nor the scientists envisioned that this research would lead to a product

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[201] This conclusion is supported by research in science policy that has attempted to measure the productivity and social benefit of different lines of inquiry. See Neal et al., *Beyond Sputnik* for an overview of these studies.

[202] See Shane Greenstein, "Nurturing the Accumulation of Innovations," In *Accelerating Energy Innovation: Insights from Multiple Sectors*, Rebecca Henderson and Richard Newell eds. Chicago: University of Chicago Press, 2011 for a history of the development of the technology that became the Internet.

with non-military applicability or commercial potential – let alone one that would be so transformative.

But for every project that delivers such transformative results, many others will fail to deliver even the benefits that are reasonable to expect and will result in a waste of resources. The Superconducting Super Collider project and the big push to eradicate cancer in the 90s are both examples of expensive disappointments. The problem is that it remains difficult to predict in advance which projects will deliver – and when.[203]

That the results of scientific research are unpredictable is not an argument for or against public funding for science – although it is can be used to support both – but it poses a significant obstacle to justice-based justifications of funding. If the outcome of a particular funding decision cannot be predicted with any degree of confidence, it is difficult to maintain that it is nonetheless required by justice. Policies that are required by justice must be justifiable to those who will be bound by them and their justification must involve some evidence that the intended justice-enhancing results are likely to be attained through the proposed policy. Certainty cannot be expected of course, but it must be possible to give reasons about what outcomes can be expect from the policy. I argued in earlier chapters that the uncertainty of scientific research raises significant challenges to justifying policies on technical issues on purely on the basis of expertise. The uncertainty in funding decisions for basic science far exceeds the uncertainty in these specific policy proposals.

When the uncertainty is so great, it no longer makes sense to ask whether justice might require it; the relevant questions are how much risk a society would be

[203] Neal et al., *Beyond Sputnik*; Rebecca Henderson and Richard Newell eds., *Accelerating Energy Innovation: Insights from Multiple Sectors.* Chicago: University of Chicago Press, 2011.

willing to take to produce justice-enhancing outcomes and how much confidence citizens have in the policy's likelihood of delivering the desired results. These are effectively questions about how much faith citizens have in scientific research. Once the issue is framed in terms of collective risk-taking, it seems clear that the provision of less risky public goods that are more likely to produce intended justice-related outcomes should be prioritized over risky investments that may or may not even turn out to serve justice, insofar as justice is the goal. It can't be a requirement of justice that citizens take risky bets over safe ones – or that they have put their faith in science. This also suggests that the justice argument would be more likely to lend support for applied research with more certain short-term benefits over basic research that is uncertain and might only deliver results in the long run. It is very difficult for funding for basic research to overcome the indeterminacy objection.

The alternative to justifying funding for basic scientific research as a requirement of justice is to justify it as a discretionary public good: not required by justice, but desired or valued by citizens. There are several different grounds on which the value of science can be defended; some are instrumental and some intrinsic. First, there are the material benefits mentioned above: the trickle-down from abstract scientific research can lead to improvements in all areas of life, from improved health to agricultural productivity, better air quality to more efficient transportation, to breakthroughs in computing that can improve the living standards for everyone around the world. Although the indeterminacy problem persists, it does not pose the same kind of obstacle for the justification of discretionary goods as it does for goods required by justice. That a majority of citizens want a good and believe a policy will deliver it is enough to justify the provision of discretionary goods. This, after all, is a key feature that distinguishes the two categories: discretionary goods are justified exclusively on the basis of the preferences and values of the majority of citizens, whereas goods required by justice must meet a higher standard of justification grounded in a theory of justice.

A second line of argument for scientific research would focus on the political benefits. Scientific knowledge can improve the quality of policy-making, help create a more informed citizenry and raise the quality of public discourse.[204] Science can bring new issues or problems to the attention of the public, either by pointing out a problem people didn't know they were facing, such as in the case of climate change, or by making new discoveries that demand attention because of the political or moral issues they raise, as in the disputes over stem cell research and biomedical technologies. It can also improve the quality of policies by providing useful information on complicated technical matters, such as on issues of public health or environmental quality. A shared interest in the quality and effectiveness of policies can be another way to defend the value of public funding for science.

Finally, citizens might value scientific knowledge for its own sake. Satisfying our curiosity and deepening our understanding of the world around us is valuable in itself. A society might be a better one simply because its citizens have a deeper understanding of the world and dedication to freely pursuing the unknown. Furthermore, the cultivation of curious individuals who are dedicated to the pursuit of truth may be necessary for the realization of all of the practical benefits of science mentioned above. The free pursuit of knowledge develops the cognitive powers of

[204] Pragmatists have always emphasized this point. See, most obviously, John Dewey, *The Public and Its Problems*. Chicago, IL: The Swallow Press, 1927. More recently, Cheryl Misak, *Truth, Politics, Morality: Pragmatism and Deliberation.* New York, NY: Routledge, 2000; and Robert Talisse, *A Pragmatist Philosophy of Democracy.* New York, NY: Routledge, 2007 have made similar arguments.

individuals and sustains a vibrant intellectual atmosphere of discovery and experimentation in which further research can continue and flourish.

These are some of the possible justifications for supporting science with taxpayer money. The next section turns to the historical reasons given to justify the vast system of public funding for science put in place in the United States after World War II and examines their philosophical basis.

III. Autonomy, Dissent and Scientific Progress

At the end of World War II, President Roosevelt asked Vannevar Bush to produce a report outlining a new vision for how the government might support scientific research in the postwar period.[205] The report that Bush produced in response – *Science: The Endless Frontier* – became the single most influential document imagining the role of science in a large modern democracy.[206] Bush's argument and the structure of public support for science erected largely following its recommendations had two key features. The first was the justification of public support for science almost entirely on the basis of instrumental benefits. "Advances in science when put to practical use mean more jobs, higher wages, shorter hours, more abundant crops, more leisure for recreation, for study, for learning how to live without the deadening drudgery which has been the burden of the common man for ages

[205] For more background on the debates around science in this period see Daniel J. Kevles, *The Physicists: The History of a Scientific Community in Modern America*. New York, NY: Knopf, 1978; James D. Savage, *Funding Science in America: Congress, Universities, and the Politics of the Academic Pork Barrel*. Cambridge: Cambridge University Press, 1999; Daniel L. Kleinman, *Politics on the Endless Frontier: Postwar Research Policy in the United States.* Durham, NC: Duke University Press, 1995; Alfred K. Mann, *For Better or For Worse: The Marriage of Science and Government in the United States*. New York, NY: Columbia University Press, 2000.

[206] Kleinman, *Politics on the Endless Frontier.*

past,"[207] Bush wrote. "Advances in science will also bring higher standards of living, will lead to the prevention or cure of diseases, will promote conservation of our limited national resources, and will assure means of defense against aggression."

This was a clever strategy to address the dilemma facing science at the end of the war. The American public appreciated the role that science and scientists had played in wining the war, but lacked a clear non-military vision that could justify continuing to spend large amounts of public funds on abstract scientific research. At the same time, the continuation of large amounts of public funding had become more crucial than ever for scientists because military investment in basic research during the war had rendered cutting edge science dependent on large amounts of money. Bush's challenge was to come up with a persuasive narrative for what science could do to improve the lives of ordinary citizens that would ensure continued investment in basic research.

The second key tenet of the report was the necessity of granting scientists a high degree of autonomy from political processes, including giving them control over the distribution of public funds for science.[208] Bush claimed that the public would benefit most from science if scientists were left free to pursue abstract research into areas that interested them. "Scientific progress on a broad front results from the free play of free intellects, working on subjects of their own choice, in the manner dictated by their curiosity for exploration of the unknown,"[209] Bush wrote. Politicians might decide on broad issue areas to be given priority based on the interests and needs of the

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[207] Vannevar Bush, *Science: The Endless Frontier.* Washington, D.C.: National Science Foundation, 1945.

[208] See Douglas, *Science, Policy and the Value-Free Ideal*; and Philip Kitcher, *Science, Truth and Democracy* for more on this ideal.

[209] Bush, *Science: The Endless Frontier.*

public, but scientists would determine the allocation of funds among particular research topics, without political considerations.

The report did not provide much evidence of the link between basic research and improvements in the quality of life for ordinary citizens, nor did it spell out why the greatest public benefits would be realized this way. Citizens were asked to take these on faith. Given the high levels of trust in science and scientists after the war, this did not prove difficult. But since this report effectively shaped the structure of science funding for the next half century and since public trust in science has fluctuated over the course of this period, it is worth looking beyond its persuasiveness at the time and analyzing the argument on its merits. This will require imagining what might fill in the blanks in Bush's argument.

What could justify giving scientists a high degree of autonomy over the allocation of public funds for research projects? The answer depends on the specific goals that will guide decisions for allocation and the principles that should be used to realize them. One possibility is to aim for public benefit as the ultimate goal and give funds to projects more likely to realize it. Under this approach, giving autonomy to scientists over funding decisions would have to be justified at least in part on the grounds that scientists are the best judges of what kind of research is most likely to benefit the public. Scientists will be more qualified to determine areas of inquiry that are likely to prove more fruitful and the benefits that can be expected from different projects, so this justification is partly right. But scientific qualification alone cannot be enough to fully justify autonomy because scientists making funding decisions will also need to make assumptions about what the public interest is in order to decide what research would serve it best. The superior knowledge of experts will not be sufficient to give them the authority to decide on what the public interest is. This line of argument would have to conclude that funding decisions must involve at least some democratic input from those with a legitimate claim to representing the public interest.

The second possible approach to allocating funding is to focus solely on the scientific merits of competing proposals without even considering public benefit as a factor at the stage of funding decisions. Scientists would evaluate projects on the likelihood of solving an important puzzle, contributing to scientific progress, stimulating further research or opening up new possibilities for future inquiry. The underlying assumption of this approach is that public benefits would follow if scientists pursued the most important scientific advances. While the first model awards funds to projects that can demonstrate how they might advance the interests of society, the second mimics Adam Smith's invisible hand argument: scientists pursuing their interests and curiosity produce the most important breakthroughs, which in turn, produce the greatest public benefits downstream.

In practice, the motives of pursuing scientific success and pursuing public benefit may not be sharply distinguishable at the level of individual scientists or projects, but the two models have different implications for the amount of autonomy the scientific community should be given over funding decisions if the goal is to produce public benefit. While the first model requires some democratic input into funding decisions to ensure that public funds are not distributed solely on the basis of the political or moral values of the scientists on funding committees, the invisible hand model justifies leaving funding decisions entirely to scientists.

It seems clear that Bush had the second scenario in mind, but it is not clear what evidence there was to support the claim that the invisible hand argument would be valid for science. It was impossible for Bush to provide empirical support since no system of public funding for basic research of a comparable scale had ever been tried before. Scholars of innovation policy today point out that it remains extremely difficult to measure the value and impact of basic research on society.[210] Many of the benefits from science are quite abstract, difficult to quantify and valued differently by different people. It is also difficult to trace spillover effects to their origin in particular research projects, especially since most scientific advances that benefit the public are the result of multiple discoveries and innovations. Finally, even if it were possible to measure the benefits under this system, it would be impossible to say whether public funds would have been used more productively in a more targeted scheme of public funding, given the lack of a counterfactual history of the development of scientific research in the past decades.[211]

There may not have been systematic evidence about the progress of science under different funding schemes, but there were influential philosophical arguments about scientific progress that had clear implications for the distribution of funds. Bush's claim that the greatest scientific progress would be made if scientists were free to pursue their interests closely followed a theory of scientific progress developed by the scientist-turned-philosopher Michael Polanyi.

In a series of papers in the 1940s, Polanyi traced scientific progress to the activities of a community of scientists sharing methods and standards, left free from political interference.[212] He developed the argument in later work through an implied analogy between scientists and actors in a free market economy.

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[210] Neal et al. *Beyond Sputnik*; Henderson and Newell eds. *Accelerating Energy Innovation.*

[211] Neal et al. *Beyond Sputnik.*

[212] See e.g. Michael Polanyi, "The Growth of Thought in Society." *Economica,* 8 (32), 1941; Polanyi, "The Autonomy of Science," *The Scientific Monthly,* 60, 1945; Polanyi, "The Planning of Science." *The Political Quarterly,* 16 (4), 1945; Polanyi, "Cultural Significance of Science." *Nature,* 147 (3717), 1941. More developed versions of this views can be found in

"[The] self-co-ordination of independent initiatives leads to a joint result which is unpremeditated by any of those who bring it about. Their co-ordination is guided as by 'an invisible hand' towards the joint discovery of a hidden system of things. Since Its end-result is unknown, this kind of co-operation can only advance stepwise, and the total performance will be the best possible if each consecutive step is decided upon by the person most competent to do so."[213]

There were two key epistemic points supporting this argument. The first was the indeterminacy argument mentioned earlier. Polanyi maintained that it was impossible to predict where the most significant scientific advances would come from. Any attempt by a funding committee to direct the course of science toward a specific purpose would fail because of this limitation. Note the striking similarity between this argument and Hayek's argument against central planning on the basis of informational limitations.[214] Just as Hayek argued that the insurmountable information problem facing central planning showed the futility of government interventions with the economy, Polanyi argued that the indeterminacy of science meant that government interference with funding decisions would be pointless. His alternative was to leave it all to experts: "So long as each allocation follows the guidance of scientific opinion, by giving preference to the most promising scientists and subjects, the distribution of grants will automatically yield the maximum advantage for the advancement of science as a whole."[215]

The second point that held up the argument was a cumulative view of scientific progress. Polanyi subscribed to the traditional view of science moving incrementally toward a complete picture of the truth. He compared the scientific

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later works such as Polanyi, *The Logic of Liberty: Reflections and Rejoinders*. Chicago, IL: University of Chicago Press, 1951; and Polanyi, "The Republic of Science: Its Political and Economic Theory." *Minerva,* 1 (1), 1962.

213 Polanyi, "The Republic of Science."

214 Friedrich August Hayek, "The Use of Knowledge in Society." *The American Economic Review,* 1945.

215 Polanyi, "The Republic of Science."

enterprise to a giant jigsaw puzzle, with each scientist carefully watching the moves of others in order to make the new moves that became possible as a result of earlier ones.[216] This account assumes a fundamental unity in science, such that all research fits together to form a coherent whole, which corresponds to the truth about the laws of the universe. The scientist's selection of research questions is thus not really an open choice: many of the scientist's problems are given by earlier work in the area and the gaps in existing knowledge. The significance of a research agenda comes from the role of the particular finding in filling out the missing pieces in the puzzle and contributing to its completion.[217] Science benefits society simply by making rapid progress in completing the puzzle.

The indeterminacy argument is hard to dispute. But the view of scientific change as a linear and cumulative progress toward truth was radically challenged by Kuhn. In *The Structure of Scientific Revolutions,* Kuhn developed an alternative model that replaced the single unified picture of truth with multiple competing paradigms that offer different and incommensurable descriptions of the world.[218] The scientist chooses what counts as significant based on considerations about what best advances a particular scientific paradigm. The puzzles of scientists are significant only relative to the methods, standards and accumulated knowledge of the paradigm, rather than to an external standard of truth.

The important consequence of this move was to undermine the intrinsic significance of the puzzles of scientists. The esoteric puzzle-solving activity of scientists is still essential to significant scientific discoveries, but the relationship

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[216] Polanyi, "The Republic of Science."

[217] See John Dupré, "Science and Values and Values in Science," *Inquiry,* 47, 2004 for more on this philosophical position and its weaknesses. See also Kitcher, *Science, Truth and Democracy* for a discussion of these rival views of science.

[218] Kuhn, *The Structure of Scientific Revolutions.*

between ordinary puzzles and big discoveries is more complicated. Polanyi provided a linear account of scientific progress, in which each problem solved was a small step toward the truth. Kuhn replaced this with a view of scientific progress marked by alternating periods of stability and rupture. The periods of stability, which Kuhn called normal science, closely resemble Polanyi's image of scientific activity. However, the most radical and significant discoveries take place in periods of breakdown and instability, when a paradigm fails to solve important puzzles and is replaced by a new one in a scientific revolution. Normal science prepares the conditions that make the revolution possible, but the revolution itself is unintended and strongly resisted by practitioners of the old paradigm.

Although Kuhn challenged Polanyi's account of scientific progress, he nonetheless supported Polanyi's view that scientists must be left to pursue their interests, rather than encouraged to address social and political priorities.

"The insulation of the scientific community from society permits the individual scientist to concentrate his attention upon problems that he has good reason to believe he will be able to solve. Unlike the engineer, and many doctors, and most theologians, the scientist need not choose problems because they urgently need solution and without regard for the tools available to solve them," Kuhn wrote.[219]

Science moves forward from revolution to revolution when scientists are left alone to pursue their seemingly unimportant problems without interference. Paradoxically, the necessary condition for radical and creative novelty was a hierarchical, elitist and fairly conservative scientific community.

The problem with this argument was that it took for granted the continued emergence of alternative views capable of precipitating a revolution, but did not provide any reason to believe this would be true. Normal science is structured precisely to extinguish this possibility: Kuhn claimed that scientists working in a

219 Kuhn, *The Structure of Scientific Revolutions,* p. 164.

paradigm would do everything in their power to resist threats to their paradigm. It is therefore curious that he did not consider the possibility that they might succeed only too well and end up extinguishing radical innovation. The risk that normal science might stifle innovation is even more serious under a system of scientific research that depends heavily on the availability of funding. If scientists working in paradigm have a high degree of autonomy over the distribution of funds, then the easiest way to defend a paradigm is by funding projects that develop the paradigm and not funding those that challenge it radically.

One of the most original contributions of Kuhn's *Structure* was to shift attention away from the lone individual following the scientific method and onto the dynamics of a community of scientists working with shared and unquestioned standards, norms and assumptions. Yet Kuhn's famous examples of scientific revolutions were all drawn from periods that preceded the emergence of a highly professionalized community of scientists with shared and strongly enforced norms. Innovators such as Galileo, Lavoisier and Einstein, who overturned established scientific consensus emerged in scientific contexts without a professional community with institutional tools for resisting new ideas.

This mismatch between his examples and his conclusions makes it difficult to share Kuhn's faith that a closed community of specialists entrusted with complete control over funding decisions will continue to produce radical ideas that undermine their own shared assumptions and findings. The argument for giving autonomy to scientists over funding decisions runs into difficulty when we consider the possibility that the process might develop in ways that would prevent rather than encourage the free pursuit of ideas and the emergence of significant discoveries. This was a key point of Feyerabend's critique of Kuhn. Feyerabend worried that Kuhn's theory tried to excuse and encourage the most dogmatic new trends in postwar science.[220] He argued instead that the dogmatic tendencies of normal science had to be countered actively by encouraging – that is, funding – new and imaginative alternatives in order to ensure the continuation of innovative discoveries. If a highly autonomous scientific community uses control over funding to resist new ideas that might threaten existing paradigms, outside intervention may be necessary to ensure the continued possibility of radical challenges to dominant paradigms. To make another analogy with economics, this can be thought of as antitrust for science funding.

IV. Politicized Science

I have so far focused on arguments for public funding of basic research, which typically does not have social, political or economic impact in the short run. I suggested that outside intervention with funding could be necessary to ensure the continuation of important discoveries, given the difficulty of radical dissent under a funding structure that favors dominant paradigms. In this section, I will focus on funding for scientific research on issues that have already become the subject of political controversy. Some recent examples of such politicized scientific issues include research into stem cells, genetic enhancement, AIDS, climate change or nuclear weapons. In all of these cases, the results of earlier research are widely known and further expertise will be directly relevant to political debates and decision-making.[221]

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[220] Feyerabend, "Consolations for the Specialist." See also Godfrey-Smith, *Theory and Reality.*

[221] It is more common to make a distinction between pure and applied research, but this is problematic for various reasons, as pointed out in Kitcher, *Science, Truth and Democracy* and also Roger Shattuck, *Forbidden Knowledge*, New York, NY: St Martin's Press, 1996 among others. It is difficult to pinpoint exactly when an issue becomes politicized, but I think it is relatively obvious when it happens.

Funding for these issues merits separate treatment for a couple of reasons. First, there will be less uncertainty about what new research can produce, which means that these funding decisions can be more goal-directed. These decisions will be about what kinds of knowledge a society needs or wants the most for policy purposes in the short-term, rather than aiming for long-term scientific progress. Secondly, there will already be a high level of public interest and discourse around these issues, which means that the stakes for different groups will be both clearer and higher. These issues are typically marked by widespread coverage in the media, lobbying efforts by interest groups and NGOs, activism and even social movements. New scientific research will be heard by a wider audience and will provoke a reaction from the public. People will be interested, concerned, hopeful, fearful, anxious or relieved as new knowledge becomes available. I cannot provide a theory of which issues end up becoming politicized and why, but the most vivid recent issues have involved science that has threatened long-held worldviews or ways of life, demanded a significant transformation of habits, practices and values, or required sacrifices from a large number of people.[222]

In the case of basic research, it is important to ensure that public funding continues to produce significant discoveries that fulfill the promise of benefiting the public. Since a closed community of scientists sharing a paradigm may end up extinguishing truly radical views if they also control the allocation of funding, outside intervention may be necessary to ensure support for dissenting views. Beyond this general point, the choice among competing projects could be left to scientists working

[222] In many ways, these issues are similar to what Liora Salter has called "mandated science," which she defines as science produced specifically to address a political need. A fundamental difference is that she focuses only on science produced in and for government agencies, whereas I look more broadly at science produced in independent research institutes, universities and so on. See Salter, *Mandated Science: Science and Scientists in the Making of Standards.* Dordrecht: Kluwer Academic Publishers, 1988.

in the same area, both because of their superior knowledge of the scientific merits of proposals and because the public will not be particularly affected by or interested in the details of basic science until it begins to have social consequences. Politicized scientific issues are different: the public is directly and often significantly affected by further scientific developments and it is more informed about the science. The question I will address in this section is how much autonomy for scientists is appropriate on these funding decisions and on what grounds arguments for political intervention might be justified.

Funding strategies for policy-relevant issues must also be justified on the grounds of public benefit, much like basic research, but what constitutes public benefit will be different here. Section II listed three main categories of benefit from scientific research: material benefits from spillover effects, political benefits from more informed policy-making and intrinsic benefits – knowledge for the sake of knowledge. While basic research has historically been justified almost exclusively on the basis of the first of these, funding for politicized scientific issues must be justified at least in part on the grounds of political benefits.

A standard argument about the role of scientific expertise in politics is that knowledge will enhance the quality of democracy. Although the claim is ubiquitous, people are not always precise about what they mean by this.[223] One possibility is simply that more expertise leads to better outcomes, such as higher quality of life, increased wealth and prosperity, better environmental quality, less inequality and so on, which are inherently desirable. But this is not enough to explain why we should

[223] The claim (or the assumption) is virtually everywhere, from democratic theory to comparative politics, from international relations to law. But for a recent work specifically discussing this issue, see Robert Post, *Democracy, Expertise, Academic Freedom: A First Amendment Jurisprudence for the Modern State*. New Haven, CT: Yale University Press, 2012.

say that democracy is enhanced when these benefits are realized. After all, if we hand over all decision-making power to experts for the sake of better results, we would not call this democracy enhancing, although it might lead to even better results. Not just any good results that take place in a democracy can be called democracy enhancing; how the results come about is also crucial. The claim that expertise can be democracy enhancing is more meaningful if we focus on the ways in which expertise enhances the possibility for democratic self-rule. This will be related to good results, but not reducible entirely to them. Good results that come about by handing over decision-making power fully to experts or those that come about through luck would not qualify as democracy enhancing.

Democratic governance requires both that decisions be made through the appropriate political channels and also that these decisions lead to a reasonable degree of effective control over the results. Neither a society where crucial decisions are left to experts, nor one where decisions persistently fail to bring about the intended consequences can meaningfully be called self-governing. Good quality knowledge is important because it allows a society to make decisions that have a higher chance of producing the desired results. But it is equally important that the knowledge available furthers collectively established goals. Expertise will enhance democracy to the extent that it strengthens a society's ability to determine its future through its own decisions and will constrain it insofar as new knowledge thwarts democratic goals and aspirations. Whether the relationship between expertise and democracy will be a productive one depends as much on the kind of expertise that becomes available as it does on the quality and reliability of the knowledge. It cannot simply be assumed that more knowledge will always mean better democracy.

This observation has direct implications for how decisions about science funding on policy-relevant issues should be made. If the quality of expertise were the only relevant consideration, it would be acceptable to leave decisions about science funding entirely to experts. But since the kinds of knowledge that become available is also important, we need to think about how the distribution of funds could be structured to produce expertise that is democracy-enhancing, rather than democracy-constraining. In the rest of this chapter, I will propose three guiding principles for how democratic considerations should shape funding decisions for science in cases where the political implications have become clearer: The first should be rather uncontroversial; the second follows directly from arguments about democratic engagement over science developed in Chapters 1 and Chapter 3, while the third introduces a new and more provocative possibility.

First, given the scarcity of resources, trade-offs between different areas of science will be inevitable. Even if there are many scientific issues that are highly salient for policy purposes, it will be necessary to set priorities. More funding for one kind of scientific knowledge will mean less of another. Priority-setting questions, such as whether more funds should be dedicated to biomedical research or environmental studies, space exploration or oceanography, are closely tied to the values and preferences of a society. They are analogous to fundamentally political questions about how to distribute funds between education and healthcare, national defense and national parks. Since science is supported by public funds for the purpose of public benefit, and since the force of the indeterminacy argument is weaker in the case of policy-relevant science, the claim that the priorities of the scientific research agenda should be set democratically should not be controversial. Expert opinion on the likelihood of making significant progress in these scientific areas will be relevant to the decision, of course, but in the end, the ordering of priorities must be made with democratic input. And indeed, the current practice is to shape priorities for science funding on the basis of national political priorities.

It is common to draw the line for political input into science funding at this general level of priority setting, leaving the distribution of funds within each area to scientists. This is inadequate because it leaves the determination of how a particular issue will be considered in the public sphere and the alternatives that will be available to decision-makers entirely to experts. While formal decision-making power may still lie with political actors, their options are constrained by the funding decisions made earlier by scientists. As Schattschneider famously pointed out, "The definition of alternatives is the supreme instrument of power."[224] Those who make funding decisions can intentionally or unintentionally rule out certain courses of action.

The distribution of funds within an area has different political implications than priority setting at a more general level. While priority setting determines which problems will gain more traction, the distribution of funds within an area determines the range of possible answers to a particular problem. At the decision stage, laypeople can either accept one of the available scientific options, or they can reject them all, but they cannot produce new science. They have to work with the options available. This is the serious limitation of studying the relationship between science and politics by focusing only at the decision stage. In Chapters 1 and 3, I argued that public contestation between a wide range of competing alternative views is crucial for enabling citizens and politicians to examine the assumptions and weaknesses of

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[224] Elmer E. Schattschneider, *The Semi-Sovereign People*. New York, NY: Holt, Rinehalt and Winston, 1960.

different alternatives. This argument presupposes the availability of sufficiently different alternative view on the same question.

Under a system where most scientific research depends on large amounts of funding, such diversity cannot be taken for granted. Its possibility will depend not only on the absence of constraints on free inquiry, but also on an active strategy of funding that supports dissenting views and diversifies the distribution of funds among a wide variety of approaches. To leave the decision entirely to scientists' assessment of quality can deprive us of the new ideas that can challenge accepted ways of thinking and expose the errors of widely accepted views. A science court where experts defending different views examine each other will not work well unless a sufficient number of alternative approaches have enjoyed comparable levels of funding. Feyerabend's dilettantes cannot compete with organized scientific research that is backed with large amounts of funds. A commitment to robust public debate about science requires a funding strategy of supporting dissenting views and encouraging different approaches.

Priority setting and supporting dissent are two possible ways science funding decisions can be used to ensure that expertise will further democratic goals. Neither of these, however, challenges the "more knowledge, better democracy" assumption mentioned above. A more profound challenge to the conventional way of thinking about the relationship between expertise and democracy is to suggest that we might be better off by having less of certain kinds of knowledge.

We cannot assume that new knowledge is always beneficial for us. As anyone who has received bad news from the doctor, or who has been deceived by a trusted friend knows, there are many pieces of knowledge that are unpleasant and painful. It is a common view that we are always better off facing the truth rather than living in illusion, regardless of how painful the truth might be, but it is not clear why this should be true. One possible answer is that we are always better off having the truth because of the intrinsic value of truth, or of the intrinsic value of being the sort of person who courageously faces up to the truth, however painful it may be. But not everyone will place such a high value on truth or on the virtue of facing it. Perhaps more persuasive is the answer that truth is also valuable instrumentally: true beliefs help us get on better with our lives and enable us to attain other things that we value. But if we value truth instrumentally, then in cases where the truth does not enable us to get on better with our lives, where it turns out to be a burden or a threat and to cause more harm than benefit, we have to admit that we will be better off not having the truth.

The instrumental approach allows us to accept that the truth is valuable as a rule, while denying that we are better off with any and all truths. We need not be and often are not truth maximizers in all circumstances. The moral and psychological costs of having the truth may sometimes outweigh any instrumental value of having it. If a certain kind of knowledge creates great fear or anxiety, if it renders a person unable to act, or if it dramatically changes the ways in which it is possible for her to live, the person may be better off not seeking out this truth. There is an important difference between having false beliefs, which are likely to make us run into difficulties in attaining our goals in life and lead to a series of other false beliefs, and simply not investigating certain matters, or not asking certain questions. As Bernard Williams has put it, "While every belief I have ought ideally to be true, it is not the case that every truth ought ideally to be something I believe. Belief aims at truth, knowledge does not aim at completeness."[225]

[225] Williams, "Deciding to Believe."

The possibility that controlling what knowledge is produced may be just as important for a democracy as simply having more knowledge is rarely considered, but that a similar kind of control over knowledge can enhance individual autonomy is now widely accepted in the medical in the medical case, so it is worth taking a closer look at this case.

There is now a widely recognized right to for patients to refuse certain kinds of genetic tests or to decline being informed about incidental findings that physicians may discover while testing for other things.[226] One way to justify this right is based purely on the right to personal self-determination or autonomy. An individual has the right to make her own choice about the information she receives, regardless of what the physician advises and whether doing so would be in her interest. If there are easy and available treatment options, the exercise of the right not to know a diagnosis can effectively be the same thing as a right to harm oneself. Although it might be important to recognize this right, this is not the most useful or compelling line of argument.

More important is the possibility that there would be little benefit from knowledge, at least on some conception of what constitutes benefit. Respecting autonomy requires that the assessment of whether knowing or not knowing should be considered a benefit be left to the patient herself. Many people think there isn't much benefit from knowing the results of genetic tests for diseases such as Huntington's or dementia for which there is no known cure. Similarly, a person already battling a serious disease might think that knowledge of yet another disease would just be a burden to themselves and their relatives. In such cases, not knowing, or at least, having the right to choose whether to acquire the information or not, can be

[226] Susan Wolf et al. "Point-Counterpoint: Patient Autonomy and Incidental Findings in Clinical Genomics." *Science*, 340 (6136), 2013.

autonomy-enhancing because it allows the person the possibility of carrying on with her life without being weighed down by painful knowledge. The "right not to know" recognizes that knowledge may not always be beneficial for an individual, especially in cases where there isn't much to be done to improve the situation and there is significant psychological burden from the knowledge.[227]

The other point is that regardless of how someone else – e.g. a physician – may assess the benefit to the patient of having the truth based on whatever standard of well-being, the patient knows best what will in fact be good for her. Of course, the physician can give the patient advice about the matter and try to persuade the patient of the benefits of the knowledge if she thinks knowledge is in the patient's interest, but the ultimate decision about whether or not to receive the information in question should be left to the patient.

These reflect two different kinds of reasons why we value autonomy.[228] The first is that it is valuable for a person to live by choices that she makes for herself, regardless of whether they are good or bad choices. The second is that an individual is often best placed to know what is good for her, including the standards by which goodness should be judged. A subjectivist would go further to claim that what a person thinks is best for her *constitutes* what's best for her, but even if we think that goes too far and that there are some objective criteria, we can still plausibly maintain that an agent will generally know what is conducive to her well-being.

These arguments are typically taken to ground a right to decline treatment or to follow a treatment different than the one suggested by a physician. But the more

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[227] Thomas May and Ryan Spellecy, "Autonomy, Full Information, and Genetic Ignorance in Reproductive Medicine," *The Monist,* 89 (4), 2006.

[228] I draw on the conception of autonomy developed in Joel Feinberg, *The Moral Limits of the Criminal Law Volume 3: Harm to Self*. New York, NY: Oxford University Press, 1989.

interesting and novel idea here is that controlling the information one receives is another choice that can be essential to autonomy. New knowledge alters the options available to an agent. If this knowledge dramatically restricts the agent's options, the fact that she retains the right to make a choice may not be very significant because the decision may be forced by the new knowledge and its transformation of the option set. Some kinds of new knowledge can render a person incapable of pursuing projects very important to her. If an autonomous agent is defined as one who controls her own destiny and pursues projects of her own choosing,[229] then the kinds of knowledge that render an agent incapable of pursuing her projects will not be autonomy enhancing. Autonomy should therefore be understood as requiring not that a person has as much information as possible, but that she can exert some control over the kinds of knowledge that become available to her.

I want to suggest that democracies sometimes face very similar situations, at least in some extraordinary cases, where new scientific knowledge dramatically affects the lives of people, threatens the way they live and causes widespread worry, fear and anxiety. This can be because new findings threaten a deeply held belief or because they literally threaten physical destruction. These kinds of scientific findings can render certain choices impossible and shake the fundamental existence and self-understanding of a society. They can therefore be very difficult to accept, create a lot of pain and may even render the society unable to act, causing a sort of paralysis.

In such cases, it would be difficult to insist that expert knowledge nonetheless enhances democracy because democratic action is obviously hindered rather than enabled by the new knowledge. Democratic publics and politicians may respond to

[229] Feinberg, *The Moral Limits of the Criminal Law Volume 3* and Raz, *The Morality of Freedom* work with this general definition of autonomy. I think this is a widely accepted, minimal definition of the concept. Delving more deeply into different conceptions of autonomy would not be relevant here.

these cases by outright denial or by trying to find alternative, false experts that they can believe. Since it is not possible to undo or take back knowledge once it becomes known, these may appear to be the only viable alternatives to accepting the painful new knowledge.

Kari Norgaard's fascinating ethnography of climate change denial in a village in Norway documents this phenomenon. Norgaard demonstrates how the residents exhibited widespread denial of climate change, even as they witnessed its visible signs in the form of lost snow-cover and unseasonable plant growth.[230] Interestingly, they were not literally denying the science as many in the United States do; they demonstrated a clear grasp of the basic facts. Norgaard claims that they failed to respond to the information in an appropriate way, often refusing to talk about it, or resorting to humor or changing the subject when the conversation turned to it. They took no political action and did not even complain to officials. All of this was especially puzzling since the effects of climate change hurt the residents economically: the tourist ski season, which was an important source of income for the village, got shorter and shorter each year. Norgaard explains the villagers' strange response as a combination of four kinds of emotions caused by the new knowledge: fear, guilt, helplessness, and a crisis of identity. The residents did not see a feasible course of positive action on the basis of this knowledge; the bleak predictions of science that they had heard led only to these paralyzing emotions, which blocked their capacity for action. This case study provides support for the view that more information is not always empowering, even where there are clear incentives for taking action.

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[230] Kari Marie Norgaard. *Living in Denial: Climate Change, Emotions, and Everyday Life*. Cambridge, MA: MIT Press, 2011.

A conventional rationalist response is to dismiss these psychological reactions as irrational and to treat them as failings that must be corrected or overridden for the sake of rational policymaking. Even if a democracy can't be forced to hand over decision-making power to experts, the argument goes, it must be educated to accept their findings. Some go further and argue that precisely in cases like this, where the public becomes upset and emotional, allowing experts more authority based on their scientific analyses of risk is the way to overcome public irrationality.[231] This presents expert knowledge as a useful corrective to the irrationality of democracy.

There are two possible objections to this argument: the first is that even if these are signs of irrationality and ignorance, the public has a right to be as irrational and ignorant as it likes. This is analogous to saying that an autonomous individual has a right to harm herself. This may be true, but again, it is not the most appealing argument because most citizens do want the benefits from expertise, just as most patients want to be treated by their doctors and villagers in Norway want the snow-cover to last longer. There isn't much to be said in favor of a right to live in denial because sooner or later the illusion will collide with reality, with worse results for everyone.

The more interesting answer focuses on the argument that we value autonomy because an individual is better placed to know what is best for her. Democracy rests on the idea that a public may understand better what serves its needs and interests. A society will not consider it in its interest to have scientific knowledge that only brings them fear and paralysis. It is a mistake to treat the fear or paralysis of the public simply as a form of ignorance or weakness. Fear and resistance can be the symptoms of a need to protect things that are deeply important, such as values, identities and

[231] See Cass Sunstein, *Laws of Fear: Beyond the Precautionary Principle*. Cambridge, MA: Cambridge University Press, 2005.

ways of understanding the world. To dismiss these as irrationality is to wish away a real and difficult dilemma instead of trying to resolve it.[232] If democracy holds out the promise that people can live in light of their values and shape the world according to their needs and desires, then threats to these values, needs and desires cannot be dismissed; it is important to try make these compatible with new knowledge. This may not always be possible, but some level of control over the kinds of knowledge produced – and not produced – might at least reduce the tension. The argument in favor of expert knowledge is that it will allow a democracy to make better decisions. If new knowledge prevents democracy from being able to make decisions at all or force it into decisions it does not want, it would be counterproductive rather than enabling.

There is of course the possibility that the public will overcome its fear and manage to mobilize. Experts who think a certain kind of approach to the problem is best are free to try to persuade others that this is the best approach and must be pursued. This is similar to a physician's telling a patient her opinion about whether the patient should take a certain genetic test. The key point here is that the physician's opinion on this particular matter is no longer within the realm of his superior knowledge, but rather just another opinion about what would be good for the patient, just as a scientist's view about whether a certain approach to a problem would best enable democratic action is simply one opinion in public discourse among others.

I have argued that democracies should have some control over the production of knowledge and pointed out that an important reason for this is that new knowledge may not always be beneficial. Accepting this latter point raises another possibility: if

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[232] Dan Kahan et al. make a similar point from a more psychological perspective in "Fear of Democracy: A Cultural Evaluation of Sunstein on Risk," *Harvard Law Review,* 119, 2006. Their main argument is that people's reactions to new information cannot be understood simply through the lens of rational behavior and that we must also focus on cultural cognition.

knowledge does in fact appear likely to be harmful, for instance in cases where it is likely to cause widespread fear and panic, might it not be permissible, or even desirable, for experts to withhold the information from the public?

The answer requires distinguishing between two different dimensions of the issue: the harm dimension and the democratic dimension. We could accept that experts can reliably predict the harm, but reject that they have the right to make such a decision because it would be paternalistic.[233] Respecting democracy requires that no one make such an important decision about the well being of the public without its knowledge. As argued earlier, however, we can go further and also contest the claim that experts can determine when knowledge will be harmful because this falls outside their area of their superior knowledge and depends on the public's own assessment of what constitutes its interest. In short, experts withholding information would be problematic on both dimensions.

Although I have focused on a democracy's right to direct new research away from potentially harmful knowledge to more beneficial ones, this is not meant to suggest that we should remain ignorant simply because the truth seems likely to be uncomfortable or unpleasant. On the whole, ignorance might lead to more harm and worse consequences for everyone. Democracies do have the right to ignore knowledge and expertise, but this will not prove wise in the long run. What is important is to allow some democratic control over the kinds of knowledge that will be pursued, especially on high-stakes issues.

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[233] See Alvin Goldman, "Epistemic Paternalism: Communication Control in Law and Society," *The Journal of Philosophy*, 88 (3), 1991; and Kalle Grill and Sven Ove Hansson, "Epistemic Paternalism in Public Health," *Journal of Medical Ethics*, 31 (11), 2005 for more on epistemic paternalism.

The democratic input into scientific research that I have in mind is not a right to legislate truth, nor is it about censorship or the complete rejection of expertise. It consists in more direct input into which approaches should be investigated further, what kinds of solutions should be prioritized, where research effort is directed and generally what kinds of new knowledge we want more of and of what less. For instance, in the case of climate change, it can involve a decision about whether we want to focus research efforts on adaptation strategies rather than mitigation. In the case of cancer, it might mean deciding how much to spend on finding out more about prevention vs. on treatment or adaptation. An example of this sort of input was seen during the AIDS epidemic, when activists insisted on directing funding toward research that would prioritize quick drug production over more rigorous controlled experiments that might generate better data and help with the long-term scientific understanding of the disease, but fail to give many sick patients access to potentially helpful treatments in the short run.[234]

These kinds of interventions are essential to a democracy's right to shape the world around it through its own decisions, while acknowledging its dependence on expert knowledge. More democratic input into science would allow a democracy to use knowledge to enhance its control over the world around it, thus enabling democratic action, rather than overriding the public's preferences, needs, emotions and values in the name of truth and knowledge.

Conclusion

There are many ways to defend studies of duck genitalia or exercising shrimp against those who mock them as useless. One could argue that basic science can have

[234] See Epstein, *Impure Science.*

benefits in the future that are impossible to predict in advance and that these benefits will only be realized if scientists pursue seemingly esoteric puzzles; or one could point to the concrete material benefits that are likely to accrue from these particular studies. The scientists responsible for these studies followed different strategies to defend their work. Patricia Brennan, author of the duck study, emphasized the importance of basic research,[235] while David Scholnick, who conducted the shrimp study, focused on the links between the health of marine organisms and the safety of the seafood that humans consume.[236]

But there was another widespread response to these attacks, which was to denounce political interference with science and call for Congress to leave science alone.[237] This chapter rejects the view that politics must not interfere with decisions about science funding. I have argued instead that science funding is necessarily political from the general decision to support science with public funds, to specific decisions about what kinds of science to support. This does not mean that majority vote should replace peer review; my goal in this chapter was to develop a specific view about what room there is for political input into funding decisions, building on a particular conception of the justification of state funding for science, a Kuhnian view of the role of autonomy and dissent in scientific progress, and a specific account of the relationship between expertise and democratic decision-making.

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[235] Patricia Brennan, "Why I Study Duck Genitalia," *Slate.com*, 2 April 2013, Web.

[236] David Scholnick, "How a $47 Shrimp Treadmill Became a $3-Million Political Plaything," *The Chronicle of Higher Education,* 13 November 2014, Web.

[237] See e.g. Larry Greenemeier, "What Makes Congress's Latest Effort to Curb Science Funding So Dangerous?" *Scientific American,* 8 May 2014. For a discussion of the dangers of this, see Steven Epstein, "The New Attack on Sexuality Research: Morality and the Politics of Knowledge Production," *Sexuality Research and Social Policy,* 3 (1), 2006 and Abby Kinchy and Daniel Kleinman, "Democratizing Science, Debating Values," *Dissent,* 52 (3), 2005.

Since the beginning of the twenty-first century, there has been a significant increase in the share of scientific research funded by corporations, philanthropists and private foundations.[238] While this may increase the benefits from science and expand its possibilities, it also gives rise to serious worries. Perhaps most significant is the worry that the interests, needs and political agendas of wealthy corporations and private donors will shape the knowledge landscape. This chapter has considered the possibility that the direction of knowledge could be determined either by experts or more democratically, but private funders also play an increasingly important role in determining what we know and which areas of knowledge become salient for society as whole. This also threatens to narrow the domain for political action and result in the circumvention of political decision-making on an increasing number of issues. If scientific research is conducted more and more at private institutions and with private money, then the people or organizations funding science can put into effect their own vision of the common good without having to go through the process of seeking the agreement of others and getting majority support.

The increasing role of private funding makes science one more area where a familiar set of concerns about the influence of money in an unequal society become relevant. Although the wealthy are free to fund their own science, which kinds of science gets funded privately will have implications for the distribution of public funding for science. If science that benefits a small segment of society is supported disproportionately through private funds, then it might be necessary to counterbalance the effects of private science through forms of redistributive public funding for science.

[238] William J. Broad "Billionaires with Big Ideas Are Privatizing Science," *The New York Times,* March 15, 2014, Web; Daniel Greenberg, *Science for Sale.* Chicago, IL: University of Chicago Press, 2007.

Chapter 5: The Limits of Free Inquiry

> *"You seek for knowledge and wisdom, as I once did; and I ardently hope that the gratification of your wishes may not be a serpent to sting you, as mine has been."*
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> Mary Shelley*, Frankenstein*

In the last chapter, I argued that democracies should have more control over new scientific knowledge produced with public funding. In this chapter, I turn to the question of whether a democracy may restrict certain lines of inquiry altogether on the basis that they pose a serious risk of harm to society. It is widely accepted today that research projects may be restricted if they pose harm to human subjects participating in the research process. Far more controversial is the suggestion that a project may be restricted on the grounds that the findings pose a risk of harm to society, even if the research is ethically conducted and the findings are true.

Two claims justify drawing a moral distinction between harm to subjects during the research and harm to society from the findings. The first is that knowledge is never intrinsically harmful and that harm only results when knowledge is used or abused by people with bad intentions. The second is that it is not permissible to restrict the activities of scientists for harms that other people inflict using their findings. We can hold scientists responsible for the harms that they intentionally inflict on participating subjects, but nothing more.

I will argue that even if we grant the first claim, we should reject the second. In the first section, I will defend a more robust understanding of responsibility that is sensitive to the context in which scientific research takes place and that involves assigning scientists some responsibility for the foreseeable consequences of their research, even if they themselves neither inflict nor intend harm. I will argue that in cases where scientific research is likely to lead to significant harm to a large of number of people, a democratic society would be justified in preventing the research from going forward. In the second section, I will shift the focus from the magnitude of the harm to the specific people affected. I will argue that in cases where a line of research is likely to disproportionately harm marginalized groups and reinforce existing inequalities and discrimination, a democracy's commitment to ensuring the equal standing of citizens would justify limiting scientific inquiry. I will focus on the example of research in sociobiology on the differences in intelligence between races and genders to make this case.

The most serious objection to the argument that a democracy may place some limits on scientific research based on considerations of harm is on the grounds of freedom of inquiry. In the third and last section, I will consider two different justfications for protecting freedom of inquiry and show that both can accommodate the argument that we might sometimes limit inquiry if it poses a significant risk of harm.

The purpose of this chapter is not to develop a full ethical theory that can settle which scientific projects may or must be restricted in a democratic society. It is, rather, to deveop philosophical support for some principles that can guide democratic deliberation in situations where the benefits from scientific knowledge and the value of freedom of inquiry must be weighed against the need to protect society from potential harms that may come from scientific findings in non-ideal social and political contexts.

I.

There are two different instruments that a state might use to constrain scientific research: the first is outright prohibition, the second is withdrawal of public funding. Although the first appears to be a more serious constraint on liberty, the two can come to the same thing in practice in a society where most or all scientific research is publicly funded. If a significant portion of research is privately funded, on the other hand, the withdrawal of public funds without a prohibition will mean that those who have money or those who can find wealthy sponsors are free to pursue research, while the rest are not. This inequality is difficult to justify,[239] except when what is objectionable is specifically the public supporting a certain line of research, perhaps due to a concern about the expressive meaning of the act of public support, rather than the research being undertaken at all. Since private funding for science is still a relatively small part of the total funding for scientific research and an even smaller part of the funding for basic research,[240] I will assume for the rest of this chapter that the withdrawal of public funds from a research project will have a similarly coercive effect as an outright ban.

The idea of a democracy limiting science may sound problematic at first, but in reality, there are already significant constraints on what scientists can and cannot do. The most important constraint is on the grounds of harm: it is widely accepted that it is permissible to limit the freedom of science to prevent harm to others. The problem, of course, is in clarifying what consitutes harm and what kinds of harms can be grounds for interference.

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[239] This was a major problem with the Bush administration's decision to withhold funding from embryonic stem cell research without banning it. The policy meant that private fertility clinics could carry on their research while scientists dependent on federal funding could not. Michael Sandel argues that this position was contradictory: If harvesting embryonic stem cells really were tantamount to murder, then surely denying funding without banning could not be a morally defensible position. See Sandel, *The Case against Perfection*. Cambridge, MA: Harvard University Press, 2007.

[240] Greenberg, *Science for Sale*.

The rule in place today is simple: scientific projects that pose harm to human subjects during the research process are not allowed.[241] In order to prevent potentially harmful research before it happens, rather than punishing offenders after the fact, all research conducted at research institutions receiving federal funding that proposes to use human subjects must be approved in advance by an Institutional Review Board.[242] This is no trivial restriction of the kinds of things we may know: Many areas of potentially beneficial medical research, for instance, are off the table because they are impossible to conduct without testing dangerous experimental treatments on human subjects. This rule was put in place in the 1960s explicitly in response to the atrocities of the Nazi experiments and the Tuskegee syphilis experiments in order to prevent anything similar from happening again.[243] Although these restrictions on science have not always been in place, they have come to seem obvious today.

The way the boundary for impermissible research is drawn under the current structure focuses almost exclusively on harms during the research process, rather than possible harms from findings.[244] It is much more controversial to suggest that science might be restricted on the basis of harms that might result from its findings, rather than harms inflicted during the research process. The question is, what makes the distinction between harms to research subjects and harms to the general public from

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241 The National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research, "The Belmont Report: Ethical Principles and Guidelines for the Protection of Human Subject of Research." The U.S. Department of Health and Human Services, April 18, 1979.

242 There is now an expanding literature on problems with IRBs. I won't enter into that debate here, but see Philip Hamburger, "IRB Licensing." In *Who's Afraid of Academic Freedom?* Akeel Bilgrami and Jonathan Cole eds. New York, NY: Columbia University Press, 2015 and Judith Jarvis Thomson et al. "Research on Human Subjects: Academic Freedom and the Institutional Review Board," *Academe,* 92 (5), 2006.

243 "The Belmont Report: Ethical Principles and Guidelines for the Protection of Human Subject of Research."

244 Douglas, *Science, Policy and the Value-Free Ideal.*

findings morally relevant, such that the former provides sufficient ground for preventing research from going forward, while the latter does not?

Two possible answers – that the harms inflicted on subjects are more certain or that they are of greater magnitude – fail to explain the distinction. We are reluctant to prohibit science even where it is clear that findings are likely to create a risk of harm greater in scale than any possible harm during the research process. IRBs are not asked to consider the impact that the results might have in a social context; they only consider risks associated with the process. And while the level of uncertainty about the risk is a relevant consideration to whether a project ought to be prevented, it applies equally to harm that might occur during and after the research and so does not distinguish between the two.

A better explanation of drawing the line this way can be found in the following line of reasoning: Knowledge itself cannot harm and harm results only from people using knowledge for harmful purposes. This makes it impermissible to prevent potential harms by interfering with the freedom of researchers rather than trying to restrain those who would use it to cause harm. It is permissible to prevent scientists only where they will directly harm others, but not otherwise. In the rest of this section, I will challenge this latter claim.

The view that knowledge cannot be intrinsically harmful does not deny that scientific discoveries may sometimes cause significant harm to the well-being and safety of human beings. There are many examples of science causing great harm, from the development of nuclear, chemical and biological weapons to the production of highly contagious viruses and novel pathogens.[245] The point is rather that in all of

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[245] Steve Keane, "The Case Against Blanket First Amendment Protection of Scientific Research: Articulating a More Limited Scope of Protection." *Stanford Law Review*, 59 (2), 2006.

these cases, it is not science that causes the harm; it is the people who use or misuse the science, most often in its application in technology. There is no such thing as a harmful piece of knowledge, but there are many possible harmful applications. Here is one clear statement of this position by a scientist: "Science tells us how the world is. That we are not at the centre of the universe is neither good nor bad, nor is the possibility that genes can influence our intelligence or our behavior. Dangers and ethical issues only arise when science is applied in technology."[246]

Let's assume that this view is correct. This by itself does not determine what should be done to prevent the harms that result from the applications of science. We need a further argument about what constitutes acceptable ways of dealing with harms, or more specifically, what makes it unacceptable to limit scientific research to deal with harms from its applications.

There are two important arguments that aim to do this, one about responsibility, the other about freedom of speech. The argument from responsibility is that a person cannot be held responsible for the harms that come about from how other people respond to their actions and that a person's actions cannot be limited to prevent others' wrongdoing. The argument from freedom is that the overriding value of freedom of speech and expression makes it undesirable or perhaps even impermissible to prevent inquiry on the grounds of harm, as long as scientists are not directly harming anyone. It is acceptable to interfere with harmful actions, but not with "harmful" speech, assuming that scientific research can be treated the same way as speech. I will leave this second claim to the last section.

The view that scientists cannot be held responsible for the harms that result from their findings, but can be held responsible for harms that they inflict themselves

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[246] Lewis Wolpert, "Is Science Dangerous?" In *Scientific Freedom*, Simona Giordano et al. eds. London: Bloomsbury, 2012.

stems from a particular view on moral responsibility, which holds that people are responsible for directly inflicting harm, but not for the foreseeable harm that results from others responding to their actions. What anyone else does as a result of their actions is irrelevant to the rightness or wrongness of their actions.

Kant provides the purest expression of this position. His famous example is of a man who has given refuge to a friend hiding from a murderer.[247] Kant argues that if the murderer comes to the door looking for the friend, the man has a duty to tell the truth and reveal his friend. If the murderer then predictably goes ahead and kills the friend, Kant argues that the man cannot be held responsible for what has happened: "accident causes this harm," he writes.

This strikes most people as an extreme disregard for the consequences of one's actions. It is no accident that caused the harm, but the bad intentions of the murderer – intentions that were entirely foreseeable. What is problematic about this position is its inability to adjust a person's moral obligations in response to or in anticipation of the known or predictable bad intentions of others. It is insensitive to the fact that moral action takes place in a context, in which there are people with all sorts of intentions, whose subsequent actions will change the impact of our own actions, regardless of what we may have intended. Of course, we cannot control the actions of others and we cannot be expected to foresee their intentions in all cases. But in some cases, we can reasonably be expected to foresee that our actions will lead to a harmful chain of events, and it is possible for us to prevent the harm by acting differently. In these cases, it is indefensible not to take these foreseeable and preventable consequences into account when deciding what to do.

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[247] Immanuel Kant, "On a Supposed Right to Lie from Altruistic Motives," In *Immanuel Kant: Critique of Practical Reason and Other Writings in Moral Philosophy,* trans. and ed. Lewis White Beck. Chicago, IL: University of Chicago Press, 1949.

The dilemmas that arise when scientists propose to undertake research projects that will predictably cause harm in the hands of others have some of the same features as the Kant's case of the murderer at the door. In both cases, harms are foreseeable, though not intended, and they can often be prevented by not pursuing the inquiry. Think, for example, of scientific research into the painfulness and long-term effects of torture techniques. Even if the findings are not intentionally biased to serve ideological purposes, and even if the scientists themselves do not participate in torturing anyone or harm anyone in the process, any scientist can foresee that this type of knowledge will most likely be used to torture people.[248]

Ignoring foreseeable consequences of scientific research disregards the fact that science is produced in and for a specific context – a context in which there will inevitably be murderers and terrorists and many other kinds of people.[249] That scientists should be assigned some responsibility for the harmful uses of their research that can be foreseen does not mean that they should be held equally responsible as those who actually use the research for harm, nor does it mean that those who do the harm become any less responsible for the harm simply because the scientist could have foreseen that it would happen. Responsibility is not zero-sum in this way.[250] What it does require is some consideration of the social consequences, including the possibility that results will be used to inflict harm, before the research project goes forward.

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[248] The New York Times Editorial Board, "Tortured by Psychologists and Doctors," *The New York Times,* December 16, 2014, Web.

[249] Dennis Thompson makes this point in the context of public office and the responsibilities of officials. See Thompson, "Moral Responsibility of Public Officials: The Problem of Many Hands," *The American Political Science Review,* 74 (4), 1980.

[250] And nor is punishment. If two people murder someone together, they don't each get half a life sentence. Similarly, the fact that the person driving the getaway car is also given punishment in no way diminishes the full responsibility and punishment of the murderers. On this point see Nozick, *Anarchy, State and Utopia.*

These arguments will probably fail to persuade someone who holds the view that individuals cannot be held responsible for the unintended consequences of their actions; the disagreement runs deep and my purpose here is not to settle this particular question. I am less concerned with providing an account of individual morality for scientists and more with drawing the boundaries of legitimate political interference in science. No matter which account of individual morality we accept, politics cannot be insensitive to consequences because it is fundamentally concerned with the welfare and protection of society. Even theories that do not assign moral responsibility for the unintended consequences of one's actions would accept that in the realm of politics and law, it is justifiable to hold people responsible for foreseeable consequences of their actions. Since harm-prevention is one of the important purposes of politics, restricting certain kinds of actions can be justified on the grounds that they will foreseeably lead to harmful results or create a high risk of harm, even where the person has not intended the harm.

Of course, what the state may legitimately prevent an individual from doing is not entirely independent from questions about individual responsibility: for the state to be justified in preventing someone from doing something, it often needs to be the case both that there be some harm to others and also that the individual whose actions are prevented be in some way responsible for the harm. But responsibility can be interpreted differently in a political context and need not map onto responsibility according to any particular theory of individual morality. Some sort of causal link to the harm will be a necessary minimum, but beyond that, the law can assign responsibility to individuals even where it acknowledges that they have been involved in causing the harm without any fault.[251]

[251] Strict liability is the obvious example for this.

I have argued that an individual may be prevented from doing something if it will result in harm to others and that this is true even where the harm results from the foreseeable actions of others in response to it. It would be permissible to restrict some kinds of high-risk research based on this argument. This would require the usual considerations about whether the harm in question is substantial enough, whether the risk of harm outweighs possible benefits and whether this would require an unduly great restriction of individual liberty and so on. The point is that the assumption that knowledge cannot harm is not enough to argue against interference with science.

Those who agree that scientists should be attentive to the potential social consequences of their actions typically argue on the basis of the special responsibilities of scientists in their professional role.[252] Scientists are in a better position to appreciate the risks and potential social consequences of their research because of their superior knowledge and close engagement with the subject, which creates additional responsibilities for them beyond those they have as ordinary citizens. The objection to this is that it asks too much of scientists, who may just want to focus on their research without being involved in politics. I have argued here that we do not need to rely on arguments from special responsibilities to show that it might sometimes be permissible to prevent scientists from doing certain kinds of research if it involves creating risks for others that are deemed too great. Such restrictions simply follow from the general responsibility not to create a high risk of foreseeable harm.

An alternative view maintains that the special role of scientists gives reason to narrow, rather than expand, their responsibilities in their professional capacity. The

[252] William Maker, "Scientific Autonomy, Scientific Responsibility," In *Professional Ethics and Social Responsibility*, Daniel Wueste ed. London: Rowman and Littlefield, 1994; Deborah Johnson, "Forbidden Knowledge and Science as Professional Activity," *The Monist,* 79 (2), 1996; Heather Douglas, "The Moral Responsibilities of Scientists."

knowledge produced by scientists is so valuable, the argument goes, that it is best for everyone if scientists are exempted from the responsibility that other people have to consider the foreseeable harms that might result from their actions.

That scientific research is valuable, however, is not enough to explain why we should assign scientists fewer responsibilities. After all, it can't be that the value of the knowledge produced requires giving scientists an unrelated gift of reduced responsibilities. There has to be a link between the reduction in responsibility and the expected benefits from knowledge. The argument typically used to provide this link is that we cannot know what kinds of research might prove to be the most beneficial and that worries about harmful consequences might prevent research that would otherwise be of great benefit to humanity.

The problem with this argument is that it requires giving disproportionate weight to an unknown but often small possibility of great benefit, compared to a foreseen risk of significant harm. The argument that it would be better for scientists not to be held responsible for considering the consequences of their research assumes that we are always better off betting on the small probability that there will be great benefits regardless of what information we might possess about the possibility of harm. This is an inexplicable attitude toward decision-making under uncertainty, especially given that we typically prioritize the prevention of harm over the creation of benefits.

The argument here is not that scientists should weigh all the possible benefits and harms before undertaking research: We take the general value of science for society to provide pro tanto reason for all kinds of research to be freely undertaken. The point is simply that we need to consider the possibility that there might be significant harm and that we can make an exception in these cases to the general rule for allowing all scientific research to be undertaken.

II.

I argued in the last section that the risk of significant harm could justify restrictions on scientific research. Although the decision about how significant the harm should be to justify restriction is one that will be determined politically, cases that fall under this category would likely involve serious physical damage to large numbers of people. To give a few examples, research on nuclear and chemical weapons, virulent viruses, genetic engineering and cloning might fall in this category. In this section, I will focus on a different kind of case, where it is not the magnitude of the possible harm, but the particular group of people who might be harmed that justifies restricting certain kinds of science. I will argue that in cases where scientific research is likely to harm groups that are systematically marginalized or underprivileged, with the result that their equal standing as members of society would be further jeopardized, a democratic society might justifiably restrict this line of research. I will develop the argument by focusing on one example: research in sociobiology that studies the biological basis of differences in ability and intelligence between races, ethnicities and genders.[253]

Although the scientists involved in this kind of research have tried to distance themselves from the many disturbing interpretations of their findings, most people have taken this area of research to imply that differences in native ability make some

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[253] See e.g. Richard Hernstein and Charles Murray, *The Bell Curve.* New York, NY: Simon and Shuster, 1994; Edward O. Wilson, *Socibiology: The New Synthesis,* Cambridge, MA: Harvard University Press, 2000; Arthur Jensen, *The G Factor: The Science of Mental Ability*. Westport, CT: Praeger, 1998.

groups inherently more suited than others for certain roles and activities. It has also been the case historically that the groups found to have lower abilities have been those that have also been historically subordinated: blacks and women, for instance. Potential harms from this line of research therefore fall disproportionately upon these groups, and do so in a way that reinforces existing stereotypes about the inferiority of these groups.

There are many possible harms from these kinds of findings; I will roughly sketch a few. The argument does not depend on these particular claims being true, but requires some empirical support for the possibility of harm along these lines. First of all, such research might strengthen existing prejudices and discrimination against certain groups and spread negative social attitudes based on their inferiority. This can damage the self-esteem and happiness of members of groups claimed to have lower intelligence and can create feelings of inferiority and humiliation. This would make it difficult for individuals to perform well educationally or professionally, even beyond the supposed biological inferiority claimed by the data, thus making the scientific claims self-fulfilling. Such scientific findings can also be used as a basis for changing institutional structures, for instance in the education and welfare systems, in ways that compound the disadvantage of these groups. Members of these groups may face diminished employment opportunities and lower expected income. All of these would result in increased difficulties in participation in the public and political sphere, as well as leading to lower quality of life, reduced life expectancy and negative effects on health. The overall result would be the further marginalization of the oppressed, making it even more difficult for members of some groups to be regarded and treated equal citizens with full standing in the social and political realm.

As this description of potential harms should make clear, the claim is not that some questions are inherently harmful, such that they should never be pursued in any society at any time, which I think would be difficult to justify, but rather that some issues might be harmful to raise in a particular context. It is important to take into account the prejudices and background injustices of the society in which scientific inquiry will be undertaken. Against the background of a long history of racism and sexism, we have good reason to think that claims about IQ differences between groups that have the authority of science behind them might simply serve to amplify the discrimination against these groups, making it even more difficult for them to be treated as equal citizens. In a society with no problem with racism or sexism, on the other hand, research into IQ differences between races and genders may be unproblematic, whatever the results turn out be. For instance, a scientific claim that right-handed people are more intelligent than left-handed ones may not result in the mentioned psychological, economic or political harms in our society today, given that handedness is not of particular social or political salience, whereas the situation would be different in a society where left-handed people are burned at the stake for being witches. Background conditions play an important role in how new scientific claims will be interpreted in a society and can shape the attitudinal and institutional responses to the findings.

It is important to note that the argument does not rest on the certainty of the harms. In all such cases, there will be uncertainty both about what the truth will turn out to be and whether the harmful effects that we might be worried about ex ante will in fact be realized. It is always possible that things will turn out to be fine, or better: for instance, research might reveal that there is no biological basis for differences in ability between groups, thus supporting the democratic commitment to the equality of citizens, or even if it turns out that there are indeed inherent differences, this knowledge might not have the feared negative impact on society. Society might adjust well to the new findings, for instance by creating educational opportunities that enable those with lower intelligence to do better in life, thereby leading to better outcomes. It is not on the basis of the certainty of harm that the right to make a decision about restricting science is justified, but rather on the basis that where there is such a risk of harm, then the decision about whether to take it or not should be made democratically by those who are properly authorized to make these kinds of judgments in the name of the affected.

It is also important to note that the argument depends importantly on the worry that scientific research might contribute to the perpetuation of inequality. It is the fundamental role that equality plays in the legitimacy of a democratic society that justifies foregoing the possible social benefits from research. This is important for marking the boundaries of the argument: not just any kind of harm that significantly and disproportionately affects members of one group could be the grounds for restricting science. For instance, even if it could be shown that climate change research seriously harmed the owners of oil companies or research on the health effects of smoking harmed the interests of tobacco companies, these would not constitute legitimate reason to restrict science. Scientific findings almost always benefit some and harm others, but that truths about the world may harm some of our interests is not a legitimate reason to give up the pursuit of truth.

In the case of research into IQ differences between races, it is not the fact that the truth would harm people of a certain group that justifies restricting science, but that truths of this sort are likely to be misused in a particular social context, in ways that will heighten discrimination against some groups and reinforce their unequal status. The argument relies on a similar principle as the one in the last section: it is the foreseeable misuse of science in the hands of racists and others that justifies the restriction, even if scientists themselves have nothing but good intentions in studying the question. This suggests that in a perfectly just society, there would be no need – and no justification – for restricting the pursuit of knowledge.

One possible objection is that no matter what, it is always better to know more and to make our social arrangements and political decisions based on as much knowledge as possible. Even if we expect findings about IQ differences to harm the standing of some groups, we are better off allowing the research and then dealing with the consequences as best we can. But must we really know all there is to be known? To remove some of the complexities of the situation due to the special features of science, let's assume that there is an oracle on a mountain that is guaranteed to give the correct answer to any question without fail. Must we ask the oracle whether there are innate differences in intelligence between races and sexes? I think not.

An important reason why we value the truth is that it makes it easier for us to pursue and achieve the things we value. If certain truths have the effect of rendering some members of society incapable of pursuing the things that are important to them, of participating in the life of the community as equals and leading fulfilling lives, then we might think that the cost of pursuing these kinds of truths is simply too high. Especially if the benefits seem questionable as well, then it would be reasonable to decide that we would be better off not raising the question at all. As I argued in the last chapter, we should reject the blanket assumption that more knowledge is always beneficial. It would be wrong to claim, contrary to the evidence, that we have knowledge that all races and genders have equal IQ, but it would be permissible to decide not to investigate.

So far I have assumed a collective "we" deciding whether to ask a certain question, rather than individuals consulting the oracle on their own, which might not be how things work in practice. The oracle example can be made more similar to scientific research if we stipulate that it takes a very expensive journey to get to the mountain where the oracle lives and that any individual wishing to consult the oracle must receive subsidy from the state to make the trip. In this case, it makes sense to imagine a "we" deciding that our money is better spent on questions that promise to deliver more benefit, rather than on questions whose benefits are dubious and which involve a significant risk of harm. While this way of prioritizing questions in distributing scarce resources is entirely reasonable, it evades the more difficult problem of whether we can, as a rule, refuse to fund all trips that propose to consult the oracle about IQ differences.

This second question is more difficult because it involves an infringement of freedom. I leave the discussion of the objection from freedom for the next section. But even if we set aside worries about freedom, there is a further concern that a policy not to have certain questions asked could be taken as a sign that the answer to the question is indeed the one that would cause all the harm that we feared. In this case, preventing the question from being asked might appear to be an attempt to suppress an inconvenient truth rather than a decision not to know the answer on the subject because the value of the truth would not be worth the high risk of harm. This might have worse results for everyone. If this were likely to be the case, then preventing individuals from consulting the oracle would not be a good idea. The point of the argument here is not that we should deny uncomfortable truths, but simply that we might decide not to raise certain questions at all if we think knowing the answer would not be worth the risk of harm.

Scientific research, however, is unlike the oracle in important ways. The assumption about the oracle was that it would always give the right answer; this cannot be said about science. This can be a problem in two different ways. First, and more obvious, is the possibility of error in scientific research. If past studies and other empirical evidence provide good reason to think that a particular line of inquiry is fairly likely to be biased or wrong or pseudoscience, and if there is reason to think that erroneous conclusions will cause even more harm than true findings, this would weigh in favor of deciding not to fund such projects.[254]

Less obvious, though perhaps more important, is that it might be difficult to pose the question in a way that leaves out the scientist's practical judgments, thus rendering the task of reaching an objective answer impossible from the start.[255] The need to define controversial concepts such as race and gender in a way that can be called purely scientific may not be possible, even if we set aside the prejudices and implicit biases among the scientists studying the issue.[256] Both race and gender are contested and, to a great extent, socially constructed concepts. Many have argued that there is no true natural or genetic essence of race and most of the classifications in place today were developed through historic contingencies and the administrative need to classify people into simple categories for various purposes.[257] Gender is a similarly contested concept, which many today think should be treated as a spectrum

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[254] If these kinds of studies do go forward, one possible way to deal with their findings in a political setting would be to raise evidentiary standards for acceptance because of the high-stakes nature of the subject. This is an idea I explored in Chapter 2.

[255] Some sociologists of science maintain that this is always the case. See Steve Fuller, *Social Epistemology.* Bloomington, IN: Indiana University Press, 1988. I want to make the more modest claim that in some cases we might have obvious reason to suspect that this will be the case.

[256] John Dupré makes this point in Dupré, "Science and Values and Values in Science."

[257] Ibid.

rather than a binary. Finally, IQ tests themselves involve decisions about how the different components of intelligence should be weighed, which inevitably requires value judgments from scientists.[258] All of these suggest that these kinds of studies might be impossible to conduct without substituting controversial value judgments in place of neutral biological ones, even in the design of the research question. These are additional considerations that would support a decision to restrict kinds of studies. If we would be justified in not consulting an oracle, on the grounds that we live in a society in which the answer is likely to be misused to perpetuate the unequal treatment of some groups, then, a fortiori, the same question asked by fallible human scientists with a reasonable likelihood of resulting in biased findings could also be restricted.

None of this is intended to suggest that seeking the truth would not be more beneficial in most situations we face in life than remaining in ignorance. But even if we accept this as a rule, we can still identify some exceptions where undertaking the research might have the consequence of threatening the equality of some citizens. We may not know for sure that this will be the case, since we can never know unless we go ahead and investigate. There will always be arguments for and against, depending on how different people interpret the social context and the status of different groups. Still, democracies always make these kinds of decisions about the common good or the good of specific groups under conditions of uncertainty. It is necessary to make these decisions, rather than assuming that all new knowledge is inherently valuable.

[258] At the very least. A less charitable approach would argue that these tests are biased to give more weight to traits or skills associated with more privileged demographics.

III.

The main objection to the argument that a democracy may in some cases restrict science on the grounds of harm is based on considerations from freedom of inquiry. Even if we accept that scientists bear responsibility for the foreseeable harms that others cause with the findings of their research, and even if we accept that scientific research that might harm the standing of marginalized groups in society should be given special consideration, we might still conclude that the overriding value of freedom of inquiry should rule out any possible restrictions on science. The purpose of this section is to analyze the main justifications for freedom of inquiry and show why the arguments in the last two sections would not be defeated by this objection.

There is a vast literature on freedom of speech and, rightly or wrongly, many theorists assume that arguments about speech can be applied to the justification of freedom of inquiry. Although courts have not directly addressed the issue of whether freedom of inquiry is protected under the First Amendment,[259] scholars often turn to the First Amendment to discuss the appropriateness of any proposed limitations on scientific research.[260] Since this is the accepted practice, I will also start with justifications of freedom of speech and ask whether scientific inquiry shares the salient features of speech that are taken to justify its special sphere of protection. I

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[259] Keane, "The Case Against Blanket First Amendment Protection of Scientific Research."

[260] There are many many examples; here are just a few: Lori B. Andrews, "Is There a Right to Clone? Constitutional Challenges to Bans on Human Cloning," *Harvard Journal of Law and Technology,* 11, 1998; Richard Delgado et al., "Can Science Be Inopportune? Constitutional Validity of Governmental Restrictions on Race-IQ Research," *UCLA Law Review*, 31 (1), 1983; Michael Davidson, "First Amendment Protection for Biomedical Research," *Arizona Law Review*. 19 (4), 1977; James R. Ferguson, "Scientific Inquiry and the First Amendment," *Cornell Law Review*, 64 (4), 1979. My purpose here is not to provide a legal argument for whether or how the First Amendment applies to scientific inquiry, but rather a philosophical examination of whether justifications for protecting of freedom speech should apply to freedom of inquiry.

will argue that scientific research and speech are different in important ways and these differences justify giving a narrower sphere of protection to inquiry than to speech.

There are two major theories that aim to explain the special status given to speech, the first epistemic, the second political.[261] The epistemic theory rests on Millian grounds: freedom of speech is necessary for the advancement of knowledge and the discovery of truth. It is always possible, given the fallibility of humans, that what we think is true will turn out to be false and that the truth will be contained in a view compelled to silence. Only by allowing all views to be debated, all hypotheses tested, all lines of research explored can we correct false beliefs or be assured of the truth of true ones.

This argument is often taken to apply in the context of science, but there are some problems with this assumption. First of all, Mill's argument presupposes a marketplace of ideas[262] in which everyone is equally free to participate. Scientific research is not like this: only those who have funding can truly participate in the production of science and the possibility of receiving funding depends on having the right qualifications. It is therefore not a conversation open to all. This makes it uncertain that the truth will emerge from free discussion and that all lines of research will be subject to scrutiny.

Science might become more Millian through active efforts to diversify funding and support different viewpoints, especially on issues where this seems unlikely to happen naturally, but freedom from interference alone will not be enough to achieve this result. Furthermore, if certain areas of research are undertaken and funded disproportionately by those with biases, then we cannot expect that the truth will

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[261] Post, *Democracy, Expertise, and Academic Freedom*.

[262] Note that Mill himself never used this term.

emerge from this discussion. For instance, it might be the case that scientists who believe that it would be harmful or pointless to raise questions about differences in intelligence between races or genders do not pursue these lines of inquiry, thus effectively leaving IQ research on race and gender to the racists and sexists.[263] Funding or incentivizing others to undertake such research, despite their disinclination, may be one way to deal with this problem, withdrawing funding from the area altogether may be another. If we don't do anything at all, however, we cannot count on the truth emerging in Millian fashion.

Secondly, Mill never considers the possibility that the pursuit and discovery of truth might have harmful effects in some contexts.[264] The argument for placing restrictions on certain lines of research, on the other hand, applies precisely in cases where there are such harms and where the harms involved override the value of discovering the truth. This is different than preventing research because a particular viewpoint is considered dangerous, because it goes against majority beliefs, because we assume that the truth has been reached already or because religious or political authority imposes what counts as truth. The decision not to want to pursue certain lines of inquiry is not a decision to suppress the truth because its content might be objectionable, but rather a decision that considerations from possible harm override the value of truth on a particular question.

Perhaps the main reason why Mill did not consider the possible harms from the pursuit of truth is that he was concerned with a different kind of truth: religious

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[263] Kitcher argues that scientists have a responsibility not to pursue this kind of science. But this might simply mean that those who act morally don't pursue this, with the result that the field is left entirely to the immoral. This seems to be an even worse outcome than if everbody pursued it. See Philip Kitcher, "An Argument About Free Inquiry," *Nous,* 31 (3), 1997.

[264] John Skorupski makes this point: "Mill was not a thinker to whom the dangers of unrestricted open dialogue had ever occurred." See Skorupski, *Why Read Mill Today?* London: Routledge, 2006, p. 61.

truth, rather than scientific. He was interested primarily in the importance of seeking the truth about one's beliefs. This, he thought, could never be harmful, whatever the truth turned out to be. Of course, Mill was perfectly aware that people would be offended by others' words; the kinds of cases he was interested in often involved challenges, or worse, insults and ridicule to people's deeply held convictions. His point was that we should take a broader view of benefit and harm: in the long-run we are all better off allowing these challenges to our beliefs since this is how we can progress toward the truth and abandon our false beliefs, even if it necessarily involves some pain and distress in the short-run.[265]

Nothing in this chapter is meant to challenge Mill on these points: my argument that certain truths can be harmful in context uses a definition of harm that Mill would accept. Like Mill, I do not extend harm to include harms to morality, religion, tradition, offenses to people's sensibilities or other things that threaten the way things are. Harm is defined as either physical or safety harm, or harm to people's interests, especially of those who are already disadvantaged, for instance from discrimination.[266] That groups who are revealed by science to be biologically inferior might be offended by this claim is not what justifies interfering with the science. The argument is rather on the grounds that these scientific claims will lead to or amplify other sorts of harms – ones that Mill would also recognize as harms – in a society

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[265] A slightly different interpretation is that Mill thought we should count these challenges and insults as *benefits* because they help our moral progress. This is Jeremy Waldron's argument in Waldron, "Mill and the Value of Moral Distress," *Political Studies*, 35 (3), 1987. Waldron seems to have modified this position recently in his recent writings on hate speech. See Waldron, "Dignity and Defamation: The Visibility of Hate." *Harvard Law Review*, 123 (7), 2010. Although the difference between the two readings is not trivial, not much hangs on it for my purposes here, so I will not defend the point further.

[266] See Joel Feinberg, *The Moral Limits of the Criminal Law Volume 1: Harm to Others*. New York, NY: Oxford University Press, 1987 for a justifiction of defining and limiting harm in this way.

with background injustice. As a utilitarian, Mill himself argued that interference with individual liberty would be justified if (and only if) it would predictably lead to harm.

An alternative theory of freedom of speech takes the purpose of protecting freedom of speech to be primarily political, rather than epistemic.[267] On this view, free speech has overriding value because of its necessity to the communicative practices that enable democratic self-rule.[268] Only if each person has the opportunity to participate in processes of opinion formation and render public opinion responsive to their own views can they possibly regard themselves as equal authors of democratic decisions. They may not, in the end, have equal influence on public opinion, but the legitimacy of democratic decisions rests on each person having had the chance to do so.[269] This view does not necessarily preclude the Millian argument that free discussion is the best way to discover truth, but it does not require it.

If we take this to be the main purpose of protecting freedom of speech, then it is even clearer that freedom of inquiry does not share the relevant features of speech and need not have the same level of protection. On this theory, the most important purpose of protecting free speech is to provide equal opportunity for citizens to participate in political discourse, independently from the epistemic claim that this will lead to truth. This view derives its normative power from the equality of citizens that is fundamental to democracy. Freedom of inquiry cannot be justified this way because it is neither inclusive, nor egalitarian. The freedom to undertake inquiry depends on

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[267] This view is associated with Alexander Meiklejohn, *Political Freedom: The Constitutional Powers of the People.* Westport, CT: Greenwood Press, 1965.

[268] Post, *Democracy, Expertise, and Academic Freedom.*

[269] Niko Kolodny, "Rule Over None I: What Justifies Democracy," *Philosophy & Public Affairs,* 42 (4), 2014; and Daniel Viehoff, "Democratic Equality and Political Authority," *Philosophy & Public Affairs,* 42 (4), 2014.

meeting certain requirements of quality and merit, at least the way science is practiced today.

Of course, freedom of inquiry is also essential for a democracy and can be justified on political grounds, but the logic of the argument has to be different than in the case of speech. While speech is important because of its connection to equality, inquiry is important because of its role in the production of knowledge. It is through the necessity of knowledge for democracy that freedom of inquiry can be justified on political grounds. This argument follows the familiar Deweyan line that democracy needs a realm of free and organized inquiry to be able to make intelligent policies.[270]

But once we recognize that the value of inquiry is through knowledge, then the argument that it might sometimes be necessary to give up some of the benefits from knowledge where there is significant risk of harm applies. The case of speech is different because of its intrinsic connection to the equality that legitimates democracy. The equal right of citizens to participate is not a benefit, like knowledge, that might be foregone if there is great harm. Because equality is fundamental to democracy in a way that knowledge is not – even though knowledge, too, is very important – it makes sense for speech to have a broader sphere of protection, whereas inquiry can be subjected to some scrutiny on grounds of harm.

In fact, taking this line could even require restricting certain kinds of inquiry precisely on the grounds that they might endanger the ability of marginalized groups to participate as equals in the public sphere. By stigmatizing them as inferior and reinforcing existing prejudices, some scientific findings may have the effect of silencing citizens. In such cases, this theory provides clear grounds for prioritizing the

[270] Dewey wrote, "Genuine public policy cannot be generated unless it be informed by knowledge, and this knowledge does not exist except when there is systematic, thorough, and well-equipped search and record." (Dewey, *The Public and Its Problems.*)

equality of citizens over the freedom of scientists to pursue inquiry because the former is fundamental to the legitimacy of democracy.

What are the implications of choosing the epistemic theory of speech over the political, or vice versa?[271] Although both can support the conclusion that we may sometimes give up certain kinds of knowledge if they will lead to harm, an important difference between the two is that the epistemic view is not sensitive to some crucial differences between speech and inquiry. Both speech and inquiry are regarded as valuable because of their instrumental role in the discovery of the truth. A good test case for bringing out the differences between the epistemic and political view is the difference between scientists proposing to research the hypothesis that certain races have inferior intelligence and an ordinary individual claiming the same on a television talk show. The epistemic view provides no grounds for treating the two differently. On the political view, however, we can argue that the latter should be protected even if it is offensive and potentially harmful because of the importance of protecting all citizens' right to influence public opinion, whereas the former need not enjoy this protection for the arguments listed above.

This assumes that scientific research should not be interpreted as the scientist's opinion, on equal footing with any other opinion trying to influence political decisions. This seems right, since scientists are paid large amounts of public funds on the grounds that they produce knowledge. If we treated scientific knowledge as the scientist's opinion, it would be difficult to see how we could justify funding them and not the opinions of other citizens. Of course, scientists as individuals go between the sphere of public opinion, in which they participate as citizens, and the

[271] I cannot provide an argument for which of these would be the more adequate view of freedom of speech. My purpose here is only to consider the implications of these two views for the scope of freedom of inquiry.

sphere of inquiry, in which they participate as experts. A scientist can freely pronounce that the moon is made of blue cheese on television, in front of millions of viewers, but cannot make the same claim on the pages of a scientific journal unless she can demonstrate it using the methods of the discipline of lunar geology.

Conclusion

It is more common for moral philosophers and scientists to argue that scientists have an obligation to consider the social consequences of their research than to argue that it would be permissible for a democracy to restrict science based on considerations about its harmful consequences.[272] Although the goal in both cases is to make scientific research more sensitive to the broader context in which it takes place and to prevent potentially harmful research from going forward, it is less controversial to leave things to the moral judgment of the individual scientists or of a committee of scientists because state interference raises worries about infringements of freedom and the political control of science.

I chose to argue for the more controversial position because the question of what constitutes significant harm for a society and what kinds of harms are worth the benefits from science are fundamentally political and contested issues that require judgments about what a society takes to be in its best interest. These cannot be settled through the internal deliberation of individual scientists considering whether to pursue a line of inquiry in light of their own beliefs about the right thing to do and the benefit of society. Individual scientists may well have a moral obligation not to pursue certain kinds of research, but the question of significant harm for society as a whole should not be left to the determination of individual moral judgment alone. Of course, any

[272] Kitcher, *Science, Truth and Democracy* and Douglas, *Science, Policy and the Value-Free Ideal* are recent books on the subject that take the individual responsibility line.

democratic decision to withdraw funding from an area of scientific research will inevitably depend a great deal on the information and analysis provided by scientists. Scientists cannot be left out of the decision, but the ultimate decision should involve democratic input.

Conclusion

There is a scene in Stanley Kubrick's *Dr. Strangelove or How I Learned to Love the Bomb* in which the paranoid General Jack D. Ripper, who has just ordered a nuclear attack on the Soviet Union, asks Peter Sellers' Captain Lionel Mandrake if he has ever wondered why Russians only drink vodka and never water.

Ripper: Have you ever heard of a thing called fluoridation? Fluoridation of water?
Mandrake: Ah, yes, I have heard of that, Jack. Yes, yes.
Ripper: Well, do you know what it is?
Mandrake: No. No, I don't know what it is. No.
Ripper: Do you realize that fluoridation is the most monstrously conceived and dangerous communist plot we have ever had to face? [273]

This parody of the 1960s controversy over the science of water fluoridation seems more relevant than ever today. The President of the United States has recently claimed that climate change is a hoax "created by and for the Chinese to make U.S. manufacturing non-competitive."[274] Mistrust of science and scientists has become widespread in the United States. Many people resist or outright deny scientific claims on issues such as climate change, vaccines and genetically modified organisms. This situation is exasperating for those who believe in the importance of conducting public debates and making policy decisions on the basis of reliable scientific evidence. Calls for removing technical issues from the vagaries of public opinion are on the rise. At a recent conference on climate change, the author of a study on public opinion on climate change argued that the best way forward would be to remove climate policy from the influence of public opinion and address it in the domain of insulated expert

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[273] Transcription of this scene from Joel Achenbach, "Why Do Many Reasonable People Doubt Science?" *National Geographic,* March 2015, Web.

[274] @realDonaldTrump, "The concept of global warming was created by and for the Chinese in order to make U.S. manufacturing non-competitive." *Twitter,* 6 November 2012, 2:15 pm.

policymaking so that the facts about costs and benefits would not be open to negotiation by non-experts. It cannot be a coincidence that books about the incompetence and ignorance of the public, and arguments in favor of expert rule have also gained popularity.[275] Google searches for the word "epistocracy" peaked on the three days after the 2016 presidential election.[276]

Democratic responses to failures on scientific issues have mostly taken the shape of efforts to rethink how scientific findings could be communicated better to the public. That is important, to be sure, but it does not provide a robust defense of why we must remain committed to democracy on these issues instead of considering technocratic alternatives. My goal in this dissertation was to provide a more vigorous theoretical defense of the necessity of democratic scrutiny over expert claims, making an argument on both epistemic and normative political grounds. Drawing on a wide range of work in the philosophy of science, I identified the specific ways in which the judgments and purposes of experts shape the production scientific knowledge and argued that the failure to question knowledge claims publicly would result in democratic policy being guided by the judgments and values of the experts. I also gave serious thought to how democratic engagement on scientific issues could be organized to improve upon the current state of affairs. I developed an institutional proposal to facilitate interactions between experts and laypeople, focusing on how to mitigate the difficulty of non-experts examining complex knowledge in a distortive public sphere, and how to overcome the challenges of deliberation among those with asymmetric knowledge. Finally, I highlighted scientific funding decisions as the site

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[275] See e.g. Bryan Caplan, *The Myth of the Rational Voter: Why Democracies Choose Bad Policies*. Princeton, NJ: Princeton University Press, 2011; Jason Brennan, *Against Democracy*. Princeton, NJ: Princeton University Press; Ilya Somin, *Democracy and Political Ignorance: Why Smaller Government is Smarter.* Palo Alto, CA: Stanford University Press, 2013.

[276] Google Trends, trends.google.com. Accessed 21 April 2017.

for long-term and more foundational democratic influence over the direction and political impact of new scientific research, going as far as to argue that democracies might withdraw funding from certain areas of research if they posed a serious risk of harm.

If recent arguments for epistocracy have been strikingly reminiscent of the views of Walter Lippmann, who argued almost a century ago for the need to remove the influence of public opinion over the substantive judgments required for governance,[277] this project has in turn followed the example of John Dewey, who argued for more democratic engagement over technical issues precisely at a political moment when trust in science and trust in democracy both ran low.[278] As this mention of the famous Dewey-Lippmann debate suggests, however, one of the things I have left undone is to trace the relationship between my arguments for democratic input on scientific issues and early twentieth century pragmatist arguments for the same.

The early pragmatists Peirce, James and Dewey witnessed and resisted the rise of value neutrality as an ideal for science, and they did so by developing a distinctive theory of inquiry that challenged three traditional dichotomies defining the relationship between scientific inquiry and practical action: between experience and knowledge, fact and value, and belief and action. These challenges opened up the conceptual space for citizen participation in deliberation on technical issues. I have shown how the ideal of value neutrality came under attack in the decades after Kuhn, but pragmatist arguments were quite different than these recent ones. Studying the difference between the pragmatist critique and the later twentieth century critique in

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[277] Walter Lippmann, *Public Opinion.* New York, NY: Harcourt, Brace & Co., 1922; Walter Lippmann, *The Phantom Public*. New Brunswick, NJ: Transaction Publishers, 1993 [1925].

[278] Dewey, *The Public and Its Problems*.

light of the arguments of my thesis and with the experience of half a century of organized science would be productive.

My focus on scientists and the public as the two main actors in use of science in democracies has also left the role of other the key political actors unexamined. I have emphasized the importance of face-to-face interactions between scientists and ordinary citizens, but equally important is the role of elites — politicians, legislative staff, bureaucrats and the media. How does scientific information get translated for the consumption of legislators? Who determines the framing of scientific issues before they reach the stage of public political debate? How is expert opinion and public opinion on science used in actual policy processes? These questions must be answered for a better understanding of the role of science in a democracy. This would allow a normative account of the institutional structures in which scientists interact with legislators and would also contribute to improving democratic decision-making and accountability at different levels of representative government.

Finally, this project has taken the nation-state to be the relevant unit of analysis for the relationship between science and democracy. I think this is defensible since most decisions about science are still made at the domestic level and most scientific research today still depends on funds provided by states. Nonetheless, in the age of climate change – a global scientific problem, if there ever was one – the international dimension of the politics of science cannot be ignored. Many decisions on cross-border scientific issues are made through international agreements, which raises additional concerns about unelected and unaccountable experts making policy decisions that affect citizens of democracies without providing clear avenues for public influence.[279]

Moreover, science itself is increasingly international. Scientific discoveries made anywhere in the world are immediately accessible everywhere else. The emergence of international scientific bodies such as the Intergovernmental Panel on Climate Change is a testament to the existence of a well-connected global scientific community. These developments complicate my arguments about science funding. On the one hand, political strategies such as withdrawing funding for research or changing research priorities may be less effective if the same research is likely to be conducted in another country. On the other hand, considering the potentially harmful consequences of science may become even more crucial once we consider the international context. We might be confident that findings about differences in intelligence between races or genders will not be used to deprive anyone of their rights in our country, but can we be equally confident that the same knowledge will not lead to disastrous consequences in other places? These are further questions raised by this dissertation. The answers must wait for another project.

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[279] See David Kennedy, *A World of Struggle: How Power, Law, and Expertise Shape Global Political Economy*. Princeton, NJ, Princeton University Press, 2016.

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