A Social-Pluralistic View of Science Advising

by

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Author's Declaration

I hereby declare that I am the sole author of this thesis. This is a true copy of the thesis, including any required final revisions, as accepted by my examiners.

I understand that my thesis may be made electronically available to the public.

Abstract

In this dissertation, I bring together two disciplines: Science, Technology, and Society studies and the Philosophy of Science, to develop a social-pluralistic account of science advising. I use three prominent theorists in the philosophy of science to critique three prominent views in the science, technology, and society field relating to science advising. I argue that the science, technology, and society literature does not fully account for the value-ladeness of scientific research. To that end, I develop a social-pluralistic account of science advising: social, because advice should come from panels or institutions rather than individuals, and pluralistic, because we should assess the credibility of advice along several dimensions of objectivity. I then apply my view to two real world examples: first, an EPA report on the harmful effects of environmental tobacco smoke, which faced lawsuits from the tobacco industry, and second, the Government of Canada's use of Roundup Ready canola, a biotechnology, as a "value neutral" policy response to avoid discussions about the socio-cultural impact of industrial agriculture. These examples help to demonstrate the usefulness of my view in responding to real-world situations. A social-pluralistic view of science advising helps ensure that the role of values in producing scientific knowledge and science advice are legitimate, helps ensure that diverse viewpoints are actively considered as part of the advisory process, and ensures that the resulting advice is independent of any one person's views, beliefs, or values.

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Dedication

To Me. We did it! And to you Oma, now I’m Dr. Freier.

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Introduction

"Doubt is our product since it is the best means of competing with the 'body of fact' that exists in the mind of the general public."

-Executive of the Brown and Williamson Tobacco Company[1] 1969

https://www.industrydocuments.ucsf.edu/tobacco/docs/#id=psdw0147

In the latter half of the 20th century, tobacco companies found themselves under attack by anti-smoking advocates wielding a growing body of evidence that cigarette smoking was harmful to health. Fighting for survival, the tobacco industry sought to sow doubt and skepticism about evidence revealing the negative health effects of cigarette smoking by adopting the strategy espoused in the epigraph. By attacking the scientific evidence being used by anti-smoking activists, the industry sought to create doubt about the legitimacy of such research, and so, to render anti-smoking research as unsuitable as the basis for policy. Specifically, tobacco companies launched a coordinated campaign to discredit scientists, create doubt about the epistemic integrity of these scientists' research, and use epistemic uncertainty as a frame for arguing that more research was needed, despite broad scientific consensus and converging avenues of research showing that cigarette smoke was harmful to human health (Oreskes and Conway 2010, 14 – 17).

Attacks on science, such as those leveled by the tobacco industry, have prompted science and technology scholars to examine the role of science in policy and offer strategies for ensuring that government policies are based on sound evidence. However, ensuring that

[1] A more specific attribution is impossible. The source is a memo from an executive of the Brown and Williamson Tobacco Company, but the document doesn't provide names.

policy is based on the best available evidence becomes difficult when some institutions attempt to sow doubt about the legitimacy of the scientific research in question.

While those in science and technology studies (STS) have offered strategies for dealing with the problem of distinguishing credible research from untrustworthy research, these strategies have largely been legalistic, often focusing on legislative or structural changes to regulatory institutions. There are opportunities for philosophers to contribute to developing a more nuanced role for science in policy and to improve on current solutions developed in STS. Specifically, I think that philosophers of science can help articulate the powerful, positive role that values can play in shaping and delivering credible science advice.

It is common in the philosophy of science to accept that science is value laden. While the extent to which science is value-laden and, what the structure of those values are, remains open for debate, the fact that there are values, including social and ethical values, in science is no longer as controversial as it once was. While there are some, such as Gregor Betz (2013), who still defend the value-free ideal, I will not substantially engage with those views in my dissertation, as doing so would digress from the argument that I wish to make, namely, that credible science advice can be responsibly implemented in the context of value-laden science.

Many STS scholars have analyzed and explored the ways in which science and society interact, and often, values are an important part of this discussion. Values often play an explanatory role in describing why certain groups are in opposition and/or the motivation for actions by certain groups. Within STS science policy literature, the presence of values in science is frequently recognized, however some prominent views in the field do not fully recognize the implications of these values with respect to science advising.

However, "science policy" is a wide field. One can study it (or opine on it) as a scientist, a political scientist, an anthropologist or sociologist, a legal scholar, a philosopher, or as an STS scholar. Furthermore, STS itself is such a varied field, that one can be either a philosopher, sociologist, or anthropologist (to name but a few disciplines) and be said to be practicing STS. As such, when I use STS, I mean a field of study that, for my purposes here, excludes philosophers of science. I do this for conceptual simplicity. While there are philosophers of science who do discuss values, science, and policy (with Heather Douglas, Kristin Shrader-Frechette, Nancy Tuana, and Kevin C. Elliott being notable examples), one of my aims in this dissertation is to find points of conceptual overlap or similarity between the STS literature and the philosophy of science literature,[2] as well as areas where philosophy of science can make contributions to the STS field. Consequently, I (perhaps somewhat artificially) treat the STS literature and the philosophy of science literature as separate. Oftentimes these two fields study very similar topics, albeit, through somewhat different lenses, which suggests to me that there is a lot of room for discussion amongst these fields.

At first glance, it seems as though the presence of values in science, and in particular, social and ethical values, threatens the objectivity and authority of science, thereby rendering it an ineffective tool for policy. The promise of science in policy, after all, is that science provides policymakers with objective, impartial (and so non-partisan) information on which

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[2] For example, there are interesting similarities between Sheila Jasanoff's concept of "co-production," and Nancy Tuana's coupled ethical-epistemic analyses, which I discuss in chapter three.

to base policies. If social values are present in science, it is still often assumed that their presence will create the impression of bias on the part of scientists and undermine the use of scientific evidence in the policy process.

Many philosophers of science, however, as I noted above, have shown that the presence of values in science is compatible with scientific objectivity, and some, such as Heather Douglas, have sought to show the implications this has for science in policy. Furthermore, approaching science in policy from a philosophical perspective is important because even when science policy scholars recognize that there are values in science, there are, as I noted above, some prominent views in the field (e.g., Jasanoff's Boundary Actor View and Pielke's Honest Broker View) that do not deal with value-laden science as fully as they might. As such, the ways of determining what counts as credible science or who counts as a credible science advisor, offered by these scholars, often unintentionally reinforce the value-free ideal (the view that there are or ought to be no values in science). In my dissertation, I will show how dichotomous thinking about values and objectivity negatively impacts the use of science for policy.

My research question is this: given the presence of values in science, how can scientific information be responsibly used to develop policy? To answer this question, I have structured my dissertation as follows. In chapter one, I provide a literature review of the relevant science policy literature, arguing that objectivity is the de facto standard for credible research. In chapter one I focus on three influential views of science advising. The first, I call the Pure Science view. This view is an amalgamation of the attitudes of scientists toward science-based advocacy and advice, as articulated in the editorial pages of various policy-relevant scientific journals. The other two views, from Sheila Jasanoff and Roger Pielke Jr. respectively, are also frequently referenced, but offer an STS perspective. That is not to say that any of these views are the *definitive* view of science advising in the literature, but they are prominent in the field, and because of that, these views are useful foils for making my argument.

In chapter two, I discuss three influential philosophical views of objectivity and following Helen Longino, I argue that objectivity is socially constructed and that values play an important role in its construction. This argument helps to create conceptual space for a pluralistic account of objectivity, which I develop in chapter four. In chapter two I go on to lay the other piece for a pluralistic account of objectivity by, following Heather Douglas, arguing that objectivity can be understood in a variety of ways.

In chapter three, I identify two problems that science advisors and advocates should consider. I do this by bringing together the discussion from chapters one and two to identify areas where the views discussed in chapter one presume a dichotomous relationship between values and objectivity. In particular, I deploy some of the concepts developed by the philosophers in chapter two, to show how even when the theorists from chapter one acknowledge that values play a role in scientific inquiry, these views still rely on an understandings of objectivity that sees values as threatening. This is problematic for two reasons. First, if we see values as threatening to scientific objectivity, then any semblance of values in science will undermine the credibility of the research, and/or the advice being offered. This presumes an understanding of objectivity as "detached" and leads to what I call the *Credibility Problem*. Second, if we do not recognize values, or if we see their presence as threatening, we cannot, as Longino and Douglas (and others) argue, critique them. This can lead to problematic values shaping science or science advice that informs policy. Furthermore, seeing values as threatening to scientific objectivity helps to reinforce the idea that science and society are separate. This, I argue, incentivizes scientists and policymakers towards technocratic, "value-neutral," policy responses, when these responses are not value-neutral. I further argue that this requires understanding objectivity (and perhaps the scientific enterprise as a whole) as value neutral, and leads to what I call the *Value Neutrality Problem*, which is when technocratic policy solutions are proposed instead of critically examining the values involved in the production of scientific research or science advice, which can allow for social and ethical value judgments to be made under the cover of technical precision (Shrader-Frechette 1993). This perception of science and technology as a value neutral tool for science policy, incentivizes science advisors to refrain from making explicit the social and ethical value judgments that go into the advice they provide. To put it simply, in chapter three I argue that seeing values as dangerous to science and science advising has the consequence of leaving science advisors to deal with these two problems.

In chapter four I draw on the work of Nancy Tuana, Kristin Shrader-Frechette, Naomi Scheman, Kyle Powys Whyte and Robert Crease, and Kevin C. Elliott, as well as the philosophers from chapter two, to propose a social-pluralistic approach to science advising. I show how a social-pluralistic view of science advising can respond to the challenge of social values in policy relevant science. The social part comes from Helen Longino. I argue that to respond to the Credibility Problem, we should adopt her Critical Contextual Empiricist framework to subsume the goals of detached objectivity in a way that more fully embraces the positive role that values can play, and so, when a claim is said to be biased or overly shaped by values, look to what the community of experts says. I also argue that we should, per Douglas, adopt a pluralistic understanding of objectivity so that a failure along one line of objectivity, does not immediately mean that some research or advice can be discounted. Instead, we should evaluate it using the pluralist account of objectivity, using the five kinds of objectivity I identify to triangulate credible advice and advocacy.

In responding to the Value Neutrality Problem, I argue that value neutral objectivity can be better understood as a kind of procedural objectivity, and that because of this, we should, drawing on Longino again, adopt a social approach to science advising, such that advice comes from a diverse science advisory panel. By diverse, I mean that the panel should be diverse not only along social dimensions (e.g., gender identity, racial identity, etc.), but also be diverse in disciplinary expertise and knowledge, in particular by including interactional experts with knowledge of how values shape scientific research. In this respect, my view bears some similarity to the Boundary Actor View offered by Sheila Jasanoff, which I discuss in the first chapter. While Jasanoff sees science advisors as boundary actors (that is, people who work at the science/society or in this case, science/policy interface), she thinks that the boundary between science and politics must hold. I differ from her insofar as I include other boundary actors, interactional experts, who can help other stakeholders navigate this terrain.

Finally, in chapter five, I develop two case studies to illustrate both the Credibility Problem and the Problem of Value Neutrality. First, I draw on the work of Naomi Oreskes and Eric M. Conway to show how the tobacco industry used the Credibility Problem to create doubt about the science surrounding the negative health effects of tobacco smoke. Second, I draw on the work of Kelly Bronson to show how the Canadian federal government technocratized decision-making around genetically modified canola.

Before I begin my first chapter, I wish to discuss a case study that I will draw on periodically throughout my dissertation since case studies can be useful tools for working through problems. Here, I use a case study involving agricultural giant Monsanto, and their product *Roundup*, to help illuminate the importance of responsible advocacy.

In an ongoing class action lawsuit against agricultural giant Monsanto,[3] the plaintiffs' lawyer released internal company documents, showing that the company used its money and influence to inflate the scientific credibility of claims that their product *Roundup* was safe, despite the fact that *Roundup* contains a potentially carcinogenic chemical known as glyphosate (McHenry 2018, 194 – 195). Monsanto did this by hiring people to attach their names to research papers largely written or edited by Monsanto. Presumably, Monsanto did this for economic reasons, seeing that being labeled as "carcinogenic" by health authorities would lead to a decrease in their sales.

In his analysis of the Monsanto papers, philosopher Leemon B. McHenry notes that it appears that Monsanto anticipated that glyphosate would be classified as a carcinogen, and "pro-actively engaged third-party academics who acted on Monsanto's behalf as 'independent' experts into signing onto Monsanto's ghostwritten reports which were then published in leading toxicology journals and in the lay media" (McHenry 2018, 195). Ghost

[3] Bought by Bayer in 2018.

authorship, as it is used here, "is the failure to name, as an author, an individual, typically an employee of a pharmaceutical company or medical communication firm, who has made substantial contributions to the research or writing of the article" (ibid.).

By hiring ghostwriters, Monsanto obscured the origins of the research by "branding industry's products with the academic reputation and institutional affiliation of key opinion leaders[4]" (McHenry 2018, 194). Passing off industry research as being independent when it is in fact led or conducted by industry, is a clear violation of scientific norms that undermines the responsible use of science for policy. Detached objectivity, that is, disinterested, impartial inquiry, is widely regarded as essential for uncovering truth in science; so much so that most journals require researchers to disclose and real or perceived conflicts of interest, so that the community can assess whether the researchers have been suitably detached in their analysis. By ghostwriting these research papers, Monsanto, and the researchers it hired, violated this norm of scientific integrity. This has several consequences.

First, Monsanto can point to their ghost-written research as evidence that glyphosate and *Roundup* are safe when advocating for its adoption or continued use. Second, federal departments and agencies, such as Health Canada, use a variety of evidence to make assessments regarding the safety of different drugs and chemicals. Among these is information provided by the manufacturer, as well as the available third-party research (Government of Canada 2017). Health Canada is tasked by the government to assess whether

[4] This phrase appears to be Monsanto's phrase for the academic consultants they hired to attach their name to industry-led research. Monsanto's strategy was to rely on the reputations (both professional and institutional) of academics to reduce scrutiny on their research.

substances are safe for Canadians. If supposedly independent third-party research is neither independent, nor conducted by third parties, the decision on the part of regulators is going to be skewed in favour of corporations, rather than towards public health and the public interest, which runs contrary to Health Canada's mandate and values. On their website, Health Canada writes that their values are to care for the people of Canada, by, among other things, providing credible information, reliable advice and quality services (Government of Canada 2011). Third, people who have questions about *Roundup's* safety and go to Health Canada's website looking for information, may come away with an inaccurate understanding regarding the safety of the product.

As indicated above, obscuring the origins of the research affects the scrutiny we give the evidence. Research which is perceived as independent will be weighed more heavily than industry sponsored research, because there are no apparent conflicts of interest. This has significant policy implications, as demonstrated by Health Canada's determination that glyphosate is unlikely to pose a cancer risk in humans (Government of Canada, 2017). The World Health Organization reached a similar conclusion, noting that while glyphosate is "probably carcinogenic," it found that the compound was "unlikely to cause cancer in people via dietary exposure, and that "it is possible to establish safe exposure levels" (World Health Organization, 2016). Yet, because these determinations were arrived at through industry led research, we have legitimate grounds to question the acceptability of these evaluations.

A charitable interpretation of this case is that the ghostwriting was done because papers with sound science may suffer from credibility issues if members of the industry write them. For example, the community may critique the papers unfairly or fail to take the papers seriously if company scientists write them. A less charitable interpretation would be that ghost-authorship is done to intentionally lessen scrutiny on dubious research. If there are no obvious conflicts of interest, then it follows that the results will be more readily accepted 'at face value' (which is not to say they won't receive scrutiny; it is just to say that there is no red flag for increased scrutiny). However, because of the ghostwriting, it is unclear how much legitimacy and credibility we should ascribe to the claims in these papers. Furthermore, since these documents are part of the research that Health Canada uses to confirm a product's safety, it is unclear how much legitimacy and credibility we should ascribe to Health Canada's claims that the product is safe.

The Monsanto case highlights the importance of responsible science-driven advocacy and science advising. It highlights the need for standards by which policymakers can judge science-based advocacy, but also standards for scientific institutions regarding what responsible science-based advocacy looks like, particularly when values seem to play a role in such advocacy. Monsanto's advocacy reflects the value they place on keeping their product on the market, and for it to continue selling successfully. If the product is thought to be dangerous, consumers will be less likely to use it and, from the perspective of a profit-driven value system, Monsanto's investment in *Roundup's* research and development will have been wasted.

On the other hand, Health Canada, whose stated goal is to provide credible information and reliable advice to Canadians, operates from a place of public-health oriented values. From this perspective, trustworthy knowledge about the carcinogenicity of a substance is invaluable, as it helps people make informed decisions about the products they use, and helps policymakers anticipate cancer rates so that they may plan for prevention, treatment, or both. But, if Health Canada does not have access to reliable information about the carcinogenicity of glyphosate because claims about its safety have been inflated as a result of economic values, then it is much more difficult for Health Canada to fulfill its mandate.

Here is the problem that I wish to tackle. In the Monsanto case, and others like it, different values and value systems come into conflict, distorting, and otherwise muddying scientific debate. As Kristin Shrader-Frechette (1991), Heather Douglas (2009), Maya Goldenberg (2015) and others, have argued, the value-laden nature of science can cause distrust in science by making it difficult for lay-publics and policymakers to tell who is providing them with credible information or, by cloaking the value judgments of scientists and policymakers behind scientific language and/or technical precision.[5] The cost of this can be political gridlock and distrust of science.

My work here attempts to alleviate this gridlock by attending to some of the ways that credible advice can be established.[6] To that end, my work does this by responding to two normative projects which philosophers Kyle Powys Whyte and Robert Crease, have identified for philosophers of science. The first of these, called unrecognized contributor cases, calls for the development of a rich normative theory of expertise and experience that

[5] The former relating to the Credibility Problem and the latter referring to the Value Neutrality Problem that I discuss above.

[6] By "credible" I simply mean "trustworthy, in the sense of offering good grounds for belief, but not infallibly true or correct. I take this to be an ordinary, rather than technical usage of the term. With respect to "credibility," Whyte and Crease (2010), on the other hand, follow Kristin Rollin, and others, in making a distinction between "credibility" and "trust" (413). They write, "Credibility has to do with what indicators are actually used by others as the basis for trusting scientists. Trustworthiness has to do with what indicators actually constitute why someone should be trusted, like their competence and sincerity" (Whyte and Crease 2010, 413).

can explain why the various epistemic insights of diverse actors should be trusted in certain contexts, as well as how credibility deficits can be bridged by rethinking expertise (Whyte and Crease 2010, 415). The social pluralistic account I develop in chapter four contributes to this project by, following the argument made by Plaisance and Kennedy (2014), leveraging the expertise of interactional experts to mediate between the scientific and technical considerations of a problem, and the values and expertise of lay publics. This solution also draws heavily on the work of Nancy Tuana (2013). In "Embedding Philosophers in the Practices of Science: Bringing Humanities to the Sciences," Tuana argues that philosophers can play an important role in interdisciplinary contexts by helping to identify the ethical dimensions of scientific research (2013, 1961 – 1965).

My view can be further contextualized using the second project Whyte and Crease identify: responding to poisoned-well cases. Such cases occur when lay publics distrust science and no amount of technical argumentation will prevail. The task Whyte and Crease identify for philosophers, is to develop concepts to explain the social factors leading to this distrust, so that science policy can still be formulated when conditions of distrust prevail. My second case study in chapter five offers one example of such a case. The reason, briefly, is that ordinary citizens held a different set of values from those of industry and the federal government. In response, the government used a deficit model framing of these holdouts to frame them as anti-science advocates (Bronson 2018). I offer an alternative solution to the case, showing how my view could have been used to help build trust and consensus among the various stakeholder groups.

By identifying factors that make science advice credible and trustworthy, we will have better tools for recognizing credible and responsible science-based advocacy and advising.

Chapter 1

Approaches to Science Advice and Advocacy

Knowledge will forever govern ignorance. And a people who mean to be their own governors must arm themselves with the power knowledge gives. A popular government without popular information or the means of acquiring it is but the prologue to a farce or tragedy, or perhaps both.

-James Madison in a letter to W.T. Barry
August 4, 1822

1.1 Introduction

We can trace arguments for the importance of experts in a government back to at least Plato. Plato famously argued that societies should be ruled over by "Philosopher Kings" (1991). In the modern era, Karl Popper criticized this view in *The Open Society and its Enemies*, arguing that it leads to authoritarianism (2013, 127). Though Popper refers to it as a "sophocracy," when referring to scientists it is better called a technocracy. Regardless of which experts are in charge, Popper thinks that "rule by experts" obviates the need for democratic discourse because it undermines the autonomy of the citizenry to choose how their society will run.

Yet governing is not the only role for experts in a democracy. The American Pragmatist philosopher John Dewey outlined a different role for scientists. Dewey argued that scientists should be technicians or technical advisors. Dewey argues that scientists, through their inquiry, can help us understand the challenges that society faces. Dewey also argues that scientists can provide us with knowledge about how to best approach solutions. Dewey offers a helpful analogy here. He writes, "The man who wears the shoe knows best that it pinches and where it pinches, even if the expert shoemaker is the best judge of how the trouble is remedied" (Dewey 2016, 224). So, while the general populace can communicate the problems they face, scientists and other experts can help us to find solutions to these problems.

Dewey's description captures one aspect of scientific advocacy for policy. That is, his description captures instances where a policy problem is identified, by the public and/or policymakers, and scientists are brought in to help solve it. But scientific advocacy for policy can take a variety of forms, ranging from advocating that science should be an input to policy (e.g., instead of, or in addition to, religious input), to science advisors who have privileged access to policymakers and are able to advocate for their views more directly.[7] As I map out below, I see advocacy and advising as existing on the same spectrum, where the difference between these two activities is best understood as a matter of degree, rather than of kind,[8] with the most salient (for my purposes at least) differences between the two being the strength with which a person or group can advocate. In part, this is influenced by the advisor/advocate's degree of access to policymakers. Another important difference between the advocate and the advisor is that the advocate may be more proactive in identifying policy problems, while an advisor may primarily be responding to policy problems identified by policymakers. Finally, while advocacy for science can be done by scientists and non-

[7] Smart and Hovland (2007), wrote a book for Overseas Development Institute, a thinktank based out of the United Kingdom, which places these on a continuum, with advising occurring in instances of high cooperation and advocacy occurring in instances of high confrontation. I develop my own continuum for these activities in a way that better supports the arguments I want to make.

[8] I will typically use "advising" as a general term to refer to the activities of advocacy and advising and make the distinction between advocacy and advising when necessary.

scientists alike, in most cases it will be experts advocating for particular science to be used in policy and scientists with some policy experience (e.g., science advisors) who will be advocating for particular policies.

The minimum level of engagement is merely advocating for the use of scientific knowledge in policy development. I call this "Science-is-Good" advocacy. This kind of advocacy may be identified with attending rallies in support of science for policymaking, such as the 2012 Death of Evidence Rally held in Ottawa in response to a series of controversial policy decisions by the Harper government, including the closing of the office of the National Science Advisor (Goldenberg 2015, 406). Not everyone may value science as a decision-making tool, so using scientific evidence may be regarded as controversial. This may, for example, be due to religious reasons. Religious fundamentalists may value religiously-inspired decision-making models. Others may fear that more science in decision-making in a democracy will undermine the effectiveness of democratic institutions by leading us to a technocracy (broadly the worry of Popper above). "Science-is-Good" advocacy is the most general and actions at this level can be effectively done by lay-publics, scientists, and NGOs (among others).

A somewhat stronger form of advocacy is advocating for *particular* scientific evidence to be used to inform policy. I call this "Use-my-Science" advocacy. This may be done by scientists in unofficial or official capacities (e.g., as members of science-based departments and agencies). Use-my-Science advocacy may also be done by lay-publics and NGOs, and may, for example, take the form of advocating that climate policies should be based on Intergovernmental Panel on Climate Change reports rather than other climate reports. Use-my-Science advocacy presupposes that science is a desirable tool for decision-making and advocates offer their tools and knowledge to policymakers to inform their decisions. Depending on the size of the organization, such advocates may be able to secure an audience with policymakers. In this sense, the specificity of the advocacy is greater than advocating merely that science be used as a decision-making tool, but less specific than advocating for specific policies.

Third, we have advocacy for *particular policies* informed by scientific evidence. I call this "Use-my-Policy" advocacy. This may be done by a Chief Science Advisor (CSA). For example, a CSA may advocate for a cap-and-trade system of carbon reduction, rather than a carbon tax. This kind of advocacy is the strongest because it involves being committed to, and advocating for, specific policies. It presupposes both that science is a desirable tool for decision-making and which scientific discipline or research is relevant to solving the problem. This is the sort of thing that Dewey has in mind with the shoemaker example. Advocates in this position already understand that there is a problem, and are, in instances of this third kind, offering advice on how to best solve it. As we consider these different kinds of advocacy, we see that there is also an increasing importance of expertise as the strength of the advocacy increases.

However, there is plenty of room for nuance regarding who is advocating with what level of expertise and at what point on this continuum their activities fall. Science and Technology Studies (STS) scholars have offered case studies showing that non-scientists, often referred to as lay publics, can acquire scientific expertise despite having no formal scientific training. For example, AIDS advocates (often gay men who are HIV positive, but also their allies) in the mid-1980s successfully gained the expertise necessary to establish their credibility within the medical community, despite little to no formal scientific training (Epstein 1995). This type of expertise is now referred to as "interactional expertise" and refers to the ability of non-experts to speak the language of a particular discipline without having the ability to practice in that discipline (Collins and Evans 2002). So, advocacy/advising can be successfully done by non-scientists at these different levels as well. However, for simplicity's sake, I will assume that advocacy will be done by experts in their capacity as representatives of various institutions.[9] So, for example, Dr. Mona Nemer may advocate in her capacity as Canada's Chief Science Advisor when she provides advice to the Prime Minister or Members of Parliament. But advocacy may also be conducted by an external panel of experts, such as those convened by the Council of Canadian Academies (CCA) to study and provide advice on issues it is tasked with by the federal government. Recent CCA reports include advice on medically assisted dying, policing in Indigenous communities, and natural resource management. Advocacy may also be done by scientists in their capacity as government-employed scientists, and finally, advocacy can be done by non-profits and think-tanks such as Evidence for Democracy, The Union of Concerned Scientists, or the Broadbent Institute.

Scientists advocating for the use of particular scientific evidence, or for the adoption of particular policies, may be seen as offering scientific advice, especially in the context of

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[9] My phrasing here is intended to reflect metaphysical considerations about the agency of institutions as well as the relative sizes of different institutions. While we can talk sensibly about something like the Royal Canadian Society putting out a report, the report is the work of several people, and those people may speak individually in advocating for the report. In other cases, the institution is a single person, like the Chief Science Advisor of Canada. While there is an office supporting her, it seems more appropriate to refer to advocacy done in this position as stemming from the person in the position qua that position.

writing reports, where policy recommendations often follow once the problem has been articulated and understood. Advising and advocacy are similar as in both cases: the actor stakes out a claim about what the problem is, what should be done, and/or what scientific research is credible and relevant. That said, advisors and advocates do differ insofar as they have different access to and levels of influence with policymakers. Furthermore, while neither advocates nor advisors can compel policymakers to act, they both can wield institutional influence as a political tool (e.g., The Union of Concerned Scientists could mobilize its membership to campaign on an issue, while Canada’s Chief Science Advisor exerts political influence by virtue of her being a respected scientific authority and by holding a respected position). Nevertheless, the advocate and advisor positions differ in the respect that typically, the judgment of a science advisor is solicited, while that of an advocate is not. My reason for treating these two activities cohesively is due to the similarities between the two activities and the fact that advisors have in some cases been accused of “having an agenda” (see, for example Douglas 2009, Jasanoff 1990). Such accusations seem to me to imply that advisors are using their position improperly to advocate for their own views. McGarity and Wagner in their 2008 book, *Bending Science*, appear to use the term “advocates” to refer specifically to special interest groups who attempt to manipulate scientific research or science-based policies to their own ends. My own usage of the term is more neutral, meant to refer both to the advocates described by McGarity and Wagner, but also to advocates with more noble intentions.

Philosophers and science and technology studies scholars have identified the worry that scientific advocacy (or value-laden science) for policy potentially threatens the utility of science in policy (e.g., Douglas 2009, Goldenberg 2015, McGarity and Wagner 2008, Pielke Jr. 2007, and many others). If advocates are seen to have vested interests or as being motivated by ideology, the legitimacy of their claims seems to be undermined. After all, if scientific research is useful in policy because of its supposed objectivity and disinterestedness, then value commitments (especially when those value commitments are seen to be political in nature, such as in the case of accepting climate change) risk undermining the epistemic authority of science, and therefore, its trustworthiness and effectiveness as a policy tool. However, since science is such a powerful tool, and policy development is inescapably political, it is important to have guidance on how to responsibly advocate for science for policy.

The need for responsible science advocacy/advice for policy has been recognized by the scientific community. As early as 1972 the physicists Frank von Hippel and Joel Primack wrote, “Science advising, not less than scientific research, needs a code of ethics. And this code should consider the fact that we live in a democracy in which the ultimate responsibility resides not with the President, or even with the government as a whole, but with the individual citizen” (von Hippel and Primack 1972, 1169). This need, described by von Hippel and Primack, is especially recognized in fields with obvious political implications arising from research, such as the field of conservation biology. While participating in policy development is relatively uncontroversial, the nature of that participation often is. Since scientific research is an authoritative source of knowledge, much has been written about how science and scientists should be involved in the policy process. We can sort these responses into three views. These are the “Pure Science” view, the “Boundary Actor view” and the

"Honest Broker view." What is common to each of these views is that they all see value in having science-for-policy appear to be the result of wholly disinterested inquiry, which is to say, each view sees value in having science appear free from values. The Pure Science View is a composite view constructed out of my analysis of the arguments of scientists presented in policy-relevant journals. The Boundary Actor View comes from Sheila Jasanoff's work on science advising in her book *The Fifth Branch: Science Advisors as Policymakers*. I have named it the "Boundary Actor View" because of the prominent role she gives to science advisors as actors at the science and policy boundary. The Honest Broker View comes from Roger Pielke Jr.'s work on science advising in his book *The Honest Broker: Making Sense of Science in Policy and Politics*. I discuss each of these below.

1.2 Pure Science View

The "Pure Science" View articulates that scientists should advise on science related to policy, but should focus solely on communicating the science, and not on evaluating policy decisions or making policy recommendations. The idea here is that by focusing on "facts" scientists can stay above the value-laden debates typical of politics. Based on my understanding of the positions captured under this view, the Pure Science View argues that by staying value-neutral or value-free, scientists can be seen as impartial advisors or arbitrators of policy disputes. Scientists, and science itself, can be seen as being "beyond" politics. This is done to preserve the (perceived) integrity of science in the policy process.

As noted above, this view is a composite view constructed from the arguments presented by scientists in the editorial pages of policy-relevant journals. As I will show below, this view can be split into two camps: those who believe that science should be used in policy, but scientists should stop short of advocating and instead respond to requests from policymakers (the Anti-Advocacy Pure Science View),[10] and those who believe that believe that advocating for certain causes, if not necessarily policies, is permissible, so long as best practices are followed (the Pro-Advocacy Pure Science View).[11] While Robert Lackey (2006) offers a passionate defense for the former position, Judy L. Meyer and her co-authors, offer a strong argument for the latter. For the sake of simplicity, I use Lackey and Meyer et al. as representatives of their respective camps, since their articles are frequently cited. However, as these are representative, there is of course nuance and variability within each camp. Finally, the Pure Science View is characterized by a commitment to ensuring that science is used in policy but stopping short of advocating for specific policies.

Historically, there has been much debate within the scientific community about the appropriateness of scientists advocating for certain policies. This debate has taken place in the editorial and commentary sections of policy-relevant journals like *Conservation Ecology* or *Journal of Wildlife Management*. In these journals the Pure Science View is often espoused by scientists themselves. For example, Robert Lackey wrote the paper I discuss during his role as a scientist at United States Environmental Protection Agency, while Judy

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[10] This is very similar to the Pure Scientist view discussed by Roger Pielke Jr. in the third section of this chapter, which is why I have opted to call this the "Pure Science View." However, the way that Lackey characterizes his position also bears some similarities to the Science Arbiter view discussed by Lackey.
[11] This view is similar to the Science Arbiter view discussed by Roger Pielke Jr. in the third section of this chapter, but the way that Meyer et al., characterize it also draws some similarities to the Issue Advocate and Honest Broker roles discussed by Pielke. I include this view under the "Pure Science View" because their concern seems to be the role of science and scientists in policy, rather than a fulsome consideration of other inputs that may go into science-related policymaking.

L. Meyer is an ecologist at the University of Georgia. The Pure Science View is primarily concerned with preserving the integrity of science by protecting the objectivity of scientists. Oftentimes, the supposed key to protecting the objectivity of scientists is by eliminating or severely restricting the presence of values in science so that scientists may be seen as detached and above politics. The worry then, is that if scientists begin to weigh in on policy discussion, it will ultimately undermine trust in science. This happens because scientists may be seen as "hired guns" for a political agenda (whether they are hired or just 'fellow travelers') by hiding behind their scientific credibility.[12]

As I've alluded to above, one influential attempt at preserving the credibility of science, from a scientist, comes from the biologist Robert Lackey. He believes that the key to preserving the epistemic integrity (and so authority) of science is to completely remove anything remotely resembling values or normative judgment in science. For Lackey, this means that scientists should stop using words like "degradation, improvement, good, and poor" in ecology or conservation science, since they imply a preferred ecological state (Lackey 2006, 14). Despite desiring the prohibition on value-laden, evaluative language, he seems to acknowledge that there are values in science, writing, "It [science] is after all, a human enterprise, but this fact does not make all science normative" (Lackey 2006, 13). However, Lackey does not go on to elaborate as to the specifics of how science is normative or involves values. This has a curious implication. While trying to eliminate values from

[12] Although I've put these together, the former (i.e., being seen as hired guns) presupposes the latter. Some authors have worried that scientists may simply be arguing for their personal policy preferences using their scientific authority.

science, and so adhering to the value-free ideal, Lackey acknowledges that there are values in science, and implies that *some* science *is* normative. However, he does not go on to elaborate as to which science is normative and in what ways science could be normative and so, the tension in his view remains unresolved.

Yet for all this, Lackey believes that scientists should provide advice to policymakers, so long as the advice they provide is "policy neutral." According to Lackey, such advice "strives to describe the world accurately and is characterized by transparency, reproducibility, and independence" (Lackey 2006, 14). Lackey does helpfully note that scientists should not "fall into the trap of substituting debate over scientific information and interpretation of data for debate over which values and preferences will carry the day" (Lackey 2006, 16).

Lackey attempts to preserve the epistemic integrity of science and scientists by keeping science and values far apart. He sees values as intrinsically harmful to the epistemic authority of science, and so sees the inclusion of value-laden terms as undermining the credibility of scientists.

It is unclear how much support positions similar to Lackey's have in the research community. Although Lackey is frequently cited, one group of researchers, led by geographer Gerald G. Singh (2014), conducted a study highlighting the importance of understanding the appropriate role for scientists and providing guidance to scientists on how to advocate responsibly. The authors identify the "ideals of objectivity, the integrity of science, and disinterested inquiry" (Singh et al. 2014, 161.), as reasons in the literature for why such scientific advocacy is seen as controversial. In their study, Singh et al. found that environmental scientists largely believed, contrary to Lackey, that "scientists should engage in science interpretation, integration, and even advocacy" (2014, 164). Yet, despite the apparently widespread desire for guidance on scientific advocacy, the authors provide none on how to do this responsibly.

Fortunately, Meyer et al. pick up this work, highlighting the need for an answer as to the proper role of scientists in the formation of policy, writing that "[i]n advocacy, no such vetting procedure exists [i.e. peer review], either to distinguish 'advocacy-driven science' from 'science-based advocacy' or to define and promote appropriate 'norms,' or rules of behavior, for how scientists should advocate" (Meyer et al., 2010, 299). While noting that some say, "to *advocate* for specific policy outcomes imperils the hard-earned reputation of scientists as objective and trusted sources of expert knowledge" (ibid.), they take a pro advocacy stance, outlining what they believe are important considerations for scientists to make before engaging in policy advocacy. As part of this, they include a set of best practices. These practices include things like bringing scientific information to the attention of policymakers and the public, ensuring that policy design is consistent with the best available evidence, and ensuring that policymakers consider the relevant science (Meyer et al., 2010, 299 – 301). They also recommend that scientists "speak only to what they know, and to acknowledge when they are moving beyond their area of expertise" (Meyer et al., 2010, 301). While helpful, Meyer et al. only recognize objectivity as crucial to trust in scientists. As I will argue in later chapters, trust judgements are more than merely epistemic judgements, and so, ascriptions of trust are based on more than scientific objectivity. Thus, I believe that this position can be bolstered, and the best practices expanded, to build trust more effectively in scientific institutions.

At minimum, the views discussed in this section point to an ongoing need to provide a way to advocate for science to be used in policy, while doing so in a way that protects and preserves the integrity of science and the importance of objectivity as a means for doing so. However, as I will argue in chapter three, I think this view is too narrow to provide scientists, science advisors, and policymakers with the tools for supplying and assessing credible scientific advice.

1.3 Sheila Jasanoff's Boundary Actor View

Science and technology studies scholars have also offered accounts of what responsible science-based advocacy looks like. One prominent view comes from Sheila Jasanoff. In her book *The Fifth Branch: Science Advisors as Policy Makers*, Jasanoff uses several case studies to argue that the answer to responsible advocacy is through ensuring that lay-publics are involved in the shaping of policy-related issues. She also argues for ensuring that the boundaries between science and politics remains firm. In this regard, Jasanoff shares the concern of Lackey and others above. Jasanoff recognizes the need for science to be perceived as transparent, reproducible, and independent. Jasanoff writes that "[i]t is now widely recognized that the questions regulators need to ask of science cannot in many instances be adequately answered by science" (Jasanoff 1990, 7) and that "[n]evertheless, there is an unspoken presumption in many of the aforementioned works that better scientific characterization of a problem will lead to better policy" (ibid.). The problem that Jasanoff is trying to resolve is this:

> scientific uncertainty and the pressures of decisionmaking lead to a forced marriage between science and politics. Guidelines for cancer risk assessment are a typical product

> of this unnatural union, an unstable policy instrument in which the balance of scientific and political considerations can disintegrate at any moment as a result of changes in either knowledge or politics (Jasanoff 1990, 8).

We want science to inform policy, but the differing constitutive values of these two areas make their union tenuous. Importantly, Jasanoff differs from the scientists in the Pure Science View, and by Roger Pielke Jr. below, by explicitly acknowledging that science can never be wholly separate from values. However, her science advising solution still attempts to present science advice as value-free. She notes that regulatory risk management require "most policy decisions to be founded on an explicit trade-off between risks to health and the environment and the economic and social costs of regulation" (Jasanoff 1991, 3) and that

> [u]nder these circumstances, it became difficult for agency officials – seen by many as an overly powerful fourth branch of the government - to avoid the impression that they were manipulating scientific knowledge and shielding fundamental political choices behind the pronouncements of a still more inscrutable 'fifth branch' of technical experts" (ibid.).

In chapter three, I will argue that Jananoff's approach, while a helpful step forward, does not protect science advisors from accusations that they have manipulated scientific knowledge or are shielding "fundamental political choices" behind scientific credibility. In chapter four, I argue that we can better resolve this problem through diverse and interdisciplinary science advisory panels who can articulate the political choices involved in adopting one policy rather than another.

Jasanoff goes on to say that any solution to this problem must account for three major findings from the sociology of science. The first finding is that facts are constructed, and that "[w]e regard a particular factual claim as true not because it accurately reflects what is out there in nature, but because it has been certified as true by those who are considered competent to pass upon the truth and falsity of that kind of claim" (Jasanoff 1990, 12 – 13). The corollary of this, as Jasanoff notes, is that facts can also be deconstructed, and no longer considered facts. While a powerful tool for creating knowledge, science and scientists are not infallible and what counts as an accepted scientific fact at one point in time, may no longer be considered as such at a different point in time.

The second major finding that Jasanoff argues any solution must account for, is the challenge from the sociology of science of the notion that "scientific facts are tested and established with reference to objective criteria of validity" (Jasanoff 1990, 13). As a social constructivist, Jasanoff also follows Thomas Kuhn in noting that what counts as "objective criteria of validity," can be historically contingent on the paradigm one is operating in.

The third finding is that a solution must consider what is known as 'boundary work.' Boundary work refers to the ways that scientists build professional communities, define and exclude non-members, compete for resources, and assert their autonomy against external controls (Jasanoff 1990, 14). In short, it refers to the ways in which scientists preserve their epistemic authority and independence. Each of these three findings helps situate science as a social process and activity embedded in a broader cultural context.

It is important to note that Jasanoff sees these boundaries as being created, rather than part of the intrinsic structure of the science-policy or science-society relationship. Her writing here foreshadows a concept she would go on to develop in later work. The concept in question, called "co-production," describes the various ways in which science and technology shape and are shaped by the epistemic and normative meanings in a culture (Jasanoff 2007 17). Under this framework, science and technology scholars examine science as part of a broader social process, by looking at the ways that science and technology interact with the rest of society. It is a conceptual tool for understanding the relationship between the "ordering of nature" and the "ordering of society." As we discover new information about the world, we develop new ideas about how society should be organized. For example, recognition of anthropogenic climate change has, in some cases, led to new proposals for the redistribution of capital in the economy, whether this takes the form of government subsidies and investments, a tax on carbon, or some combination thereof. Co-production recognizes science as embedded in the broader cultural context. As new knowledge and technologies are created, our social institutions are reshaped, which leads to new epistemic and technological developments. Co-production, then, describes the ways that science interpenetrates the numerous concerns of society and the ways that the concerns of society shape science and technology.

Through the lens of co-production, boundary work takes on a new level of precision. Instead of merely being the study of how scientists assert and preserve their epistemic integrity and independence, which was a core focus of the Pure Science view above, under the lens of co-production, studying boundary work can show how these boundaries are shaped and negotiated by a variety of actors in a variety of contexts, and as such, allows for new ways of understanding how credibility is established. To wit, boundary work under the co-production framework recognizes the importance of non-scientists' views of this boundary as equally important to understanding its shape. This view of boundary work also allows us to examine the ways in which science and values interact, including how science can shape or make salient certain values, and how values can shape how science is done.

Understanding the relationship between science and values is *a specific way* of managing the border between science and non-science.

Historically, this border has been maintained through a scientific commitment to objectivity and has been strictly policed. However, in the 20th century and into the 21st, philosophers have renegotiated what this border ought to look like, by re-examining the relationship between objectivity and values, which I discuss in the next chapter. Yet as we have seen, many scientists continue to hold more traditional views regarding the shape of the science-policy interface. Furthermore, as we will see in Chapter two, there is disagreement among philosophers about what this border looks like. Nevertheless, as Jasanoff recognizes, any solution of the problem of responsible science advocacy will require careful negotiation of this boundary to preserve the epistemic integrity (i.e., objectivity) and credibility of science.[13]

Appropriately then, Jasanoff's solution is to ensure that the perceived integrity of the boundary between these two areas remains intact. Citing the need to balance democratic concerns against the need for technological and scientific expertise (i.e., citing the need to avoid a technocracy), she writes that "[r]egulatory practices at EPA [Environmental

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[13] While Jasanoff does not define "objectivity" in this text, I believe her view recognizes the political capital that calling something "objective" has. Naomi Scheman, who I discuss in chapter four, defines "objectivity" as way of identifying trustworthy information (2011, 24 – 25). Said differently, to call a claim objective is to say that it has epistemic integrity. I take Jasanoff to be securing epistemic integrity in her view by having stakeholder values shape the framing of the problem, and perhaps the methodologies used, but having the advice delivered by an independent, disinterested panel of scientists. This is Jasanoff's attempt to find a middle ground between the "democratic critique" of science policy, which argues that science does not speak truth to power in a value-free manner and so there should be broader democratic participation in science policy, and the technocratic view that more science is needed for policymakers to make better decisions and that too much democratic participation in science policy undercuts the perceived legitimacy and integrity of the science.

Protection Agency] and FDA [Food and Drug Administration] support the thesis that negotiation - amongst scientists as well as the lay public - is one of the keys to the success of the advisory process (Jasanoff 1990, 234). Boundary work, on the other hand, "is the casing that gives the [scientific] result legitimacy" (Jasanoff 1990, 236). It is by managing this boundary, Jasanoff argues, that science maintains its authority in the policy-context. Jasanoff goes on to say that "[t]he creation of such boundaries seems crucial to the political acceptability of advice" and that "[w]hen the boundary holds, both regulators and the public accept the experts' designation as controlling, and the recommendations of advisory committees, whatever their actual content, are invested with unshakable authority" (ibid.). By ensuring that the science being done is seen as separate from the politics of the topic in question, stakeholders can be assured that special interests are not manipulating science to suit their desires. Jasanoff adds that "the most politically successful examples of boundary work are those that leave some room for agencies and their advisors to negotiate the meaning and location of the boundaries" (ibid.).

Thus, for Jasanoff, the creation of a boundary between science and policy creates the appearance that the scientific recommendations are free from political interest, values, or bias, in the eyes of the policymakers and public.[14] However, this is not to say that the resulting advice is free from political concerns, social values, or biases. As Jasanoff puts it, "[w]hen the boundary holds, both regulators and the public accept the experts' designation as

[14] That said, Jasanoff does acknowledge that while policymakers have moved away from the view of seeing facts and values as wholly separate, they do not necessarily *openly* subscribe to the opposite view of seeing them both as integral to risk assessment and science advising. Based on my own experiences at science policy conferences and as a science policy analyst for the federal government, this continues to be true.

controlling, and the recommendations of advisory committees, whatever their actual content, are invested with unshakable authority" (1990, 236), even when the 'scientific' recommendations put forward includes social and political value judgments.[15] That is, having this boundary ensures the legitimacy and authority of science in policy by explicitly allowing values and interests only at the initial stages of the process, when the boundaries are being negotiated, so that problems and solutions may be framed before the research is conducted. The science that is done afterwards to determine a regulatory outcome is, it is supposed, insulated from accusations of bias because the boundaries of the scope of the advice are negotiated early on, when stakeholder interests are recognized and the external scientific community is engaged. However, as we will see in chapter two, values play some role in science at every stage of inquiry. Jasanoff's solution, then, is limited in that scientists remain open to accusations of bias without a more rigorous understanding of the acceptable and unacceptable roles for values in science, and the ways in which objectivity may still be preserved.

1.4 Roger Pielke Jr.'s Honest Broker View

More recently, Roger Pielke Jr. has offered a model for understanding the role of scientists in society in his book *The Honest Broker*. In the book, Pielke identifies four archetypes of science in policy and politics, and the associated roles for scientists. Unlike Lackey (and

[15] In chapter two I discuss the work of Heather Douglas. In my opinion, Douglas builds on Jasanoff's work to articulate, in a more fulsome way, the roles that values can play and how we can ensure they are playing appropriate roles, in a framework like the one laid out by Jasanoff. It is unclear whether Jasanoff would accept Douglas's developments, but in my own view, Douglas's work enrichens Jasanoff's and provides helpful clarity.

other scientists) or Jasanoff, who operate more squarely at the boundary of science and policy, Pielke's view places him more squarely on the policy-side of things, focusing on the appropriate roles for scientists once they've entered the policy realm. To that end, Pielke advocates for scientists to be value-neutral in the policy sphere. In this regard his view closely mirrors those discussed above.

Pielke, following science and technology studies scholar Daniel Sarewitz, argues that science itself offers an "excess of objectivity." What Sarewitz (and Pielke) mean by this is that "nature can be viewed through many analytical lenses, and the resulting perspectives do not add up to a single, uniform image, but a spectrum that can illuminate a range of subjective positions" (Sarewitz 2000, 90). So, because nature can be studied by a variety of disciplines using a variety of tools, techniques, and models, all of which produce objective (but potentially different) results, it is comparatively easy for special interests to find some research which supports their view.[16] Consequently, Pielke and Sarewitz see the objectivity of science as hindering, rather than helping, settle matters of policy due to this excess of objectivity. Since any policy position can be justified based on *some* evidence, science hinders, rather than helps the policymaking process. Pielke argues that this is because relying on science masks the value differences that are at play in policy debates. This differs from the earlier theorists we've looked at, who view the objectivity and impartiality of science as essential to its credibility, and so utility and acceptability, in the policy process.

[16] In chapter four, I provide a response to the problem of excess objectivity by developing a pluralistic account of the criteria for objectivity.

I will now turn to the archetypes that Pielke identifies for scientists once they cross the boundary into the policy realm. The first is what Pielke calls an “issue advocate” (Pielke Jr., 2007, 2). The issue advocate makes the case for one policy alternative over another (ibid.). Opposite the issue advocate is the titular “honest broker of policy alternatives.” The honest broker, Pielke tells us, makes “an effort to expand (or at least clarify) the scope of choice for decision-making in a way that allows for the decision-maker to reduce choice based on his or her own preferences and values” (Pielke Jr. 2007, 2 – 3). The difference between these two roles is that “[t]he Issue Advocate seeks to compel a particular decision, while an Honest Broker of Policy Alternatives seeks to enable the freedom of choice by a decision-maker” (Pielke Jr. 2007, 3). Finally, there are also the ‘pure scientist’ and the ‘science arbiter.’ The former simply offers interesting/relevant information, while the latter acts more like a concierge, answering any questions the policymaker has. Pielke tells us that the issue advocate, more than any other role, “threatens the others, particularly the Honest Broker of Policy Alternatives” (2007, 135) and this is because “[i]ssue advocacy often takes a stealth form in which scientists characterize their role as Pure Scientist or Science Arbiter, but are really using their scientific authority as a tool for advocacy” (ibid.).

Despite claiming that he thinks all these roles for scientists are acceptable, including the role of issue advocate, Pielke views stealth issue advocacy as the greatest threat to the integrity of science in the policy process and science more generally, for the reasons we saw above. Thus, if scientists are to be “Honest Brokers,” they require guidance on how to do so responsibly lest they fall into the trap of becoming stealth issue advocates. I pick up the problem of stealth issue advocacy below and highlight some of the shortcomings of Pielke's distinction between issue advocates and honest brokers in chapters three and four.

To help guide scientists, Pielke offers two suggestions for becoming so-called Honest Brokers (Pielke's preferred role). These take the form of questions. The first question is this: "If your policy recommendation is indeed based on scientific results, what scientific information would be necessary to change your policy position? (If the answer is 'no information' then why depend on science at all?)" (Pielke Jr. 2007, 141 – 142). The second question is this:

> A range of policies is consistent with particular scientific results. What is the full range of options that you see as consistent with the state of science in order to achieve particular desired ends? Within such a range, what factors other than science do you use to settle on one policy, or group of policy options, over others? (Pielke Jr. 2007, 142).

The implications of these questions will be taken up in chapter three. In chapter three, I show how Pielke's questions presuppose a Popperian view of science and that this framing overlooks many of the roles that values can play in the course of scientific research. So, while Pielke's questions are attempting to get at something important, that is, what values are shaping the advice one is giving, one could still answer these questions in a way that curates evidence in a way that supports their preferred policy options. In short, I show how honest brokers can still slip into stealth issue advocacy. For now, however, I will explore what motivates Pielke to settle on the Honest Broker as the ideal role for scientists in the policy process.

Pielke writes that the roles that we expect experts to play in a democracy will hinge on how we think a democracy should operate. This requires us to make two considerations. The first is the role is of science in society, and the second is the roles for experts in a democracy.

In response to the first consideration, Pielke rightly criticizes what is known as “the Linear Model” of science in policy. This model is most closely associated with the American engineer Vannevar Bush (see Douglas 2009; Jasanoff 1990, Greenberg 1968, Sarewitz 1996) and his 1945 report for President Roosevelt, *Science: The Endless Frontier.* In his report, Bush outlines how support for basic research can help democracies flourish by providing a resource for decision-makers. The promise that Bush made was that by supporting basic research, society would benefit democratically and economically in the long-term. By having a pool of knowledge, scientists would be available when called upon to provide answers to technical, policy-related questions. Bush also promised that basic research would provide economic opportunity as basic research led to new technologies and innovations. The Linear Model lends itself to a technocratic understanding of the role of science in decision-making: policy decisions will be settled by relying on science rather than on public debate. Thus, the Linear Model undermines our democratic responsibilities.

In the latter half of the 20th century, research from STS and policy scholars has shown that the Linear Model is empirically false (e.g., Jasanoff 1990; Cairney 2016). Science and policy interact in several ways, at a number of points throughout the policy process. Furthermore, funding basic research has not led to the reservoir of knowledge promised by Bush, and while advances in basic research do lead to advances in applied research, advances in applied research also lead to advances in basic research (e.g., Sarewitz 1996; Greenberg 1966).[17]

Pielke also rejects the Linear Model of science policy, calling it “descriptively inaccurate and normatively undesirable” (2007, 13). Instead, he offers the Stakeholder Model, which “holds not only that the users of science should have some role in its production but that considerations of how science is used in decision-making are an important aspect of understanding the effectiveness of science in decision-making” (Pielke Jr. 2007, 14), in contrast to the Linear Model, where lay-publics are not usually given a significant role in the science-policy process except, perhaps, as background actors in shaping the political considerations of policymakers. The stakeholder view described by Pielke is very much in keeping with the view described by Jasanoff above. The concepts of co-production and boundary work recognize that there is a myriad of stakeholders beyond scientists and policymakers, that value and use scientific information in a variety of ways and contexts, as Jasanoff’s case studies show.

On the question of “how should a democracy function, and what should the role of experts be?” Pielke associates two of the archetypes (the Pure Scientist and the Issue Advocate) he discusses with one of the founding fathers of the United States, James Madison.[18] Under a Madisonian view of democracy, Pielke tells us, different groups engage

[17] One example of applied research leading to advances in basic research supplied by Sarewitz is how research into tracking German U-Boats in World War II led to magnetic mapping of the seafloor, which led to the plate tectonics revolution in geology (1996, 37).
[18] In the epigraph of this chapter, I used a quote from Madison written in a letter responding to one W.T. Barry about the Kentucky legislature’s financial appropriations to implement a public education system. Whether Madison himself believed what he wrote, or not, the quote is helpful to illustrate my own belief on the

in political debate, and if they reach a compromise, it "reflects the best possible balancing of conflicting demands" (Pielke Jr. 2007, 11). Pielke calls this 'interest group pluralism'. When the Madisonian view of democracy pairs with a Linear Model of science policy, we have his archetype of the "Pure Scientist." Pielke says that this archetype is found in myth more often than reality, but in principle, the Pure Scientist would do research and different interest groups would be able to draw on this research to argue for their interests (2007, 15). When we pair the Madisonian view of democracy with the Stakeholder Model of science policy, we have the Issue Advocate, where a scientist focuses on the implications of their research for a particular political agenda (Pielke Jr. 2007, 15). In such a case, the role of the Issue Advocate is to ally with their preferred faction and lend their expertise.

The second view of democracy Pielke offers comes from E.E. Schattschneider, who offers an alternative to the Madisonian view. The two remaining roles that Pielke identifies, the Science Arbiter and the Honest Broker, fall under this view. Pielke tells us that while Schattschneider also sees democracy as a competitive system, the public is allowed to participate by voicing its views on the alternatives presented to it. Thus, under this model, the policy alternatives come from experts, rather than from the populace. Pielke writes "It is the role of experts in such a system to clarify the implications in the form of policy alternatives to decision-makers who can then decide among different possible courses of action" (Pielke Jr. 2007, 12). When we pair the Schattschneiderian view of democracy with the Linear Model of science policy, we have the role of Science Arbiter. When we pair it with a

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importance of making knowledge accessible to all members of a society so that they can make informed policy choices. Here Pielke is drawing from a different area of Madison's corpus.

Stakeholder Model of science policy, we have the Honest Broker. Pielke tells us that the Science Arbiter "seeks to focus on issues that can be resolved by science, which may originate in questions raised by decision-makers or debate among decision-makers" and "avoids normative questions and thus seeks to remain above the political fray" (Pielke Jr. 2007, 16). The Honest Broker, on the other hand, "seeks explicitly to integrate stakeholder concerns in the form of alternative possible courses of action" (Pielke Jr. 2007, 17).

Pielke maps out his roles like so:

		View of science	
		Linear model	Stakeholder model
View of democracy	Madison	Pure Scientist	Issue Advocate
	Schattschneider	Science Arbiter	Honest Broker of Policy Alternative

Fig. 1 – Taxonomy of roles for scientists in Pielke's view. [19]

Obviously, Pielke's categories are meant to be idealizations. So, democracy can have Madisonian-like tendencies, and Schattschneiderian-like tendencies. Neither, however, will fully capture all the dynamism of a modern democracy. His claim that all the roles for

[19] Jasanoff 2008

scientists are acceptable makes this the most charitable and likely interpretation, since two of the roles fall under the Madisonian view of democracy. However, Pielke has a clear preference for a Schattschneiderian view of democracy, given the title of his book.

After reviewing Pielke's taxonomy of the roles, and their emergence from the interaction of different considerations, it is unclear why the roles of the pure scientist, or the science arbiter are included, given his rejection of the Linear Model. The reason for their inclusion is that the Linear Model still receives 'buy-in' from scientists and policymakers. That is, despite its shortcomings, these audiences, especially scientists, still find it compelling (Pielke Jr. 2007, 81) and this, Pielke tells us, is what leads to stealth issue advocacy, since many times scientists see themselves as playing the role of Pure Scientist or Science Arbiter while also arguing that a certain policy response necessarily follows from their research (2007, 7).

While Pielke discusses values, he does so in the context of policy, not science. Indeed, Pielke makes no mention of the roles that values can play in choosing a research topic, choosing a methodology, or interpreting data. However, he does, in a way similar to what we will see from Douglas in chapter two, suggest that values can guide our decision making in the face of uncertainty. Pielke recognizes that usually policy debate (i.e., politics) is incredibly value-laden, as different interest groups compete for their position to win.

Pielke distinguishes between two contexts of politics: Tornado Politics and Abortion Politics. Tornado Politics, so named because of the comparatively simple politics and policy Pielke associates with tornado disaster-response, refers to decision-making contexts where "[t]he scope of the choice is unambiguous, discrete, and bounded," "[n]o ambiguity exists about the desirability of different outcomes," "[n]o ambiguity exists between alternative actions and desired outcomes,"[20] and where "improving information on which decisions are based promises insight into understanding the relationship of alternative courses of action and desired outcomes" (Pielke Jr. 2007, 24).

On the other hand, Abortion Politics, so named because of the more value-pluralistic nature of the associated politics and policy, refers to contexts where "[t]he scope of the choice is ambiguous, but it is also contentious," "conflict exists about the desirability of different outcomes, because there are multiple possible outcomes and multiple conflicting interests," "[c]onsiderable ambiguity exists in the relationship between alternative actions and desired outcomes," and finally, where "improving information promises little insight into the course(s) of action likely to lead to a desired outcome, at least over the near term. But other options are available that might lead to desired outcomes" (Pielke Jr. 2007, 28). Thus, while Pielke acknowledges the role that values play in politics and policy, he makes no mention of the role of values in science and as we will see in chapters three and four, it is the presence of values in science which leaves science advisors open to attacks. As we will see in chapters two and three, values in science play an important role in contextualizing the appropriate roles for scientists, and, how to act responsibly in those roles. Furthermore, we can see how Pielke's preferred role for scientists, the titular 'Honest Broker,' implies a value-

[20] I think Pielke drastically oversimplifies the distinction between tornado and abortion politics. As I state in my introductory section, there are well documented instances of special interests creating doubt and ambiguity about the strength of the evidence and desired outcomes (e.g., *Merchants of Doubt* (2010), and *Bending Science* (2008)). Furthermore, while it is certainly sensible to evacuate when there is a tornado warning, people may hold conflicting values and beliefs which makes the decision more difficult. People may not believe the risk of a tornado to be great, may not believe it will affect them, may not want to leave their home, or may have a family member with mobility issues which makes leaving challenging, all which conflict with their (presumed) value of self-preservation.

neutral position for scientists on policy questions. I develop this point further in chapter three to explicitly show how the Honest Broker correlates to a value-neutral position for scientists on policy questions.

1.5 Analysis and Conclusion

In this chapter I traced out three general views of scientists as advocates and advisors in the science and technology studies literature. While there is general agreement that scientists should be involved with policy formation in some capacity, the extent and nature of that capacity is still the subject of debate. Each of the views I've discussed in this chapter recognizes the need that the public and policymakers have for credible information. However, each of these views differs in how "credibility" gets cached out. In Lackey's view, it is by not using value-laden language, in Meyer et al.'s it is by being impartial, factual arbiters, in Jasanoff's it is allowing stakeholders to make political judgments at the beginning of the advisory process so the resulting advice can be seen as objective, and for Pielke, credibility means being open and honest about what policy options are consistent with the available evidence, and what non-scientific factors led you to settle on those options. What seems common to all of these is a notion of objectivity-as-impartiality. While some form of impartiality is important, as I will argue in chapters three and four, "impartiality" can be cached out in at least five different ways, and the views discussed here tend towards either detached objectivity or value-neutral objectivity, while other kinds of objectivity may be more appropriate and useful.

Furthermore, the theorists under examination try to keep values and science separate to preserve the epistemic authority of science. They contend that objectivity is essential to scientific authority and seem to imply that the presence of values in science undermines this authority, despite a general acknowledgement of the presence of values in science. I examine these views more fully in chapter three using tools I identify from the philosophy of science literature in chapter two. Among the scientific community, we have those like Lackey, who believe that science should remain distinct from policy and that it is inappropriate for scientists to advocate, although advising is acceptable in politically neutral terms. Yet we also have those who say that such behaviour isn't inherently harmful to science, so long as best practices are followed. However, in both cases, trust and objectivity are conflated. In the science and technology studies context, we have Jasanoff, who argues that scientists at the science-policy interface are boundary actors, and that they must be careful to keep science and policy clearly demarcated when operating at this boundary. That is, science advisory bodies must keep up the appearance of their scientific recommendations as objective and free from values, although they are not.[21] The way to do this, Jasanoff tells us, is to include stakeholders early, so that the locations of these boundaries may be established, which thereby helps to ensure that the research is done within the established bounds. Finally, with Pielke, we see that issue advocacy appears to threaten the utility of science in the policy and that scientists should instead strive to be honest brokers. However, Pielke's analysis, like the others, fails to fully consider the implications of the presence of values in science, and that

[21] I further develop this analysis in chapter three.

values are not a threat to objectivity, and this raises questions about the applicability of his solution, as it means that even Honest Brokers may inadvertently act as issue advocates.

The majority of science and technology studies literature on science advocacy/advising has several gaps. One such gap is a robust account of relationship between objectivity and values in science. By framing objectivity, particularly objectivity as detached or disinterested, in opposition to values, objectivity becomes a tool that can be weaponized against legitimate, value-laden science. Moreover, as we will see in chapter four, objectivity plays a political role as well. Chapter two will examine three distinct approaches to values in science that philosophers have developed. I will use these approaches to draw out lessons about the nature of objectivity and values that are especially relevant to policy advocacy and advising, and so, relevant to the views of the authors discussed above. In chapter three, I apply the concepts in chapter one to identify shortcomings for the views discussed here, in chapter one. In particular, I argue in chapter three that each of the views here, to varying degrees, sees values and objectivity as oppositional. The consequence of this is that these views inadvertently reinforce the idea the science and society are wholly separate. This, in turn, delegitimizes the important role that social and ethical values can play in science and science advising.

Chapter 2

The Nature of Values and Objectivity

The goal of producing results of research that are value-free is part of the notion of the ideal mind as a mirror that can reflect a world that is 'out there,' ready-made. In this view, value-free objectivity can locate an Archimedean perspective from which the events and processes of the natural world appear in their proper places.

-Sandra Harding 1991, 157-8

2.1 Introduction to Values and Objectivity

We saw in chapter one that objectivity plays an important role in ascribing credibility to scientific claims. In this chapter, I will examine the views of three prominent philosophers who have each articulated different ways of thinking about values and objectivity. Each argues that values and objectivity should not be regarded as wholly oppositional and here I explain the arguments they make for this conclusion. From this, following Helen Longino I argue that objectivity is socially constructed and that values play an important role in the production of objective knowledge, as this premise helps create the conceptual space to argue for a pluralist conception of objectivity in chapter four. A concern one might immediately have here, is that adopting a social constructivist view seems to strike a fatal blow to the utility of science as a tool for policymaking in a similar way that the problem of excess objectivity seems to pose a challenge to science advising. That is, one might worry that adopting this view expands the number of policy choices so greatly that we cannot possibly determine the best course of action. However, in this chapter I will show that this is not the case.

To begin, I will briefly describe the value-free ideal (VFI). This is important, as the three views I discuss all provide alternatives to the VFI. The three views I discuss are those of Helen Longino, Heather Douglas, and Janet Kourany. Each theorist provides an interesting and useful way of thinking about the relationship between values and objectivity. Each of these views accepts that objectivity is socially constructed. What I mean here is that each view sees "objectivity" as a social kind; the kind of conceptual resource that we can usefully revise for certain theoretical and political purposes, much like Sally Haslanger argues is the case for "race" and "gender" (2000, 34). In chapter four I get to the core idea of my dissertation, and use the work of Longino, Douglas, and Kourany (among others), to develop a framework for applying different notions of objectivity and the role of values to the issue of science advice.

2.2 The Value-Free Ideal

The value-free ideal is the view that science is, and ought to be, free of social, ethical, and political values. According to this ideal, there may be ethical values that guide the responsible conduct of research, but these values should not affect the evaluation and acceptance of the conclusions that scientists arrive at through their research (Douglas 2009, 45; Longino 1990, 85). To the extent that values do influence the evaluation and acceptance of the conclusions of scientific research, it should only be epistemic values and cognitive values playing this role. Epistemic values are values relating to the goal of science (true, or at least reliable, knowledge), and includes things like internal consistency, predictive competency, and empirical adequacy (Douglas 2009, 94). Cognitive values, on the other hand, are values that aid in theory choice, such as the simplicity of an explanation, the explanatory scope of a theory, or the consistency of a theory with other areas of research (Douglas 2009, 93). Furthermore, according to this ideal, scientists should not be expected consider the societal implications of their research (Douglas 2009, 54). Instead, such conclusions should be evaluated according to the internal, constitutive values of science as determined by the goal of science; namely, an accurate understanding of the natural world (Longino 1990, 85).

Philosophers have long recognized the role of epistemic values in science. For example, Thomas Kuhn’s 1977 book *The Essential Tension: Selected Studies in Scientific Tradition and Change* represents one early recognition of the role that epistemic values can play in science. In this book, Kuhn talks about how certain values, such as simplicity or having a broad explanatory scope, can aid in theory choice. Since then, philosophers have challenged the value-free ideal by elucidating more ways that values play a role in scientific inquiry, and even specifically including roles for social and ethical values. Yet the introduction of non-epistemic or cognitive values into scientific inquiry ostensibly eliminates the objectivity of scientific research, particularly when these values go beyond things like explanatory scope to include social and ethical considerations. Philosophers, in turn, have attempted to preserve objectivity by offering more nuanced accounts of how and why we should consider some claims to be objective, even when there are social and ethical values involved.

Finally, although there is arguably near consensus among philosophers of science that non-epistemic values are permissible in many core aspects of science (Lusk 2020, 102), there are a few prominent holdouts for the value-free ideal. Oftentimes, contemporary arguments for the value-free ideal take the form of, what Greg Lusk calls, “the political legitimacy argument” (2020, 103). This argument, in its general form, concludes that “[p]ermitting something other than truth to influence scientific reasoning would lead to an erosion of trust in scientific institutions” (Lusk 2020, 103). This kind of argument can be traced back at least to W.E.B. Du Bois, with Liam Kofi Bright (2018) offering a recent explication of Du Bois’s argument for value-free science. Other contemporary arguments in this vein come from Gregor Betz (2013) and Robert Hudson (2016). Betz, for example, writes that “[a]s political decisions are informed by scientific findings, the value-free ideal ensures–in a democratic society–that collective goals are determined by democratically legitimized institutions, and not only by a handful of experts” (Betz 2013, 207). The sentiment being expressed by Betz is similar to the one expressed by Jasanoff above.

One of Jasanoff’s aims is to develop an account of science advising that rests in the middle ground between technocratic decision making by unelected experts, and democratic decision making without due consideration of the scientific and technical inputs. In her view, social, ethical, and political values may play a role in the development of risk assessments or other scientific advice, but for this advice to be legitimate, it needs to appear free from any such values. In this way, the collective goals can be determined by democratically legitimate institutions, and these institutions can draw on the advice of experts to achieve those goals.

In chapter four I show how political legitimacy can be achieved in the context of value-laden science. I do this by offering a competing account of credibility and trust in the context of value-laden science. Consequently, I see my argument in this dissertation as an argument against the political legitimacy argument.

2.3 Helen Longino's Critical Contextual Empiricism

2.3.1 Longino's View of Values

Helen Longino offers a view she calls "critical contextual empiricism." The contextual aspect comes in because Longino makes a distinction between constitutive values and contextual values. Constitutive values are epistemic values that establish what constitutes acceptable scientific research. Contextual values, on the other hand, are personal, social, or cultural values. The contextual empiricist position, therefore, is aimed at understanding how contextual values shape our observations and interpretations of data.

Longino tells us that there are five ways in which contextual values can shape a research program. These are:

1. *Practices.* Contextual values can affect practices that bear on the epistemic integrity of science.
2. *Questions.* Contextual values can determine which questions are asked and which ignored about a given phenomenon.
3. *Data.* Contextual values can affect the description of data, that is, value-laden terms may be employed in the description of experimental or observational data and values may influence the selection of data or of kinds of phenomena to be investigated.
4. *Specific assumptions.* Contextual values can be expressed in or motivate the background assumptions facilitating inferences in specific areas of inquiry.
5. *Global assumptions.* Contextual values can be expressed in or motivate the acceptance of global, framework-like assumptions that determine the character of research in an entire field. (Longino 1990, 86)

Longino notes how some of these, such as practices or questions, can be problematically shaped by contextual values. We can see how contextual values bear on the epistemic integrity of scientific practices by looking at the Monsanto case. The contextual value of "profit" was *a* motivating factor (if not *the* motivating factor) for Monsanto paying prominent scientists to attach their name to Monsanto's own research. This violates some of the norms of scientific practice such as disclosure of conflict of interest and peer review.

More concerning, however, is Longino's example of how contextual values can motivate the adoption of global, framework-like assumptions. Longino tells us that "[a]ny science…must characterize its subject matter at the outset in ways that make certain kinds of explanation appropriate and others inappropriate" and that "[t]his characterization occurs in the very framing of questions" (1990, 98). Citing historians of science, Longino tells us that the mechanistic philosophy of the sixteenth and seventeenth centuries conceptualized matter in certain ways that made certain kinds of questions appropriate to ask and certain kinds of explanations appropriate. The ways in which the subject (i.e., "matter") was constructed, reflected the analogy that natural philosophers drew between natural phenomena and machines (Longino 1990, 92 – 98). This meant that the primary qualities of nature that were mathematically quantifiable (e.g., size, motion, quantity) were positioned as explanatorily important features of nature as they were mechanically significant properties with familiar causal powers (ibid.). As mechanistic explanations met with success (i.e., they were empirically adequate, made successful predictions, and importantly, allowed for successful manipulations of the natural world), their success was compounded by their ability to facilitate the needs of the craftspeople who constructed, among other things, buildings, infrastructure, clocks, and maps (Longino 1990, 96). These instrumental successes lead to the widespread and general adoption of mechanist philosophy and the development of modern physics.

However, Longino tells us that instrumentality is not an epistemological notion (1990, 93). It does not guarantee truth, and, when it is part of our desideratum for theory acceptance, it can lead to contextual values transforming into constitutive values (Longino 1990, 92 – 98). For example, by adopting a mechanistic philosophy towards matter, we stop asking teleological questions, as such questions are considered an unacceptable way of framing the observed phenomena and do not provide acceptable kinds of explanations in a mechanistic research program. Thus, contextual values are transformed into constitutive values, by constructing the object of inquiry in ways that limit the kinds of questions that are asked, the ways in which they are asked, and the kinds of things that count as a good explanation. In Kuhnian terms, we might say that contextual values shape the research paradigm. This transformation of contextual values to constitutive values will be important in chapter three.

2.3.2 Longino's Views on Objectivity

Turning now to her conception of objectivity, Longino argues that objectivity comes from interactions within the scientific community. While the scientific method itself may not wholly control for the influence of the values of the individual researcher, the submission of that researcher's work to the broader scientific community can help identify where certain value judgments or background assumptions are playing a role. However, we will not necessarily know what these background assumptions are until we enter discussions with other people. One way this is done through peer-review and publication in a scientific journal, so the research is subject to scrutiny by the community of scientific inquirers. If that community is appropriately constructed, then it is more likely that the values connecting the evidence to the hypothesis will be detected and critically evaluated. Longino notes that appropriately constructed communities are diverse communities that include historically marginalized groups, such as women and people of colour. In addition to the ethical significance of having historically marginalized groups as part of our epistemic communities, Longino notes that diverse communities have access to greater critical resources for detecting unidentified assumptions (2001, 132). Subsequent research (from other value-positions) can then help us pick the right values to include in our inquiry. Longino tells us that the requirements for this community are that there be recognized avenues for criticism, a shared set of standards, community response to criticism, and a relative equality of intellectual authority (1990, 79 – 81; 2001, 129 – 133). Claims are therefore objective to varying degrees, to the extent that they emerge from a community that resembles the one articulated by Longino.

Criticism plays an important role in Longino's view, with Longino identifying two basic types: evidential criticism and conceptual criticism (1990, 71). By evidential criticism, Longino means the degree to which a hypothesis is supported by the evidence through considerations of accuracy, extensiveness, and performance conditions for the experiments and observations offered as evidence, as well as the analysis and reporting of results (ibid.).

Conceptual criticism, on the other hand, refers to the more theoretical grounds for critiquing a view. She tells us that there are three kinds of conceptual criticism. The first kind refers to the conceptual soundness of a hypothesis (Longino 1990, 72). By this she is referring to the critique of some hypothesis on the grounds that there is a perceived conceptual flaw in it, such as Einstein's rejection of quantum theory on the basis of randomness (Longino 1990, 72). The second is the consistency critique, whereby some hypothesis is criticized based on being inconsistent with other widely held beliefs about the world, such as when the heliocentric view of the solar system was rejected because it seemed inconsistent with Aristotelian physics (Longino 1990, 72). Finally, there is the relevance critique, which is essential to Longino's view (1990, 73).

The relevance critique focuses on the relevance of evidence presented in support of a hypothesis (Longino 1990, 73). For example, Longino offers the case of Thelma Rowell, who questioned the relevance of observations made in the study of animal behaviour because the observations assumed a universal male dominance (1990, 72 – 73). Longino considers this to be a conceptual critique, rather than an evidential one because the object of debate is not the data, but rather the assumptions from which the data are interpreted or taken to be evidence (1990, 73). That is, the contextual values that have shaped the interpretation of this evidence. Furthermore, this critique is best understood as a critique of the fourth way that science and values interact, namely, at the level of specific background assumptions. This is important for Longino because this is the kind of critique where value-laden background beliefs and assumptions are subject to scrutiny by the community of inquirers and so, from whence objectivity comes (1990, 73). The debate and critique at this point reduces the influence of subjective preferences, but only at the level of background beliefs (Longino 1990, 73). Longino goes on to note that while this may not eliminate subjective preference at either the individual or community level, it does limit the influence these preferences have in the formation of scientific knowledge (1990, 73). The consequence here is that when these criticisms are raised, one may modify their background assumptions in response (Longino 1990, 73). In this regard, the criticism is transformative. For Longino, the important point is that these background beliefs and values can be articulated and subject to criticism, and that once public, they can be defended, modified, or abandoned in response to this criticism. It is through this process that an idea becomes labeled as scientific knowledge (Longino 1990, 73). Essentially, it is through this critique among diverse perspectives that subjective values and preferences can be given legitimacy. The knowledge generated by the community is no longer reflective of any individual's preferences, but is instead, the considered product of critical discourse by the scientific community (Longino 1990, 73).

Finally, the homogeneity of scientists as a class offers a limiting condition of this kind of objectivity, as noted by Longino herself (1990, 80). If the members of the scientific community are not sufficiently diverse, this may lead to homogeneity in the community's background assumptions. So, for example, Rowell's critique of universal male dominance as a background belief may not receive uptake in a male-dominated scientific community that already holds the background belief that males are dominant. As such, to ensure that the relevance critique receives uptake, we need to have communities of inquirers that include a diversity of perspectives, irrespective of the social or economic power of an individual or community, so that the hypothesis is subject to the broadest range of criticism (Longino 2002, 131 – 132). As noted above, Longino thinks we ought to include groups who have been historically actively excluded from the scientific community, such as women and people of colour (2002, 132).

To sum up, there are a few important points to keep in mind from Longino's view going forward. These are that objectivity comes from community interaction, diversity is important, and that contextual values can shape how we conceptualize our object of inquiry. Each of these pieces will be important in the following chapters. I now turn to consider the view of values and objectivity offered by Heather Douglas.

2.4 Heather Douglas – Direct and Indirect Values and a Pluralist Approach to Objectivity

2.4.1 Douglas's Views on Values

In her book, *Science, Policy, and the Value-Free Ideal,* Douglas outlines two general ways in which values play a role in scientific inquiry: direct roles, where values directly shape our inquiry and indirect roles, where values play a background role in inquiry. When values play a direct role, their acceptability in scientific inquiry is limited (Douglas 2009, 96). One way in which values play an acceptably direct role in science is when deciding which topic to research. For example, a scientist's desire to study the intelligence of owls may stem from the value that this scientist places on owls. They may find them interesting animals, have been inspired by the Roman myth of Minerva (goddess of wisdom, often associated with owls), or have just always loved them, ever since they were a child. This, in turn, helped our scientist decide what to study while in school, what field they chose when they graduated, and what research to pursue in their field. This is a relatively uncontroversial role for values in science, dating back to at least Michael Polanyi (1962).[22]

The other legitimate direct role for values is in choosing a research methodology (a point echoed in Lacey 2013). As both Douglas and Lacey point out, certain methodologies are better suited to different sorts of questions (Douglas 2009, 99; Lacey 2018, 4). For example, ethnographic studies are better suited to answering qualitative questions about what different cultures believe, whereas statistical surveys are better for understanding how widespread a belief is. Furthermore, certain methodologies may face ethical challenges if human or animal subjects are involved.

Values, particularly social and ethical values, play an illegitimate direct role when it comes to evidence. Evidence should be regarded as primary and should not be replaced by the values of the researcher. We do not want scientists to desire a result so badly that they make up data, nor do we want them not wanting a result so badly that they ignore data (Douglas 2009, 102). This seems to be the worry of Lackey and the others above.[23]

However, Douglas argues that social and ethical values do and should play an indirect role throughout scientific inquiry and in particular, in situations where we need to make a

[22] Interestingly, it may also imply a policy view for the scientist if the species they study is threatened.
[23] They seem to be worried that the presence of values will undermine the norm of disinterestedness and that science will lose its epistemic authority and become a tool for politics, rather than policy, in the sense described by Pielke above.

decision, but our evidence and reasons for making the decision are incomplete (2009, 96). For example, in contexts where there is high uncertainty because there has not been enough time to collect all the evidence, but a decision needs to be made (as is often the case in the policy context) social and ethical values can play an indirect role by helping us consider the consequences of making an erroneous choice, or by estimating the likelihood that an erroneous choice would remain undetected (Douglas 2009, 96). Importantly, in such circumstances values do not replace evidence. Instead, they help us bridge the gap between the evidence we have available to us and our need for action. Lastly, Douglas argues that as our evidence increases, the need for values in their indirect role decreases.

Like Longino, Douglas recognizes that values may influence the interpretation of data. For example, when interpreting data, the scientist will use cognitive values to help them decide how to proceed. Recall from the start of this chapter that cognitive values are ones meant to play a solely epistemological role in scientific reasoning.[24] They provide guidance on things like theory choice (Kuhn 1977). Perhaps the most famous cognitive value is simplicity. Kuhn famously offers the example of the mathematical simplicity of a Copernican system of astronomy in comparison to the Ptolemaic system (1977, 324). However, an example more relevant to policy may be that of a scientist who decides not to include an event if it is unclear whether that event occurred, because of this value of simplicity, believing that the inclusion of such data may complicate the analysis. Other cognitive values

[24] Although as I will argue in the next two chapters, we should be skeptical of reasoning claiming to only be concerned with epistemological issues, at least in certain circumstances.

identified by Douglas (and others, such as Thomas Kuhn) include explanatory power, scope, and predictive precision (2009, 93).

In other cases, such as when we are evaluating the toxicity or carcinogenicity of a substance, ethical values or social values may come into play. Scientists must decide on how much evidence is enough to either 'prove' or 'disprove' a claim. In the context of policy, this standard is often lower than it would be in the academic context, since stakes are typically higher, and timelines are typically shorter. Since, in the policy context, human health and well-being are often at stake, decisions must be made in a context of greater uncertainty and Douglas correctly believes that either ethical or social values (i.e., contextual values) should play a role in such decisions, especially when there is high uncertainty and a high cost associated with getting the facts wrong, because such values can help us determine when there is enough evidence to make a decision.

One reason that science advocacy and science advising seem worrisome is due to the concern that contextual values, such as social and ethical values, will either distort the otherwise objective findings of science, or, that they will outright replace scientific evidence. One frequently cited example of this is that of Trofim Lysenko in the Soviet Union. Lysenko criticized mainstream genetics research as "bourgeois" and incompatible with Marxism (Wolfe 2018, 21-22). The Communist Party came to see Lysenko's views as superior to Western capitalist science and in 1948, this culminated in the Communist Party officially endorsing the views of Lysenko, with "Lysenkoism" becoming the party line with respect to genetic research (Wolfe 2018, 24). Soviet scientists pursuing the mainstream genetics program had to do so under different names for the research or risk arrest and imprisonment (ibid.). However, as Douglas points out in her analysis of the roles for values in science, situations where this occurs seem to be a result of values displacing evidence, rather than values playing their appropriate roles. This limited role for values, however, may not satisfy those worried about the integrity of science in the face of advocacy. They may be concerned that any social or ethical values in the latter stages of inquiry are problematic and biasing. While Douglas argues that social and ethical values can and should play a role in the early stages of inquiry, I will extend this argument to show that these choices have important consequences for science advising, by showing, in chapter three, how certain social or ethical value choices at the early stages of research can impact how a science advisor curates the research they believe is relevant for consideration of the problem they are advising on.

Social values appear to be the most controversial kind of contextual value. Social values appear to threaten the objectivity of science, and so, the legitimacy of its use in the policy process. If science is value-laden, then it may not appear suitably detached. It may be seen as partisan or as a tool for special interests. As we saw in the previous chapter, supposed impartiality seems vitally important to preserving the epistemic authority of science. The presence of values seems to render the boundary between science and policy less clear, and thus the integrity of science in the policy process is diminished because it seems as though political or institutional biases are unduly shaping science. It is typically believed by those who subscribe to the value-free notion of objectivity that "[o]nly false beliefs have social causes – human values and interests that blind us to the real regularities and underlying causal tendencies in the world, generating biased results of research" (Harding 1991, 158).[25] And yet, as we saw above with Helen Longino, social values can also be epistemically productive as they can be subject to criticism and lead to more rigorous inquiry. This fact challenges the presumptions of the views in chapter one, namely that objectivity and values are in opposition and that the presence of social and ethical values in science should render it uncredible. Longino shows how (at least one kind of) objectivity is socially constructed through interactions within epistemic communities and the important role that social and ethical values play in this construction. In the next section, Douglas's work supports the thesis that objectivity is socially constructed, as she argues that any definition of objectivity must be comprehensive enough to include at least the seven different kinds of objectivity she identifies. The social construction of objectivity, the numerous ways it can be constructed, and the importance of social and ethical values in constructing it are the ingredients I will use in chapter four to argue for a social-pluralistic view of science advice.

2.4.2 Douglas's Views on Objectivity

Douglas identifies eight different kinds of objectivity, including an important, three-way distinction between detached objectivity, value-neutral objectivity, and value-free objectivity. Douglas argues that detached objectivity and value-neutral objectivity are often conflated with value-free objectivity (2009, 121). This conflation is one that seems largely taken for granted in the science policy literature discussed in the previous chapter.

[25] This is Harding's description of an assumption of the value-free ideal, not a representation of Harding's views.

The first kind of objectivity discussed by Douglas is manipulable objectivity. We have manipulable objectivity when our conceptual models allow us to successfully (i.e., reliably, and predictably) intervene in the world. For example, DNA is a conceptual tool that explains what traits an organism has (Douglas 2009, 118). But this tool also allows us to manipulate the genetic information of organisms to assign them particular traits, such as making crops more resistant to cold or heat. When our conceptual/theoretical tools allow us to actively intervene in the world in reliable and predictable ways, this suggests that we have found a genuine causal mechanism, even though it may not be one which we fully understand. However, as Longino notes above, “usefulness” is not an epistemological notion. So, while manipulable objectivity may be useful, it should not be considered a wholly authoritative marker of credibility on its own.

The second type of objectivity discussed by Douglas is convergent objectivity. We have convergent objectivity when different avenues of inquiry arrive at the same or similar conclusions (Douglas 2009, 119 – 120). For example, evolutionary theory is well supported by information gathered in paleontology, genetics, and geology. When we have convergence among disciplines and inquiry methods, we have good reason to think that we have discovered a genuine fact about the world. However, one problem with manipulable and convergent objectivity not discussed by Douglas, is that in both cases, a claim will be underdetermined by the evidence, leaving them open to dispute in the policy context. This has important implications. As we will see in the coming chapters, many science policy controversies arise because entrenched interests capitalize on the uncertainty inherent in underdetermination to create doubt about the credibility of the research being done.

Next Douglas discusses the objectivity of individual thought processes. It is in this section that Douglas makes her three-way distinction between detached objectivity, value-free objectivity, and value-neutral objectivity. The first kind, detached objectivity, refers to an "intellectual distance" between the inquirer and their area of inquiry (Douglas 2009, 121 – 122). By this, Douglas means not wanting a result so much that we either ignore or overemphasize evidence. We can think of it as a mental "stance" meant to combat confirmation bias. As Douglas writes, "[v]alues should not protect us from unpleasant or inconvenient evidence. Evidence needs to be able to challenge our current ideas and preconceptions and be able to overturn them" (2009, 122). This is part of the reason we value science and why it is generally regarded as trustworthy. That is, scientists, in their capacity as scientists, are expected to change their mind in response to new evidence. If scientists are not suitably detached, and want a particular outcome too much, they may discount evidence which weighs against it. If scientists are not suitably detached, and are opposed to a particular theory, they may discount evidence weighing in favour of a particular theory. In short, this ideal points to the importance of limiting the role of personal bias and to accurately represent the world. Yet, it can be difficult to determine whether our scientist has been suitably detached. This consideration will be discussed further in chapter four.

The next type of objectivity Douglas discusses is value-free objectivity. She says that this kind of objectivity is often conflated with detached objectivity. The presumption behind this conception is that social and ethical values are subjective, and thus contaminate the objectivity of science (Douglas 2009, 122). Douglas correctly rejects this kind of objectivity, arguing that "[h]iding the decisions that scientists make, and the important role values should play in those decisions, does not exclude values. It merely masks them, making them unexaminable by others" (2009, 123).[26] Instead, Douglas recommends that we be vigilant in ensuring that values are playing the appropriate roles (discussed above) and are not supplanting evidence (ibid.).

Next, Douglas considers value-neutral objectivity. This type of objectivity, Douglas writes, is also sometimes conflated with value-free objectivity, and refers to when scientists take a neutral position with respect to a spectrum of values (2009, 123). This type of objectivity is typical of writing a literature review, where the author summarizes different positions in a debate, but remains agnostic with respect to who is correct (Douglas 2009, 124). Douglas also correctly notes that in the case of sexist or racist values, value-neutrality is unacceptable, as there are good moral reasons for not accepting such values (ibid.). The issue with value-neutral objectivity is that value judgments are required to arrive at this agnostic position. I will explore this point in greater detail in chapters three and four, arguing that the value-neutral objectivity can be better understood as a kind of procedural objectivity and that the reflective value judgments used to arrive at a value neutral position can be used to curate the evidence considered by policymakers in ways that might exclude socially and ethically significant information.

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[26] In chapters three and four, I argue that this is true of both value-neutral objectivity and detached objectivity.

Next, Douglas explores how social processes can underline the objectivity of a claim. Douglas writes that there are three senses in which we might call something objective according to social processes. These are: procedural objectivity, concordant objectivity, and Critical Contextual Empiricist objectivity.

The first social process Douglas refers to is procedural objectivity. We have procedural objectivity "if the same outcome is always produced, regardless of who is performing the process" (Douglas 2009, 125). This kind of objectivity is useful because it eliminates personal judgment and idiosyncrasy, however, it does not guarantee the elimination of values (ibid.). The values might instead be coded into the process (Douglas 2009, 126). The inflexibility of the process might also mean that nuances, important to particular cases, will be left out (ibid.).

The second social process that Douglas examines is concordant objectivity. Like procedural objectivity, Douglas says that this aspect is also concerned with uniform outcomes across groups of people (2009, 126). We have concordant objectivity when "some set of competent observers all concur on the particular observation" (ibid.). While agreement among observers can be persuasive, it does not guarantee that the observers were correct in their observations (Douglas 2009, 127). It may be the case that the observers share a set of values that will lead them to discount certain evidence (ibid.). As such, groups must be suitably diverse to limit this possibility, as with Longino above.

The final sort of objectivity discussed by Douglas is Critical Contextual Empiricist objectivity. This is the kind of objectivity developed by Longino above, wherein objectivity is a property emerging from the interactions and structure of the community.

The key takeaway from Douglas's account of objectivity seems to be this: objectivity can be established in a myriad of ways and each way of thinking about objectivity is limited in certain respects. Douglas argues that any notion of objectivity that we adopt must be at least as complicated so as to include the seven kinds of objectivity she believes are tenable (i.e., excluding value-free objectivity; 2009, 116) and that articulating when and why we should ascribe objectivity to a claim can help us understand why we should consider a claim persuasive (2009, 132). In chapter four, I will argue that our conception of objectivity can be less complicated, as detached objectivity and value-neutral objectivity can be better understood through other kinds of objectivity discussed by Douglas. Thus, we can make do with five kinds of objectivity. These five kinds of objectivity form the basis of the "pluralist" side of the social-pluralistic view of science advising I develop in chapter four. These five kinds of objectivity can be used by policymakers to triangulate credible advice, and, by science advisors to explain to policymakers why their advice is credible.

2.5 Janet Kourany – A Two Pillared Approach to Credibility

Janet Kourany, a feminist philosopher of science, takes a more radical view of the role of values in science than either Douglas or Longino. Kourany argues that science ought to be more socially responsible and socially engaged. Kourany offers an ideal for socially responsible philosophy of science. According to this ideal, science is explicitly political and politicized (Kourany 2010, 106). While science is politicized under Kourany's view, she argues that this does not threaten the integrity or objectivity of science. Conscious of the specter of Lysenkoism, Kourany articulates what Matt Brown calls "the principle of joint necessity of evidence and values" (Brown 2013a, 68). According to Kourany, "science should be judged by two kinds of standards, moral/political standards as well as by epistemic standards" (Kourany 2010, 85).

The epistemic standards, as we have seen, are uncontroversial and essential to the functioning of science. According to Douglas, moral and political values are acceptable, so long as they play an indirect role. Kourany, on the other hand, goes on to say, "science should be found wanting if it fails sound epistemic requirements, but it should also be found wanting if it is shaped by unacceptable social values" (2010, 85) and that it is "only when both kinds of requirements are fulfilled should talk of scientific advance or scientific progress be considered appropriate" (ibid.). Finally, Kourany writes, "[t]he upshot is that the epistemic requirements of the idea of socially responsible science were not fulfilled by Lysenko's science even if the social requirements were, and hence, the failure of Lysenko's science forms no critique of the ideal of socially responsible science" (2010, 87). So, for Kourany, social/ethical values take on a much more direct role and become part of the standards by which we judge a theory.

While science is explicitly political in Kourany's account, evidence and epistemic rigor still play a central role. Social and ethical values cannot and should not replace evidence but should be considered equally important to epistemic standards in our considerations of what constitutes good science. If science meets the social or ethical requirement, but not the epistemic requirement, we should not say that we have made scientific progress. Likewise in the case that scientific research meets the epistemic requirements, but not the ethical requirements. Of all the views of values in science discussed here, Kourany's is the most radical, and seems to pose the greatest threat to the objectivity and authority of science in policy.

What Kourany seems to be offering is a rejection of value-neutral objectivity. By arguing for the joint necessity of evidence and values, Kourany echoes Douglas' point above that value-neutrality ought to be an untenable position in the face of things like racist or sexist values. I say ought to be, because, as Kourany points out this has not always been the case. For example, she shows how "value-neutral," "curiosity-driven" science has been done allegedly showing that women do not perform as well on intellectual assessments, is based on biological differences. This research is then used to justify a number of problematic social arrangements. Kourany shows how value-neutrality can encode racist or sexist values, hence the need for both epistemic and moral evaluations of science. Kourany, provides us with an articulation of the ways in which value-free/value-neutral/detached objectivity gets weaponized, a notion I pick up in chapters three and four. Drawing on Kourany's analysis as a starting point, and incorporating the work of Elizabeth Anderson, I develop a thought experiment to show how value-neutrality can be used by Honest Broker's in Pielke's framework to curate the exclusion of socially and ethically relevant evidence from consideration by policymakers.

2.6 Analysis – How do we Deal with Values and Objectivity?

Longino, Douglas, and Kourany offer us three distinct accounts of the role for values in science. Longino sees values as inherent to the functioning of science but notes the importance of transformative criticism to the scientific community so that problematic values can be recognized and corrected. Similarly, Douglas sees values as unproblematic, except in cases where they play an inappropriate direct role such that they replace evidence. Additionally, she shows that we can still maintain objective science in the face of such values, outlining six kinds of objectivity that can be appealed to, including among them the intersubjective objectivity identified by Longino. Kourany sees the absence of appropriate values as problematic and offers a much more prescriptive view, giving social and ethical values the most explicit role of any of the theorists in her account, requiring not only that scientific claims meet epistemic rigor, but also, meet the egalitarian demands of anti-racism and anti-sexism. It is only when both conditions are met that we can say that we have objective science.

In this section, I will discuss the ways that values and objectivity interact in the three views discussed above. For Longino, objectivity depends on values. Transformative criticism in an appropriately constructed community can help elucidate the background value assumptions so that they can be critically assessed with respect to their usefulness to inquiry. To the degree that the community is successful in identifying which values are useful to inquiry, the claims coming out of that community can be said to be objective to greater and lesser extents. The reason for this is that the values shaping the research and evidence are no longer idiosyncratic, but ones which have been adopted by the community.

Turning now to Douglas, we see that when one speaks of "objectivity" one can be referring to several things. These are: manipulable objectivity, convergent objectivity, detached objectivity, value-free objectivity, value-neutral objectivity, procedural objectivity, concordant objectivity, or Critical Contextual Empiricist objectivity. Further, Douglas tells us that at least some kinds of objectivity are susceptible to value-influences. For example, procedural objectivity may inadvertently encode individual, idiosyncratic values into the process. She also acknowledges that values may shape how one interprets evidence, for example, whether to include a particular event within a dataset. Finally, Douglas tells us that values can be helpful in the policy context, where we must go beyond the evidence to make decisions about what to do. Values are primarily only problematic for Douglas when they replace evidence.

Turning finally to Kourany, we see that social and ethical values can potentially play an even more direct role in scientific inquiry. Kourany argues that scientific claims should only be considered credible when they meet both evidentiary and normative standards. It is only when a claim meets both standards that it should be regarded as objective. In this way, Kourany sees her view as setting a higher bar for credibility than more typical views.

Another way in which these theorists differ is with respect to the role that values play. Kourany gives a very prominent role to social and ethical values, making theory acceptance dependent on them. Douglas, on the other hand, thinks that values only have a very narrow legitimate direct role to play. Above, I referred to Kourany's position as "the joint necessity of evidence and values." This comes from Matthew Brown's 2013 article "Values in Science beyond Underdetermination and Inductive Risk." In this article, Brown contrasts Douglas and Kourany's views. He refers to Douglas's view as "the lexical priority of evidence over values" position (Brown 2013b, 830). According to Brown, these views differ in an important way, namely, that in Douglas's view, when evidence and values conflict, evidence takes priority, while in Kourany's view, both epistemic (i.e., evidential) standards and moral standards must be met. If evidence and values conflict, we should, according to Brown's reading of Kourany, continue our inquiry until we have achieved a claim that meets both criteria (Brown 2013b, 837).

While Brown accepts the joint necessity principle, he differs from Kourany by thinking that value judgments are rationally revisable things. Drawing from John Dewey, Brown argues that if values and evidence clash, we can revise and refine both our evidence and our values, and ultimately our theories. Under Brown's conception (and Dewey's), both empirical inquiry and value formation are fallible, revisable processes (Brown 2013b, 837). The one qualifier that Brown adds, is that in revising our evidence, theories, and values, we must actually resolve the problem that prompted our inquiry (Brown 2013b, 837). Furthermore, if this revision occurs at the level of the community of inquirers, then it seems to me that we arrive at something very similar to the process described by Longino. Longino's criteria for community composition seem to fit nicely with the evidence, values, theory matrix described by Brown when considering this matrix at the community level.

Douglas's view, on the other hand, seems to regard values as static things. Although her view does not logically exclude revising our values, Douglas's solution focuses more on deliberative processes for collectively deciding what values should shape scientific research and/or risk assessment practices. This fits well with her focus on science in the policy context. Further, in certain contexts it may mean revising our values so that they better aid in inquiry, but the sense one gets is that Douglas's preferred scenario is that these democratic deliberative processes occur prior to inquiry. The reason for this is to reduce the number of science-policy controversies. If the parties involved all agree on the terms of reference for a study, as well as what methods should be used and how controversial evidence should be interpreted, then there should be fewer instances of controversy since all will have inputted and shaped the procedures for inquiry. In this respect, Douglas's solution is very similar to Jasanoff's in chapter one.

Whether we accept the principle of the lexical priority of evidence over values or the joint necessity of evidence and values has important consequences. Under the lexical priority principle, values are acceptable only insofar as they "close the gap" between evidence and theory or evidence and action. This principle is motivated by the problems of underdetermination and inductive risk. The lexical priority view seems to implicitly endorse the position that values are "dangerous" to inquiry, as when evidence and values conflict, evidence should win out. This way of thinking can therefore be seen as a more moderate instantiation of the value-free ideal. Said differently, we can understand the lexical priority view as being shaped or motivated by the same values as the value-free ideal. The joint necessity principle, on the other hand, seems to wholeheartedly embrace the importance of values to scientific inquiry and in doing so, deflates their dangerousness. Under the joint necessity principle, values are simply a standard tool of inquiry. Further, like evidence and processes, they are something we can be fallible about, and revise as necessary. For these reasons, I see the joint necessity view, as articulated by Brown, as a more useful tool for understanding the role of values in science, since it presents a more complete departure from the narrative in which values are the bogeyman of rigorous inquiry and better captures the close relationship that exists between science and society. It should not be the case that merely claiming that social and ethical values have shaped advice or research is enough to dismiss it from consideration. Instead, we can, as Brown, Longino, and Douglas argue, look at what those values are and consider whether those are appropriate. Finally, the joint-necessity principle is preferable in the science advisory and science policy process, since policy setting, and development, is a value-laden process.

Although each theorist in this chapter identifies a different relationship between values and objectivity, all three theorists agree that values shape objectivity. Furthermore, at least Longino and Kourany have shown how values can actually play a role in strengthening objectivity and Douglas shows us that objectivity is not a single thing. It can be appealed to in a variety of different ways, in a variety of contexts, and each one communicates the trustworthiness of the claim through different justifications. The upshot of this discussion of objectivity is that "objectivity" is socially constructed and that it can be, and has been, constructed in a variety of ways. Consequently, we need to be precise when saying that something is "objective." This is important because credibility and trustworthiness are closely connected to objectivity, especially in the scientific context. Yet, what all of these kinds of objectivity seem to have in common is a shared attempt at ensuring that knowledge is never relative to a single person's point of view. In chapter three, I will evaluate how the theorists from chapter one use various kinds of objectivity to establish the credibility and trustworthiness of scientific knowledge in the policy context.

On the other hand, the upshot of all this discussion about values is this: values reflect our priorities. They communicate the things that are important to us, both individually and as a society. And, because they play an important role in scientific inquiry, they also play an important role in how we understand and construct our reality. Since science policy controversies often come down to disagreements about what the world actually looks like, discussions of values are just as important as discussions of evidence when we are constructing policies, and this includes discussions of how values have shaped the evidence. While views like Jasanoff's and Pielke's note the importance of values in discussing policies, often they do not have a discussion of how values shape the production of evidence. This is not to say that science and technology studies scholars are unaware of how values shape the production, or at least interpretation, of evidence in a policy context. This point is well-trodden in the field. Much of the research coming out of science and technology studies looks at scientific controversies and the role that values play in these controversies, especially in areas like responding to things like biotechnology and environmental harms.[27] What I think is

[27] For a few examples, see Jasanoff 2007, *Designs on Nature: Science and Democracy in Europe and the United States*, Kinchy 2012, *Seeds, Science, and Struggle*, Parthasarathy 2011, "Whose Knowledge, What Values? The Comparative Politics of Patenting Lifeforms in the United States and Europe," and Bronson 2018, "Excluding 'Anti-biotech' Activists from Canadian Agri-Food Policy Making: Ethical Implications of the Deficit Model of Science Communication." My project here

missing, however, is a tool for evaluating the credibility of advice in the context of value-laden science. This is what I provide in chapter four. The theorists here in chapter two show that we can and should more fully embrace the value-laden nature of scientific inquiry and the dynamism between science and society when developing models for, and providing, science advice.

2.7 Conclusion

In this chapter I examined three ways of constructing the relationship between values and objectivity. An emerging theme out of chapter one was that science is useful and desirable as a policy tool because it purports to eliminate personal biases through its objectivity. In theory, this goal of science dovetails nicely with the goals of a democracy, where the whims of a single person should not dictate decision-making. People in a democracy have a right to collective self-determination. Science then, in adopting a "view from nowhere," positions itself as an independent resource for collective decision-making, and importantly, one that is able to "speak truth to power." If facts are independent of human interests, then everyone ought to agree on them, and so if a government adopts an undemocratic approach to governing, science provides a powerful tool for public protest. This is because disagreement with science carries with it a sense of bad faith. However, what this chapter has shown is that both scientific knowledge, and its claimed objectivity, are socially constructed and shaped by values in a variety of ways. As such, neither is independent of human interests.

Where does this leave us? On the one hand, eliminating personal bias is important to the acceptability of scientific knowledge. On the other hand, different kinds of objectivity can obscure the role that personal biases play and sometimes these personal biases reflect systematic disparities in institutional power. In chapter three, I will use the theorists discussed in this chapter to show how each view from chapter one implicitly accepts (to varying degrees) a view that sees values and objectivity as oppositional, rather than as complementary.

Chapter 3
The Credibility Problem and the Problem of Value Neutrality

> *[W]e are led to remark that the consequences are of two kinds, those which affect the persons directly engaged in a transaction, and those which affect others beyond those immediately concerned. In this distinction we find the germ of the distinction between the private and the public. When indirect consequences are recognized and there is effort to regulate them, something having the traits of a state comes into existence.*
>
> -John Dewey, *The Public and its Problems*

3.1 Introduction

In the previous chapter I showed how objectivity is socially constructed, while discussing three influential views on the relationship between values and objectivity. One theme common to all the scholars in the preceding chapter is that objectivity is important because it eliminates idiosyncratic personal values and biases from scientific knowledge. From this perspective, to call something objective is to say that it is trustworthy because individual biases are not present and because it does not solely represent an individual's point of view. Objectivity is an important tool because we typically think of scientific authority as being derived from objectivity. However, objectivity can be understood in various ways. One conclusion I will draw from this chapter is that certain kinds of objectivity are not helpful because they can obscure the role that social and ethical values are playing.

I reach this conclusion by critiquing the science policy views from chapter one, using the different kinds of objectivity discussed in chapter two. These views are the *Pure Science view*, the *Boundary Actor view*, and the *Honest Broker view*. Finally, I use my analysis of how certain kinds of objectivity can obscure the role of social and ethical values, to show how recognizing the role of values in science and the ways that objectivity and values interact allows us to identify two pitfalls that a responsible science advisor or science advocate should avoid.

3.2 Strengths and Weaknesses of the Pure Science View

In the Pure Science view, there is general agreement among scientists involved in policy, that scientists ought to be involved in the policymaking process but disagreement about the extent to which this ought to be the case. On the one hand, we have those who argue that although scientists should be involved with policy development, they should stick to providing facts and should avoid any appearance of not being totally impartial. Call this the Anti-Advocacy Pure Science view (AAPS). On the other hand, there are those who believe that scientists can and should engage in advocacy but should do so according to best practices so that scientists retain the trust of policymakers and the public by remaining being seen as impartial. Call this the Pro-Advocacy Pure Science view (PAPS). So, while there is disagreement within the Pure Science view as to the extent to which scientists should be involved in the policymaking process, there is agreement that impartiality is essential to scientists' involvement. I take the work of Robert Lackey as representative of the AAPS and work by Meyer, Frumhoff, Hamburg, and de la Rosa as representative of the PAPS. I will discuss each of these views in turn.

3.2.1 Anti-Advocacy Pure Science View

Lackey's concern with ensuring that scientists appear impartial extends to the use of value-laden language when describing the results of research. His worry is that value-laden language could imply a preferred policy view on the part of the scientist, or pre-emptively sway the value judgments of policymakers one way or another. For example, under Lackey's AAPS view, if a scientist were to describe the effects of smoking tobacco, such effects should merely be enumerated, rather than categorized as either positive effects or negative effects. Lackey believes that adhering to rigid standards like these is the only way of ensuring that scientists remain seen as impartial parties above the political fray.

As we saw in the previous chapter, values operate at all stages of scientific inquiry and may do so in unproblematic ways. Therefore, Lackey's argument for a blanket prohibition on value-laden language is too broad. Both government and academic scientists do research in the public interest, such as toxicity studies, health science, or environmental science. Such fields are useful precisely because we want to know whether something has *negative* health effects or *degrades* soil quality. Yet under Lackey's conception of science-based advocacy such descriptions would be unacceptable.

Philosophers have long argued that value-laden words have a role in scientific inquiry.[28] Elizabeth Anderson (2004) has argued in her paper "Uses of Value Judgments in Science: A General Argument, with Lessons from a Case Study of Feminist Research on Divorce," the

[28] Brendan Larson, in his 2006 paper "An Alien Approach to Invasive Species: Objectivity and Society in Invasion Biology," is also critical of value-neutral descriptions of natural phenomena, at least in the field of conservation biology, arguing that a firm line between "facts" and "values" is untenable, and that 'neutral language' may ignore implicit values (950).

use of thick evaluative concepts in science does not threaten the epistemic integrity of science. Thick evaluative concepts are hybrid descriptive-evaluative terms. They are descriptive in the sense that they track something in the world. They are evaluative because they carry with them some normative content. Words that Lackey would have us refrain from using, such as “negative,” or “degrades,” are excellent examples of these kinds of concepts.

Anderson discusses two research programs on divorce and shows how contextual feminist values reconceptualized “divorce” as something that can have both positive and negative impacts, rather than something entirely negative that results in conditions like “stress” and “trauma.” How the object of inquiry is conceptualized, leads researchers to look for certain kinds of evidence and to adopt certain methodologies to collect this evidence. For example, by conceptualizing divorce as entirely negative, researchers were inclined to look for evidence of stress or trauma. Researchers looked at whether people felt stressed resulting from divorce, and because “stress” is understood as something bad, they found evidence for the negative impacts of divorce. Note however that the normative, value-laden character of “divorce,” “stress,” and “trauma” did not prevent researchers from doing sound science. In fact, these concepts are essential to research. This is because the object of inquiry is directly related to human interests and the value of the knowledge being produced has a clear impact on human wellbeing. In fields like environmental science and divorce research where the role of values is clear and obvious, the questions and answers will also always be political. However, under Lackey’s conception of science advocacy, much of the research coming out of a field like environmental science can be framed as value-laden and thus, not trustworthy (see McGarity and Wagner 2008 and Oreskes and Conway 2010 for examples).

Lackey is concerned with ensuring that the public continues to regard scientists as impartial and trustworthy sources of information. However, he conflates detached objectivity with the value-free ideal. Lackey's prohibition against any value-laden terms is motivated by the concern of "normative science." Science is normative, according to Lackey, when it is "developed, presented, or interpreted based on a tacit, usually unstated, preference for a particular policy or class of policy choices" (Lackey 2007, 13). Lackey warns that such science is often "not perceptibly normative to policy makers or even to many scientists" (ibid.) and using terms similar to Pielke, describes this as stealth policy advocacy (ibid.).

For Lackey, the issue here is that normative science appears to transgress the norms of both detached and value neutral objectivity. Lackey seems to be suggesting that values are replacing or unduly shaping the production of evidence by shaping scientific procedures to justify a particular policy, or class of policy, options. His solution is to adopt a kind of value-neutrality instead. He does this by emphasizing the need to describe the world in the most neutral way possible. Thus, according to Lackey's view, since the natural world is seen as independent of human interests, sentences claiming to describe it can be thought of as impartial or detached from human bias.

However, as we saw in chapter two, Douglas and Kourany both argued that value-neutrality is not always a tenable position. Further, science that is value-laden is not necessarily bad science. In cases where thick evaluative concepts are appropriate, such as Anderson's case study, it would, to Kourany's point, be irresponsible to adopt the kind of neutrality that Lackey is arguing for. We would not get the information that we desire. Thus, it is not necessarily a problem that values play a role in the production of scientific evidence.

Further, as we can see through Jasanoff's co-production thesis (i.e., the proposition that science and society are shaped by each other, rather than mutually exclusive areas of activity), and the work of Anderson, science will never be entirely independent of human interests. The problem is when values replace evidence (Douglas 2009, 45). Yet with the solution that Lackey proposes, scientists or science advisors who do use thick evaluative concepts can be said to lack credibility for letting *any* values influence their science. Lackey's view then, is too narrow, since it would exclude a lot of research that is relevant and useful to human interests.

3.2.2 Pro-Advocacy Pure Science View

Contra Lackey, Meyer et al. believe that the role for scientists in policy extends beyond mere accurate descriptions of the world. They believe that scientists can, and should, participate in policy discussions. That is, scientists should be actively working to shape policy. To that end, Meyer et al. provide a list of best practices to guide scientists in these activities. These best practices, recall, include:

1. bringing scientific information to the attention of policy-makers and the public,
2. ensuring that policy design is consistent with the best available evidence,
3. ensuring that policy-makers consider the relevant science,
4. speaking only to what you know, and
5. acknowledging when you move beyond your area of expertise (Meyer, Frumhoff, Hamburg, & de la Rosa 2010, 299 – 301).

These best practices seem to reflect the presupposition that evidence is value free and that scientists are, and should be, wholly detached observers. While Meyer, Frumhoff, Hamburg, and de la Rosa acknowledge that scientists should be transparent about their values (e.g., about the importance of science in policy making, the benefits of conserving biodiversity, etc.), these acknowledgements seem to reflect scientists' values as they relate to policy, not necessarily how scientists' values have shaped the science that they have done.

Furthermore, the best practices that Meyer et al. argue for are best practices for bringing science to policymakers, not best practices for doing value-laden, policy-relevant science. If this understanding of their position is accurate, then "science" as conceived by Meyer et al. seems to be a value-neutral tool for policymakers to draw on. However, doing science can also challenge policies and established social structures merely by asking certain questions and undertaking certain research. Such research can be met with opposition and criticized as value-laden, no matter how rigorous it is, because it threatens entrenched, often economic, interests. Consequently, research criticized as value-laden may be viewed as partisan, and so, unreliable, rather than merely inconvenient. Thus, while the imperative to bring science to the attention of policymakers is an important one, its aim to do so in a value-neutral way, from the perspective of a detached observer, is impossible if values and objectivity are seen as oppositional.

Furthermore, how does one determine the "best" evidence? "Best" is not a value-neutral term. Reasonable people may disagree about what constitutes the best available evidence. While there are usually shared standards about what constitutes *good* evidence, determining which evidence is *best* requires judgment from the science advisor or advocate.[29] For example, if the available *good* scientific research from toxicology shows that some substance is likely to be harmful to humans, based on animal studies showing a strong correlation between exposure and illness, but the *good* scientific research from epidemiology shows only a weak correlation between exposure and illness, how does one determine which is the *best* available evidence? Since determining the *best evidence* requires judgment, contextual values are likely to influence this assessment. Meyer et al might reply, "This is why it is important to acknowledge the limits of your knowledge as well as when you're moving beyond these limits." But there are two problems with this response.

First, what policymakers consider as relevant, or "the best," evidence may differ from scientists (Cairney 2016, 23). This is because when policymakers consider policy solutions, they are at least equally concerned with what is feasible from a political perspective, as compared to just a technical perspective (ibid.). Consequently, while scientists may have one idea about what constitutes the "best available evidence," policymakers may not share this understanding. Thus, while the imperative for scientists to provide policymakers with the best available evidence is well-intentioned, it does not fully recognize the implications of value-laden research nor the role of values in the policy process itself.

Secondly, the uncertainty that follows from acknowledging when scientists and advisors are moving beyond the limits of their knowledge can be exploited by bad actors who feel

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[29] A further consideration: why would an advocate provide anything less than what they assess to be the best available evidence for the position they are advocating for? This doesn't mean that they share the standards of *good evidence*, only that they have an interest in presenting the strongest case they can for their view.

threatened by legitimate, rigorous research (Elliott 2017, 103). Naomi Oreskes and Erik Conway in their 2010 book, *Merchants of Doubt*, document multiple cases in the decades following World War II of special interests capitalizing on the uncertainty inherent in the scientific process to create doubt about things like the effects of tobacco on smokers and those who inhale it second-hand, acid rain, and global climate change. In these cases, people protecting their economic and political interests not only attempted to undermine the credibility of science showing harmful or otherwise negative effects, but also tried to promote science which favoured their views. For example, Oreskes and Conway show how the R.J. Reynolds Tobacco Company ran a funding program for research that would support the claims of tobacco companies that there was no causal link between tobacco smoking and the harmful health effects that other research was beginning to show (Oreskes and Conway 2010, 14 – 21).

So, what can we take away from this discussion? Both the AAPS and the PAPS recognize that there is political value in science appearing politically neutral. However, neither the approach of Lackey nor Meyer et. al, fully appreciates the role political considerations play in either the production of scientific knowledge, or the use of scientific research in public policy. Value judgments in the context of discovery have important consequences for both the context of justification, and the adoption of science by policymakers. But, as we saw above, political values interact with science even at the level of "what questions get asked." Thus, responsible advocacy and advising requires us to interrogate the ways in which scientific knowledge is produced, and the ways in which the asking or framing of certain questions serves different interests. This means that we can add detail to the practices that

Meyer et al. outline. Instead of merely bringing evidence to the attention of policymakers, scientists should, as Douglas (2009), Tuana (2010), Shrader-Frechette (1993) and Elliott (2020) all argue, be able to explain the values that go into their research as well as how different groups would be differently impacted by the proposed policy.[30]

A final consequence that we can take away from this discussion is that those who provide science advice are faced with what we might call *the Credibility Problem*.[31] One important point coming out of the discussion in the previous chapter is that values and objectivity are not necessarily in opposition. However, the Credibility Problem occurs when we think of them in opposition, with "objectivity" as a straightforward indicator of trust, and "value-laden" as a straightforward reason for distrust.

The Credibility Problem can occur in two ways: either by inappropriately inflating or deflating the credibility of scientific evidence or conclusions. Deflating credibility can happen when one party attempts to discredit another by claiming that values have influenced the scientific conclusions and consequently, that the science is unreliable, and an unsuitable basis for policy. In these cases, one uses a credibility indicator (value-ladeness) to persuade policymakers and the public of other researchers' incompetency or insincerity. On the other hand, inflating credibility occurs when one party uses another credibility indicator (objectivity) to legitimize science that should be scrutinized more carefully.

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[30] I discuss the work of Tuana and Shrader-Frechette further in the next chapter.

[31] In his book, *Tapestry of Values*, Kevin C. Elliottt describes the credibility problem without naming it. He writes, "And even if the evidence for one side of a controversy is misleading or of dubious quality, as in the case of the Marshall Institute's climate change report, an interest group can still disseminate their interpretations of the evidence widely as long as they have enough financial or political resources at their disposal" (Elliottt 2017, 103).

One way that credibility inflation can occur is through ghostwriting, to create the impression of convergent objectivity, but another way is by providing alternative research, that is presented as impartial, after deflating the credibility of competing research because it is value-laden. Dubious science is often science that supports the economic interests of the promoter, and may be conducted by the industry itself, or by people with close ties to that industry. In areas of controversy, science advisors are likely to face both kinds of challenges. Similarly, an advocate will be required to avoid these challenges when advocating for their goal. Let us now turn and consider Sheila Jasanoff's Boundary Actor view.

3.3 Strengths and Weaknesses of Jasanoff's Boundary Actor View

While Jasanoff acknowledges the presence of values in science, she does not offer a solution that fully realizes the implication this has for policy. She writes, "If science in the policy setting is always colored by values, then what role should scientists, who are professionally committed to impartiality, expect to play in decision-making?" (Jasanoff 1990, 7). This is the crux of the problem.

As we saw in chapter one, Jasanoff's solution is to ensure that the boundary between science and policymaking is maintained. One way in which her solution is successful is through her recognition that the choice of what to study and how to study it is a political act. Jasanoff recognizes this and argues that her case studies suggest that while the boundaries must remain intact, the location of those boundaries (i.e., what questions to pursue, how these questions are framed, and the methodologies being used) can be negotiated. Said differently, Jasanoff argues that different values can legitimately play a role in the early stages of inquiry, and that for science in policy to be recognized as legitimate science, actors with different interests need to have a role early on in shaping what values are reflected in the choice of research questions and methodologies. However, once these are negotiated, the science being done must be perceived as impartial (i.e., value-neutral) to maintain its scientific integrity.

There are several opportunities for improvement with Jasanoff's Boundary Actor view. As we saw with chapter two, values play a legitimate role in all stages of scientific inquiry, not just in the setting and framing of research questions. Furthermore, while Jasanoff does acknowledge that the choice of research question and how these questions are framed may be value-laden, she does not fully address the fact that the interpretation of data, and the choice of which evidence is relevant and important to policymakers, is also value-laden. Nor does she acknowledge the impacts these decisions have on the availability of evidence for policymakers. By keeping the boundary free from political influence, the research being done maintains the appearance of being politically neutral.

Jasanoff's boundary work view is limited in that it obscures the role that values are playing, rather than fully dealing with the implications that values have for science in policy. By ensuring that the boundary appears impermeable, science appears to be value-free, when as we have seen in chapter two, it is not. Thus, contra to the views of Longino and Douglas in chapter two, there is no clear avenue for critique or public debate about which values ought to guide the considerations of advisory panels, and any accusation of bias will appear legitimate, as values continue to appear contrary to the mission of science. For both Longino (1990, 2002) and Douglas (2009), public debate about which values are appropriate, is crucial to the acceptability of the research, advice, or policy. Jasanoff's work in this book shows how the advisory panels created by these agencies to advise on different questions were often criticized for having a proregulatory bias, often, because stakeholders felt that they were not sufficiently consulted in the production of the advice.

However, the bias of committees is not always seen as proregulatory. Oreskes and Conway also show that perceived political bias towards anti-regulatory policies is a frequent source of controversy in science advising. One example offered by Oreskes and Conway, is the acid rain panel put together by the Reagan-occupied White House. This panel's mandate was to review research from joint US-Canada working groups struck to explore the possibility of stronger controls on transnational pollution linked to acid rain. Reagan was elected on a platform that promised to reduce regulation and his administration saw the possibility of stronger pollution controls as a threat to its campaign promises. Thus, the Reagan administration's goal, seemingly, in establishing yet another review panel was to build a scientific case that such regulation was not necessary. To that end, the Reagan administration appointed two physicists to key roles on this new panel: Bill Nierenberg as chair, and Fred Singer. Both physicists had reputations as anti-environmentalists (Oreskes and Conway 2010). Furthermore, although Nierenberg attempted to put together an ostensibly impartial committee (his criteria seemed to be trusted and respected scientists of renown with the relevant expertise), the White House rejected many of the names he put forward for inclusion on the committee (Oreskes and Conway 2010, 86). Despite the presence of more qualified suggestions from Nierenberg, the White House pushed for Singer as the final member.

Despite the placement of these sympathetic scientists in key positions, the penultimate version of the report did not reflect the interests of the White House. After reviewing the evidence, Singer remained the only committee member skeptical of the relationship between sulfur dioxide and acid rain (Oreskes and Conway 2010, 91). While there was some disagreement among committee members on finer points, it made strong recommendations about how to proceed to curb the impact of acid rain.

Singer, who was responsible for writing the final chapter of the report, took a drastically different tone, softening the message, and expressing skepticism toward both the causes of acid rain and the need for regulation of sulfur dioxide levels (Oreskes and Conway 2010, 91). Singer's chapter ended up being included as an appendix, as the committee members would not sign off on a chapter so contrary to the rest of the report (ibid.). In this regard, the White House was partly successful, as it created a fracture in the consensus of the report, and so, created uncertainty about the link between sulfur dioxide and acid rain, which allowed the Regan administration to continue to hold a "business as usual" posture. The White House further justified this position by having the Office of Science and Technology (part of the Executive branch) edit the report with suggestions from Singer and without consulting the panelists. Nierenberg then unilaterally approved the changes to "soften the tone" of the report (Oreskes and Conway 2010, 100).

Jasanoff's Boundary Actor view is useful because it gives us a means of identifying instances, like the one described by Oreskes and Conway, where science is being politicized. By recognizing the importance of a boundary between science and policy, we can helpfully articulate why certain cases seem problematic. For example, in the case above, the Reagan administration violated the science-policy boundary by filling the committee positions with people it deemed sympathetic to, or at least not problematic for, its cause. In addition to Singer, the White House's suggestion, Nierenberg secured six of the people he had suggested to the White House who had the relevant expertise, and whom he deemed prestigious enough to give the committee credibility in the scientific community. Oreskes and Conway also note that those who had been vocal about the dangers of acid rain, and who had the relevant expertise, were rejected by the Reagan administration (Oreskes and Conway 2010, 86). The other way that the White House violated the science-policy boundary was by editing the report in such a way that the final report, released to the public, was markedly different in tone and implication as compared to the penultimate version approved by the committee.

The issue with Jasanoff's view, however, is that the Boundary Actor view seems to give credence to the view that science is value-free, and that science is distinct and isolated from society. That is to say, the Boundary Actor view implies that scientific advice is *only acceptable* if it *appears* to be value free. It does this by seemingly only allowing value-judgments at the early stages of the science-policy process, to shape how and where the boundaries around "science" and "not science" are, as well as how questions are constructed and framed. Jasanoff writes that,

> If negotiation is the engine that drives the construction of regulatory science, boundary work is the casing that gives the result legitimacy. Boundary work by scientists grows out of a premise that seems diametrically opposed to the concept of negotiation and yet is equally essential to the closure of controversy. By drawing seemingly sharp boundaries between science and policy, scientists in effect post 'keep out' signs to prevent non-scientists from challenging and interpreting claims labeled as 'science.' The creation of such boundaries seems crucial to the acceptability of advice. When the boundary holds, both regulators and the public accept the experts' designation as controlling, and the

> recommendations of advisory committees, whatever their actual content, are invested with unshakeable authority (1990, 236).

This follows a paragraph where Jasanoff argues that the most politically successful examples of this are cases which leave room for regulatory agencies and stakeholders to negotiate the location and meaning of such boundaries (Jasanoff 1990, 236). My understanding of Jasanoff here, is that science in policy spaces must *appear* to be value-free and/or politically neutral. However, this attitude gives credence to the notion that science advice can be free from political, social, or ethical considerations, ultimately skirting the issue of what responsible science advice in the context of value-laden science looks like. While Jasanoff recognizes that science and values are not inherently in conflict, if we adopt her recommendations uncritically, we risk perpetuating the myth of the value-free ideal by legitimizing the perception that policy science is entirely free from political considerations. We got a glimpse of how this could occur in a fairly significant way chapter two, where Longino identified how contextual values can shape global, framework-like assumptions. As Jasanoff's own later work shows (using the co-production idiom), science and society are not separate and so, are shaped by each other. This leads to what Nancy Tuana calls "coupled ethical-epistemic issues" (Tuana 2010, 481).[32]

Coupled ethical-epistemic issues are problems where there is a significant ethical dimension to the epistemological problem under investigation. One example of this comes from Tuana (2010) regarding the presence of values in scientific modeling. Tuana and her

[32] Although this is not to say that all coupled ethical-epistemic issues are consequences of contextual values shaping global framework-like assumptions.

colleagues looked at the role of values in cost-benefit analysis (CBA) calculations. These CBAs examined possible outcomes with respect to climate management. According to the standard CBA model, if the benefits outweigh the costs, then that course of action should be taken, regardless of the consequences to the climate. However, another model sets a threshold on the probability of the collapse of the North Atlantic meridional overturning circulation (MOC), a system of currents that bring warm, salty water to the northern hemisphere and colder water to the southern. If the threshold is passed, and collapse becomes probable, then the set of actions that lead to collapse should not be pursued, regardless of the net benefits to society.

While it would be *epistemically* legitimate to use either model, social or ethical values will not only impact how models such as these are constructed, but also, which model policymakers choose. A policymaker who places significant value on economic growth will more likely choose the one that allows the MOC to collapse, while one who is more environmentally and socially conscious will choose the one which prevents the collapse of the MOC. Furthermore, despite the epistemic legitimacy of each model, we can see how easily value-ladenness can be used against the CBA model that protects the MOC from collapse. The critic might say, “This model is biased. It makes a pre-emptive judgement that the collapse of the MOC is bad, rather than weighing the economic benefits versus the environmental costs, which is what a CBA is designed to do!” The implicit claim here, is that the CBA that does not allow the MOC to collapse is not a result of disinterested inquiry. And fair enough. The critic is correct in saying that it is not the product of disinterested inquiry.

Rather, it is an objective way of modelling the impacts on the MOC relative to certain, pre-established interests (that is, its continued circulation). Said differently, it is a way of modelling the costs and benefits while acknowledging that there are certain costs we are unwilling to pay. The competing, supposedly more objective model, which does not give the MOC privileged standing, is a different way of modelling the same situation. This model is arguably just as biased by privileging an economic outlook where everything is for sale and under which there is some point where the costs associated with the course of action will be paid for by the economic benefits that accrue. This way of looking at the situation assumes that the worth of ecosystems and natural processes can be entirely captured in economic terms. It is these economic contextual values that end up acting like global, framework-like assumptions, and under these assumptions the logic of calling the model that prevents the collapse of the MOC biased makes sense.

The issue of contextual values shaping global, framework-like assumptions, the co-production idiom provided by Jasanoff, and the coupled ethical-epistemic issues of Tuana all point to the same conclusion: science and society are not separate entities. While Jasanoff may be correct that the appearance of a boundary gives policy-relevant science legitimacy, having a clear boundary actually reinforces the values-objectivity dichotomy. Further, by having this boundary, researchers and policymakers are able to focus solely on the epistemic aspects of a problem, with no discussion as to the ethical aspects. The creation of a boundary puts ethics beyond the scope of consideration, effectively rendering the ethical concerns invisible. We can call this the *problem of value-neutrality*. But Jasanoff provides no account for how non-epistemic values are used to evaluate the trustworthiness of scientific research, except to show numerous instances where values and evidence seem to come into conflict. Therefore, while it is important that there is no political interference with the production of scientific research, we need to avoid criteria for evaluation that reinforces the values-objectivity dichotomy.

3.4 Strengths and Weaknesses of the Honest Broker View

Finally let us consider Pielke's "Honest Broker view." The lessons from chapter two regarding the role of values in science complicates Pielke's analysis of the role for scientists. In this section, I argue that Pielke assumes a clear distinction between facts and values. Consequently, social and ethical value judgments can creep in under the guise of epistemic judgments if we follow Pielke's view.

Pielke's aim, broadly speaking, is to help illuminate the role that values play in the *policy process*. However, the issue with Pielke's view is that he does not account for the role that values play in *science*. Recall that Pielke thinks that there are four roles for scientists in policy development. These are the roles of pure scientist, science arbiter, issue advocate, and the honest broker. Pielke believes that the issue advocate most threatens the integrity of science in the policy process as scientists may engage in stealth issue advocacy by presenting themselves as pure scientists. But fully realizing the implications of value-laden science has the consequence that there is no clear conceptual distinction between honest brokers and issue advocates since, as we saw in the previous section, many issues are coupled ethical-epistemic issues.

As we saw above, values have legitimate and important roles to play at all stages of inquiry, including the questions being asked, the way that questions are framed, the research methods chosen, and how data are interpreted. The background assumptions and values that go into scientific research in these various stages have important consequences. If we apply these lessons to Pielke's view, this means that even the role of "pure scientist," that he outlines has political consequences and so, pure scientists are always acting as issue advocates.

The work of Elizabeth Anderson above showed how the values that researchers brought to their work influenced how researchers framed their research questions, looked for data, and provided explanations. She contrasts this with a feminist research program on divorce, which acknowledged a plurality of family types, rather than holding up the nuclear family as an ideal. Anderson further shows how this led to different research questions, data collection, and interpretation. These different empirical approaches are helpful in illuminating important consequences for Pielke's view.

It is useful to consider how an Honest Broker would provide advice to policymakers regarding these two research programs. Retrospectively, it seems obvious that the feminist research program described by Anderson is superior. Divorce does not have the same social stigma it did thirty to forty years ago. Nor is the default assumption that women and children are worse off following a divorce. But, if we consider a science advisor being asked to weigh in when this was a more controversial question, the answer is less obvious. If we are regularly faced with coupled ethical-epistemological issues like this one, then providing advice seems less straightforward even if we follow Pielke's recommendations.

Recall from chapter one that Pielke's recommendation for encouraging scientists to be honest brokers is that we should ask scientists two questions. The first question is that if our policy recommendation is based on scientific results, what scientific information would be necessary to change our minds? (Pielke Jr. 2007, 141) The second question is that since there may be a range of policies consistent with particular scientific results, we should ask "what is the full range of options that is consistent with these results, and, what non-scientific factors lead you to settle on one policy or group of policy options, over another?" (Pielke Jr. 2007, 142). Let us consider the scenario offered by Anderson to answer each of these questions in turn.

Anderson has presented our hypothetical honest broker with two competing research programs, with the feminist program offering evidence that should, in keeping with Pielke's recommendation change people's minds. But, for several reasons, it may not. First, the feminist research program outlined by Anderson does not straightforwardly falsify the results of the traditional research program. Instead, this feminist research program offers conceptual criticism by questioning the background assumptions of the traditional research program and adds context to the existing research by asking different questions. Consequently, this change in background values changes the interpretation of the available evidence and suggests new questions to ask. For example, both research programs showed that women typically face greater financial challenges after a divorce. However, the feminist research program found that woman have more control over the money they do have, and so, reported a higher quality of life (Anderson 2004, 15).

So, Pielke's question does not fully recognize the subtleties of scientific progress. My understanding of Pielke is that his view assumes a kind of logic of falsifiability. The framing of Pielke's question implies that facts are value-free, and so, are open to a kind of straightforward Popperian refutation. However, because of the nuanced relationship between values and scientific facts, such a straightforward falsification may not always be possible. In the case of divorce research, I take Anderson to be describing not a straightforward refutation and rejection of a hypothesis, but rather the reframing of research questions and methods, to contextualize existing research in a way that may more closely resemble a Kuhnian paradigm shift. In short, Pielke's view cannot easily account for how conceptual criticism, resulting in a change in the values shaping research, can change and recontextualize the evidence being produced. Nor does it recognize the importance of this for science being used in the policy context.

Secondly, people may disagree with the values informing this feminist approach to divorce research, and so, may not even recognize the research program as legitimate science. But rejecting the values that go into the feminist research program seems different from claiming that there is no evidence that would change your mind. Here, it is not the case that there is no evidence that would change our hypothetical science advisor's mind. It is saying that *this* evidence (i.e., evidence produced within such a research program) would not change their mind. It is a rejection of the process by which the evidence was produced, as well as the values which informed the production of such evidence. So, Pielke's first question, while important, fails to acknowledge the value-laden ways in which science is produced prior to being used in policy and so ignores the important value-laden context in which science advisors provide recommendations.

If we consider Pielke's second question, that is, what sorts of non-scientific considerations go into our policy recommendations, we are similarly poorly off. Continuing with our thought experiment, if our honest broker adopts the Precautionary Principle (i.e., the position that policymakers have a duty to prevent harm, especially in the face of uncertainty), then divorce seems to be a social ill which has negative effects for women and children, full stop, and so, our science advisor is likely to recommend a policy or range of policies based on research from the traditional research program. This is something that can be articulated by our policy advisor, and something which, at the time, likely would have seemed eminently reasonable to a significant number of people. However, it has several problematic consequences.

The first consequence is that these policies are unlikely to help single mothers and their children and may in fact compound their struggles. Secondly, it also shows the limits of the Precautionary Principle in identifying harm. Our other values will shape how, where, and to whom we look for harms. Thirdly, it contributes to an institutional-level distrust of policymakers, and potentially, to science itself. The reason for this is that women's experiences are not being adequately recognized. Our honest broker of policy alternatives may, as we saw in my analysis of Pielke's first question, reject feminist science as legitimate science. This mirrors the concern I raised against the Pure Science view above regarding what constitutes "the best evidence." A feminist research program may be seen as biased and so excluded from the evidence-base informing the advice as being not even good evidence, let alone “the best available evidence.” This curation of evidence reflects the non-scientific value considerations that the honest broker makes, and these judgments can, and do, legitimize certain values, research, and policies, intentionally or not. At the same time, these non-scientific value considerations are obscured by ostensibly “pure” scientific considerations, as noted by Kourany in chapter two. Yet we have seen that feminist research perspectives can be epistemically productive, if not epistemically superior.

But, even if this feminist research program is not perceived as biased and is included in the evidence-base, auxiliary values, such as the Precautionary Principle, may lead a well-meaning advisor to “play it safe” and make conservative policy recommendations to “protect the children.” Moreover, sexist contextual values can shape the global framework like assumptions that shape one’s use or interpretation of the Precautionary Principle such that it gets interpreted in this way. This is a less obvious form of sexism that may not be recognized by the science advisor or the policymaker because it can be hidden under the guise of value-neutrality. As such, the policies that the science advisor recommends, and that the policymaker may adopt, may be rightly met with resistance and distrust, as they use (perhaps inadvertently) oppressive science to justify (perhaps inadvertently) oppressive policies that reject evidence from people’s lived experiences.

To sum up, the previous discussion demonstrates how Pielke’s second question is limited. While some of the non-scientific considerations may be articulated, the relationship between values and scientific research is much more nuanced, so, certain scientific judgments can carry with them normative content that may not be recognized and articulated.

These issues partially arise because Pielke assumes a clear distinction between facts and values. An honest broker is making evidentiary judgments, but these are also value-laden judgments. Furthermore, as seen with the Pure Science view, some facts, or descriptive claims, carry normative content and as we saw in the Boundary Actor view, many issues are coupled ethical-epistemological issues. In cases of both judgment and descriptive claims, the thick evaluative concepts discussed by Anderson are at play. Pielke does not seem to fully appreciate that some genuine scientific questions may be value-laden or politically charged. He sees that there are some politically charged questions which science is asked to answer, but not the reverse. Asking about the human cost of climate change or the health effects of certain substances on human beings can threaten certain interests and consequently, be seen as partisan.[33] Alternatively, merely asking whether glyphosate is linked to cancer could be viewed as a threat to Monsanto's interests. Yet Pielke, with his Honest Broker, aims for something like the reflective centrism discussed by Douglas above (agnosticism with respect to two ends of a value spectrum). This kind of value-neutrality is presumably seen as politically desirable, as the responsibility (partially) of a science advisor is to be non-partisan. But as we saw above, this reflective centrism is problematic. When science is saturated with thick evaluative concepts and asks value-laden questions, one cannot be apolitical. Similarly,

[33] In *Merchants of Doubt* Naomi Oreskes and Erik Conway show how anti-Soviet policymakers saw environmentalism (partly because of its criticisms of military and defense spending) as supporting Soviet goals. These policymakers seem to have adopted the philosophy "if you're not with us, you're against us." Thus, environmentalists were painted as radical activists because they were seen as asking politically charged, anti-American questions. In a similar vein, Audra J. Wolfe shows how apolitical, value-free science, was used as a propaganda tool during the Cold War to support American interests abroad.

one cannot seem apolitical when merely asking certain questions is seen as activist or partisan.

So, given the roles of values in science, the distinction between Issue Advocate and Honest Broker seems blurrier. According to Pielke, the key difference between the Honest Broker and the Issue Advocate is that the Honest Broker offers scientific knowledge in the context of a variety of policy options (Pielke Jr. 2007, 2 – 3). Pielke characterizes this as "expanding choice." And yet, due to the problem of excess objectivity, the act of "expanding choice" must be an act of limiting choice as well. If there is, as the problem of excess objectivity says, scientific evidence that will support a broad range of policy options (including contradictory evidence and contradictory options) the Honest Broker must act to limit choice. Pielke recognizes this, acknowledging that some value judgments will have to be made. However, he adds that there is a difference between providing one policy option, which he says is characteristic of the Issue Advocate, and providing several policy options, which is characteristic of an Honest Broker. Yet my analysis above shows that even providing a range of options can take the form of stealth issue advocacy since the act of limiting choice is not solely an epistemic act and under the guise of value neutrality, contextual values and judgments can be made to look like purely epistemic ones. The available scientific evidence will play some role in limiting choice, but as I have shown above, value judgments will play a role in the evaluation and inclusion of evidence, the questions that are asked, and the ways in which evidence is gathered, so limiting choice will also require value judgments to reach this point of "policy neutrality." Thus, if values are variously encoded into the process of science advising itself, then even honest brokers, as

Pielke describes them, are stealth issue advocates. Consequently, by seeking to depoliticize science, Pielke ignores and obscures the politics of an immensely political role. However, Pielke does seem to recognize this danger and adds that advice is more trustworthy when it comes from an advisory panel or committee or institutionalized bodies such as national academies or professional societies. I will take up this part of Pielke's work in the next chapter.

3.5 Conclusion – The Credibility Problem and the Value Neutrality Problem

The main point arising thus far is that science is inescapably political. Although this point has been well-established by others, I raise this point because it is a crucial piece of my argument since it has important implications for science policy. As we saw with Longino in chapter two, the contextual values that affect which questions are asked can, in turn, transform into constitutive values for an area of research by regulating what kinds of questions get asked, what kind of properties are considered relevant and interesting, what kind of data is important, and what kinds of explanations are acceptable. Furthermore, Longino has shown us how contextual values can shape global, framework-like assumptions, and so, can shape science in ways that reflect some of the more problematic aspects of our society. The consequence is that we are frequently faced with coupled ethical-epistemic issues and as such, the very way that science is done has political implications that should not be ignored when advising on policy. We have seen examples of these things throughout the chapter, such as above with the hypothetical science advisor providing advice on divorce policy. Yet by focusing solely on the epistemic considerations, important ethical and political considerations can be, and sometimes are, ignored.

With the Pure Science view, we saw that attempting to remove all value influences from science was untenable, while attempts to acknowledge the role of values but preserve political neutrality did not go far enough in realizing the implications of the various value-science interactions. This allowed me to identify the Credibility Problem. The Pure Science View has no clear way of determining what the "best" evidence is, and it does not have a useful way of determining whether some information is objective, and if so, in what ways that information is objective. This, in turn, leaves science advisors open to the kinds of credibility attacks that I described above.

With Jasanoff's Boundary Actor view, we saw that maintaining the appearance of a boundary between science and politics seems crucial to the political acceptability of science advice. However, we saw how this presumption helps reinforce the belief that values are threatening to science. Reinforcing the belief that values are threatening to science ignores the many legitimate ways that values can be useful to scientific inquiry, obscuring the various ways that science and values interact, and potentially allowing problematic social values to shape the scientific advice being provided to policymakers.

Finally, with Pielke's Honest Broker view, we see how even being an "honest broker" in the way that he outlines, allows scientists to make covert judgments that can serve political interests. Furthermore, despite Pielke's attempts to correct for this by having scientists articulate what values and other non-scientific considerations go into their policy recommendations, and since scientific research uses thick evaluative concepts, value judgments can sneak in under the guise of typically accepted scientific or epistemic considerations. Thus, even Pielke's "honest brokers" may act as stealth issue advocates by curating the kind of evidence that is considered relevant, important, and sufficiently rigorous. This is the Problem of Value Neutrality. While Pielke is right to note that we need to know what kinds of non-scientific considerations go into policy advising, it would be helpful to acknowledge how these affect the production of scientific knowledge from the beginning.

Pielke and Sarewitz's diagnosis from chapter one is correct: advisors are faced with the problem of excess objectivity. This means that there are multiple avenues and areas of research that scientists and policymakers can draw upon to inform policy that do not add up to a single, unified picture of the world. So, there is research that can be used to support any number of policy positions. This means that science advisors must evaluate the available scientific evidence and use value judgments to narrow the scope of the research they will draw on, since not all positions will be technically or politically feasible. Because of this, even in situations where advisors attempt to be "honest brokers," they in fact act as "stealth issue advocates."

The presence of excess objectivity is important because that is what leads to the curation of evidence. Science advisors may legitimately use cognitive value judgments to curate evidence, but in doing so, may limit the scope of policy options in ways that reproduce structural oppression (be it gender identity/sexuality, race, or class-based). For this reason, it is important to also consider social and ethical value judgments when narrowing the scope of research. If a science advisor uses science without considering the social and ethical dimensions of the research, they cannot be seen as objective, be it defined as independent or impartial or disinterested, since as philosophy Naomi Scheman writes, it is "irrational to expect people to place their trust in the results of practices about which they know little and that emerge from institutions - universities, corporations, and government agencies - which they know to be inequitable" (Scheman 2011, 43). Science that is produced by unjust institutions cannot be regarded as objective by *everyone* and consequently, cannot be regarded as wholly trustworthy or credible. Thus, while each of the views discussed in chapter one attempts to ensure that biases are not present in science and science policy, we have seen here ways in which biases and values may be present and unaccounted for.

Chapter 4
Developing a Social-Pluralistic View of Science Advising

The neutrality of science to the uses made of it renders it silly to talk about its bankruptcy, or to worship it as the usherer in of a new age. In the degree in which we realize this fact we shall devote our attention to the human purposes and motives which control its application. Science is an instrument, a method, a body of technique. While it is an end for those inquirers who are engaged in its pursuit, in the large human sense it is a means, a tool. For what ends shall it be used? Shall it be used deliberately, systematically, for the promotion of social well-being, or shall it be employed primarily for private aggrandizement, leaving its larger social results to chance?

-John Dewey, Science and Society, *Philosophy and Civilization*

4.1 Introduction

My goal in this chapter is to develop recommendations for science advising that maintain the authority of scientific knowledge while avoiding the politicization of science and the technocratization of policy, all while recognizing the roles of values in science. I draw on the literature review and analysis of the roles of science in policy that I developed in chapter one, showing that a responsible science advisor must contend with two pitfalls, which I refer to as the Credibility Problem, and the Value Neutrality Problem. In this chapter, I develop ways to respond to these problems using the analysis of the literature on objectivity that I presented in chapter two. In discussing these problems and solutions, I ultimately show that to maintain the authority of scientific knowledge, while avoiding the politicization of science and technocratization of policy, we should adopt a pluralist conception of objectivity and embody our scientific advising institutions in panels, rather than in chief science advisors or chief scientists. Finally, since the value-free ideal has been recognized as neither achievable, nor desirable, I will exclude it and the anti-advocacy view that seems to rely on it from further analysis.

4.2 How the Pro-Advocacy Pure Science View is Susceptible to the Credibility Problem

The Pro-Advocacy Pure Science View, which holds that scientists should advocate for and advise on policy, articulated by Meyer et al. in chapters one and three, is useful to scientists because it offers a concrete set of tools in the form of five best practices for science advisors and advocates. These are:

1) to bring significant scientific information to the attention of policy-makers;
2) to ensure that policy-makers consider the relevant evidence;
3) to ensure that policy design is consistent with the best available evidence;
4) to speak only to what you know; and
5) to acknowledge when you are moving beyond your area of expertise (Meyer et al. 2010, 299 – 301).

However, these best practices also have drawbacks and limitations. While the latter two practices are important and useful, the first three practices fail to account for the presence of values. Words like “significant,” “relevant,” and “best” indicate the need for judgment which implicates values.

But how do we consistently determine what is significant to the public and to policymakers? Returning to the Monsanto example, if there is a weak correlation between glyphosate exposure and cancer, is this information significant to the public and to policymakers? It is certainly *relevant* information for policymakers and regulators when considering regulatory options. It is less clear whether a weak correlation reaches the threshold of "significant."

Alternatively, if we consider the word "relevant," how do we consistently determine whether something is relevant to the public and to policymakers? For example, if an advisor is providing advice related to the harmful effects of tobacco smoking, is it relevant for policymakers to consider the economic importance of cigarettes, or the size of the industry? Answering these questions will depend on the values one holds and one's beliefs about the role of science advisors. If one believes that science advisors should "stick to the science," regarding the harmful effects, then one likely believes that economic considerations are irrelevant to the question of whether tobacco consumption should be regulated.

Relatedly, words like "best," indicate the need for a similar kind of judgment on the part of an advisor or advocate. As I argued in chapter three, determining what constitutes the "best" can be difficult. Returning to the Monsanto example, if exposure studies in animals show a high correlation between exposure to glyphosate and cancer, while epidemiological studies show a weak correlation, what do we take to be the *best* evidence? The ambiguity surrounding the word "best" means that different parties may judge different evidence to be "best." Furthermore, corporations could claim that regulators have *not* considered the best available evidence, due to a pro-regulatory bias, and that any regulatory action is therefore unjustified. When this happens, alternative evidence is often provided which is instead said to represent the best available evidence. Thus, the issue becomes one of competing experts: who has the best evidence? This is what drives the politicization of science, as scientists and/or corporate lobbyists can cloak their value judgments under the guise of “scientific objectivity.”

The specific type of objectivity in question seems to be detached objectivity, rather than value-free objectivity, since values play a role in shaping one’s views of what constitutes the “best” evidence. Furthermore, detached objectivity is all about having the right kind of attitude with respect to evaluating evidence. It is not wanting a particular result too much or too little that our interpretation of the evidence becomes skewed.

To summarize, the issue with the Pro-Advocacy View is that its theorists’ advice assumes that there are obvious objective criteria by which we can judge standards of “significant,” “relevant,” and “best.” But, because these terms require judgment, reasonable people may disagree about which evidence is significant, relevant, or best, so there may be real or perceived failures of detached objectivity. The Pro-Advocacy View leaves scientists, advocates, and science advisors open to illegitimate attacks on their credibility, since they may be accused of not being suitably detached, and therefore, of inappropriately judging the evidence. Consequently, the Pro-Advocacy View does not give us the tools to determine whether these attacks are legitimate or not. However, if we adopt a pluralistic view of objectivity, and recognize values as a normal and useful part of the scientific process in the ways outlined by the philosophers in chapter two, then we can equip science advisors and policymakers with better tools for determining if such criticisms are legitimate. As I will argue below, by recognizing that values shape evidence, and that objectivity is a matter of both degree and kind, we can have more nuanced discussions about the strength of a claim, and the mere presence of values will not be a reason to easily discredit research.

4.3 How the Honest Broker and Boundary Actor Views are Susceptible to the Value Neutrality Problem

In chapter three, I discussed Roger Pielke Jr.'s Honest Broker View. His view is useful for several reasons. First, as with Jasanoff, Pielke recognizes that the formative technocratic linear view of the relationship between science and policy espoused by Vannevar Bush is untenable. He also highlights the importance of bringing scientists and policymakers (and presumably other stakeholders) together at an early stage of the policy development process to make sure that there is a common understanding of the questions to be asked. Finally, he recognizes that to be trustworthy, science advisors and advocates should practice honesty and transparency in their work.

To address this last point, Pielke proposes a method to make these social and ethical value judgments explicit in science advising: the Honest Broker View. However, as we saw in chapter three, social and ethical value judgments can be obscured by epistemic judgments, such as in the case of divorce research. This makes it difficult for scientists and science advisors to achieve the desired level of transparency using the Honest Broker View.

Further, Pielke's solution does not adequately account for the distinction between epistemic/cognitive and social/ethical values. As we saw in chapter three, Pielke's Honest Broker is expected to curate the science relevant to a particular policy question, by a) articulating the range of policy options that are consistent with the scientific evidence, and b) list the non-scientific considerations which led them to settle on *those* options. What is missing, however, is a list of scientific and non-scientific considerations which led them to settle on that evidence. As I showed in chapter three, a science advisor may curate what science they consider relevant based on epistemic judgments (which may or may not be scientific considerations). However, because many science-policy issues are coupled ethical-epistemic issues and use thick evaluative concepts, simply using epistemic considerations in making these curatorial decisions is insufficient. Relying solely on epistemic judgements when curating "relevant science" can obscure the ethical dimensions of an issue and may result in a science advisor inadvertently overlooking other relevant and useful research. In less honest cases, advisors may rely solely on epistemic considerations to intentionally obscure the ethical dimensions of an issue to pursue a specific agenda while continuing to be perceived as credible and trustworthy.

For example, in the study of divorce from chapter three, one could simply fail to see the ethical dimensions of the case, and so, focus solely on the epistemic considerations such as the methodology or the size of the sample when evaluating the available research. Alternatively, one could see the ethical significance of this research and try to discredit it by intentionally emphasizing epistemic dimensions such as the "disinterested" nature of competing research. In this case, one could see how the positive effects of divorce for women could be used to justify policies that make it easier for couples to divorce and/or justify policies that support the economic independence of women, and, because of one's belief in traditional gender roles, one could emphasize the importance of epistemic considerations (e.g., rigorously produced, evidence free from values) to curate the evidence to undermine the credibility of these results.[34]

Ultimately, cases with coupled ethical-epistemic issues and thick evaluative concepts are not readily addressed by Pielke's solution. His view suggests that values, and especially social and ethical values, only play a role in the policy process, and the epistemic evaluation of relevant scientific research seems to fall more squarely on the "science" side of science policy. This mirrors the false divide between science and policy in both the Pro-Advocacy Pure Science View discussed above and Jasanoff's Boundary Actor View, which I turn to now.

Jasanoff's Boundary Actor View offers many helpful lessons, although two are especially significant. First, she helpfully notes that science advice rarely sticks to technical issues and advisors are often required to make a number of social and political judgments. Secondly, her view *does* give us some way of articulating when the advice from science advisory institutions should be regarded skeptically. Having a science-policy boundary, permeable though that boundary may be, allows us to articulate a variety of ways in which political considerations may have unduly influenced the advice being provided. As we saw in the previous chapter with the example from Oreskes and Conway, the White House clearly interfered with the acid rain panel chaired by Nierenberg. The science-policy boundary was

[34] One may even do this without realizing the irony of advocating for value-free science, and the social values that are shaping their advice, particularly if one takes traditional gender roles as "a given."

transgressed, and the resulting report lacked credibility. But Jasanoff's view does not capture *all* the ways in which this boundary can be transgressed. It does not capture cases where social and ethical value judgments sneak in under the guise of epistemic value judgments such as in the example of the differing research programs on divorce. Since, as Jasanoff herself has argued, science advice rarely sticks to just the science, science advising often includes, overtly or not, both ethical and epistemic issues. Consequently, social and ethical value judgments can be obscured by appealing to scientific objectivity, or be cloaked in scientific, technical language. This obfuscation of social and ethical value judgements by claims of objectivity is what happens in the Value Neutrality Problem.

4.4 Standards of Objectivity and their Implications for the Credibility and Value Neutrality Problems

Each of the three views that I have presented recognizes the public's and policymakers' need for credible information, but differs in how to realize this credibility. Further, each view recognizes the presence of values at the science-policy interface, albeit to varying degrees and with varying levels of tolerance, highlighting different ways that values and science can interact. These variances in the relationship between values and science complicates Douglas's position discussed in chapter two: wherein values are only politically salient when scientists advise on the risks associated with certain courses of action in contexts of high uncertainty.[35]

[35] Neither Lackey, Meyer et al., Jasanoff, nor Pielke engage with Douglas's book, in the works of theirs that I am drawing from. In most cases, this is largely attributable to temporal reasons. For example, Lackey's article was published in 2007, Jasanoff's book in 1990, and Pielke's book in 2007. Douglas's book, by comparison,

By evaluating the discourse of scientists (e.g., to only provide "the best evidence") relating to the science policy interface, as well as the role that values can play in the creation of advisory bodies, we can see that objectivity plays an important role in determining the credibility of a person or statement. In addition to this, we can also see that politically relevant value judgments occur at various stages in both the scientific and advisory processes.

Recall the eight kinds of objectivity discussed in chapter two: manipulable objectivity, convergent objectivity, detached objectivity, value-free objectivity, value-neutral objectivity, procedural objectivity, concordant objectivity, and interactive objectivity (i.e., Longino's contextual empiricism, from here called Critical Contextual Empiricist objectivity). However, in the remaining views of science advocacy and advising (i.e., the Pro-Advocacy Pure Science View, Jasanoff's Boundary Actor View, and Pielke's Honest Broker View), value-free objectivity does not seem to be the operative standard. Instead, we see both detached objectivity and value-neutral objectivity at play. As such, I will analyze the usefulness of these two kinds of objectivity to show how using them as *the* operative standard of objectivity leads to the Credibility Problem and the Value Neutrality Problem respectively.

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was published in 2009. Meyer et al. published their article in 2010. While Douglas has written articles prior to the release of her book, these are also not cited by Lackey, Meyer, or Pielke. However, Lackey does cite an article from Pielke. Meyer et al., cite Lackey's article and a different article from Pielke, but not Pielke's book. Notably, Douglas does draw on Jasanoff's work for developing her own view, but does not draw on Pielke in her book. My intention in writing this dissertation has been to bring these interesting and prominent views into greater conversation with one another.

Detached objectivity plays a significant role in shaping the advice of the Pro-Advocacy Pure Science View. Meyer et al. assume that individual scientists can impartially judge which evidence is significant, relevant, and "the best." As I will argue below, whether scientists *can*, is less important, since the adoption of detached objectivity as a standard for evaluating trust leaves science advisors and advising institutions open to criticism that they have not been suitably detached (whether such a critique is true). Consequently, using the ideal of detached objectivity undermines trust in both scientific and democratic institutions.

On the other hand, value-neutral objectivity is operative in the Boundary Actor and Honest Broker views. Jasanoff's Boundary Actor View has the advantage of including more democratic elements than Pielke's Honest Broker View, and therefore in principle, provides greater controls for ensuring more trustworthy outcomes. However, neither view goes far enough in recognizing the myriad of ways that values and objectivity interact. This has the consequence of inadvertently reinforcing the idea of an impermeable science-society boundary, and delegitimating valuable and important uses of ethical and social values in science and science advising.

In the remainder of this chapter, I further develop these two pitfalls that responsible science advisors must avoid. First, I show that in the case of adopting a detached view of objectivity, as is the case with the Pro-Advocacy View, both values and objectivity can be used against credible science. I call this the *Credibility Problem*, in which experts, each claiming to have the "best science," compete for the attention of policymakers and politicize science in the process. Secondly, I show how the science/value dichotomy can lead science advisors and advocates to offer technological, “value-neutral” policy advice which then obscures the value judgments being made and fails to consider the ways in which science and society influence each other. I call this the *Value Neutrality Problem*. From my perspective, responsible science advising means, at minimum, successfully navigating both of these pitfalls.

4.5 The Credibility Problem

While I have referred to the Credibility Problem in the singular for convenience, there are two situations where the problem occurs. Broadly speaking, the problem occurs when the perceived credibility of some claim is manipulated to either be greater or lesser than is deserved. The first occurrence of the Credibility Problem arises when advocates promote dubious science that supports their research, unjustly *inflating* the credibility of a scientific claim. The second occurrence is when advocates attack the credibility of research that threatens their interests, unjustly *deflating* the credibility of competing claims. In the following section I will provide examples of how these occurrences arise and the roles of objectivity in addressing the Inflating and Deflating Credibility Problems.

Both occurrences of the Credibility Problem can arise when scientists and science advisors use detached objectivity as the standard for assessing credibility. Since the Pure Science View regards science as a value neutral policy tool, and detached objectivity as the guiding principle for science advisor’s behaviour, value judgments made in the regular course of scientific inquiry can be leveraged to undercut the credibility of legitimate research being done. As we know, the Pure Science View does not recognize that science is political, therefore when scientists aim to provide the “best” evidence, this is under the guise of being apolitical. In this instance, science advisors and advocates may be incentivized to inflate the credibility of research that supports their political views. Secondly, the Pure Science View fails to acknowledge that science requires value judgments, it creates a guise of value-neutrality. As such, scientists and science advisors may be incentivized to use the real or perceived presence of values in competing scientific research to deflate its credibility. Both kinds of credibility problems have, at their root, a general distrust toward values, and a high premium on certain kinds of objectivity.

One example where the presence of values was used to discredit research comes from the “Climategate” email hacks in 2009. Content from these emails was taken out of context and used by climate change deniers to claim that climate researchers had changed the results of their research to make climate change appear worse than it is (Leiserowitz et al. 2012, 819). In one series of emails, climate scientists Phil Jones and Michal Mann discuss a “trick” to hide a decline in warming over the preceding fifty years (Leiserowitz et al. 2013, 819). The decline in question was present in the dataset pulled from tree rings, but not in dataset from thermometer readings, while the “trick,” involved combining the data in a way that hid this divergence and was done because Mann and Jones were worried that this divergence would be taken out of context by climate deniers to argue that there was no problem (Grundmann 2013, 70 - 81).

Mann made a value judgment that it was better to hide the decline rather than giving climate skeptics data that they could take out of context to argue that climate change was not caused by humans. As we saw in chapter two, value judgments about how to interpret and convey data are common in the course of scientific research. However, it was not the appropriateness of the value judgment that climate critics questioned. Instead, it was the fact that there were any value judgments being made at all the critics took issue with, to argue that climate change was a hoax and that scientists were unduly manipulating data.

4.6 Credibility Inflation and Objectivity

In cases where credibility problems occur because of dubious science, bad actors manipulate public perception by presenting their own research (or research that favours their interests) as more objective than it is, especially in comparison to the research done or provided by advisory bodies and advocates. This over-inflation of credibility positions industry research as more objective by creating an illusion of detached objectivity and an illusion of convergent objectivity. In the following section I will discuss how the illusions of detached objectivity and convergent objectivity contribute to the Credibility Inflation Problem using the Monsanto case.

As presented in the introduction, the first instance of inflating credibility is by paying prominent researchers to attach their names to industry conducted research. This tactic relies on the illusion of detached objectivity to make Monsanto's research seem more credible. Recall that detached objectivity may be thought of as something like "disinterestedness" and is described by Douglas as, "not wanting a result so much that we create or ignore evidence" (Douglas 2009 122). As Douglas uses "detached objectivity," it refers to a kind of psychological distance between the researcher and the object of researcher.[36] However, since Monsanto has a financial stake in the product it is researching, it may be difficult for the company to impartially judge the evidence. This is in fact what the Monsanto documents show, according to a 2021 analysis from Leland Glenna and Analena Bruce. In their article, "Suborning Science for Profit: Monsanto, Glyphosate, and Private Science Research Misconduct," Glenna and Bruce provide evidence from internal company documents, predominantly emails, to show Monsanto executives were becoming increasingly aware of concerns about the toxicity of glyphosate but found reasons to dismiss the findings and avoid further regulation, rather of calling for further research (2021, 7). Instead, Monsanto hired academics to lend their credibility to research and documents ghostwritten by Monsanto employees to support its claims about glyphosate's safety.

The second way that credibility is inflated in the Monsanto example is through the illusion of convergent objectivity. Glenna and Bruce note how agricultural firms offer "materials and funding to university scientists with the goal of building a body of independent, peer-reviewed publications to demonstrate that their products are safe and efficacious when it comes time for regulatory review" (2021, 2). Since different authors' names were strategically associated with different papers, all supporting Monsanto's product, there is an illusion of arriving at the same conclusion from separate lines of inquiry.[37] Having

[36] Said differently, "detachedness" is a kind of phenomenological state.
[37] Philosopher Leemon B. McHenry makes a similar point in his article on this subject, writing that Monsanto responded to claims of the carcinogenicity of glyphosate by pointing to four decades worth of evaluations from a worldwide community of experts that glyphosate is safe (2018, 195). McHenry responds to this by noting the irony of this claim when the Monsanto papers reveal that the company not only anticipated a carcinogenic

such a disparate community of researchers, plausibly from different fields, creates the impression of independent convergence of research that the product is in fact safe. When the perception of detached objectivity is paired with the perception of convergent objectivity, as we have seen in the Monsanto case, the claims of Monsanto appear credible, but at the cost of human health.

4.7 Credibility Deflation and Objectivity

The second occurrence of the Credibility Problem also relies on detached objectivity. As such, presentations of how values have, or have been perceived to, affect the research will deflate its credibility. This is because the researcher or the program no longer seems detached, and science no longer seems value neutral. As I will discuss in this section, the two kinds of credibility problems that I have outlined are used jointly – one claims their research is detached and objective (inflating credibility) while levying criticism against competing research as biased or otherwise value-laden (deflating the credibility). Further, recall from Douglas that detached objectivity and value neutral objectivity are frequently conflated with the value-free ideal. I would extend this point to include detached objectivity and value neutral objectivity are also often conflated with each other. The result of all this conflation is that it can be difficult to achieve clarity on the boundaries of these concepts. My proposed solution will help clarify and distinguish between these two terms. Often, such as the EPA versus Big Tobacco case which I will discuss in the following chapter, we have experts

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classification of glyphosate, but also contracted third-party academics to act on Monsanto's behalf as "independent" experts to sign on to reports ghostwritten by the company and published in academic journals and the lay media (2018, 195).

competing for who has the "best" evidence using these inflationary and deflationary strategies.

Using values to undermine the credibility of research or an institution can occur when one uses emotionally charged language to insinuate that an organization and/or its researchers are biased, and so have arrived at some preconceived result to support their bias. By using the presence of values to question the integrity of the researchers or the organization of which the researchers are members, the objectivity of the research results can be called into question. This creates the impression that the research is neither independent nor disinterested, by insinuating that the researchers were not suitably detached. By extension, any policy justified based on this research may not be seen as credible or legitimate.

However, these attacks only work if we assume that values are opposed to objectivity.[38] If objectivity is understood as aperspectival, or as a detached "view from nowhere," then there is no room for values in scientific inquiry. Consequently, suggesting that since research is from a perspective, a non-detached "view from somewhere", it is value-laden and not desirous to include as part of the evidence-based for decision-making. Perspectives therefore would seem to undermine the disinterested nature of scientific inquiry itself. Furthermore, if this "view from nowhere" requires a psychological distancing on the part of a researcher, then a failure (or even the insinuation of a failure) to adhere to this psychological distancing may be seen as biasing research.

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[38] A point made by Longino (1990), Douglas (2009), Goldenberg (2015), Whyte and Crease (2010) and others.

Detached objectivity seems to be the operative standard in both versions of the credibility problem. In cases where the presence of values is used to deflate the credibility of some evidence or conclusion, one argues that the organization or persons doing the research were not suitably detached; they wanted a particular result so bad that they skewed the research in their favour. In cases where the credibility of dubious science is inflated, those promoting dubious science typically emphasize their scientific attitude, highlighting their dispassion, detachedness, and reasonableness. Based on how I have constructed these credibility problems, one may argue that the operative standard of objectivity in them is the value-free ideal, rather than detached objectivity, since any suggestion of values influencing science will be a threat to the credibility of that science under this view. I address this concern in the next section.

4.8 The Credibility Problem and the Value-Free Ideal

If it is the case that value-free objectivity is the operative standard in the credibility problems, and not detached objectivity, there does not seem to be a need for my view, since we already have ways of responding to the credibility problems, such as Douglas's role for indirect values in cases where evidence is insufficient. However, an individual using either of these strategies could accept that values, including social and ethical values, are a part of science but contend that the organization or researcher was not suitably "distanced" from the research they produced. By embracing the fact that humans do have values, detached objectivity allows us to have a more nuanced conversation about the role that values are playing, at least in comparison to value-free objectivity, even if detached objectivity still regards them suspiciously. The issue then becomes a matter of degree – how detached a researcher was from their research, and whether it was enough.

However, "enough" is a vague term, and like all vague terms, it is difficult to determine (and to agree) when we have reached a point where we may legitimately use it. The reason we have this trouble stems from Douglas's construction of the ideal as one of individual thought processes. In the view of detached objectivity presented by Douglas, there is no obvious mechanism for assessing the degree to which one has maintained "enough" psychological distance, and the degree to which their personal biases and idiosyncrasies have influenced the research. One must take the researcher's word that they have achieved the appropriate psychological distance. But it is unlikely that researchers will be able to perfectly do so or know when they have done so "enough." They may have gone into a certain field because they love animals, or the natural world, and such attachment may make them a great scientist, but also sway them toward one interpretation of the data or set of policy options rather than another. Furthermore, whether the researcher *can* achieve it or not is moot, as they are always susceptible to the sort of credibility attacks that I have described above.

However, let us grant that there is a scientist who can achieve this perfectly detached mental state. Since it is difficult to know with certainty whether they *have* achieved this detached mental state, this standard of objectivity is not helpful with respect to determining whether the researcher or institution is biased. Someone whose interests are threatened by the scientist's research, or the advisor's acceptance of certain research, can claim that they are biased, and the policymaker may have more trouble weighing the credibility of the science they are presented with.

4.9 Solving the Credibility Problem: Pluralistic Objectivity and Critical Contextual Empiricist Objectivity

A partial solution to the Credibility Problem is to understand "detachedness" as a community property.[39] Longino's Critical Contextual Empiricism offers an alternative way of understanding "detachedness." Longino's pillars of transformative criticism and community responsiveness to such criticism play the same role as detached objectivity. It is through critique that knowledge is constructed since part of the role that transformative criticism plays is evaluating the influence of values. Longino's view takes for granted that all enquirers bring with them certain values and biases when doing research. When these individual values are open to public scrutiny, members of the community are responsible for critiquing these values and determining whether they are good for inquiry. What results is a form of detached objectivity, insofar as the values in question are no longer specific to an individual, but now represent the community's position.

So, while Douglas is correct in recognizing that detachedness expresses an important norm of scientific practice, I differ from her in considering it as a norm of community rather individual thought processes. If we understand objectivity as socially constructed, then adopting the Critical Contextual Empiricist framework offered by Longino helps us achieve the aim of detached objectivity though the elimination of personal bias. We can therefore remove detached objectivity from our framework and instead subsume its goals within the standard of Critical Contextual Empiricist objectivity. It should not be the case that merely

[39] Roth and Lee (2002) have shown how community properties can exist, using the concept of "science literacy" based on a case study a British Columbia community.

claiming that values have unduly shaped some advice, or some piece of research, is enough to discredit research. Instead, we should look at what the community of experts concludes with respect to the values, evidence, and theory matrix described by Brown in chapter two, as well as the degree to which the expert community matches with Longino's ideal. Yet as I have said, this only partially resolves the problem of competing expertise, insofar as it recommends regarding individual scientists claiming to be more objective than their peers with increased skepticism. Subsuming the goals of detached objectivity into the standard of Critical Contextual Empiricist objectivity allows us to consider cases such as those described by the Credibility Problem more carefully. We can resolve this issue more completely by attending to the role that objectivity plays with respect to assertions of trust and credibility.

4.10 The Role of Objectivity in Assertions of Trust and Credibility

As we saw in the first chapter, the most frequent standard for evaluating expertise and knowledge claims in science policy scholarship, is objectivity. This reflects an historically close connection between objectivity and rationality in Western societies.[40] It is precisely because of this close association with rationality (and the corresponding association of values with irrationality, or a-rationality) that objectivity can be used to inflate the credibility of claims. When claims are made that threaten established interests, these claims can be made to look irrational or otherwise non-objective by those wielding institutional power. Additionally, since objectivity is so trusted, those who can make objective claims appear

[40] This association goes at least as far back as Plato. He famously thought that we could understand the ultimate forms through reason.

irrational or non-objective can create doubt and distrust about claims that threaten their interests, leading to the two kinds of attacks I have described above. These are the kinds of attacks that have been well documented by science and technology scholars, such as Oreskes and Conway (2010) and McGarity and Wagner (2008). I believe that my account helpfully advances this literature by providing names to these attacks and a description of how they can occur, and the kinds of objectivity involved.

Philosopher Naomi Scheman's views on objectivity as trustworthiness are a useful addition to this discussion. She notes that "[a] sustainable attribution of objectivity serves to underwrite a significant degree of - objectively refutable - authority, and it does so by rationally grounding trust" (Scheman 2011, 25). She also writes that objectivity "is a normative concept with which we evaluate our own and each other's assertions and beliefs" (Scheman 2011, 24) and that to call something "objective" is "to recommend it to others, and, importantly, to suggest that they ought to accept it" (ibid.). Objectivity, according to the science and technology scholars discussed in chapter one, acts as *the* normative standard by which we should evaluate expert claims, and experts fail to be objective when values and subjective preferences take precedence over evidence (or even just appear to). However, the corollary of Scheman's point is that to say that something is non-objective, is to say that it is not trustworthy. Scheman thus problematizes the traditional relationship between trust and objectivity and in doing so, provides a functional account of objectivity. That is, she explains what objectivity does and why it is valuable by describing how it functions in our evaluations of experts and the research they produce.

Scheman's point is that to call something objective is to say that it should be regarded as authoritative, while also being open to refutation, and as such, should be regarded as trustworthy (a similar point is made by Douglas). Scheman believes "[b]y attending to the function of objectivity in rationally grounding the trust we are called on to have in what scientists do, we can get a handle on understanding both why and how scientific practices are objective and why and how they are not" (Scheman 2011, 27). Scheman also tells us that when we trust what scientists come up with, what we are trusting them to do is to conform to the norms of good scientific practice. And we trust their results to the extent that we believe that they have done so while conforming to those norms reliably lead to the truth (Scheman 2011, 28).

If we regard the scholars from chapter two through the lens that Scheman provides, we can not only see that there are different ways of constructing objectivity, but also, that each offers a different standard for justifying the trustworthiness of a claim. How are we to decide which standard to use? Our answer has important consequences. As we saw above, adopting a standard of detached objectivity leads to politicized science. Furthermore, it is vital for science advisors to hold the trust of policymakers. The purpose of science advisors is to provide policymakers with actionable information. If the advisor is seen as untrustworthy, or rather, non-objective, or in competition with other experts, then the advice they offer may not be taken seriously. Where does all this leave us then with respect to objectivity?

4.11 Adopting a Pluralist Conception of Objectivity

Longino and Kourany offer us two ways of understanding not only how objective knowledge is constructed in science, but how it ought to be constructed. Longino demonstrates that the production of scientific knowledge is a social process and consequently, she limits her conception of objectivity to a social process - transformative criticism. Social processes and relationships are complex, and there are numerous, and at times, competing, values at play in a given community. As we saw above, objectivity is valued and understood in different ways by different people. For people studying science policy from an STS perspective, or even by scientists themselves, detached/disinterested objectivity appears to play an important role because it seems to offer assurance that personal biases are minimized. By focusing on the social processes, Longino's view helps ensure that the knowledge that science produces is detached from any single person's interest.

However, the upshot of recognizing that objectivity is socially constructed, and done so in multiple ways, as we saw in chapter two, is that a deficit along one dimension of objectivity should not automatically count against the trustworthiness of a particular claim or position. For example, several epidemiological studies may be done about the carcinogenicity of substance X, with some studies finding a correlation between exposure and cancer, and others finding no correlation. Assuming that all the studies are methodologically sound, in such a case, I would argue that there is a deficit with respect to the standard of convergent objectivity. However, this deficit, on its own, should not license us to dismiss the claim of carcinogenicity. We should look at how the claim of carcinogenicity fares along other dimensions. We might consequently look at dose-response studies and see whether high doses of a substance X in laboratory settings are associated with cancer (Shrader-Frechette 1993, 34; Elliott 2017, 125 – 127). If they are, not only does the claim become credible along the lines of manipulable objectivity, but it also helps fill the deficit that exists with respect to convergent objectivity. Furthermore, different policy questions may be better served by different kinds of objectivity, so we should not fault a position for failing to meet all, or even one's preferred, standard for objectivity. Consequently, those receiving and/or evaluating science advice must look for different kinds of objectivity, and judge whether the evidence is strong enough to merit the influencing of policy.

What is not yet clear from this discussion is the role of social values. So long as they play an indirect role in inquiry, as Douglas recommends, they seem unproblematic. But if we follow Kourany in thinking that science must meet social and ethical standards as well as epistemic, science's utility as a tool for policymaking seems diminished. The claims made by scientists will no longer appear to be detached, since they will be required to meet these social and ethical standards that may not be agreed upon by everyone, or even a majority, of people.

Liam Kofi Bright (2018) articulates an argument made by Du Bois in support of value-free science wherein if science is to be a guide for decision making in a democracy, scientists must only be, and appear to be, motivated by a search for truth (2235). While I do not argue for the value-free ideal, I do think this argument raises a legitimate point of concern if we adopt Kourany's view. If social and ethical values are adopted in as prescriptive a way as

Kourany describes, it seems likely that Du Bois's worry will come to fruition. This is not to say that the anti-racist and anti-sexist egalitarian values that Kourany argues for have no place in scientific inquiry, but making them core requirements in the way the Kourany does can easily be used to deflate credibility in the ways I have discussed above with respect to the Credibility Problem. Under Longino's Critical Contextual Empiricism, egalitarian values like those Kourany has in mind can be used to show the shortcomings of certain research programs or the framing of certain research questions. Then, as Brown argues, our evidence and/or theories can be revised and refined by the community so that we are able to answer the question that originally prompted our inquiry. For this reason, the Critical Contextual Empiricist objectivity of Longino ought to play an important role. Critical Contextual Empiricist objectivity represents a kind of community agreement.[41] This agreement should be regarded as a necessary, though not (usually) sufficient condition for regarding a claim as authoritative and trustworthy.[42]

4.12 Consequences of Adopting a Pluralist Conception of Objectivity

Adopting a pluralist conception of objectivity has several consequences. First, a responsible science advisor ought to be able to point to the composition of a community to justify to a policymaker why some claim should be considered credible or not. That is, they ought to be able to explain why a diverse community of inquirers is epistemically important and

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[41] I call this community agreement for readability. What I mean to capture here is the (perhaps temporary or highly contingent) acceptance of certain values within a community following the process of transformative criticism and community responsiveness discussed by Longino.

[42] However, this raises the problem of determining community agreement and dealing with "maverick" scientists who rebel against it. Such mavericks may be seen as "whistleblowers," which may increase their credibility (Grasswick 2008, 397). I do not take up this problem here but leave it for future work.

understand the composition of the scientific communities that produce the data they are using. Relatedly, a policymaker (and the public) should be wary of an advisor or organization who says that diversity of thought is not important.

But community composition is not the only marker of trustworthiness that philosophers have identified. As we saw in chapter two, Heather Douglas identifies eight kinds of objectivity as markers of trustworthiness. Above, I showed that this number can be reduced to six (manipulable, convergent, concordant, Critical Contextual Empiricist, value-neutral, and procedural).[43] A claim may be objective along one dimension, to greater and lesser degrees. But a claim may be objective along multiple dimensions as well, and to varying degrees. Policymakers can use this pluralist account of objectivity to calibrate the credibility they assign to a particular claim and science advisors can use these kinds of objectivity to help articulate why they consider some evidence to be credible or not. Consequently, a science advisor who can articulate to a policymaker the different dimensions along which a claim may be regarded as objective, and the degree to which a claim is objective along these various dimensions, ought to be regarded as a credible source of information. By adopting a pluralistic notion of objectivity, we are able to address the Credibility Problem by being better able to determine the "best" available evidence.

One may ask here, "how do I tell the difference between genuine skepticism and the illegitimate use of objectivity and values to inflate or deflate credibility?" I believe that the

[43] Although I acknowledge that there may be more, but, in the next section I show how we can better think of value neutrality as a kind of procedural objectivity, thereby reducing this number to seven. Marianne Janack in "Dilemmas of Objectivity" (2002) identifies no fewer than thirteen different uses of the word "objectivity."

two concepts I have deployed above, that is, pluralistic objectivity and Critical Contextual Empiricist objectivity, are sufficient for responding to this concern. In cases where there is skepticism about some area of research, the community agreement around the values, evidence, and theory matrix will be crucial. If there is no community agreement, either about which values are appropriate or which evidence is relevant, then one can judge the credibility of a claim or conclusion based on these other kinds of objectivity. The tools for assessing credibility that I have provided are not meant to eliminate these kinds of discussions, but to help us have them in a more meaningful way. I turn now to the second pitfall facing science advisors: the problem of value-neutrality and technocratic policy solutions.

4.13 The Value Neutrality Problem

The Value Neutrality problem arises when we adopt value neutrality as our operative standard for objectivity. The problem, simply stated, is that the value neutral standard of objectivity necessitates curating evidence in a way that curates values and evidence from the "extreme" ends of some value spectrum, through the use of some value judgments. As such, it is disingenuous to act as though the advice is value neutral.

However, curation requires not only epistemic and cognitive value judgments, but also social and ethical value judgments. For example, many people would agree that racist and sexist evidence should be excluded from science advising but excluding these is an explicitly ethical value judgment. However, a science advisor may choose to exclude research done from an explicitly feminist or anti-racist perspective as well, seeing these as being the opposite "extreme" of the value spectrum, and so, equally biased. But this type of research can be useful in illuminating socially and ethically significant dimensions to a policy, and so, salient evidence could be excluded from policymakers' consideration. Yet if the rationale for excluding these value positions is that they are "biased," this seems like an epistemic judgment rather than a social or ethical one about the appropriateness of these values. The judgment being made in this case has less to do with the harmful nature of sexism or racism, and more to do with the corruption of sound scientific evidence. Nevertheless, value neutrality is still regarded by some as a legitimate standard of objectivity, which has two important implications for science advising. The first is that using the standard of value neutrality can cloak value judgments in technological precision (Shrader-Frechette 1993, 7), and the second, is that the standard of value neutrality may cause policymakers to favour technological interventions to social problems, rather than societal changes (Elliott 2017, 47). This is because such interventions are seen as apolitical due to this veneer of technical precision.

As we saw in chapter two, value neutrality is another ideal that is frequently conflated with the value-free ideal. As with detached objectivity being understood as a form of Critical Contextual Empiricist objectivity, we can understand value-neutral objectivity using another form: procedural objectivity. Doing so allows us to more clearly see how value-neutrality can play a political role in obscuring important value judgments and social and ethical considerations by technocratizing policy making. Understanding value-neutrality as a kind of procedural objectivity also adds conceptual clarity to the discussion by eliminating the confusion that can arise through value-neutrality's conflation with the value-free ideal.

Value-neutrality, as we saw in chapter two, is a position of agnosticism with respect to a range of values. Douglas acknowledges that value judgments (including cognitive, social, or ethical values) play some role in arriving at this "reflectively neutral" position. The example Douglas offers is a literature review. In doing their review, researchers must chose a methodology (e.g., for a literature review, one might use a critical review, to contrast a number of perspectives, or a conceptual review, which organizes the material thematically), which will shape where one looks for data (e.g., which database one uses), what kind of data one looks for (e.g., what search term one uses), which data are included, and, how to interpret the data once it is collected (Douglas 2009, 88). Thus, while this kind of work might be useful, it seems a misnomer to call it "value-neutral."[44] Value-neutrality, like the other kinds of objectivity I have discussed, is a construction. But in reconstructing it here, we can see that value-neutral objectivity may be thought of as a kind of procedural objectivity;[45] through structured reflection, we can curate the evidence available for consideration.

Recall that procedural objectivity refers to processes which aim at eliminating personal judgment and idiosyncrasies, whereby consistent outcomes are produced, regardless of who performs the process. This "consistency of outcome" seems to be the aim of value-neutral objectivity, especially in cases such as a literature review. Among other things, the aim of a literature review is to provide a foundation, or knowledge base, from which to draw conclusions from and *in theory*, if everyone is looking at the same evidence, then they would

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[44] Douglas writes of value-neutrality "[w]hile the overview may in fact incorporate values in how it presents and views this topic, it does not allow extremes in those values" (2009, 124).
[45] Kourany also argues that values are incorporated into the *process* of arriving at some reflectively centrist position.

arrive at the same conclusions. Furthermore, literature reviews are important to science advising. Science advisors may not have the time or resources to conduct primary research, and so, may rely on the work of others to provide their advice. Of course, one could not include all the relevant literature, but ideally, one would include a representative sample of the relevant literature. But, as we have seen, value choices come into play when choosing which evidence is relevant, as well as in evaluating the evidence that once decides on. Having shown how value-neutral objectivity is best understood as a kind of procedural objectivity, in the next section I show how value judgments throughout this process can curate evidence.

4.14 How Value Judgments Curate Evidence in Value Neutral Positions

Since scientists, science advocates, and science advisors are required to make value judgments, one could curate evidence in a way that excludes morally or scientifically significant evidence, or curate it such that it favours evidence which furthers a particular agenda (even while avoiding the extremes of a value spectrum).[46] For instance, in the divorce example from chapter three, our science advisor excluded research from the feminist research program, on seemingly epistemic grounds, yet the feminist research program on divorce offers morally and epistemically significant evidence for consideration. Alternatively, let us consider the acid rain panel from chapter three. We can imagine a nearby possible world with membership more supportive of the White House's position. In this possible world, we can

[46] Further, this curation can be used to inflate or deflate credibility, although that is not the focus of my discussion.

imagine the panel deciding on seemingly epistemic grounds, to include evidence strongly supportive of the view that acid does not have anthropogenic causes.

The point here is this: value neutrality is never value-free. Reaching a point of value neutrality requires a certain amount of reflection, and/or the following certain procedures. If these procedures fail to appreciate the legitimate roles that social and ethical values play, and the ways that they intersect with epistemic values, the social and ethical values that go in to reaching this value-neutral position may be obscured and the process may, perhaps inadvertently, encode problematic values.

As we saw above, a science advisor must curate evidence, including which evidence they consider *relevant*, and which they consider *good*. By placing values and objectivity in opposition, science advisors are disincentivized from making explicit the social and ethical value judgments that go into making these curatorial decisions.

4.15 Value Judgments and Risk Assessments

One way in which these social and ethical value judgments have historically been obscured is through the move towards risk assessment in U.S federal decision making in the 1970s. Broadly speaking, risk assessment refers to the process of making an inference about the risk of harm something poses to either environmental or human health. The value judgments that go into risk assessment have been a key focus of philosophers studying policy-relevant science, with Kristin Shrader-Frechette's *Burying Uncertainty: Risk and the Case Against Geological Disposal of Nuclear Waste* (1993), Heather Douglas's *Science, Policy, and the Value-Free Ideal* (2009), and Kevin Elliott's *A Tapestry of Values* (2017) being notable examples. Schrader-Frechette and Douglas in particular show how historically, risk assessment had been (wrongly) thought of as a way of providing objective, value-free information to policymakers. However, my focus here is to discuss risk assessment as a policy tool more broadly.

Risk assessment is a process that has three main parts: risk identification, risk estimation, and risk evaluation (Shrader-Frechette 1993, 31). First, one identifies the risk. Next, one undertakes various forms of analysis to estimate what, if any, risk the object of inquiry poses. Finally, the acceptability of the danger must be evaluated relative to other hazards (Shrader-Frechette 1991, 36). Once the analysis is complete, then policymakers must decide on the appropriate actions (e.g., legislation, banning, warning labels) to take to effectively manage the risk.

We can see that risk assessment reflects a very linear view of the role of science in policy. Furthermore, as one might imagine, it does not always work this smoothly. Public controversy erupts over issues like tobacco smoking, genetically modified organisms, or climate change. Value judgments enter risk assessment at various stages in both the scientific and policy stages of the process. Value judgments are required for deciding which substances to examine, which methods to use, what constitutes a significant result and/or risk, and what constitutes an appropriate response (Shrader-Frechette 1993, 28 – 38; Elliott 2017, 166). Further, as Douglas aptly shows, value judgments are required at various stages of the risk assessment process, which can lead to the two kinds of credibility attacks described above, undermining the legitimacy of the procedure.[47]

Risk assessment is intended to depoliticize decision-making by providing value neutral evidence and to ensure consistency of outcome. If everyone was looking at the same studies, then they should all reach the same conclusions. If everyone agreed on the procedure (e.g., the studies to be included, the significance of the results of each study, etc.), it may well have succeeded. However, due to the need for multiple value judgments (of the curatorial nature I described above) throughout the process, it did not achieve the desired end (Douglas 2009, 139 – 140). Instead, it 'technocratized' decision-making by making a risk of harm the sole sufficient condition required for a policy response. This, in turn, allows risk assessment procedures to be criticized for 'cherry picking' evidence or being otherwise biased. Yet there may be (and there often are) legitimate reasons for excluding some evidence.

4.16 How Value Neutrality Leads to Technological Solutions to Social Problems

However, risk assessment is not the only way in which science and policy intersect. Another example of the value neutrality problem, and of how value judgments can be obscured under the veneer of technical precision, is through the use of technologies to "solve" social problems (Elliott 2017, 47). One way this could occur is through the use of genetically modified crops to help address nutrient deficiencies. For example, Kevin C. Elliott in his

[47] Douglas provides a satisfactory solution to the problem of using science for policy as it relates to risk assessment, however, it should be clear by now that science for policy extends beyond risk assessment. Longino shows how contextual values can be included in global, framework like assumptions, and this can have a profound impact on the production and interpretation of evidence.

book, *A Tapestry of Values*, cites the case of the golden rice crop as one example of this (2017, 44). This strain of rice was developed to respond to vitamin A deficiencies in developing countries. Vitamin A deficiency can lead to negative health outcomes, including blindness, anaemia, and the worsening of infections leading to death (World Health Organization 2009, 1 – 2).

The rice was genetically engineered to contain beta-carotene, a precursor to vitamin A and pigment that gives plants a red-orange colour (Jasanoff 2016, 112 – 113). The addition of this gene is what gives the rice its golden hue (ibid.). When beta-carotene is processed in the body, it produces vitamin A. Rice was chosen as the delivery vehicle because it is a staple in many diets around the world.

Technologically speaking, golden rice was successful (Wight 2019, 192). However, it has not been successful as a policy response (Elliott 2017, 44). As of 2019, roughly twenty years after its development, it had only received regulatory approval in four nations: Australia, New Zealand, the United States, and Canada (Regis 2019). More importantly however, is that vitamin A deficiency

> develops in an environment of ecological, social and economical deprivation, in which a chronically deficient dietary intake of vitamin A coexists with severe infection, such as measles, and frequent infections causing diarrhoea and respiratory diseases that can lower intake through depressed appetite and absorption, and deplete the body stores of vitamin A through metabolism and excretion (World Health Organization 2009, 1).

This suggests that even if golden rice had been more quickly and widely adopted, it would not completely resolve the problem of vitamin A deficiency. Golden rice only addresses the “chronically deficient dietary intake of vitamin A” dimension of the problem, without addressing the ecological, social, and economic dimensions of the problem. These other dimensions of the problem may contribute to chronically deficient dietary intake, but they also contribute to other barriers to achieving healthy levels of vitamin A, such as infection rates. This means that golden rice, in the absence of ecological, social and economical changes, is not a sufficient solution to achieving sufficient levels in vitamin A in at risk populations.[48] Unfortunately, despite the complexity of the problem, one continues to see claims that the failure to adopt golden rice is a "crime against humanity" (Potrykus 2010, 466).[49]

The issue here is that by asking certain questions, and not others, we curate the kinds of evidence that are available to decision-makers.[50] In the case of golden rice, the question seems to have been "how can biotechnology address vitamin A deficiencies?" instead of "what factors lead to vitamin A deficiencies?" and "what social changes can be made to reduce vitamin A deficiencies?" In part, this curation is done by narrowing the kinds of expertise that are considered relevant. As Elliott argues, technical and hard science expertise, which are seen as "less political," are more frequently relied upon (2017, 47).

The larger issue here that I am trying to articulate is this: in attempting to appear value-neutral, policymakers and science advisors may see complex social problems as technological or scientific problems, and so, see these as problems that can be resolved using technological or scientific solutions. In such situations, science, it seems, is regarded as

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[48] This is not to say that golden rice could not still play a role in some solution. It is only to say that a sufficient solution must look at all aspects of the problem.
[49] Ingo Potrykus is one of the inventors of golden rice.
[50] The question in this case was "How can technology be used to solve the problem of food insecurity?"

distinct from society, and so able to come in and resolve all of society's problems. However, as the co-production[51] lens from chapter one shows us, science is not separate from society.

Regarding science as distinct from society further plays into the oppositional framing of values and objectivity. If science is not a part of society, it is not subject to society's values or biases, and so, is able to provide objective, factual information. In this way it can be seen as an impartial arbiter, detached from political or ideological concerns and positioned as an apolitical (i.e., value neutral) tool for policymaking. In practice, this allows politicians and policymakers to 'choose science' to avoid making overtly political choices. As we have seen, this results in a reframing of a discussion from a political one, to one about the science, which in turn, leads to the two kinds of attacks I have described above. "Choosing science" also prioritizes a certain kind of technoscientific advice, focusing on ideas and solutions from the natural sciences and technology, rather than information from the social sciences. Consequently, this allows science advisors to offer a very narrowly focused kind of advice, without any considerations of the social context. In short, it allows science advice to appear value neutral by promising to secure a 'consistency of outcome.'

But science is political, from the questions that get asked, to the ways in which the data is interpreted. And the choices and actions of a science advisor have consequences. If we focus solely on the technoscientific or "hard science" aspects of a policy, we risk overlooking important social aspects. If a risk assessment looks at the likelihood of a substance causing

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[51] "Co-production is shorthand for the proposition that the ways in which we know and represent the world (both nature and society) are inseparable from the ways in which we chose to live in" (Jasanoff, 2004, p.2)

cancer and finds it 'reasonably safe if used as directed,' but not at the ways that populations use the substance, or the ways in which risk and scientific research is communicated, the advisor has overlooked important information in providing their advice. Similarly, if an advisor looks at the feasibility of a technology, such as a GMO crop, but not the social context which gives rise to a problem, or the ways in which different populations may be impacted, the advice they provide will contain important gaps.

As in the divorce example from chapter three, the crux of the Value Neutrality problem in each of these cases is one of curation.[52] Value judgments come into play in deciding which questions get asked, how they get asked, which data are collected, and which data are relevant. The Linear Model[53] of science in policy emphasizes a technocratic approach to decision-making, and consequently, can emphasize a technoscientific approach to science advice and policies. These technoscientific 'solutions' are able to appear value neutral, if not value free, by following certain established procedures and protocols, whether they are risk assessment procedures, or a long-standing tradition of seeing only certain kinds of science as relevant.

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[52] Philosopher Heidi Grasswick makes a similar point in her 2010 paper, "Scientific and Lay Communities: Earning Epistemic Trust through Knowledge Sharing," arguing that scientific communities have a responsibility to share "significant" knowledge with lay communities. This, of course, requires scientists to make judgments about what constitutes "significance" resulting in the curation of scientific research to lay communities.

[53] The Linear Model, recall, claims that governments should fund basic research so that scientists have a pool of knowledge from which to draw from when called upon to answer questions of public importance.

4.17 The Value Neutrality Problem and Stealth Issue Advocacy

As I discussed in chapter one, Roger Pielke Jr. has argued that the Linear Model has incentivized scientists to become "stealth issue advocates," whereby scientists assume that because the science shows one thing, a particular policy option necessarily follows. It is easy to see why this might occur. If a risk assessment, for example, shows that there is a somewhat significant risk that glyphosate is linked to cancer, the common-sense thing to do is to ban, or at least severely limit, access to it among the general population.

Pielke argues that the Linear Model incentivizes a kind of technocratic, value-neutral decision making because it removes the onus of policy decision-making from policymakers and gives it to "science" and "scientists." The idea here is that we can eliminate political uncertainty to arrive at a policy decision if we can eliminate scientific uncertainty. Furthermore, because science is regarded as objective (i.e., value-free), by having an objective understanding of the problem, we can develop an objective (i.e., non-partisan) policy solution. This causes political debate to become debates about the science, which in turn incentivizes bad actors to systematically create doubt about scientific research that threatens their interests (by using the two attacks described above) which in turn can lead to stealth issue advocacy. However, as I argued in chapter three, the problem of stealth issue advocacy is even more complicated than Pielke acknowledges.

If the aim of science advice is *only* to provide policymakers with actionable information, then focusing only on epistemic considerations *is* effective. But it is only effective precisely because it obscures the moral, ethical, and social considerations involved. Yet, as the co-production lens offered by Jasanoff shows, science and society are not distinct, but rather, shaped by one another. Thus, by not being reflective on the impacts that science and technology have, and 'leaving these larger social results to chance,' we risk curating the factors under consideration by policymakers to only epistemic ones, while ignoring the social and ethical ones.[54]

We have seen in this section that the promise of value-neutrality leads to evidence being curated and to the issue of stealth issue advocacy. Aiming for value neutrality is therefore a pitfall that science advisors must avoid. However, we have also seen that doing so is not easy. The linear, technocratic view of science in policy, combined with the opposition of values and objectivity can act as incentives for hiding the value-judgments made by science advisors. Further, we have seen two ways in which this may play out in the policy context. By appealing to value-neutrality and technoscientific solutions to policy problems, science becomes a battleground for political debate, and the social impacts of such solutions are not adequately recognized. I turn now to my solution for the problem of value-neutrality and technocracy.

4.18 Science Advisory Panels: Solving the Value Neutrality Problem

As we have seen, being 'value neutral,' requires value judgments. One must decide where the ends of a value spectrum are, and where one draws the line to be neutral between those

[54] This is not to say that policymakers *do* only consider epistemic factors. Ample research shows that they do not (e.g., Cairney 2016). I am saying that when a science advisor emphasizes epistemic considerations, the policymaker has plausible deniability with respect to their responsibility for the social and ethical considerations, as they are merely "listening to the science."

extremes. Such advice is not independent, in the sense of 'free from values,' but rather dependent on the idiosyncratic values of the person(s) or institutions offering the advice. As such, there is no value neutrality and so, science advisors must consider these values and the subsequent social impacts of their recommendations. As with the Credibility Problem, the solution to this problem is two-fold. First, science advice should come from panels, rather than individual science advisors. Second, there must be people on these panels with the expertise to recognize how value judgments shape science and science advice.

Instead of merely providing policy advisors with actionable information, the role of science advisors ought to be to help identify the social impacts of science and technology policies. To that end, science advisory bodies ought to not merely include scientists, but science and society studies scholars as well. Philosophers, historians, and sociologists of science, for example, can provide information about how different policies may affect different groups. This is the type of information required if science advisors are to be genuine "Honest Brokers." Longino notes the importance of epistemic communities in ensuring that a claim is justified. If a community is suitably diverse, implicit assumptions and biases are more likely to be recognized and appraised. The same goes for providing advice. Advice from a single person (say a whistleblower) may carry idiosyncratic values or assumptions that shape the advice being given. Such values may enter an advisor's understanding of the problem, considerations of relevant kinds of data, or appropriate solutions. We saw this worry at play in the previous chapter, where the science advisor who offers advice on divorce policy provides a dubious analysis of the situation or above in the risk assessment and GMO examples where underlying social problems are ignored.

To avoid these issues, advisory institutions should not be embodied in individuals but rather in advisory committees. Advisory bodies should include members who represent a diversity of perspectives and expertise. Rather than having a chief science advisor, governments should have a council for science advice, or an office of science policy[55], whereby individuals representing a number of expertise including, but not limited to science, advise the policymakers. Non-scientific experts who may be on the council include historians, philosophers, and sociologists of science, that is, those with interactional expertise, who understand the permeable boundaries between science and the rest of society and who can provide advice on the social impacts of the adoption of some technology or policy. Interactional experts, who I briefly mentioned in chapter one, are experts who can "speak the language" of what Henry Collins and Robert Evans call "contributory experts," were contributory experts are those who contribute knowledge to the field in question (Collins and Evans, 2002; Plaisance and Kennedy 2014, 61). Interactional experts, then, are those who although they may or may not also contribute to the field, understand the field well enough to interact interestingly and fruitfully with contributory experts (Plaisance and Kennedy 2014, 65).

[55] Or some similar kind of institution. The important part is that science advice is arrived at through some kind of community deliberation.

Ideally, all these non-scientific experts should also have *specific expertise* relevant to the question at hand. However, as long as there are scientists and/or social scientists with the *relevant expertise* on the panel, this is not absolutely essential. These non-scientific experts may instead contribute broader expertise toward understanding the relationship between science and society. Part of the promise of a panel is the diversity of knowledge, experience, expertise, and skillsets that are brought to the table, so too much uniformity in the panelists would be counterproductive to the goals of having science advisory committees instead of chief science advisors. In their response to the work on interactional expertise done by Collins and Evans, Plaisance and Kennedy note that one of the benefits of using the interactional expertise concept is that it allows us to identify individuals and groups who can converse fluently with contributory experts, but who also hold a different set of assumptions and viewpoints than the contributory experts (2014, 63). For this reason, interactional experts make excellent additions to the kinds of science advisory panels I have in mind. They help diversify the knowledge and expertise that can be brought to bear on a policy problem.

A policymaker may ask, "When do I know that I have the right group of people?" The answer is when we have a group of people with different values and perspectives. Above, I've argued that these panels should not only have a diversity of expertise, but also of knowledge and experiences. To achieve this, these panels would likely need to have people with diverse identities and representation from stakeholder groups, including members of lay publics. The reason for including lay publics is that, as Elliott writes, "community members have unique experiences, [so] their values sometimes differ from scientific experts, and those values lead them to collect different sorts of information or to ask different questions" (2017, 144).[56]

However, there may be cultural challenges in including lay publics on advisory panels. This problem is also discussed by Kyle Powys Whyte and Robert Crease in their 2010 article, "Trust, Expertise, and the Philosophy of Science." Whyte and Crease discuss what they call "unrecognized contributor cases." These cases occur when certain people "with scientifically relevant knowledge, experiences, and perspectives are excluded as potential contributors to scientific activities" (Whyte and Crease 2010, 415). They go on to write that these contributors often go unrecognized "because scientists and other actors in the controversy hold a narrow conception of scientific expertise" (ibid.).

One problem that results from this, according to Whyte and Crease, is that since the scientists do not recognize lay person's ability to contribute, lay persons may see this as a reason to distrust scientists. Plaisance and Kennedy (2014) argue that interactional experts can help ameliorate this 'rational distrust,' that, in addition to Whyte and Crease, has been identified by feminist philosophers such as Heidi Grasswick (2010) and Naomi Scheman (2011). Interactional experts can do this, argue Plaisance and Kennedy, by identifying and meaningfully including new groups in scientific and decision-making processes, especially

[56] Put differently, we can draw again on the Dewey quote from chapter one, "The man who wears the shoe knows best that it pinches and where it pinches, even if the expert shoemaker is the best judge of how the trouble is remedied" (Dewey 2016, 224). Lay publics are able to tell policymakers and experts "where it pinches."

by reducing the exclusion of historically marginalized groups, as well as by mediating between scientific communities and important stakeholders (2010, 63).

This problem is also discussed by STS scholar Shobita Parthasarathy. Activists and lay publics face what she calls, the "expertise barrier." In her 2010 article, "Breaking the expertise barrier: understanding activist strategies in science and technology policy domains" Parthasarathy defines the expertise barrier as "the formal and informal rules of a science and technology policy-making domain which make it difficult for those without technical expertise to engage as equals" (Parthasarathy 2010, 355). She argues that lay public activists face this barrier because "insiders often argue that the average person operates at a knowledge 'deficit' and cannot properly comprehend the complex issues under discussion" (Parthasarathy 2010, 356 – 357). However, this "insider" reasoning is problematic, as lay persons bring a different set of expertise to the table. These are the people whose lives will be directly impacted by the policy or advice in question. They can bring a more practical, in situ understanding to bear on the problem. Furthermore, if interactional experts are included on advisory panels, they can, as Plaisance and Kennedy argue, help to mediate between the contributory experts and the members of the lay public, at the very least with respect to the technical expertise required.

One example of how lay publics can provide an in situ understanding of a problem comes a pacific northwest community referred to as "Oceanside."[57] Residents of Oceanside worked with scientists to develop a plan to manage the Henderson Creek watershed, a river that runs

[57] The authors of the study the example comes from used pseudonyms for people and places.

through the community. In their 2002 article, "Scientific Literacy as Collective Praxis," Wolf-Michael Roth and Stuart Lee found that when experts spoke to lay audiences, control over the salient issues in the conversation changed and what was relevant at that moment and how the information being relayed, became related to the world, and emerged from the interactions between the presenter and the audience (40). For example, at a community meeting, a water technician who was relatively new to the community provided a graph on how the quantity of water in the creek changed throughout the year. During the technician's presentation, a long-time resident of the community asked questions and helped to determine the salient features of the conversation using his knowledge of the farming practices in the community and his historical knowledge of changes in the watershed.[58] In this way, Roth and Lee argue, the resident was able to shift the conversation toward information and topics salient to the community (2002, 40). In the case of science advisory panels, lay experts can similarly bring a set of expertise and the values relevant to their community to the deliberations and so, should be included on advisory panels. Having members from lay public stakeholder groups helps increase the diversity of expertise, knowledge, and values on the panel (Plaisance and Kennedy 2014, 63). Consequently, it contributes to the overall diversity of the panel and helps ensure that policymakers receive robust science advice.

This case is strengthened by the work of Sheila Jasanoff. Recall that Jasanoff argues that stakeholders ought to be included at various stages of the science-policy process. Jasanoff's

[58] This example might count as an instance where the unrecognized contributor case discussed by Whyte and Crease (2010) could have occurred but did not.

work shows that policies are effective when interested parties are included from the early stages of the process. This ensures that their understanding of the situation helps shape the way the problem is understood by policymakers and scientists, and consequently, the way questions get asked or framed, the methodologies used, and the evidence that gets collected, and ultimately results in more useful information.

In some respects, my proposal here is similar to that of Harry Collins and Robert Evans in their 2017 book *Why Democracies Need Science*. Collins and Evans argue for science advice to come from social scientists of science, since these researchers understand the relationship between science and society (2017, 76 – 78). Natural scientists, on the other hand, need to be and ought to be, focused on finding the truth. Social constructivism, which Collins and Evans describe as the received view (for the last several decades at least) of social scientists of science, is incompatible with the ability to do natural science (2017, 76). The reason for this, they argue, is that scientists must believe that they are seeking truth and that there is a chance of finding it (ibid.). Yet because social scientists of science are able to understand the production of scientific knowledge, and, importantly for Collins and Evans, scientific consensus, they should be the ones providing scientific advice to policymakers. They call this hypothetical group of social science advisors, the Owls.

The role of these social scientific advisors is to provide policymakers with advice relating to the *practical* scientific consensus on a matter (Collins and Evans 2017, 85). Collins and Evans acknowledge that metaphysical *Truth*, if it is possible to discover, requires work beyond the policy timeline (2017, 88). As such, *total* consensus may not emerge in the available timeframe. Scientific “mavericks” may continue to hold out as most scientists reach some consensus on a topic, but the role of the Owls is to understand the nature of scientific controversy, and, to provide advice to policymakers about which ideas are accepted by mainstream science for the purposes of policymaking. Further, these advisors can also highlight for policymakers where there is consensus among those with opposing viewpoints. Importantly, Collins and Evans note that by asking what consensus is, we are not asking a technical question about the nature of the world, but a social scientific question about the substance and strength of the consensus (2017, 85). Finally, since social scientists of science are interactional experts, rather than practicing scientists of the field being advised on, Collins and Evans believe that they will be able to provide the most disinterested advice to policymakers (ibid.).

Related to this last point is that by having the court of Owls made up of social scientists of science, the advice from this institution may seem more trustworthy to policymakers. This is because they have no “skin in the game,” so to speak, on the subject under debate. As such, these Owls can comment on matters of controversy (e.g., vaccine safety or anthropogenic climate change) and speak authoritatively to what the scientific consensus is.

Another idea to emerge from Collins and Evans’ proposal is that the Owls will provide grades for the strength of the consensus (2017, 90). For example, ideas that have gain acceptance in large, mainstream, peer reviewed scientific journals, and which have gained complete or almost complete acceptance, would achieve the highest rank, “A.” Ideas that appear in fringe journals might receive a “C,” and ideas that appear on amateur blogs or on conspiracy theory websites would be graded “E” (ibid.). These grades would be made public, and the work done in arriving at these grades would be transparent, and politicians can decide whether to base decisions off the Owls’ recommendations or not (ibid.). Collins and Evans argue that a policymaker who decides to ignore an A grade claim when constructing a policy will have a much more difficult time explaining their decision to do so than when they decide to ignore an E grade claim when constructing a policy (ibid.).[59] In this way, the Owls can help policymakers identify the best available evidence.

However, my vision for an advisory body differs from that of Collins and Evans in several important ways, mostly pertaining to the value and role of social scientists of science as advisors. Namely, I think that social scientists of science, as well as humanities scholars who study science, can help identify how latent biases or values.

The role I envision for social scientists and humanities scholars on this body is more than merely providing expert advice on the *practical* scientific consensus to policymakers. Such a role undervalues the contributions that social scientists and humanities scholars of science can make. A significant part of STS research is understanding how science and technology shape, and are shaped, by society. A sociologist of science may, for example, study how agricultural policies and technologies have shaped the development of agricultural

[59] I believe this grading policy functionally resembles my pluralist account of objectivity. Both seem to be aimed at establishing the credibility of certain claims. I think that this grading could be one function of the advisory body that I have described in my view.

practices. Such knowledge may be useful in identifying potential gaps and areas for innovation in policymakers thinking about the problem.

Alternatively, those in the humanities, like philosophers, may help scientists understand the ways that values shape their work. There have been numerous examples of this throughout my dissertation. Recall from earlier in this chapter how Nancy Tuana has worked with data scientists to show how value considerations affect cost benefit analyses with respect to the Meridonal Ocean Current.

So, science and technology studies scholars, whether they are in the social sciences or humanities, have more skills and knowledge to offer than simply explaining scientific controversies and reporting on scientific consensuses, important though those roles are. These experts from outside the realm of the natural sciences can provide policymakers with valuable insight on the probable social impacts of a policy or to warn the policymaker when some field of research provides incomplete, technical solutions to complex problems with both social and technical dimensions. These scholars can also help identify the ways in which values have shaped some research program and then articulate these considerations to policymakers.

Another way I differ from Collins and Evans is that I would include natural scientists on this advisory body. One reason for this is that as Jasanoff has argued above, the credibility of advisory bodies is more likely to be recognized when various stakeholders are included. Natural scientists may not recognize the legitimacy of an advisory body that includes only social scientists and humanities scholars. Natural scientists and lay publics might wonder, “what makes these experts qualified to advice on matters relating to the natural sciences?” Including natural scientists addresses this worry. Furthermore, any advisory body is likely to require calling on the expertise of outsiders, as no advisory body will be so comprehensive as to cover all disciplines. Thus, including natural scientists also broadens the network through which the science advisory body I argue for can gather knowledge to inform its advice.

One might wonder here whether the inclusion of natural scientists undermines the perceived impartiality of the advisory body that was highlighted by Collins and Evans. I do not believe that it does. The presence of social scientists and humanities scholars provides a balance against the self-interest that may be perceived from a panel of entirely natural scientists.

4.19 Conclusion

In this chapter, I articulated my social-pluralistic view of science advising. My view is useful because it allows us to respond to the Credibility Problem and the Value-Neutrality Problems. My view instructs that when faced with these problems, we consider two things: which dimensions of objectivity are relevant to assessing the trustworthiness of a claim, and what is the advice of a robustly interdisciplinary and diverse expert panel? Answering these two questions allows scientists, advocates, policymakers, and citizens to have more productive discussions about what the science says and what policy options to pursue.

Furthermore, my view addresses the primary shortcoming of the science and technology studies science policy literature discussed in chapter one, namely, that values threaten the objectivity of science and should be kept firmly in check. It resolves this problem by giving values a more robust role to play in science advice. One role for values is supporting the production of objective knowledge and by having us reflect on what our policy goals are and the ethical dimensions of research and policy. Instead, I have followed the lead of Douglas (2009) and Elliott (2017), among others, to show, as they have, how values are not oppositional to objectivity and in fact beneficial to scientific inquiry, in turn growing this into an account of how values can be beneficial to science advising or detrimental to science advising if their role is not recognized.

Additionally, my social-pluralistic view offers a solution to the problem of excess objectivity that I discussed in my first chapter. Recall that the problem is that there are many analytical lenses through which to understand nature and each of these can be used to support a number of subjective positions. By offering a pluralistic account of objectivity, we can better identify which of these subjective positions is more credible. My view raises the standard for what counts as objective, credible knowledge.

Finally, my view builds on the existing views and helps to fill in areas where there are gaps. For example, like the pro-advocacy view, my view recognizes that significant information ought to be brought to the attention of policymakers. However, by investing the responsibility for advice in an independent body of experts, rather than in a specific individual, it recognizes that words like “significant” and “best” are value-laden terms whose meaning require a certain amount of negotiation. Secondly, like Jasanoff and Pielke’s views, my view recognizes that technical considerations can smuggle in social and ethical value judgments. However, my view ensures there is an appropriate challenge function to those judgments by having advice come from the inclusion of non-technical experts on advisory panels.

In the next chapter, I show how my view can be applicable to the real world. I do this by examining two case studies for proof of concept, and by showing how my proposed solutions, diverse science advisory panels with pluralistic views of objectivity, would respond to each case. In this way, we can see what responsible science advising could look like in practice.

Chapter 5
An Application of the Social-Pluralistic View of Science Advising

If one wishes to realize the distance which may lie between "facts" and the meaning of facts, let one go to the field of social discussion. Many persons seem to suppose that facts carry their meaning along with themselves on their face. Accumulate enough of them, and their interpretation stares out at you. The development of physical science is thought to confirm the idea. But the power of physical facts to coerce belief does not reside in the bare phenomena. It proceeds from method, from the technique of research and calculation. No one is ever forced by just the collection of facts to accept a particular theory of their meaning, so long as one retains intact some other doctrine by which he can marshal them. Only when the facts are allowed free play for the suggestion of new points of view is any significant conversion of conviction as to meaning possible. Take away from physical science its laboratory apparatus and its mathematical technique, and the human imagination might run wild in its theories of interpretation even if we suppose the brute facts to remain the same.

-John Dewey, *The Public and its Problems*

5.1 Introduction

The Dewey quote which opens this chapter reflects a common attitude toward science, particularly toward science for policy. Facts are thought to speak for themselves, and more importantly in the policy context, facts are often thought to be determinative of policy solutions. It is this attitude which lies at the heart of many science-policy controversies, and indeed, which lies at the heart of the linear view of science for policy. As should be clear by now, the linear view, the view that we simply need to "get the science right" to develop policies that benefit all persons, is an unsatisfactory way of accounting for the role that science does and should play in informing policy. Dewey astutely observes that facts do not "speak for themselves," rather, they must always be interpreted. As we have thus far seen, values can shape these interpretations in a variety of ways.

The aim of this chapter is to demonstrate the potential usefulness of my view. To that end, the key consideration for this chapter is how my solutions of pluralistic objectivity and interdisciplinary science panels track real world cases and respond to the Credibility Problem and the Value Neutrality Problem. I will begin this chapter by providing a summary of the discussion from the previous chapter. I will then turn to two case studies which I believe provide good examples of the kinds of problems described in chapter four.

5.2 Summarizing Chapter Four and Looking Ahead

In chapter four, I identified two problems that responsible science advice should avoid, as well as two solutions to these problems. These are the *Credibility Problem* and *the Value Neutrality Problem*. The Credibility Problem occurs when one party claims that their science is more credible than competing science, when it is in fact not, or when one party claims that another party is peddling dubious science, when they in fact are not. On the other hand, the Value Neutrality Problem occurs when science claims to be neutral with respect to social and ethical values, but social and ethical values are smuggled in under the guise of value neutrality.

To respond to the Credibility Problem, we should adopt a pluralistic conception of objectivity. By doing so, we will be able to evaluate the credibility of a particular claim along a variety of dimensions. Briefly, these dimensions are manipulable objectivity, convergent objectivity, procedural objectivity, concordant objectivity, and intersubjective objectivity.

Under my view, a failure along one dimension of objectivity is not grounds for immediately dismissing a claim as untrustworthy. Instead, my view adopts a more holistic approach and examines how a claim fares along multiple dimensions to triangulate how much credibility we should ascribe to the claim. Furthermore, we should pay particular attention to claims which have a high degree of intersubjective objectivity, as such claims will have undergone rigorous critique and are likely to represent the community’s position.

To address the Problem of Value Neutrality, science advice should come from science advisory committees, rather than individual science advisors. Furthermore, this advisory committee should include science, technology, and society scholars with an understanding of the relationship between science and values. This could include philosophers who work on this topic, such as Douglas, Tuana, or Elliott, but it could also include those, like Kelly Bronson, and other sociologists of science who have a good knowledge of the role that values play in issues of science policy controversy. Such scholars have training in understanding the social impacts of science and can help articulate the possible value judgments that could be informing a policy or technology adoption, which will enrich the advice provided to policymakers.

The two case studies I discuss below highlight some of the dynamics I have tried to capture in formulating these problems. The first case study I discuss relates to how corporate interests tried to undermine science on the harmful effects of environmental tobacco smoke (i.e., second-hand smoke). The second case study examines how debate and discussion regarding genetically modified canola was technocratized by framing the discussion in "value neutral," scientific and technical terms, rather than discussing which social and ethical values should be shaping policy.

In the first case study, I draw from Naomi Oreskes and Eric M. Conway's book *Merchants of Doubt* to show how the tobacco industry and their allies try to weaponize objectivity against the Environmental Protection Agency in the United States to undermine the credibility of the agency. In this case study, *Credibility Problems: The Controversy of Second-hand Smoke*, the U.S. Environmental Protection Agency (EPA) is accused of curating evidence to fit its "regulatory agenda." The key issue in this study is that values and objectivity are weaponized to delegitimize the research of the EPA.

In the second case study, I draw upon the work of sociologist Kelly Bronson to show how the Government of Canada adopted a pro-biotech approach to agriculture, despite opposition from farmers. In this case study, *Technocratizing Agriculture: Value Neutrality and Biotechnologies*, farmers in Saskatchewan take issue with the technocratization of farming through the adoption of principle of substantial equivalence to assess the risk of genetically modified organisms. The key issue in this study is that the government tried to frame GMO canola as a value neutral policy solution to the problems of modern agriculture. They did so by emphasizing the scientific nature of the risk assessments they had conducted and portraying critical members of the public as scientifically illiterate and "anti-science," rather than facilitating and participating in national discussions about the role of biotechnologies in the national economy and the appropriate way of conducting risk assessment. This opposition was rooted in social values about the role that farming plays in local communities and concerns about the social values shaping commercial biotechnologies and agriculture. However, because of the dominant model of agricultural production favouring maximal economic output, and the successful framing of anti-GMO farmers as ignorant and "anti-science," the government saw the problems associated with the dominant model as merely technological problems requiring technological fixes.

5.3 Credibility Problems: The Controversy of Second-Hand Smoke

In the introduction to my dissertation, I used a quote from an anonymous tobacco company executive to illustrate the active disinformation campaign that entrenched economic interests have waged against sound regulatory science. The quote, "Doubt is our product since it is the best means of competing with the 'body of fact' that exists in the mind of the general public," illustrates how tobacco companies sought to weaponize values and objectivity to preserve their economic interests, even after the industry's own research showed that smoking tobacco, and inhaling tobacco smoke second hand, caused serious health problems. The industry continued to call for "sound science" before any regulatory action be pursued (Oreskes and Conway 2010, 143), despite there being sound science on the matter.[60]

The controversy reached its pinnacle in 1998 when the U.S. Department of Justice in the sued prominent members of tobacco industry, including the country's largest manufacturers, for violations of the Racketeer Influenced and Corrupt Organizations (RICO) Act. Among other things, the violations related to the hazards of smoking and the suppression of this

[60] In developing this case study, I draw primarily on Naomi Oreskes and Eric M. Conway's recounting, as discussed in *Merchants of Doubt* (2010). When appropriate, I draw from primary material in the form of the legal decision.

information. In 2006, a federal court delivered a guilty verdict, and, as remedy, required that tobacco companies offer “corrective statements” to claims they have previously made, to clearly communicate the dangers of tobacco smoking.

While this case was before the court, a related legal battle was reaching its peak. In 1993, the Environmental Protection Agency (EPA) published a risk assessment report on the harmful effects of environmental tobacco smoke (ETS). This prompted members of the tobacco industry to file a lawsuit against the EPA. The lawsuit was successful. The court found that the EPA did not meet the statutory requirements it was obligated to under the *Radon Act* (US District Court, Middle District of North Carolina). Specifically, the act required that the EPA have an advisory panel that included representatives of industry. However, due to the cost associated with conducting the research and developing the assessment, the court ruled that this violation was only significant enough to warrant a legal remedy if industry consultation would have impacted the conclusions of the report, or, the conduct involved in the production of the report (US District Court, Middle District of North Carolina). Otherwise, it would amount to a waste of taxpayer money. The remedy that the tobacco industry sought was to have the report quashed.

The court found that the EPA did not provide suitable documentation to justify certain assumptions it made in the report (US District Court, Middle District of North Carolina). These assumptions informed the way that the EPA conducted the research, and consequently impacted the conclusions. The tobacco industry convinced the court that had there been industry representation, it would have questioned the assumptions made by the EPA, and so, the way the research was conducted would have been substantially changed. Thus, the report was rendered null and void. In 2002, the EPA successfully appealed this decision, arguing that its report did not constitute regulatory action and was therefore outside of judicial review. This resulted in the assessment being reinstated.

Following the tobacco companies' successful lawsuit, the Fraser Institute, a Canadian thinktank published a book titled, *Passive Smoke: The EPA's Betrayal of Science and Policy.* In the book, the authors use slanted language to position the controversy of the EPA's ETS assessment as symptomatic of an over-zealous, pro-regulatory attitude sweeping across North America, using "junk science" to justify governmental control (Gori and Luik 1999, VII). Furthermore, the authors claim that in some instances, such as the ETS assessment, this "junk science" is being produced by regulatory institutions themselves (Gori and Luik 1999, XIII).

The Fraser Institute's book weaponizes objectivity by arguing that research organizations like the EPA have a pro-regulatory bias, and therefore, are incapable of being impartial arbiters of the truth. Furthermore, they weaponize objectivity in a variety of ways. In one instance, the authors seem to weaponize both procedural objectivity and detached objectivity. The authors weaponized procedural objectivity in statements such as, "The EPA's science simply does not exist. As the Court found, the EPA never asked whether ETS is or is not a risk for lung cancer, but began with the unsupported affirmation that it is, and then set out to bolster its claim by whatever means" (Gori and Luik 1999, XIII). They go on to add "It cannot be said that the EPA acted in good faith and was merely technically incompetent or careless to the point of dereliction of duty, for its interactions with the Court show a conceited attempt at deception" (Gori and Luik 1999, XIII-XIV). All this follows a passage, a few pages earlier, where the authors weaponize detached objectivity, writing, "Increasingly, regulation of chemicals is being governed by political responses to public fear and hysteria rather than careful, objective scientific evaluations of the actual risks and benefits posed by environmental hazards" (Gori and Luik 1999, IX).

The claim being made by the Fraser Institute's authors is that the procedural norms of the scientific method were violated and that the EPA's researchers asked a biased question. Instead of beginning with an impartial hypothesis, per an idealized model of scientific inquiry, the authors allege that the EPA asked a biased question and subsequently cherry-picked the data to support it. However, as Oreskes and Conway discuss the case, it is not clear that any violations of procedural objectivity with respect to scientific inquiry occurred. The EPA failed to follow the appropriate legal processes (at least according to the initial ruling), but this does not mean that they failed to follow the appropriate scientific procedures. Furthermore, even if the Fraser Institute's account of the situation is correct (that is, correct insofar as the EPA began with the assumption that environmental tobacco smoke is carcinogenic), this still would not clearly be a transgression of objectivity. Tobacco smoke in smokers had, by this time, been identified as carcinogenic (Oreskes and Conway 2010, 137; EPA 1993, 1-2).[61] So, on that fact alone there are reasonable grounds for the EPA to *hypothesize* that ETS would be harmful as well. What this research shows is a convergence on the link between ETS and cancer.

[61] The carcinogenicity in smokers is also acknowledged in the Fraser Institute's book.

Furthermore, as we saw in chapter four, detached objectivity is not a useful standard by which to assess the credibility of a claim. This is because its requirement that individual researchers be "suitably detached" from their research may not be achievable, and if it is achievable, it requires a community of inquirers to assess. Therefore, we ought to use standards of interactive (i.e., contextual empiricist) objectivity instead. On this measure it is harder to evaluate the EPA's report, particularly in retrospect given the now widespread acceptance of the harms of ETS. Another difficulty is that the report does not list the expertise of all the authors and contributors, and given its publication approximately thirty years ago, such information is not readily available. However, those who contributed to or reviewed the report whose expertise I was able to confirm through an internet search, came from a variety of fields including epidemiology, respiratory science, environmental medicine, environmental science and engineering, medicine, cancer research, and architecture. While these, and other fields listed (e.g., "environmental health") may seem closely related, we can organize them into at least three groups, each reflective of a different dimension of the problem. In the first group are the medicine and cancer researchers, who bring an understanding of how exposure to tobacco smoke affects the body. In another group we have environmental scientists and epidemiologists who understand how disease moves through and affects communities. In the third group we have the engineers and architects who may provide an understanding of things like ventilation and airflow in enclosed environments. Additionally, according to the EPA at least, comments from science advisory board and public consultations were included in the revisions to the report for publication. This suggests that there was ample opportunity for criticism of the assumptions that the EPA had made in constructing the report.

In summary, there is no evidence that the EPA failed to follow the appropriate procedures. A hypothetical science advisor, if asked to provide advice on the health effects of tobacco smoke, could make the following claims about the credibility of the EPA. Contra the claims from the Fraser Institute, the EPA did not curate evidence to fit a pro-regulatory agenda. Rather, they appear to have followed legitimate scientific procedures. The science advisor could go on to say that while there were value judgments that were made, these judgments were sound and drew on existing correlations between tobacco smoke and carcinogenicity, and so within the realm of acceptability. The science advisor could also add that the results of the EPA study corroborate the findings of previous research, demonstrating convergent objectivity and that the report received critique from external members of the scientific community who had expertise relevant to evaluating the science, assumptions, and value judgments that went into constructing the report. Finally, with respect to the work of the Fraser Institute, our science advisor could note that the rhetoric of the Fraser Institute's book demonstrates characteristics of the Credibility Problems discussed above (i.e., language that suggests the EPA was biased) and note that this book appears to be aimed at deflating the credibility of legitimate research.

Situations where there are competing scientific claims are exceedingly common. Yet this case study allows us to see both what the Credibility Problem looks like in practice, and, how the pluralist view of objectivity can be used to assess the credibility of competing claims in such cases. While there may be cases where such a determination is more difficult, this case study is useful in showing what my view looks like when put into practice.

5.4 Technocratizing Agriculture: Value Neutrality and Biotechnologies

I turn now to my next case study, in which I discuss the history of biotechnology in Canada, and in particular, the technology of genetically modified canola. Drawing from her article, “Excluding ‘Anti-Biotech’ Activists from Canadian Agri-Food Policy Making: Ethical Implications of the Deficit Model of Science Communication,” I use sociologist Kelly Bronson’s research into the use of genetically modified canola by farmers in Saskatchewan to show how decision making gets technocratized under the guise of value-neutral science. This case study is an instance of the poisoned-well cases described by Whyte and Crease (2010) in the introduction. Recall, these cases occur when lay publics distrust science and no amount of technical argumentation will prevail (Whyte and Crease 2010, 418). This is because different stakeholders have such entrenched social positions that there is no room for technical expertise. Through her research, Bronson demonstrates how farmers who were resistant to the use of genetically modified canola were framed as “anti-science activists,” while the government continued to maintain that its decisions were grounded in “sound science” (2018, 235). This allowed the federal government to position itself as simply “following the science” when it came to regulatory policy on genetically modified (GM) seeds and crops, without articulating the broader set of values and considerations that led to the adoption of the policy.

In 1996, Monsanto's "Roundup Ready Canola" seeds began to be available in Canada. The seeds had been genetically modified to work with specific chemical herbicides (Bronson 2018, 236). This meant that if the farmers used the correct combination of seeds and herbicides, the crops would be able to withstand the chemicals while the surrounding plants died. This practice is common in "productivist" methods of farming, which are "technologically intensive and aim at high-yield production of commodity crops destined for export markets." (Bronson 2018, 242).

But the farmers interviewed by Bronson rejected the productivist model of agriculture, often preferring an organic approach which rejects the use of chemical herbicides. This organic approach envisions "healthy farming as farming that works with and for others," and which extends "beyond microscopic features of the land to include interpersonal relationships." (Bronson 2018, 241). Bronson writes that oftentimes, these farmers "also see themselves as having an intergenerational responsibility to care for the land and as promoting a decentralized food system that is responsible to local ways of life and sustaining environmental health" (Bronson 2018, 242). The farmers who were the subject of Bronson's research rejected biotechnologies such as genetically modified canola because they see these technologies as remedies to "problems that might be more correctly recognized as inherent instabilities stemming from the technologization of nature under the productivist system" (Bronson 2018, 242).

Bronson argues that the government used the Deficit Model of Public Understanding of Science, which framed these holdouts as ignorant, anti-scientific "activists," and that the government used this framing to exclude these holdouts from public discussions about the risks posed by these biotechnologies (Bronson 2018, 226). This argument is supported by other sources, including Eric Montpetit and Christian Rouillard, who, in their paper "Culture and the Democratization of Risk Management: The Widening Biotechnology Gap Between Canada and France," write that one civil servant was incredulous at the idea of citizens participating in the risk evaluation process, insisting that only scientists can evaluate the risks associated with biotechnologies (Montpetit and Rouillard 2008, 921). This kind of attitude suggests a preference for technical decision-making, rather than deliberative or discursive processes. At the same time, the Government of Canada positioned itself as making decisions based on "sound science," pointing to the risk assessments it had completed on various GMO crops, and in particular, Roundup Ready Canola. Bronson writes that the farmers she interviewed were concerned with the fact that there was no process for public deliberation on how risk was defined in the regulatory context, and that this obscured the fact that the decisions were being made by people in a particular social and political context, namely, long-standing federal investment in biotechnology (Bronson 2018, 248).

In fact, government documents dating to the 1980s are explicit about the economic importance of investing in biotechnology. In 1980, the Ministry of State for Science and Technology published a background paper on biotechnology in Canada. Among other things, the report highlights the important role that biotechnologies could play in industrial development and increases in efficiency for the exploitation of natural resources (Ministry of State for Science and Technology 1980, 6). Furthermore, after the release of the background paper, the Minister of State for Science and Technology, John Roberts, established a Task Force on Biotechnology. This task force was primarily responsible for advising the Minister on policies and programs designed to allow Canada to take advantage of the opportunities offered by biotechnology (Brossard et al. 1981, iv). Further the report from the Task Force claims that "[i]f Canada's resource industries fail to innovate through biotechnology, their competitiveness in world markets will be jeopardized" (Brossard et al. 1981, 3).

But how does the government define risk with respect to genetically modified crops, and does this way of defining risk favour economic objectives, as the farmers in Bronson's case study argue? To be introduced to the market, "plants with novel traits," a category that includes species derived through selective crossbreeding as well as through genetic engineering, must undergo a risk assessment by the Canadian Food Inspection Agency. This risk assessment looks at five criteria:

1. the plant's potential to become a weed or to invade natural habitats;
2. the potential consequences of gene flow to related species;
3. the potential for the plant to becomes a pest plant;
4. the potential impact on non-target organisms, including humans; and,
5. the potential impact on biodiversity (Canada Food Inspection Agency 2018).

However, these criteria are evaluated using "the principle of substantial equivalence," meaning that all plants with novel traits are evaluated in comparison to their non-novel counterparts. Thus, in effect, a plant must be found to have "no more risk" than its non-novel counterpart, if it is to pass the assessment (Eaton 2013, 36).

Yet the principle of substantial equivalence is not the only approach one could take to risk assessment and is in fact at odds with the approaches taken by other jurisdictions. For example, in Europe, countries adopt the Precautionary Principle as the guiding principle for risk assessment of GMO foods. As discussed in the previous chapter, the goal of the Precautionary Principle is to prevent harm, especially in the face of uncertainty. Furthermore, the principle of substantial equivalence is at odds with independent advice in Canada. The Royal Society of Canada argues the government should adopt the Precautionary Principle rather than the principle of substantial equivalence in their 2001 report: *Elements of Precaution: Recommendations for the Regulation of Food Biotechnology in Canada*. In this case, the Precautionary Principle, as articulated in the 2001 report, means assessing the "potential for causing harm to the environment or to human health" (Barret et al. 2001, x).[62] This is importantly different from the principle of substantial equivalence.

This "principle of substantial equivalence" framing for the risks associated with genetically modified crops supports the long-term investments in biotechnologies made by the Canadian government because it sets a higher threshold for harm than the Precautionary Principle. Under the principle of substantial equivalence, harmful effects to humans and the environment are tolerable, so long as they are no more harmful than the organic version of a plant. This means that if a non-novel plant has potential to cause harm but is not regulated, the novel plants should be treated the same. However, under the Precautionary Principle, if the evidence suggests that harmful effects on humans or the environment are likely, the

[62] And defined above as "the duty to prevent harm, especially in the face of uncertainty."

product will not receive regulatory approval. Consequently, under the Precautionary Principle, plants with novel traits would be assessed on their potential to cause harm independent of whether its non-novel counterpart is harmful. Although both approaches may be legitimate, the Precautionary Principle adopts higher safety standards for the public, while the substantial equivalence approach aims at efficiently bringing products to market.

This is an important distinction. Choosing the principle of substantial risk versus choosing the Precautionary Principle is a value judgment. These principles are thick evaluative concepts, meaning that while either one can tell us about the world, each carries an important ethical implication. On the one hand, we can choose something that efficiently brings products to market but risks a greater number of harmful products slipping through, and on the other hand, we can choose to prioritize harms to individuals, communities, or the environment at the cost of economic efficiency. By choosing the principle of substantial equivalency, the government chose the former. This allowed the government to curate the kinds of evidence that were produced and used in discussions about the safety of genetically modified crops. This, in turn, allowed the government to cloak its claims in technical precision to add to their credibility and legitimacy.

However, as Bronson shows, the farmers in her case study were resistant to biotechnologies due to the values embedded in commercial biotechnological science and questioning the values shaping how risk is framed is a reasonable critique of the risk assessment process. But, as Bronson constructs the case, there was no space in public consultations for this discussion to occur. Instead, the government framed these holdout farmers as anti-science while using the objectivity of science, and in particular risk assessments, to obscure the economic value judgments being made by regulators.

Using Longino's analysis of how contextual values can shape global, framework-like assumptions, we can see how under the productivist model of farming, economic values position biotechnology as a way of helping maximize economic output and it is against this backdrop of economic values that biotechnology itself appears value neutral. These contextual values mean that maximizing economic output is a given and this leaves little room for critiquing this approach to agriculture. Furthermore, these framework-like assumptions mean that risk assessments play an important role since, when guided by the principle of substantial equivalence, it creates a context which validates the decision made by the government. This, in turn, allows policymakers to treat the "inherent instabilities" in the biotechnological framework as merely techno-scientific problems that can be managed through risk assessment and further technological innovation, rather than as problems grounded in social values. In the above case, the farmers, in addition to having reservations about productivist farming, also took issue with the values that were involved in shaping the risk assessment process. This process, shaped primarily by economic values, in turn shaped the discourse on whether and how genetically modified crops should be adopted. Consequently, the government was able to continue to invest in biotechnologies while ignoring the concerns of farmers, since it could claim that the farmers were "anti-science," even though their concerns were not directed at the science itself, but rather that values that were shaping how risk was being assessed in this context.

If we imagine a science advisor providing advice to the government in this scenario, it is easy to envision that the advice would relate to which biotechnologies are most promising, rather than relating to the appropriateness of "substantial equivalence" as a frame for assessing risk.[63] In this context, the contextual values shape the global, framework-like assumptions that effectively curate the kinds of evidence and expertise that are relevant and available. For example, advice about whether to adopt biotechnologies would require the advisor to see the concerns of the organic farmers as legitimate. Yet under the circumstances described by Bronson, it is difficult to imagine that such concerns would even make it to the science advisor, given the framing of organic farmers as deficient of the required scientific knowledge. Furthermore, there is also a presumption here as to what kind of knowledge is required to have "legitimate" opinions about agricultural biotechnologies. We can see in this case how focusing on epistemic considerations (i.e., the technical problems associated with productivist farming) can obscure the relevant social and ethical values at play. Furthermore, this obstruction undermines democratic processes by excluding interested parties from deliberative processes.

However, as I argued in chapter four, we can at least partially alleviate these problems by adopting interdisciplinary science advisory panels. Work like Bronson's can help illuminate the differences in values that are at play in different matters of controversy. It is for this reason that I argued for the inclusion of social scientists and humanities scholars who study

[63] In fact, we have seen both kinds of advice. The former is precisely the kind of advice the Task Force on Biotechnology supplied, and this has shaped how the government has looked for and curated evidence relating to GMO crops for the last 40 years. The latter is the kind of advice the Royal Society of Canada provided (among other recommendations).

science on advisory bodies in the previous chapter. Such scholars can provide epistemic "counterpoints" that can challenge the prevailing assumptions and attitudes (Medina 2013; Fehr 2011; see also Fricker 2007) and offer the kind of coupled ethical-epistemic analyses argued for by Tuana. This, in turn, can help us reflect on the values that are playing important roles in discussions and decision-making. Furthermore, such work can help illuminate the likely broader societal impacts a policy may have, such as how different parts of the population may be affected. That is, it can help identify assumptions that are so fundamental we may not recognize that we are making them.

5.5 Conclusion

In this chapter I have demonstrated how the social-pluralistic view I developed in chapter four can help us make sense of two high-profile cases. I did this by examining two case studies which capture the dynamics of weaponized values and objectivity, as well as value neutrality and technocratization. In the first case study, we see how objectivity gets weaponized by pro-tobacco/anti-regulatory interest groups to undermine the credibility of otherwise trustworthy research. However, adopting a pluralistic conception of objectivity allows us to identify not only where the specific failure of objectivity is said to have occurred, but it also gives us the tools to make nuanced considerations about how credible a particular claim is. In the second case study, we saw how agricultural biotechnologies were framed as value neutral, technological solution, and related investments were framed as value neutral policy solutions, to the problems present in the productivist agricultural model.

Furthermore, work like Bronson's helps demonstrate the added value that the inclusion of social scientists and humanities scholars can have in the production of science advice.

My social-pluralistic view captures the dynamics in these two case studies. The problems of credibility and value neutrality that I have described are present in these case studies, while the solutions that I have proposed, diverse science advisory groups employing a pluralistic concept of objectivity, help provide valuable insight into determining the credibility of competing claims. They also can help identify the values that are shaping both scientific research and policy interventions. These case studies, therefore, demonstrate the feasibility of my ideas and suggest a fruitful program for future research.

Conclusion

> *It is plainly not enough to remove as much of present evil as lies in our power. We must look to producing new good things, better, more active and harmonious ways of living, individually and socially. So far science has hardly touched these fields.*
>
> J.D. Bernal "The Social Function of Science" 1938, *Modern Quarterly*

In my dissertation, I have sought to answer the question, "how does one responsibly advocate for, or advise on, science-based policies, in the context of value-laden science?" To that end, I have attempted to provide standards which we can use to assess the behaviour of science-based advocates and advisors. These standards are a pluralist conception of objectivity and diverse science advisory panels. To develop these, I examined the literature surrounding science advice, focusing on three influential views: the Pure Science view, Jasanoff's Boundary Actor view, and Pielke's Honest Broker view. In this analysis, I found that objectivity plays an important role in legitimating scientific advice. The reason for this, is that "objectivity" functions as a way of asserting that some information is credible, and therefore, trustworthy.

Since objectivity plays such a central role in these views for the role of science in policy, I considered three relevant views of objectivity from the philosophy of science literature. One idea that emerged from this analysis was that objectivity is socially constructed and is constructed in a variety of ways. This shows two things: first, that objectivity is not one thing, but a constellation of concepts all aimed at establishing standards for credibility, and second, that objectivity is itself a value. Consequently, values and objectivity cannot be wholly oppositional. In fact, objectivity and other values interact in a myriad of ways. In this chapter I also showed how Longino and Kourany's views complement each other and that values, like our theories, can be revised in response to new evidence or considerations. This is important because the values that we choose will shape which science is done, how it is done, and what we do with our discoveries. Therefore, it is important that we adopt the Critical Contextual Empiricist view of objectivity and treat values as equally important to objectivity and as something revisable if they are not doing what we need them to do, especially if our theories are not helping us achieve our desired social and ethical ends. Science, if used well, can be a powerful tool towards achieving those ends, as intimated by the Dewey quote in the epigraph of chapter five.

I then used these views on objectivity and values to critically examine the perspectives on the roles of science and scientists in policy making. I found that none of the views on science in policy provided sufficient acknowledgement of the different kinds of objectivity that exist, nor sufficiently appreciated the implications that value-laden science has on science-based policy. I noted that one highly influential view, offered by Roger Pielke Jr., had attempted to provide a solution that gets people to reflect on their values, but that his view did not sufficiently distinguish between epistemic and social or ethical values. This analysis allowed me to articulate two pitfalls the science advisors and advocates should avoid: the Credibility Problem and the Value Neutrality Problem.

In chapter four, I showed that in order to responsibly advocate for, or advise on, policies in the context of value-laden science, advocates and advisors must address the Credibility

Problem and the Value Neutrality Problem. Science advisors and advocates must avoid relying on a single standard of objectivity to assess whether a claim is credible, or, to make their own claims credible, while also addressing the ethical side of coupled ethical-epistemic issues. My suggestions for avoiding these threats to the advisor's credibility are having diverse advisory panels and using a pluralistic conception of objectivity.

Finally, I provided case studies to successfully demonstrate the utility of my view, from both the perspective of articulating the problems, as well as how my solutions would resolve them. However, these case studies are meant as a proof-of-concept, rather than definitively proving my view. I believe my social pluralistic view of science advising can be usefully applied to other cases as well. For example, I can see my view being useful and applicable to the fields of forestry and resource management or pandemic prevention and management, as these fields are often areas where there are a number of competing values impacting not only how evidence is interpreted, but how policy is developed based on this evidence. In forestry and resource management, economic values and conservation and other ecological values are often at the centre of policy discussions. In pandemic prevention and management, as the world has recently seen, economic and individualistic values often came into conflict with values relating to public health, safety, and collective wellbeing. My view would allow policymakers and citizens to better assess the credibility of information and reflect on which values should be shaping the policy.

Throughout my dissertation, I have noticed several threads that I think warrant further inquiry. Many of these have their roots in the curatorial nature of value judgments and the technocratization of policy under the guise of value neutrality.

One idea for further work relates to objectivity.[64] Specifically, I wish to explore how my pluralist conception of objectivity can be refined. In a footnote, I noted that others have identified as many as thirteen kinds of objectivity, whereas I only identify five. I think it would be an interesting project to see how these lists can be further reconciled. Research in this topic could more broadly consider the ways that we use the word "objectivity," mapping out how these uses overlap with each other so make our categories of objectivity more refined and precise. In my dissertation, I showed how two of the uses for "objectivity" identified by Douglas can be better understood using other categories of objectivity. Conceivably then, some of the other uses of "objectivity" that have been identified in the literature may fall under other categories of objectivity. Such refinement could allow for more nuanced and sophisticated discussions about why something is objective.

Another idea for further work, is to elucidate the relationship between thick evaluative concepts, coupled ethical-epistemic analyses, and coproduction. In my dissertation, I've treated them as all gesturing at the conclusion that science and society are not distinct, but I do not go into detail as to how they specifically relate to, and/or, reinforce each other. Working on this problem from the conceptual engineering view that I have adopted in my

[64] The ideas in this paragraph could either be interconnected or separate ideas. I've grouped them together because they all relate to objectivity.

dissertation could provide clarity toward understanding the boundaries of these concepts, similar to how I used conceptual engineering to redraw the boundaries between different kinds of objectivity in my dissertation. This work would continue the work in my dissertation toward bringing philosophy of science and science and technology studies into closer conversation and strengthen the presence of philosophy of science in the field of science and technology studies.

If the ideas of thick evaluative concepts, coupled ethical-epistemic analyses, and coproduction overlap significantly, a unified understanding with elements of all three could provide a useful tool for analysis in the science policy field for understanding tensions and controversies at the science-policy interface. Alternatively, if these ideas cannot be completely reconciled into a single idea, there may be an opportunity to network them to expand their reach in further research. For example, it may be the case that thick evaluative concepts and coupled ethical-epistemic analyses are ideas that can be grouped under the coproduction concept. However, because coproduction is itself draws from a number of approaches to studying science, technology, and society, there may be ways to link thick evaluative concepts and coupled ethical-epistemic analyses to these other concepts.

The project I am describing here is largely topographical. It aims at understanding the landscape around these ideas, the routes one may take to move between them, and other stops one may make along the way. Consequently, it is difficult to say where exactly this research would go and what interesting things may arise from it. However, each of these concepts is useful individually, so I believe it would be worth exploring how they could be useful cumulatively.

The penultimate thread I wish to explore is the ethics of panel construction. The crux of this problem is that when governments put together expert panels, they must strike a balance between competing values, such as panelists who are established and recognized members of the field, versus the need for a diversity of perspectives (including race, gender, sexuality, academic discipline, geography, and career stage) and opportunities for underrepresented groups. However, since institutions have historically benefited straight white men, these are often the established and recognized members of the field. This has interesting implications for the how to establish panels whose credibility is accepted. The work of Helen Longino (1990, 2002), Elizabeth Anderson (2012), Kristi Dotson (2012), and Naomi Scheman (2011) would be helpful in understanding and addressing this problem.

Relatedly, the final thread I am interested in exploring is the role of the "whistleblower" scientists discussed by Grasswick and how this relates to the credibility of such panels. Grasswick argues that whistleblower scientists may be seen as more credible than what we might call "establishment" scientists, and it would be interesting to examine such cases through the framework I have developed here. One could imagine a possible world where the acid rain panel discussed in chapter three pushes back against Singer, and Singer, feeling slighted, claims that the panel has already decided what their advice would be and has not duly considered the evidence. If Grasswick is correct, then Singer would likely be regarded as more credible since he would appear as a whistleblower to the broader public. Yet this way of making credibility judgments would seem to pose a problem to responsible science advice.

As the world transitions from a COVID-19 pandemic to COVID-19 being endemic, it is important to engage with responsible science advising. During the pandemic, lay publics were faced with a tremendous amount of misinformation. Furthermore, different jurisdictions took different measures to prevent the spread of the virus and advisory bodies in different countries provided different advice to their policymakers. Science advising played a very public, global role. Further still, the advice from public health officials would sometimes change quickly without a clear rationale to the public. The pandemic was a prime example of how science cannot, and should not, be separated from social and ethical considerations. The advice early in the pandemic to stay home and "flatten the curve," was an evidence informed value judgment to protect people's health, particularly the health of the most vulnerable. As the pandemic progressed, social Darwinist, "survival of the fittest," arguments became more common and radical groups stoked fears about the threat to individual liberty and instead of vaccination, argued for "herd immunity."

In such circumstances, policymakers and the public need to be able to assess the credibility of what they are hearing. My view offers these groups tools for such assessment. By placing values central to the discussion of credibility, my view helps science advisors and policymakers communicate their basis for providing their advice and for making their decisions. What I believe my view shows is that scientific inquiry is always shaped by social values and social context. Therefore, we ought to consider our values and our ends, and then see how science can support them.

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