Concerning the Politicization of Climate Science: Epistemic Dependency, Trust in Expert Testimony, and Determining What We Ought to Believe

by

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ABSTRACT

Belief in climate change does not divide into a simple dichotomy of (good) believers and (evil) nonbelievers. An unclear view of skepticism arises when the differences between empirical and normative claims are revealed. Developing responsible beliefs on matters of which we possess no expertise requires reliable expert testimony. However, trust and objectivity are integral factors for belief in expert consensus. A reduction in public opinion regarding the reliability of climate science, due to politicization, enables the dismissal anthropogenic climate change. Understanding politicization from both Pielke and Douglas clarifies a negative role that politics can play in the doing of science. The risks that politicization pose, mistrust for one, do not undermine the necessary role of values in science. The role of values within scientific enquiry must be restricted and acknowledged for trustworthy science to be produced, and for scientific findings regarding climate change to be accepted by nonexperts, including policymakers.

KEYWORDS: Anthropogenic climate change, scientific skepticism, ethics of belief, epistemic trust, epistemic dependence, moral authority, social epistemology, feminist theory.

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PREFACE

Regarding the tension between the demand to act[1] on anthropogenic climate change and resistance to changing the status quo, Bruno Latour asks: "How can we not feel rather ashamed that we have made a situation irreversible because we moved along like sleepwalkers when the alarms sounded?" (Latour 9). In his first lecture, "On the Instability of the (Notion of) Nature," he writes that we[2] ought to have responded by dramatically changing our relationship with nature, such that our way of life would have greatly changed long ago. He writes that we have received many warnings of threats to our current ways of life on Earth and have responded oddly: with absolute stoicism, responding that "this too will pass" (Latour 7). The alarms have been sounding for years, and yet we have chosen to do nothing, or very little. Latour frames this as an odd response in the history of humanity. He writes that generally when there are immediate threats to one's own well-being, to one's survival, or to the well-being of those we care about, "we tend to err in the direction of security" (Latour 10)[3]. He claims that, generally, when our

[1] Here is it important to ask, what kind of action? There are a wide variety of answers provided, some of which are easily acceptable (like not buying a gas-guzzling Hummer and instead buying a hybrid) and some of which might be more difficult (like doubling the price of gas, turning away from all fossil fuel burning, or heavily taxing industries that pollute more than others). Further still, it is important to ask, who knows which actions are the best, the most economical, the most acceptable and the most sustainable?

[2] The use of 'we' is complicated. In March 2017, Lorraine Code gave a conference talk at the University of Ottawa on "Who Do We Think We Are?" regarding the difficulties of using the word 'we' to discuss climate change culpability. There are varying degrees of causation and guilt regarding climate change internationally. The use of 'we' in this thesis is roughly in reference to those of us in Western contemporary society who are able to make judgments about the accuracy and reality of anthropogenic climate change. There are many assumptions embedded in the use of 'we' which makes its use problematic.

[3] Although there is an interesting question here: have we tended to err in the direction of security? Or is it that we like to maintain the status quo, keep things normal and regular? Humans, it might be argued, have a history of staying even the most uncomfortable of courses rather than pursuing the unknown.

bodies and our belongings are at risk, we act. We go to the doctor, we build walls, we create borders, but when it comes to the current environmental crisis, we have failed to act—failed to dramatically change our interactions with the world. Even those who trust the science, Latour accuses of squeezing their eyes shut, not wanting to see their own obligations: "this humanity has remained passive" (Latour 10).

Latour writes that, in the face of surmounting evidence pointing to dramatic changes in global climate, there is "one segment of the public—some intellectuals, some journalists, helped occasionally by certain experts—[that] has decided to plunge little by little into a parallel world in which there is no longer either any agitated nature or any real threat" (11). They want us "'to keep our heads'" (Latour 11). But Latour argues that "We can no longer say 'this, too, will pass.' We're going to have to get used to it. *It's definitive*" (13). If the alarm bells have been ringing for decades, if climate scientists are telling us that there are extreme threats regarding the global climate, why are we not doing anything and how can this segment of the public not believe the matter at hand? As I will show in the coming argument, the stance that Latour takes combines many different claims—claims which are both empirical and normative. To state that we ought to have already acted assumes many things: that climate change science has been confirmed, that science can dictate political action, that political action can help, and that we know the best ways to change our current situation.

INTRODUCTION

This thesis will not assess the scientific data leading to the conclusion that climate change is happening. Neither will it examine the reasons that I personally believe in climate change. Nor will it address the question of *why* we do believe certain things about climate change. This thesis is concerned with the specific question, what *should* be believed regarding anthropogenic climate change? The simplistic answer is: we should believe the consensus of the experts. This is a complex answer to address, and the following elements must be parsed in order to conclude that the most justifiable belief to hold is that anthropogenic climate change is real: epistemic trust and dependency, the relationship between experts and nonexperts, the question of moral expertise, the politicization of climate science, the role of the IPCC (Intergovernmental Panel on Climate Change), and the necessary role of values in scientific enquiry and objectivity.

Given that social epistemology frames testimony as an important source of knowledge, I will claim that nonexperts are epistemically dependent on the expert testimony of climate scientists; that is, we "know" that anthropogenic climate change is happening and is a significant threat because the experts tell us this is the case. However, this expert testimony will only be believed if, in the public eye, climate scientists are considered *trustworthy*. I have observed that some members of the public do not trust climate scientists and therefore feel justified in disbelieving some or all the claims made by climate scientists. Some of this distrust is due to specific critiques of the politicization of climate science. I will show that there are significant concerns regarding the role of politics and values in the doing of climate science; a balance that

must be struck between the misuse of values and politics in climate science and the ethical and value-based judgments that are necessary for producing reliable and just science.

The politicization of science and the role of values in scientific enquiry are distinct concerns. In this thesis, I will consider the unacceptable *politicization* of climate science from two different perspectives. The first is formulated by Roger A. Pielke Jr.. He defines politicized science as the improper use of science as a political tool by scientists. Pielke argues that too often scientists allow scientific debates to morph into political debates. He claims that "In many instances science, particularly environmental science, has become little more than a mechanism of marketing competing political agendas, and scientists have become leading members of the [A]dvertising campaigns" (Pielke 406). He also notes that, increasingly scientists have been "equating particular scientific findings with political and ideological perspectives" (Pielke 405). He argues that this sliding from science into politics has negatively changed both the perspectives of the public and policymakers regarding climate science: "scientific debate and political debate on many environmental issues already have become indistinguishable, and such cases of conflation limit the role of science in the development of creative and feasible policy options" (Pielke 405-6). It is unlikely that many would argue that science ought not to be "predetermined by political perspectives" (406); inevitably predetermined politicized science would be both bad science and produce bad policy (406).

The second definition of politicized science that I will be considering is formulated by Heather Douglas. In *Science, Policy, and the Value-Free Ideal*, Douglas divides the role of values in scientific enquiry into either acceptable roles or unacceptable roles. From this division, she provides a concise description of politicized science. She writes that, "In the cases of politicized science, the norm against a direct role for values in the decisions about empirical

claims is violated" (Douglas, *Science*, 112). As I will explore in the third chapter, the norm against a direct role for values ought to be upheld, without the assumption that a value-free ideal is required. In short, values ought not play a direct role in the doing of science; however, an indirect role is necessary and unavoidable. Douglas explains: "Allowing a direct role for values throughout science is a common way to politicize science, and to undermine the reason we value it at all: [it is one way] to provide us with reliable knowledge about the world" (*Science*, 112).

According to Douglas, politicized science undermines the integrity of scientific enquiry. She writes:

> "When scientists suppress, or are asked to suppress, research findings because the results are unpalatable or unwelcome, values are playing a direct role in the wrong place in science. When scientists alter, or are forced to alter, the interpretations of their results because they are unwelcome by their funders or their overseers, values again are playing an unacceptable, direct role." (Douglas, *Science*, 112)

Douglas claims that "unacceptable, politicized science occurs when values are allowed to direct the empirical claims made by science" (*Science*, 113). The immersion of values and politics in science can direct the stated outcome of the research. When this happens, scientific findings cannot be considered empirical evidence. Douglas writes, when science is politicized, "we can only stand aghast at the deliberate attempt to wish the world away" (*Science*, 112).

It is important to stress that neither Douglas nor Pielke are advocating for a *value-free ideal*. Pielke writes, "Utopian views of clearly separating science from politics, facts from values, are not helpful" (406). Douglas concedes that "All science is value laden" (*Science*, 113). The humanness of enquiry does not allow for values to be removed from the doing of science. However, the role of these values must be both acknowledged and maintained. For this thesis, I

am considering politicization from both angles: from Douglas, that politicized science occurs when values are used in inappropriate roles in research, and from Pielke, that politicized science is a political tool used to manipulate policy. In sum, politicized science, whether a function of politics dictating evidence or evidence being manipulated to dictate policy, politicized science is unacceptable science and is damaging to political debates regarding sound scientific evidence. From this two-fold definition, I will argue that when we say the science is politicized and that this is a bad thing, there might be a tendency to misunderstand the actual role of values in enquiry. That is, avoiding politicization does not preclude the role of values. Thus, the direct (and inappropriate) role of politics and values in science will be contrasted with the indirect (appropriate and unavoidable) role of values in scientific enquiry.

As I will explore in the final chapter with Douglas, there are three kinds values which impact science and must be maintained and divided into indirect, appropriate roles and direct, inappropriate roles. The three kinds of values Douglas analyses are ethical values, social values, and cognitive values. Ethical values might include elements such as equality, goodness, and justice. Social values can be considered elements that are for the good of society, such as sustainability and economic stability. Cognitive values aid in the process of doing science, such as simplicity or feasibility. These will be explicated in chapter three. It is important to note that these values can hold indirect roles which can aid in the doing of science, and these same values can hold more direct roles which can detract from the science. This is the crux of my argument: understanding politicization from both the perspectives of Pielke and Douglas clarifies a negative role that politics can play in the doing of science, but politicization ought not to undermine the necessary role that values must play in doing science, which must be constrained in indirect but active roles.

This thesis will progress in three chapters: the first chapter will examine varying degrees of belief in anthropogenic climate change, and address why some people believe in anthropogenic climate change and why some people do not. However, this division is not simplistic. Often those who do not believe in climate change are stereotyped as "lunatic" "deniers," but there are many different claims made about the facts of climate change and our obligations to do many different things which can be reasonably rejected or held in suspension. The division between belief and disbelief is not stark. Following the dissection of this false dichotomy, I will use the work of social epistemologists to discuss expert testimony as a valuable source of knowledge and reinforce the idea that the epistemic relationship between expert and nonexpert is dependent on *trust*. For expert opinions on socially constructed knowledge and the ethics of expertise, I will rely on Richard Corry, David Coady, Alvin Goldman, Richard Moran, Lorraine Code, Ben Almassi, John Hardwig, and Linda Zagzebski. I will argue that the most reliable way to form justified true beliefs about climate change is to appeal to expert testimony. This is a complex and vulnerable relationship because those without expertise in the domain are dependent on the experts. For this relationship to function, the issue of trustworthiness must be addressed by climate scientists and adequately understood as an important element in maintaining this relationship. I will also give consideration to the separation of empirical claims from political claims about climate change, and discuss how a lack of certainty regarding moral and political authority makes belief in expert opinion on the politics of climate change uncertain.

The second chapter of this thesis will address concerns that climate science is politicized from the perspectives of both climate change skeptics and from philosophers who critique climate science. Online commentary from nonexpert and fringe climate change skeptics will be considered. This standpoint is the most often appealed to stereotype of those who do not believe

in climate change. Although these opinions seem to be both outlandish and ungrounded conspiracy theories, I concede that not *all* conspiracy theories ought to be outright rejected as epistemically flawed.

From here, I will move into considering reasoned arguments against the *linear model of expertise*—science compelling particular policy on a get-the-facts-then-act model—coming from Pielke and Silke Beck; that is, some philosophers have concerns about climate science and climate policy interacting in ways that politicize the science and ultimately overestimate the relationship between empirical science and policy decision-making. They claim that the linear model of expertise ignores the intricacies of the relationship between science and politics. Pielke's article, "When scientists politicize science: making sense of controversy over *The Skeptical Environmentalist,"* strongly critiques the linear model. He writes, "Because scientific results always have some degree of uncertainty and a range of means is typically available to achieve particular objectives, the task of political advocacy necessarily involves considerations that go well *beyond* science…. Science never compels just one political outcome" (my emphasis, Pielke 406). The outcome is neither obvious nor simple.

Making clear the role of politics in science, and the role of science in politics, is crucial for developing a trusting epistemic relationship between experts (who provide testimony to nonexperts) and nonexperts (who are dependent on expert opinion for matters of which they have no expertise). The concerns that Pielke and Beck raise regarding the use of science in developing clear public policy on climate change need to be understood. Both experts and nonexperts ought to be wary of the slide from science to politics, and the slide from politics into science. For their arguments against the politicization of climate science and concerns regarding climate science's impact on policy-making, I will refer to the arguments of Michael E. Zimmerman, Roger A.

Pielke, Jr., and Silke Beck. For an example of the politicization of climate science, I will outline the East Anglia "Climategate" scandal and its effect on public trust through Myanna Lahsen's account in "Climategate: The Role of the Social Science." Then, I will use the arguments of Beck regarding the new role of the IPCC. In sum, chapter two will examine two claims: science ought not to become overtly and overly politicized because politicization provides a distinct context in which the issue of trustworthiness of expert opinion arises (and rightly so). Therefore, various skeptical positions might be warranted about the moral and political expertise which emerge from the political claims of scientists.

The third chapter of this thesis will counter the underlying epistemological assumptions that might arise in eschewing the role of politics in science: mainly, the assumption that any amount of values in science undermine the quality of research produced. There are many perspectives on the normative dimensions of science and additional clarification of the value of values in scientific enquiry is needed in domains of policy-relevant science, like climate science. For this, I will consider three of these perspectives: Bruno Latour, Lorraine Code, and Heather Douglas. In contrast to the concerns raised by Zimmerman, Pielke, and Beck on the relationship between politics and science, I will consider the arguments of Latour and Code about the epistemology that is employed when doing policy-relevant science. I will then argue for Douglas' clarification regarding appropriate and inappropriate roles of values in scientific enquiry to argue that the presence of values in climate science ought not to undermine the trustworthiness of climate scientists. The role of values within scientific enquiry must be restricted and acknowledged for trustworthy science to be produced, and for scientific findings regarding climate change to be accepted by nonexperts, including policymakers. I will

additionally outline Douglas' seven alternative understandings of objectivity to illustrate that, when we account for values in scientific enquiry, objectivity in science is not threatened.

In whole, I will be arguing that given the nonexpert's dependence on expert opinion, given the risks inherent in this relationship, given the facts of the matter regarding politicization of scientific enquiry, and given the necessary and unavoidable role that values play in scientific enquiry, a simple definition or example of climate change skepticism is not possible. There are some arguments regarding the ways in which climate science is done that gives room for skepticism to flourish. I will conclude that the politicization of climate science has damaged the public's trust in climate scientists, which is directly related to public belief in anthropogenic climate change. Thus, we must be very clear on the role of both politics and values in science. The risks and avoidance of politicization in climate science does not preclude the epistemically valuable role of values in scientific enquiry in general.

CHAPTER ONE

1. Expert Testimony, Epistemic Trust, and Moral Authority

Generally, when we speak of "believing in" anthropogenic climate change, we mean we believe what the scientists are telling us: that human activity has caused global climate temperatures to rise *and* we must act now in order to best adapt to the change and attempt to prevent environmental catastrophe. When we label someone a "climate change skeptic," we generally employ the stereotype that they don't believe these claims. However, this simplified definition of "skeptic" conflates many different elements. Concerning the claims regarding anthropogenic climate change, we must be very clear: there are empirical facts, researched within the field of climate science, and there are normative claims about what we ought to do about anthropogenic climate change. The distinction between empirical fact and normative claims have often been blurred. This section will parse the various empirical and normative elements of "belief" in climate change to understand that "skepticism" is not a blanket term that can be applied to any and all forms of dissention. I will also claim that the epistemology of expert testimony is not as simple as accepting what the experts tell us to believe. Expertise must be understood as an imbalanced relationship between those who possess knowledge and those who do not. This expertise can be assessed by nonexperts to varying degrees. It is also important to examine the testimony coming from experts in the context of their expertise. Our willingness to believe expert testimony coincides with our trust in expert credibility, honesty, and reliability in general.

1.1: Accepting and Rejecting the Orthodox View of Anthropogenic Climate Change

To elaborate on the distinctions between beliefs regarding climate change, I will use the phrase "the orthodox view of anthropogenic climate change," in the same way that David Coady and Richard Corry do in their text, *The Climate Change Debate: An Epistemic and Ethical Enquiry*. This perspective is comprised of four claims:

> "[1] The climate is changing; in particular the world is getting warmer (on average) over the long term.
>
> [2] This change is largely caused by human activity.
>
> [3] This change is a bad thing.
>
> [4] Something should be done to mitigate this change." (Coady and Corry, *The Climate Change Debate,* 3)

Most environmentalists, philosophers of the environment, climate researchers, and the public who believe in climate change accept these four claims. It is the foundational perspective of former Vice President Al Gore's 2001 documentary *An Inconvenient Truth*. It is a belief many people feel they are justified in holding. In this section, I will pull apart the orthodox view of climate change to illustrate that the stark stereotyping of anyone skeptical about any of the claims of the orthodox view is overly simplistic and eliminates finer distinctions that need to be made about these claims: that is, some of these claims are empirical and some are normative.

The label "skeptic" fails to acknowledge the varied perspectives of those who might not believe one or more of these claims. Not all skeptics reject all four of these claims, because they are vastly different claims. Claims 1 and 2 (that climate change is happening and that it is human-caused) are empirical claims. They are proven or disproven through empirical evidence. Claims 3 and 4 (that climate change is bad and that something must be done) are value-based

claims. Is someone who doesn't believe that a carbon tax is the solution to anthropogenic climate change a denier on the same level as someone who thinks that a cold winter is proof that climate change is a lie? No. There are a variety of ways to not believe in the orthodox view of anthropogenic climate change; one does not either accept or reject anthropogenic climate change. There are varying degrees of belief and disbelief: a broad arc, ranging from the denial of those who believe that nothing is out of the ordinary to the belief that human life on Earth (as we know it) will end in ten years due to climate change.

To be clearer, skepticism regarding climate change is varied. There are not simply believers and skeptics. To give a few examples of the many ways to "believe" in climate change, one might fully accept claims 1 and 2 but not believe that the warming climate is a bad thing [3]. These individuals accept the science, but do not accept the value judgements of the third and fourth claims. One might not believe we are ethically obligated to do anything or are capable of doing anything about these claims [4] but accept that climate change is happening [1], that it is human caused [2] and that it is largely a bad thing [3]. One might believe nothing can be done, nothing ought to be done, or that nothing we can do is affordable. One might believe that climate change is happening [1] but doubt that human activity has anything to do with it [2]. Further still, one can reject the fundamental claim: that climate change is happening at all [1].

Considering the problems with dividing the debate between two sides, and with the understanding that this division both conflates varying degrees of belief and non-belief and conflates the division between empirical and normative claims, I will be problematizing the debate between two sides of the climate change debate: those who feel justified in believing all four claims of the orthodox view of anthropogenic climate change and those who feel justified in being skeptical of any of the four claims of the orthodox view of anthropogenic climate change.

This becomes more problematic when discussing Zimmerman, Pielke, and Beck; while their critiques of climate science ought not fall into the category of skepticism regarding anthropogenic climate change, I will be using their critiques as reasons for why some individuals feel that some skepticism regarding anthropogenic climate change is justified. There is a polemical stance which the proponents of the orthodox view of anthropogenic climate change often take: that is, anyone who questions any of their four claims are labelled either skeptics or deniers. My aim, in using the term "skeptic," is to provide a broad definition of the vilified stance of questioning climate science. I will explain further.

A non-partisan approach for talking about skepticism is concisely advocated for in the draft Introduction to the forthcoming book, *Climate Change Skepticism: A Transnational Ecocritical Analysis*, by Greg Garrard, Axel Goodbody, George Handley, and Stephanie Posthumus.[4] In this text, the authors examine climate change skepticism as a literature, and do so from a centrist's perspective. Even though the authors consider themselves "righteous environmentalists" (Garrard, et al. 5), they have set out to "understand climate skepticism, not to vilify or even overcome it. Without surrendering our difference of opinion, [they] want to try to see the world from the perspective of climate sceptics" (Garrard, et al. 5). In doing so, the non-confrontational approach implemented by the authors is an attempt to avoid reinforcing stereotypes and take climate change skeptics seriously "*as writers*" (their emphasis, Garrard, et al. 12). This approach to assessing and understanding skepticism the authors have dubbed "reading climate skepticism agnostically" (Garrard, et al. 13).

There are legitimate concerns about taking a non-critical, agnostic approach to discussing climate change skepticism. The authors note that this approach is not only disappointing for the

[4] This is available on academia.edu through Greg Garrard's profile. It is due to be published by Bloomsbury Academic, in February 2019.

liberal side, it risks "implying that sceptical perspectives on climate change are equivalent to those of scientific experts" (Garrard, et al. 13). The concern expressed by the authors is that implications of equivalency regarding the two sides is epistemically flawed. The authors note that many scholars who are doing work on climate skepticism do "acknowledge the diversity of climate skepticism, but then offer analysis that tend to minimize or eliminate it" (Garrard, et al. 14). The effort to eliminate this diversity is overly simplistic. As I will explore in this thesis, skepticism about anthropogenic climate change ranges from questioning the accuracy of charted global temperatures to debates regarding the best way to mitigate the warming. Taking a partisan view of the debate divides it into a false strict dichotomy of belief and disbelief.

Garrard and his co-writers note that there are two general reactions to their work: "liberal academics" often react with fear, which "symbolizes a wider challenge to the social value of expert knowledge" (6); resistance from the skeptics indicate that skeptics "do not expect a sympathetic hearing" (6). This indicates a growing polarization between those who believe in anthropogenic climate change, and those who are skeptical of any of the four claims of the orthodox view. Examining the wide-ranging ways of handling the debate over anthropogenic climate change, these authors claim they take the same stance as Arlie Russell Hochschild: "to challenge political polarization by scaling the 'empathy wall' between environmentalists and sceptics" (Garrard, et al. 25). In other words, fully understanding the scale and scope of the climate change debate requires a non-partisan approach. As the authors put it, "climate sceptics cannot be dismissed as dummies who reject science, and they are unlikely to be persuaded by more, and more brilliantly communicated, science" (Garrard, et al. 27). Thus, as Garrard, Goodbody, Handley, and Posthumus do, I will maintain a non-partisan perspective of both

climate change "believers" and climate change "skeptics" with the understanding that skeptics cannot be considered one unified body of one ideological approach.[5]

Many philosophers who fully embrace the orthodox view of anthropogenic climate change reject that the term *skeptic* ought to be used at all, and any rejection of the four claims needs to be accurately labelled as *denial*.[6] For present purposes, what this should illuminate is that climate change denial is not a simple issue and addressing those who don't believe in climate change is complicated. The underlying claim of the debate over terminology is that *denial* must be used to refer to anyone who disagrees with any of the claims of the orthodox view of anthropogenic climate change is that deniers are *blind to the facts of the matter at hand*. This is complicated further when we make divisions between empirical facts and normative claims. It is additionally problematic when we consider the sources of these "facts": who decides which claims (both empirical and normative) ought to be and must be accepted by the public?

Understanding the expected role of scientists is important in these distinctions. There are some, Pielke and Beck for instance, who want to maintain a distinction between the scientist's

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[5] There are further debates regarding what to do once we agree on the four claims of the orthodox view of anthropogenic climate change. Most scientists agree, "Something should be done to mitigate this change" (Coady and Corry, *The Climate Change Debate,* 3); there is no consensus on what ought to be done. As discussed by Warren Pearce, Reiner Grundmann, Mike Hulme, Sujatha Raman, Eleanor Hadley Kershaw, and Judith Tsouvalis, in their *Environmental Communication* response, "A Reply to Cook and Oreskes on Climate Science Consensus Messaging,"[5] there is a consensus on "the minimalist fact that human influence on a changing climate" (Pearce, et al. 736); however, there is no consensus regarding normative dimensions of policies related to climate change mitigation (Pearce, et al. 737). Two examples the authors provide give insight into what disagreements are still taking place among climate scientists: "the debate over the hiatus/pause in global temperature increase" and the ongoing "debate regarding how much carbon dioxide can be emitted while keeping global temperature rise below 1.5°C" (Pearce, et al. 736). When consensus is being discussed, these kinds of disagreement are often ignored; some claim ignoring these on-going (esoteric in nature, as we will explore with the arguments of Goldman) debates is important for making effective and immediate policies to mitigate climate change (Oreskes and Cook), others do not (Pearce, et al.). There is an important consideration to make here regarding consensus in the scientific realms of enquiry as it relates to public policy: these authors, Pearce et al., write that we do not need to insist that "knowledge of a single number is a pre-condition for political progress" (737). Others, like Oreskes and Cook, claim that one single message ought to be sent to the public, outlining that climate change is happening and it ought to be mitigated, without allowing the other disagreements to enter the debate.

[6] See: Torcello, Lawrence, "The Ethics of Belief, Cognition, and Climate Change Pseudoskepticism: Implications for Public Discourse"; Washington, Haydn and John Cook, *Climate Change Denial: Heads in the Sand*; and, Coady, David and Richard Corry, *The Climate Change Debate: An Epistemic and Ethical Enquiry*.

role in telling us what is (empirical claims about the world) and what we ought to be doing (political claims about how we ought to act in the world). Others, like Latour, Code, and Douglas, argue that scientists are just as interested and situated in the world of ethics and politics as the general public. And, at times, scientists are the best situated in the debate regarding what to do next, to be making prescriptive judgements. The importance that is placed on scientific expertise requires a great deal of trust in the experts. In the coming section, I will argue that we should make determinations about which beliefs ought to be accepted based on expert testimony.

1.2: Determining What to Believe

In determining what should be believed about anthropogenic climate change, I will parse the role that trust and dependency take up in the relationship between experts and nonexperts. In his aptly titled book *What to Believe Now*, David Coady explores the imbalanced relationship between experts and nonexperts: when we lack expertise, it is difficult to determine what to believe and whom to believe.[7] He writes that "Often we can't work out what to believe (or how confident we should be of our beliefs) on our own. In these circumstances, we may seek the guidance of experts" (Coady, *What to Believe,* 27). Experts, in their role as experts, must both maintain a current awareness of the questions and answers within their own growing domain, and maintain a relationship with the nonexperts for their expertise to remain trustworthy and reliable. The main question within this strained co-dependence is, how does the nonexpert determine what to believe when they themselves are not an expert?

[7] It should be noted that not only the nonexpert is dependent on the experts. Experts rely on other experts for their expertise both in their own fields and in other fields where they too are part of the laity.

Situating expertise in the history of epistemology, Coady writes that both the empiricist and the rationalist traditions of epistemology[8] "downplay the extent to which we are dependent on others, especially experts or those we judge to be experts, for many of the things we believe and many of the things we claim to know" (*What to Believe,* 38). This issue is taken up in the field of social epistemology as *epistemic dependence* and, within the framework of social epistemology, this reliance on others is not considered a *definitive* weakness. Here I will explore what it means to rely on experts from the perspectives of Coady, Goldman, Moran, and Code[9], then I will discuss why we believe or don't believe expert opinion. That, according to Hardwig and Ben Almassi, comes down to trust, and trusting or not is a matter of character.

As a starting point for defining an expert, I turn to Coady's most basic definition: being an expert is "simply a matter of being well-informed about a subject" (*What to Believe,* 28). In other words, "someone laypeople can go to in order to receive accurate answers to their questions" (Coady, *What to Believe,* 30). For Coady, relying on expert opinion regarding empirical claims is justifiable. He writes, "Believing a proposition, or increasing our confidence in it, on the grounds that an expert believes it, or has a high degree of confidence in it, is justifiable insofar as we want to increase our chances of believing, or having a high degree of confidence in, the truth" (Coady, *What to Believe,* 30). In the domain of anthropogenic climate change research, that question is: what should we believe when our limited scope of knowledge prevents us from being fully justified in our own determinations? I am not able to make expert calculations to know if global temperatures are increasing or what the exact threats are regarding

[8] While this thesis is not intended as an outright critique of more traditional epistemological frameworks, there are good critiques of the empiricist and rationalist traditions in much of the literature, for example in both Coady and Code's work.

[9] There are finer arguments between philosophers regarding expertise which will not enter discussion here. For instance, Goldman's critique of Hardwig and C.A.J. Coady and their willingness to "stem the potential tide of testimonial skepticism" (Goldman 87). There are some arguments regarding a *prima facie* acceptance of expert testimony, all things being equal.

climate change. Instead, I *must* look towards expert opinion if I want to increase my chances of believing and increasing my confidence in my held beliefs.

In Alvin Goldman's "Experts: Which Ones Should You Trust?," an expert is defined by three properties: a fount of knowledge, the ability to apply this knowledge to form accurate answers to new questions within the expert's domain, and extensive knowledge of both primary and secondary questions regarding this domain. The first element is relatively simple: the expert has acquired an extensive knowledge of accurate information within their domain of expertise. The second element requires the expert to not only have extensive true beliefs regarding their domain of expertise, they must also be able to harness this knowledge to address new questions arising in this domain. The third element separates questions regarding the expert's domain into two categories: primary questions, the "principal questions of interest to the researchers or students of the subject-matter," and secondary questions, questions regarding "the existing evidence or arguments that bear on the primary questions, and the assessments of the evidence made by prominent researchers" (Goldman 92). For Goldman, an expert should possess extensive knowledge of both primary and secondary questions regarding their field of expertise.[10] In sum, according to Goldman, an expert "will claim to have [a wealth of information within this domain of expertise] and set of methods, and will claim to have true answers to the question(s) under dispute because he has applied his fund and his methods to the question(s)" (92).

[10] Goldman concedes that there is another, weaker sense, of expert: he defines this, in consideration of the work of others, as an expert who possesses extensive knowledge of the secondary questions regarding this field of knowledge. They might hold mostly wrong opinions regarding the primary set of questions, but their extensive knowledge of secondary questions allows for some consideration of the expertise (Goldman 92).

There are some who are concerned about our reliance on expertise in determining what to believe. Of them, Coady writes that there are three good reasons why someone might be skeptical of claims of expertise:

> [a] they are skeptical of reputational experts (they distrust who claims to be an expert, or who has the reputation of being an expert);
>
> [b] they are resistant to the implicit elitism in accepting some opinions as more valuable than others;
>
> [c] if we rely on experts, we are also relying on their testimony. That is, we rely not only on what they know, but what they are telling us. (Coady, *What to Believe,* 31)

These three concerns regarding expertise can be clarified through consideration of those who are skeptical of any of the four claims of the orthodox view of anthropogenic climate change.

In the first reason [a] regarding the legitimacy of claims regarding expertise, it is tempting to simply say that climate scientists are experts because of their education, their accreditation, and their publications (which indicate that their knowledge is widely accepted by their peers), but one can ask two questions: what kind of expert are we talking about and who labelled them an expert? The first question becomes complex when we consider the range of claims that climate scientists are making: some of us can easily accept that climate scientists are experts who we can rely on for accurate, empirical statements regarding the long-term global temperature trends; however, should we believe that these same scientists possess the moral expertise needed to guide our policies regarding what to do about the empirical findings? This skepticism will be taken up in chapter two from the perspective of Pielke and Beck regarding scientists' roles in policy-making. What kind of expertise or special insight do climate scientists

have that gives them credentials to be making policy-relevant decisions? Some, such as Pielke and Beck, argue that this misconstrues the scientist's expertise. Others, such as Latour, argue that through enquiry into the natural world, human responses and ethical obligations emerge.

The second question that arises when discussing the validity of expertise concerns the accreditation, or the labelling, of such an expert as an expert. Goldman makes an interesting assessment of the accreditation of experts. He writes:

> "I treat ratings and credentials as signaling 'agreement' by other experts because I assume that established authorities certify trainees as competent when they are satisfied that the latter demonstrate (1) a mastery of the same methods that the certifiers deem fundamental to the field, and (2) knowledge of (or belief in) propositions that certifiers deem to be fundamental facts or laws of the discipline." (Goldman 97)

The accrediting bodies and meta-experts determine who is considered an expert. These bodies make assessments based on the processes and procedures already in place within the domain, and these new experts are judged based on the experts already working within the domain. In this sense, experts become experts by becoming familiar with the knowledges, belief, and propositions that are *already held within the domain.* As noted previously, these experts must also use this body of knowledge to answer new questions and come to create and come to accept new knowledges which enrich the domain. To maintain a certain level of credibility within an expert domain, experts must update their "known" information, and be recognized by their peers as doing so. Goldman stresses that "ratings and conferred credentials ultimately rest on basic agreement with the meta-experts and certifying authorities" (97). In other words, experts are

considered experts when they are recognized within their own domain of expertise, by other experts.

The second reason [b] that one might be skeptical of expertise stems from an automatic hierarchy which is established between those with expertise and those without. Implicit in this hierarchy is the belief that some opinions are worth considering more than others. The charge then levied against those who trust the experts is that they are falling into an elitist trap. This (again) should be addressed with the question, where does one's expertise lie? Scientists' opinions are better informed than nonexpert opinions regarding the changing climate, just as a doctor's opinion about my heart health is more valuable than my accountant's opinion. However, my doctor's opinion of my finances might not be the type of expertise I should rely on. We must be cautious about whose opinions we regard as superior on which subjects. That is, skepticism of the testimony of experts can be justified when this testimony applies to a field outside the expert's domain. This can also be seen within the climate change debate when climate scientists make policy recommendations.

The last good reason for being skeptical of expert testimony is that we are relying on what the experts say, not just what they think [3]. Coady argues that we ought to "defer to experts insofar as [our] goal is to believe the truth" (*What to Believe,* 33).[11] To address this concern, we ought to consider the likelihood that the experts are telling the truth, are capable of knowing the truth, and want us to know the truth. Not only do we require experts to come to know the truth, but we must trust that they are *telling* us the truth as well. In terms of the climate change debate, we must rely on testimony to know what is happening to the global climate. Just

[11] Belief in testimony has long been written about, both from the perspective that hearing testimony is not gaining knowledge (as written about by John Locke) and the perspective that testimony is at times our only access to knowledge (Coady, both in *What to Believe Now* and with Richard Corry in *The Climate Change Debate*).

as Coady states about complex mathematical theories, we cannot only rely on our own senses or opinions to tell us about global climate temperatures, warming trends, or the effects of increased carbon in the atmosphere. He writes that "Our own powers of observation and reasoning are not more reliable than the testimony of putative experts. And it is simply egoism to think otherwise" (Coady, *What to Believe,* 35). The complexity of relying on, not only experts, but what the experts say presents a problem for those who already have difficulty trusting: one could ask, "How do I know the experts[12] are telling the truth?"

In terms of discussing expertise related to anthropogenic climate change, I will mostly be referring to the Intergovernmental Panel on Climate Change (IPCC). The role of the IPCC might appear simple. From their webpage, the IPCC defines itself as an international body whose purpose is "assessing the science related to climate change" (*IPCC*). Their role is to provide policymakers with accurate assessments of the current impact and future risks of climate change, and provide various suggestions for how to adapt to climate change and mitigate the damage caused by climate change. They claim that this collective body provides "rigorous and balanced scientific information to decision-makers" and it does so through the work of its 195 members, "open to all member countries of the WMO [World Meteorological Organization] and United Nations" (*IPCC*). The assessments and recommendations coming from the IPCC are written by

[12] In some of the literature on climate change skepticism—Washington and Cook, and Oreskes and Conway—there is a history of manipulating who is perceived as an expert to sway the debate. For instance, Washington and Cook claim that those in the business of manufacturing denial use fake experts to bolster the illusion that most scientists do not agree that the climate is changing. Like the fake experts who were "used extensively by the tobacco industry to counteract the growing evidence on the harmful effects of second-hand (passive) smoke" (Washington and Cook 12), fake climate experts have been contesting the (near) scientific consensus. Washington and Cook writes that one study (the Petition Project published by the Oregon Institute of Science and Medicine) purports to prove that 31,000 scientists reject the claims of climate science; however, Washington and Cook claim that these scientists were comprised of "anyone with a BSc or higher" (Washington and Cook 45). The fields of these "experts" ranged from mechanical engineering to computer science. This study claimed that those with expertise in any area were qualified to comment on the facts of climate change.

"hundreds of leading scientists who volunteer their time and expertise" (*IPCC*). These assessments and recommendations are written by "assessing published literature" (*IPCC*). Rather than publishing their own scientific findings, the IPCC assessments "point to areas of well-established knowledge and of evolving understanding, as well as where multiple perspectives exist in the literature" (*IPCC*).

The IPCC is composed of the foremost working climate scientists across the globe. The aim of the IPCC is to include authors who "reflect a range of scientific, technical and socio-economic views and backgrounds" (*IPCC*). These teams are required to be composed of a mix of various experts coming from all different areas, from developed and developing countries, as well as a balance of male and female participants. Authors may additionally include experts from both industry and non-profit groups (*IPCC*). These groups are formed following a call to governments and organizations associated with the IPCC for nominations of experts. The authors who are chosen are chosen "on the basis of their expertise" (*IPCC*).

The IPCC was initially formed to make recommendations to global leaders regarding the right policies to be implemented. Quoted, at length for clarity, from their webpage:

> "The initial task for the IPCC as outlines in UN General Assembly Resolution 43/53 of 6 December 1988 was to prepare a comprehensive review and recommendations with respect to the state of knowledge of the science of climate change; the social and economic impact of climate change, and possible response strategies and elements for inclusion in a possible future international convention on climate" (*IPCC*).

The role of the IPCC has expanded as the group itself has grown. Today, they define their role in the relation between scientific expertise and global governance as:

> "to assess on a comprehensive, objective, open and transparent basis the scientific, technical and socio-economic information relevant to understanding the scientific basis of risk of human-induced climate change, its potential impacts and options for adaption and mitigation. IPCC reports should be neutral with respect to policy, although they may need to deal objectively with scientific, technical and socio-economic factors relevant to the application of particular policies" (*IPCC*).

In chapter two, I will consider Silke Beck's critique of the IPCC and the current ways it relates to its expertise and global leaders. The role of providing expert opinion to global leaders is more complex than simple, and there is on-going debate regarding the ways in which and the extent to which expert opinion ought to affect policy decisions.

1.3: Trusting Expert Testimony

Testimony has a long history of being underappreciated as a source of knowledge. In "Testimony, Advocacy, Ignorance," Lorraine Code's social epistemology directs new attention towards the validity of testimony as a source of knowledge, from which I will argue that we are justified in believing in anthropogenic climate change because expert testimony tells us that anthropogenic climate change is happening. Code situates testimony as one of the "principal, most reliable sources of knowledge" in the tradition of Anglo-American epistemology; however, she notes that "Testimony's relegation to last place on [the] list signals its lesser ranking…because reliance on testimony has long been thought to compromise epistemic self-reliance" ("Testimony," 2). Social epistemology reconfigures this troublesome relationship with testimony and insists that testimony (expert testimony) can be a legitimate source of knowledge. I will consider this perspective from Hardwig, Code, Moran, and Almassi.

In “Doubt and Denial,” Lorraine Code explores the relationship between expert and nonexpert in the field of social epistemology. Code writes, “the growing significance of testimony in social epistemology creates spaces for epistemologists and moral-political theorists to engage philosophically with situations in the division of intellectual-epistemic labour in western societies where ‘we’ are commonly reliant on other people to ‘know’ for us” (Code, “Doubt” 851). However convenient this relationship might be, there are complexities to trusting the experts. Code continues to say, it is in our ignorance that this reliance is risky: “we do not and perhaps cannot know well enough to know whether they are knowing responsibly or to judge how to place our trust wisely” (Code, “Doubt” 851). The relationship between expert and nonexpert is uneven; there is no counter reliance. Due to the nonexpert’s epistemic reliance on the expert’s knowledge, our belief in the conclusions of the experts necessarily involves trust. That is, in order to believe the testimony of experts regarding claims outside our own knowledge domains, we must trust that they are capable of making reliable judgments and that they are telling us the truth. In short, full belief in expert conclusions follows from trusting the expert.

Trust is a contributing difficulty in deciding what to believe about the climate change debate; often political skepticism is rooted in a distrust of the experts. In “Toward an Ethics of Expertise,” John Hardwig observes that often he finds himself believing things for which he has no evidence: climate change, wiring issues, technicalities regarding nuclear waste, heart disease, child mental health, and so forth. The finer details, the evidence leading to expert knowledge, is massive. Hardwig grants that he might be able to research one, maybe two, of these fields and come to his own determinations, but he could not gather all the evidence for all the things he believes but does not know.

Harwig writes:

> "Too much is known; the evidence is too extensive; much of it is available only to those with special aptitudes and skills honed over years of study and practice. And I lack the competence, the skills, the sheer intellectual capacity, as well as the time. Usually, I lack the ability to critically evaluate the merits of evidence presented to me. Often I cannot even understand it." (Hardwig 2)

As we must as well, Hardwig must rely on others who possess expert knowledge of things he cannot learn himself. As Goldman proposed, expert opinion, while not definitively correct, is more reliable, less likely to be wrong, and less likely to be misunderstood. To refuse the opinion of experts, and instead determine what to believe for one's self, Hardwig writes, "would be to hold relatively crude, unchecked, unreliable, and therefore irrational beliefs" (2). We may not be able to understand the evidence that leads experts to believe what they believe, we can still be justified in believing their expert testimony.

In the face of both expert opinion and non-expert opinion (whether they can tell the difference or not), non-expert climate change skeptics appear to believe the testimony supporting the beliefs they already hold (that climate change is not happening) and tend to reject the expert opinion that is too technical, too difficult, and too demanding. They perhaps even rely on their own observations and calculations rather than the opinion of experts. Hardwig calls this "an attempt at epistemic self-reliance" (3). This attempt at epistemic self-reliance is a rejection of the testimony of the experts, through which individuals attempt to come to their own decisions regarding what to believe, independently. As we will see going forward, some who have been labelled climate change skeptics can practice some epistemic self-reliance (such as philosophers who are trained to question the assumptions implicit in theoretical work) and non-expert climate

change skeptics do not possess the critical abilities to practice epistemic self-reliance. However, it is important to ask again, is self-reliance more justifiable in moral domains? Skeptics sometimes reject the propositions of climate experts in favour of others who claim to be experts: for example, those found on fringe news sites like Breitbart and some who might have vested interest in oil markets or fossil fuels.[13] Other skeptics still rely on their own phenomenological observations, choosing to selectively believe only what they can see and feel for themselves.

By the nature of expertise, there is an inequality in the relationship between experts and nonexperts, an *epistemic difference*, to use Ben Almassi's term. Hardwig contends that there are problems with forming beliefs based on expert testimony that are lodged in this inequality. Nonexperts are vulnerable: "A doesn't know what B knows" (Hardwig 5). The divide between A and B on subject X is where doubt and denial can fester; the relationship between expert and non-expert (in the expert's specialized field) must be based in trust. Without trust, expert testimony will not be believed. Often a climate change skeptic's disbelief in climate change is rooted in a social distrust of the role that scientific experts play in society. There is a vulnerability in trusting; Hardwig writes, "this trust in experts is, like any trust, subject to abuse. There are untrustworthy experts and many who fraudulently or mistakenly claim more expertise than they possess. There are many cases where experts have proven to be inaccurate, biased, of limited vision, and even dishonest" (6). Within the possibility of deception lies our vulnerability. To accept the testimony of the experts regarding empirical matters, we must trust that they are doing their science correctly, and trust that they want us to know the truth.

[13] What Pearce et al. refer to as "climate disinformers" (737). Also see: Naomi Oreskes and Erik M. Conway's *Merchants of Doubt: How a Handful of Scientists Obscured the Truth on Issues from Tobacco Smoke to Global Warming*.

Code claims that an "ingrained distrust of testimony" ("Testimony," 2) is counter to the fact that rarely are the facts we purport to know based on first-hand encounters. It is wholly counterproductive for epistemological frameworks to view testimony as the weakest form of knowledge when, as Code argues, so much of our knowledge claims are based on the testimony of others. Enquiry within social fields[15] generally operates so that testimony "becomes the stuff of which knowledge is made and on which policies, practices, attitudes, and actions rely" (Code, "Testimony," 2). The field of social epistemology corrects this negative positioning of testimony to the extent that, Code writes, "Social epistemology claims its title, in large measure, from the centrality it affords to testimony and to knowledge-conveying exchanges in the real [social] world" ("Testimony," 3). The sociality involved in our pursuit of knowledge is integral to our ability to acquire it. These social relations always involve an assessment of what Code calls *extra-epistemological issues*, such as, "trust, power, advocacy, negotiation, and epistemic community" ("Testimony," 3). It is testimony (both the giving and receiving[16]) which is tied up in these issues. Many of these questions arise when determining what to believe about climate change.

To bolster these claims, Code uses the work of Richard Moran and his essay "Getting Told and Being Believed." In this essay, Moran structures reliance on testimony as tangled up with concepts of epistemic dependence, epistemic vulnerability, and risk (cited in Code, "Testimony," 3). These elements are crucial to understanding why belief in expert testimony is

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[15] I am extending this analysis to include policy relevant science, which I frame as a social field of enquiry in which issues of politics and issues of science are prone to be conflated. The threat of politicization is all the more relevant in policy relevant science because these are domains in which the science is directly tied to politics. And policy-makers must include scientific findings, of which they might not possess any expertise, when making decisions.

[16] There are important considerations to be made, which this argument does not necessarily tackle, but ought not be made without at least reference to *testimonial injustice*. Proposed by Miranda Fricker, in *Epistemic Injustice*, there are some testimonies that are privileged above others. There is a long-standing history of injustice regarding the handling of the testimony of people of colour, women, and queer individuals. This is not largely ignored by Code or others working in the field of feminist social epistemology.

more difficult for some: it involves trust, dependence, vulnerability, and risk. Moran writes, "when I take someone's word for something I am peculiarly dependent on the will or the discretion of the speaker" (Moran 7). It is not only that I believe the object of what I am being told, it is that I believe the speaker (Moran 3). And the speaker might not always be reliable. As Moran writes, "People are known to lie, exaggerate, and otherwise speak in ways that do not express their genuine beliefs" (6). For this reason, forming justified beliefs based on trust in expert testimony involves an element of risk—the speaker might be deliberately deceptive. When we are dependent on the testimony of others, we are dependent on "their discretion, sincerity, good intentions" (Moran 6).[17]

In "Climate Change, Epistemic Trust, and Expert Trustworthiness," Ben Almassi argues that public knowledge regarding anthropogenic climate change is dependent on expert opinion and epistemic trust. This dependency, according to Almassi, "need not thereby make our knowledge of climate change somehow intellectually or ethically suspect" (30). However, when nonexperts are epistemically dependent, we are at risk of being manipulated, exploited, or deceived. Almassi argues that, because of the risk within epistemic trust, whether we believe in anthropogenic climate change is *dependent* on our trust relationship with climate experts, and this trust compels specific "duties of trustworthiness" (Almassi 30) from both the expert and nonexpert. That is, trust must be earned and maintained, not simply given. This requires effort on the behalf of the expert; asking for trust is not enough. The category of expert is complicated by the indiscernibility of whose expertise counts: experts can be influenced by a variety of influences, including governments, industry, and other funding agencies.

[17] Elements of power and oppression against come into play here. When trust becomes an integral element to being believed, there are many opinions which will be discounted and ignored due to prejudices founded in racism, sexism, ableism, bigotry, and homophobia, though this list is not exhaustive. *Who* is being believed can be a very important factor in determining *what* is being believed.

Trust is difficult and can be a breeding ground for what Almassi refers to as *moral rot*. He writes, “Trust is not like a contract voluntarily adopted” (Almassi 42). In other words, if climate scientists and environmentalists want to garner public trust, they will have to acknowledge and be explicit about the inherent risks in this epistemic dependence and recognize that a reciprocal relationship is needed for trusting expert/non-expert relations to develop in healthy ways, rather than exploitative or manipulative ways. Almassi assesses the risks that come with trusting from the perspective of Annette Baier. Baier writes, “‘Exploitation and conspiracy, as much as justice and friendship, thrive better in an atmosphere of trust. There are immoral as well as moral trust relationships, and trust-busting can be a morally proper goal’” (qtd. in Almassi 44). Climate change skeptics ground their skepticism in a distrust of climate scientists possibly because of the risks involved. As explored earlier, in the epistemic difference between the experts and the nonexperts, the nonexperts ought to be wary of being tricked. The risks of epistemic trust are particularly present in unbalanced relationships of epistemic dependency because of the “potential [for] expert exploitation” (Almassi 45).

Despite resistance to scientists acting as advocates for certain political positions, there are two good reasons for climate scientists to strive to create trustworthy relationships with the public: one, because this trust ought not to be taken for granted and, two, because of the politically-charged climate of climate change research. Almassi argues that fostering epistemically trustworthy expertise requires not only “a commitment to transparency, reliability, and absence of ill will” (46) but also an understanding that one’s opinion is being trusted. Being in the position of being trusted requires the expert to respond conscientiously to the “‘expectation that the one trusted will be directly and favorably moved by the thought that we are counting on her’ (Jones 1996)” (qtd. in Almassi 46). In other words, the expert must be aware of and engaged

with the public's dependency on their expertise. Climate scientists ought to foster and maintain "morally healthy epistemic trust relationships among scientific communities and the broader public" (Almassi 46). These trust relationships enable public faith in the testimony of experts. Trustworthy experts "offer public testimony of scientific claims in ways that are not just reliably formed and transparent but also favorably responsive to non-expert members of the public in our vulnerable condition of epistemic dependence" (Almassi 46). The expert must be engaged with their moral responsibilities and obligations as an expert. This includes engaging with nonexperts and being aware of the epistemological risks that accompany their position as both *trusted* expert and *trustworthy* expert.

Almassi argues that the public is epistemically dependent on experts to determine what to believe regarding climate change, and he extends this dependency to even those who have some understanding of the climate science. He writes, all our beliefs "regarding contemporary climate change, the extent of human contribution to it, and proper mitigating responses are grounded substantially in our epistemic trust in expert testimony" (Almassi 47). Much of the dependence on and acceptance of expert testimony relies on our *ability* and *willingness* to trust the experts. Almassi writes that, in a move towards a better trusting relationship between expert and layperson, "in addition to striving for transparency and reliability in climate science and its public presentation...[,] we would do well to recognize a genuinely engaged affective trustworthiness, by both expert and non-expert parties to our relationships of epistemic dependency, as epistemologically and ethically significant" (47). This relationship with the public, particularly with the most skeptical, has been complicated by claims that climate science has been politicized. If we take what Almassi warns seriously—if there is a significant risk of

moral rot, deception, and manipulation within epistemic trusting relationships—should we trust the testimony of climate scientists?

In the coming two sections, I will examine the question: how do I know which experts to trust? The recommendations coming from Goldman and Coady are helpful not only for determining which experts to trust when the experts disagree, but they are also helpful for addressing skepticism regarding any expert opinion. Because belief in an expert's claim follows trust, trust is an integral component for the nonexpert when deciding which expert opinion to believe. It is important to know the ways in which the nonexpert can and do make assessments of whether experts should be trusted.

1.4: When the Experts Disagree

When determining what to believe, the nonexpert is epistemically dependent on expert testimony regarding complicated matters which require expertise to come to hold justified beliefs. The difficulty of the nonexpert/expert relationship is most apparent when two experts (or groups of experts) provide contradicting claims. This is taken up by both Goldman and Coady; they ask, how can the nonexpert determine what to believe when experts disagree? This is complicated by the fact that the nonexpert is dependent on the expert because the nonexpert cannot assess the truth-value of either expert's claims. Lacking expertise means that the nonexpert is unable to parse the truthfulness or warrantedness of an expert's claims. The question is: "Can novices [understood here as the nonexpert], while remaining novices, make justified judgments about the relative credibility of rival experts? When and how is this possible?" (Goldman 89).

When trying to determine who to believe when there is disagreement between expert opinions, Goldman provides five considerations: consider the arguments which support one view and critique the other; check for agreement among other experts; examine their credentials as

experts through the appraisals of meta-experts; check for interests and biases of the experts; and look at the experts "track records" (Goldman 93). Coady condenses Goldman's considerations more succinctly in *What to Believe Now*. Coady provides three options: we can go by the numbers, we can consider the experts' arguments leading to their conclusions, and we can look for evidence of dishonesty. This list is not exhaustive, but these are the most obvious ways we can allay distrust of expert opinions and aid in determining which experts are most likely to lead us towards forming justified true beliefs.

One of the ways nonexperts can assess the arguments of experts in disagreement is to calculate the number of experts on each side and determine which argument is the most justifiably true. Coady writes that it is intuitive to calculate which experts to believe based on the relative numbers on each side of the debate: "All else being equal, it would seem, if there are more experts on one side than on the other, it is rational for a layperson to take the side of the larger group of experts" (*What to Believe*, 38-9). However, this position becomes more complex when we think about how experts themselves come to their opinion. According to Goldman, as cited in Coady, "there is only evidential significance in the fact that the numbers are on one side of an issue if, and to the extent that, those on the side formed their opinions *independently* of one another" (my emphasis, cited in Coady, *What to Believe*, 39). Goldman's argument is that the numbers are irrelevant for forming a belief regarding which expert opinion to believe unless each individual expert came to their own conclusions without relying on the opinions of other experts. Goldman writes, "If two or more opinion-holders are totally *non-independent* of one another, and if the subject knows or is justified in believing this, then the subject's opinion should not be swayed—even a little—by more than one of these opinion-holders" (99). The risk identified by Goldman is the cascade effect: one scientist [A] comes to conclusion [Z]. Another scientist [B]

believes scientist [A] is right and truthful about conclusion [Z]. Another scientist, [C,] trusts scientist [A] because scientist [B] trusts that [A] is correct. Scientist [D] then feels justified in trusting [Z] because scientists [A], [B], and [C] believe the conclusion [Z]. However, Goldman's claim is that only one scientist ever came to this conclusion through original research; therefore, belief in [Z] because three or four scientists believe it based on only the work of one isn't justified. In other words, that ten, twenty, or a hundred scientists believe climate change is happening is irrelevant, according to Goldman's claims, unless each scientist came to their own conclusions from their own work. In order to form a belief regarding climate change, unless each scientist worked out the evidence for themselves, Goldman claims that we ought not hold our beliefs based on the numbers alone.

Coady provides good reasons to object to Goldman's line of thinking. He argues that even if every single climate scientist believed in anthropogenic climate change based on one individual scientist's work and opinion, he "may still be rationally swayed by the numbers favoring global warming" (Coady, *What to Believe*, 40). He writes, even if all expert opinions are based on the expert opinion of one scientist, because experts can adequately judge the quality of that one researcher's expertise (meta-expertise), the nonexpert is justified (much of the time) in going with the conclusions that most experts agree on. Thus, we are still justified in believing that the consensus is most likely to be accurate. He writes that "Although expertise and meta-expertise are logically distinguishable, they overlap to a large extent. Because experts typically work closely with other experts on the same subject, we can usually assume that experts will be able to recognize other experts, and be able to recognize those who have greater expertise than themselves" (Coady, *What to Believe*, 46). In other words, experts can rely on their own expertise to judge the expertise of others. This is referred to as *meta-expertise*—expertise on

expertise. Experts can recognize other experts and are better equipped than nonexperts to judge the quality of each other's work. We can justifiably believe expert opinion on other experts. Thus, according to Coady, a nonexpert can use the number of experts on each side of the argument to assist in determining which side to believe.

Additionally, Goldman's rejection of weighing the numbers ignores the fundamental dependence that experts have on one another. It is not possible for each expert in each field to come to know everything about that field without relying on the testimonial knowledge imparted by other experts. It is not only nonexperts who should trust the experts, experts must rely on other experts as well. This is taken up by Hardwig also; he explains that expertise relies on specialization, and to remain an expert in these specialized fields, they must remain current "by assuming the role of laypersons and accepting the testimony of other experts, standardly in the form of books and journal articles" (Hardwig 3). This becomes complicated. Hardwig writes that he does not intend to "call a full professor of chemistry at Harvard a layperson in chemistry" (3), but that all knowledge of chemistry is reliant on "the opinions of other chemists and by deferring to their judgement" (3). Experts happen to be in a better position to assess the claims of other experts, but they too must trust, at times, the testimony of other experts even within their own domains. Relying on expertise is something both the non-expert and the expert must do in pursuit of accepting true beliefs. We are justified in believing the opinions of the experts, even if this opinion is reliant on other expert opinions.[18]

Another strategy that Coady proposes for determining what to believe in the face of expert disagreement is to assess the arguments experts use to support their conclusions. Even if a

[18] What is at stake here is the debate regarding consensus. Often, when arguing over anthropogenic climate change, one side will claim that there is no consensus, while the other will claim that there is a consensus. Even consensus is problematic within the realm of climate science, as illustrated with Pearce et al. There are important questions regarding the nature of scientific consensus and the cascade effect which are beyond our present purposes.

nonexpert is unable to determine if expert conclusions are correct, they may have the ability to assess the argument's strengths and weaknesses to determine which expert opinion to believe. Coady concedes that nonexperts who cannot "work out the truth-value of the expert's claims" are not going to be able to work out the validity of an expert's supporting arguments (*What to Believe*, 46); some expert arguments will be inaccessible and incomprehensible to nonexperts. However, using Goldman and his claim that "Not every statement that appears in an expert's argument need be epistemically inaccessible to the novice" (Goldman 94), Coady gives some insight into how a nonexpert might go about determining the validity of an expert's arguments. Goldman frames the difficulty by separating out categories of *esoteric* statements and *exoteric* statement.

In line with Goldman, Coady writes that esoteric statements remain within the specific realm of expertise and are often outside the nonexpert's ability to assess. Exoteric statements, however, are outside the field of expertise and can be assessed by a nonexpert. Within the climate change debate, I, as a nonexpert, cannot evaluate the truth-value of the esoteric claim that a twenty year pause in warming is not cause to doubt that global temperature are still increasing, but I can assess the exoteric claim that increasing weather instability negatively affects vegetable and berry growth in North Ontario. It is possible for nonexperts to be able to make some judgments about expert conclusions by assessing the exoteric proposition used in their arguments; what about esoteric claims?

Despite that Goldman claims that nonexperts are "ill-placed to assess the support relations between the cited evidence and the proffered conclusion" (94), he offers some solution for how a nonexpert might be able to determine which expert ought to be believed. Dividing justification into two categories—direct justification and indirect justification—Goldman argues

that, even though a nonexpert cannot produce direct justification because of a lack of knowledge of the esoteric information needed to come to such a conclusion, a nonexpert can form an indirect justification for believing an expert. If one can directly justify their belief in an expert's claim, they are able to assess the truth-value of each proposition leading to the conclusion, and accept the conclusion. This is not possible for nonexperts most of the time. A nonexpert may be indirectly justified in his belief in one expert over another (without needing to assess the truth-value of esoteric propositions) through their assessment of the speakers "dialectical superiority" (Goldman 95). Goldman writes that, "this dialectical superiority might be a plausible indicator" (95) that this expert ought to be believed over the other. In other words, nonexperts can make their best possible guess—the most plausible conclusion—by judging which expert argues best for their belief.

Similarly, building off Goldman's argument, Coady recommends that we ought to weigh an expert's delivery of their argument by their ability to respond to counterevidence. He writes that we should consider the number of rebuttals, the speed of rebuttal and the smoothness with which the argument is delivered; then, "all else being equal, the greater the confidence the novice should have in the expert's expertise" (*What to Believe*, 47). This is complicated however when we consider the reliability of performance and authenticity. Coady writes, "A novice who is ill-placed to evaluate the plausibility of the premises of an expert's argument or the strength of its inferences may still sometimes make a reasonable judgement about whether to accept it on the basis of the expert's performance" (*What to Believe*, 49). However, I would advise the nonexpert to consider authenticity, by whatever definition one might ascribe to the concept, over and above the smoothness of one's tongue, the sparkle of one's charm, and so forth. The ability to smoothly argue might not necessarily indicate greater expertise. Truth might not always follow confidence.

Coady's position bolsters my caution. He writes that "All else being equal, an expert's responses to ostensive counterevidence should be *reasonably* quick and smooth, but not *excessively* quick and smooth" (his emphasis, Coady, *What to Believe*, 49). Additionally, if an expert is too complete in their absolute rebuttal, this leaves some room for doubt as well. If experts disagree, there ought to be evidence supporting both claims, and should be taken into both experts' considerations. If *all* counterevidence is rebutted by the expert, the nonexpert is justified to reserving some doubt regarding the expert's claims. This is because when experts disagree, some counterclaims must have some validity. It is highly unlikely that an expert, well versed in the field, would hold so outrageous opinions that all their opinions could be dismissed by an expert in disagreement (Coady, *What to Believe*, 49-50).

The third recommendation that Coady makes for the nonexpert who is determining which of the disagreeing experts to believe is to look for signs of dishonesty. This is discussed in Goldman as well, who recommends that the nonexpert check for vested interests, biases, and examine the expert's track record. Coady writes that sometimes it is not about determining which expert knows the truth, but which expert is most likely to *tell* the truth. One way of determining which experts are likely to not provide truthful opinions is to look for interests or biases which might indicate the expert is likely to lie or likely to have come to false beliefs.

Goldman writes that the nonexpert ought to look for "evidence of distorting interests and biases that might lie behind a putative expert's claims" (104). However, lying is not the only concern: "interests and biases can exert more subtle distorting influences on experts' opinions, so that their opinions are less likely to be accurate even if sincere" (Goldman 104). The effect, implicit or explicit, of interests and biases are always a concern regarding scientific enquiry, particularly in climate science. If we determine that the expert we are assessing has either

interests or biases related to the object of study—for instance climate skeptics who are employed or funded by fossil fuel companies—we ought to be more cautious regarding their testimony (Coady, *What to Believe*, 50). Not only can vested interests and biases affect the likelihood that the expert is telling the truth, but they also increase the chances that the work has been compromised through implicit bias. It is not only direct deception that the novice must be wary of, but also the integrity and accuracy of the expert's work. Coady writes that, "people sometimes believe things, because it is in their interests to believe them" (*What to Believe*, 50). Interests might not only be a reason for an expert to lie, it could also be the cause of the experts themselves coming to false conclusions despite attempting to provide honest testimony.

In short, despite that the nonexpert is, at times, unable to assess the validity of expert propositions and conclusions, there are various assessments the nonexpert can still make when determining which expert opinion is most justifiable to believe. The nonexpert can examine the expert's history and situatedness for hints of error or bias; the nonexpert can judge an expert's ability to debate the details of their argument and challenge counterarguments; and, the nonexpert can look to the number of experts who also believe in this expert's proposition. None of these are, of course, guaranteed; however, Coady's three elements, and Goldman's extended five, are able to offer some guidance for how best to determine whether to trust or believe expert opinion. Additionally, these considerations show that the nonexpert is still often vulnerable and dependent on the testimony of experts. That there are expert disagreements, that we must be aware of signs of dishonesty, and that some arguments are more convincing than others, illustrates that nonexperts are still at the mercy and whim of the experts a great deal of the time. This dependence indicates that we are therefore always potentially misled.

Further still, we must ask, what are the claims that climate experts are asking us to believe? Claims 1 and 2 of the orthodox view of anthropogenic climate change (that climate change is happening and is human caused) are empirical claims. They are claims based on empirical, scientific evidence. Claims 3 and 4 of the orthodox view of anthropogenic climate change (that climate change is a bad thing and that something must be done about it) are normative claims. Are climate scientists the experts we ought to be appealing to regarding normative or political claims about climate change? Linda Zagzebski's account of the debate regarding the "moral epistemic authority" (2) is helpful to consider here.

1.5: The Question of Moral Authority

The question of moral authority is crucial when talking about expert opinion on anthropogenic climate change. Yes, scientists possess expertise regarding scientific data that tell us climate change is happening and it is human caused; however, we must ask, are climate scientists experts regarding normative claims? And deeper still, must we rely on scientific experts to tell us what can be and ought to be done about climate change? In the chapter "Moral Authority," from *Epistemic Authority: A Theory of Trust, Authority, and Autonomy in Belief,* Linda Zagzebski makes a defense of what she calls "epistemic authority in the moral domain" (2). To begin her argument, she situates the debate: "No domain of epistemic authority is more sensitive than the domain of the moral. People who accept scientific authority without question frequently balk at the idea that they ought to accept authority in any of their moral beliefs" (Zagzebski 2). As applicable here, when thinking about skepticism regarding the normative claims of the orthodox view of anthropogenic climate change, we must ask, based on what expertise and on what authority are scientists dictating the values, politics, and policy that ought to address climate change? Blanket stated, there are two conflicting perspectives on moral authority related to

climate change: scientists ought not to wade into political and moral debates or scientists are best positioned to make normative judgments on the best way to mitigate and adapt to climate change and claim that it is right to do so. I will provide a short overview of the arguments against moral epistemic authority and make clear Zagzebski's objections to these arguments.

A simplified version of Zagzebski's argument is as follows: there are two reasons for accepting the testimony of a moral epistemic authority. Either I can believe what the authority tells me to believe, or I can consider the authority's advice through my own moral filters. She proposes two distinct senses of taking authoritative advice: [1] "The authority of another person's moral testimony for me is justified by my conscientious judgment that I am *more likely to form a true belief and avoid a false belief if I believe what the authority tells me* than if I try to figure out what to believe myself" (my emphasis, Zagzebski 3); or [2] "The authority of another person's moral testimony for me is justified by my conscientious judgment that if I believe what the authority tells me, *the result will survive my conscientious self-reflection* better than if I try to figure out what to believe myself" (my emphasis, Zagzebski 3). In other words, Zagzebski argues that moral testimony from an epistemic moral authority is a reliable way to both form true moral beliefs and avoid false moral beliefs, *either* because their authority is best situated to dictate the most likely true beliefs *or* because I ought to consider another person an authority because their judgments will survive my self-reflection better than my own ability to work out what to believe.

To emphasis the debate regarding the claim that there can be moral authority , I will consider three arguments against Zagzebski's propositions which she presents in this chapter: [i] that moral truths don't exist, [ii] that moral experts don't exist, and [iii] taking moral belief on authority diminished our moral autonomy. The argument that moral truth [i] is different from

empirical truth is an important consideration. Of that, Zagzebski says, her first argument for accepting epistemic moral authority "assumes that we aim to make our moral beliefs true, and epistemic conscientiousness is the quality of exercising my faculties in the best way I can in order to satisfy my desire for truth. But that is irrelevant to moral beliefs if moral beliefs do not have truth value, or if truth in the moral domain is constructed rather than discovered" (Zagzebski 4).

The second reason for not accepting testimony from moral authority [ii] is the belief that moral authority does not exist, which Zagzebski roots in moral egalitarianism. She concedes that there are many way in which our moral beings can be considered equal. She writes, "one of the sources of resistance to recognizing some persons as superior to others in their ability to get moral truth is the thought that that might lead to violating the dignity of some persons" (Zagzebski 4). She continues, "There certainly is no necessary connection between dignity and the accuracy of one's moral judgment, but the connection is close enough in the consciousness of many people that there is a (probably reasonable) fear that the public recognition of lack of equality in the ability to make moral judgments will lead to the treatment of persons as unequal in dignity" (Zagzebski 4-5). In other words, if we prioritize one person's moral judgments over another's, we might diminish the treatment of the latter's moral dignity. She clarifies: "I am suggesting that moral-epistemic egalitarianism is motivated, at least in part, by the fear that the protection of the public recognition of equality in dignity requires a public stance of moral-epistemic egalitarianism" (Zagzebski 5). That is, one opinion is valued more than another.

In the domain of philosophy, the concept of the moral expert is often eschewed. Zagzebski says of this avoidance, that "it is thought important to deny the existence of moral experts indicates a fear that some deep values are at stake" (5). Precisely with the use of the term

expert, it "suggests publicly recognized authorities to whom others defer in some domain of human knowledge, and who claim that others should defer to them in this way" (Zagzebski 5) and if there are moral experts, the expectation will be that their beliefs about the moral must be accepted as truth. Should we accept their moral expertise, the fear is that this is "but a short step to telling us what to do" (Zagzebski 5). Thus, philosophers have concluded, "there had better not be any moral experts" (Zagzebski 5).

The third reason for not accepting the concept of a moral authority is the risk posed to one's autonomy [iii]. Zagzebski describes this argument as follows: "there might be a special problem with taking moral beliefs from another person since autonomy is sometimes thought to require direct control over the moral domain of one's life, including the epistemic part of that domain" (6). This direct control is believed to be forfeit when deferring to a moral authority. If we consider this perspective in reference to Zagzebski's first thesis—that I am more likely to come to true beliefs if I take an authority's word instead of my own—autonomy could be at stake. However, if we consider her second thesis regarding moral authority—that I can be form true beliefs and avoid false beliefs by conscientiously reflecting on the moral testimony of an authority—it is possible to consider another's moral guidance without sacrificing autonomy. I would not lack autonomy in forming moral beliefs if I defer to someone else based on *my* reasonable judgment that their belief about what I should do is likely to be correct (Zagzebski 6).

The debate over moral expertise cannot be resolved here. What is important here is that there are questions regarding the moral authority of scientific experts when determining how best to mitigate and adapt to climate change. Some of the reasons why someone might be skeptical of the normative claims embedded in the orthodox view of anthropogenic climate change are that

they doubt the existence of moral truths, moral experts, or fear their moral autonomy will be lost by accepting the notion of moral authority.

Going forward, I will examine claims that the *politicization* of climate science has reduced public trust in expert consensus. I will do so with various examples from Twitter and climate change skeptic websites, as well as academic examples from Michael E. Zimmerman, Roger A. Pielke, Jr., and Silke Beck. I will consider the arguments of the former to be the thinking of the nonexpert community of climate change skeptics, or *fringe skeptics*. The latter I will consider to be expert opinions on climate science regarding public trust and the politicization of climate science. In the third and final chapter, in contrast to Zimmerman, Pielke, and Beck's perspectives on climate change (acknowledging that epistemic trust in climate scientists has been damaged by instances of politicization, like the East Anglia email scandal), I will consider the arguments of Bruno Latour and Lorraine Code about their perspectives on the appropriate epistemology to employ when doing policy-relevant science, and then argue for Heather Douglas' clarification that not all values in climate science ought to render the production of climate change knowledge untrustworthy. As Douglas argues, the role of values within scientific enquiry must be restricted, understood, and acknowledged for reliable, accurate, and just science to be produced.

CHAPTER TWO

2. The Politicization of Climate Science

If public trust in expert testimony is needed to believe in the orthodox view of anthropogenic climate change [claims 1, 2, 3, and 4], it becomes all the more important that climate scientists foster trustworthiness in the public's view. I am concerned that the public's trust in climate scientists is waning, and I attribute some of this decline to public opinion that climate science has been politicized or value-infused. There are two distinct claims coming out of the critiques of climate science that are related to the relationship between science and policy. The first is the concern that politics are influencing the scientific outcomes of climate science. The second is the idea that the *linear model of expertise*—an operating platform that some accuse the IPCC of operating from which models science as a dictator of public policy, instead of policy informer—constrains both the science of climate change and the politics of climate change action. It blurs the lines between scientific findings and public policy decision-making. The linear model misconstrues science's role in policy-making. As we considered above, many are concerned with the role of science in determining what we ought to be doing regarding climate change. Both accusations regarding the relationship between science and politics appear to reduce public trust in climate science and climate science experts.

Rather than empirical data collection of climate change skeptics, most of the statements made about skepticism have been drawn from various academic resources and logically deducing what belief in climate change is, rather than stereotyping what climate change skepticism is.

Rather than assessing common mediums for climate change skeptic discussions, I will mainly consider skepticism from the perspective of *not* believing the four claims of the orthodox view of anthropogenic climate change. That is, an absence of belief rather than the formation of a negative belief, as we can find in the rhetorical strategies of internet trolls, advocates for oil companies, and some republican politicians in the United States. This angle is in part to avoid stereotyping all skepticism under the same headings and ignoring the nuanced positions that can arise from being critical of environmental movements and withholding judgment regarding the empirical and moral testimony of climate scientists.

First, I will refer to the opinions of nonexpert online climate change skeptics. Second, I will use the academic work of climate science critics Zimmerman, Pielke, and Beck to discuss the problems with the polarization of the climate change debate and climate science adopting the "linear model of expertise," specifically the assumptions embedded in this model. Zimmerman argues that the climate change debate has been polarized, both sides demonizing and vilifying each other. Then I will move to discuss both Roger A. Pielke, Jr.'s critique of the linear model of expertise and the similar critique made by Silke Beck regarding the role of the IPCC. Their discussions of the trouble with climate science—specifically the IPCC in that it often operates from a linear model of expertise—are highly useful for understanding nonexpert-expert relationships in terms of trust and dependency, as well as understanding why some people are skeptical of climate experts. Additionally, I will illustrate what is meant by the politicization of climate science with a discussion of the University of East Anglia email scandal commonly known as "Climategate" and consider why the risk of politicization negatively affects the trustworthiness of expert opinion on climate change consensus. Lastly, I will consider the

suggestions made by Beck on alternative ways for science to address public policy needs without relying on the linear model of expertise.

The purpose of this chapter is to discuss a variety of concerns that have been raised regarding the relationship between science and politics which have risked the public's view of expertise regarding anthropogenic climate change. It is important to understand the concerns that Pielke raises regarding the politicization of science, which I will then contrast with Douglas' assertion that there is an important role for values in scientific enquiry. Distinct from Pielke's understanding of the politicization of science, Douglas presents the role for values in science as neither good nor bad; instead, she divides between the acceptable, indirect role for values in scientific enquiry and the unacceptable, direct role for values in scientific enquiry. Values will always be present in science, but we ought to be explicit about their positioning to avoid allowing science to become politicized.

2.1: The Picture of a Climate Change Denier

Recalling the warning given by Garrard, Goodbody, Handley, and Posthumus, the stereotypical image of the climate change denier is easy to conjure. Coupled alongside Holocaust denial, claims that the moon landing was faked, or that the Earth is flat, the image of a climate change denier is that of unstable, lunatic ramblings. However, I hope to have parsed this image into a variety of perspectives on skepticism that range from "it's all made up" to "what is really going on with the claim that we can mitigate climate change with a tax?" Many of these perspectives have been clustered together under the single heading "denier." In this section, I will present the more outrageous claims found online, to illustrate a comparison between these claims and those made by philosophers critiquing climate science.

These arguments are difficult to counter because there is little evidence in support of these conclusions. I think these climate change conspiracy theories are not worth considering, as do many others. In *Climate Change Denial: Heads in the Sand*, Haydn Washington and John Cook argue that "Refusing to accept the overwhelming 'preponderance of evidence' is not skepticism, it is denial and should be called by its true name" (2). Believing that climate scientists make things up for a living, that they are invested in a tax scam, that they use pseudoscience as a fear mongering technique, or that they are funded by "Green initiatives" who are looking to increase their profit margins are unfounded claims. Washington and Cook cite sociologist Eviator Zerubavel, who writes that "'denial is inherently *delusional* and inevitably distorts one's sense of reality'" (qtd. in Washington and Cook 3). The large conspiracies that are raved about in online forums and on social media are rarely rooted in evidence, in expert opinion, or any type of proof. These opinions are not really the concern of this thesis; instead I am looking to dispel this image of denier in order to parse the actual subtler range of skepticisms regarding the empirical and normative claims of the orthodox view of anthropogenic climate change. Without wading too far into the *#climatehoax* depths of Twitter, the claim that environmental science is derived from money and manipulated by government interests is easy to find. Here I will present a selection of tweets and blog excerpts to provide an image of what far fringe skeptics have perceived as the "bad science" of climate science.

On 3 January 2016, economist Alan John Moran posted to his twitter feed: "The debacle of climate science — PC "climate scientists" funded by govt. make things up for a living" (@alan_john_moran). Here, Moran is not just suggesting but outright claiming that the climate data is made up, and that the government is paying these scientists to make up their predictions. This is a claim I keep coming across: that the government wants the public to believe in climate

change, and that they are paying scientists to fake their data to encourage this fear. Another example of those who believe that the science is influenced by politics and vested interests, a twitter used by the handle @MrGmoneygp tweeted to the Canadian Minister of the Environment the following: “Can’t wait till your [sic] out in 2019 and #climatehoax and #carbontax scam can be stopped #taxscam” (@MrGmoneygp). The climate hoax that @MrGmoneygp is referring to is the idea that climate change has been invented as a theory and a threat by the government to increase taxes in the form of a carbon tax.

On 31 of October 2017, twitter user @JWspry tweeted, “MORE rampant #globalwarming fear-mongering to scare the gullible into belief. Failed #climate models are proof of nothing other than pseudoscience and propaganda. #climate #UN #WEF SCAM #Auspol” (@JWspry), also indicating his lack of belief in the “purity” of climate science. @JWspry claims that climate models are propaganda and a government funded conspiracy. He claims that the models prove nothing and are failures. We can see the belief that the government is interfering here again, trying to induce fear.

An extensive blog kept by JoNova, the author of *The Skeptic's Handbook* and a self-described science presenter, writer, and speaker, details many of the arguments used against climate models and evidence of anthropogenic climate change. Here she posted a flow chart titled *The Climate Change Scare Machine*. The image shows various influencing “Green” bodies that “create” climate change data for their own gains, and “duped” the “well intentioned public [to] pay for it all” (JoNova). She lists the primary stakeholder in this “false” belief in climate change as the “Industrials”: “Renewable energy, nuclear power, electric cars, batteries, hydroelectric, geothermal, desalination plants” (JoNova). She claims (without sources) that renewable energy has received $243 billion in 2010, that nuclear energy is “valued at $217

billion" (JoNova) in 2010, and that the solar PV market was worth $80 billion in 2010. With arrow symbols, she illustrates the flow of money from the public purse to these green industries. Arrows move from these industries to financial houses, green foundations, green groups, smear sites, and the media. With further arrows placing government funded activists, the IPCC, universities, scientists, and public broadcasters taking money from the public, indicating that the government is taking public money and funding scientists to increase the "scare" (JoNova). She claims that "$79 billion [goes] to the scare" (JoNova), while "Independent scientists' criticisms are ignored or called 'fringe', 'extremist' and 'in denial'" (JoNova).

This is a general sampling of the claims coming from climate change "deniers": government interests, large funding packages, and the politics and values of scientists producing climate change data are called into question, while also fundamentally being ignorant of or ignoring the scientific data. This small subsect of the population, who I think we can properly call "climate change deniers," claim that they *know* that scientists are paid to manipulate their data to fool the public into believing in climate change. For those of us who accept the orthodox view of anthropogenic climate change, these positions appear outlandish, conspiratorial, and foolish. These opinions are not of the majority, but rather a sampling of the fringe deniers whose image we take up when discussing climate change skepticism. Further still, there are interesting elements within these claims that ought to be pulled apart. Recalling the list of claims that comprise the orthodox view of anthropogenic climate change, the opinions shown above through "tweets" claim that climate scientists have partisan political views which influence the quality of their scientific findings. These skeptics are claiming that scientists want the public to believe that something ought to be done about climate change [claim 4]. Thus, they are working backwards from this claim, and manipulating their empirical data [claims 1 and 2] to fit these political goals.

Rather than moving from empirical findings to political outcomes, these skeptics claim that scientists begin with political motivations and doctor their science to manipulate public opinion towards their own partisan views on climate change.

Washington and Cook claim that the irrational and emotional disagreement with climate scientists is to "assume all those scientists are involved in a *vast conspiracy to deceive*" (their emphasis, 44). While there is nothing inherently wrong with conspiracy theories as epistemological positions,[19] the claims of these fringe "climate change deniers" border on an epistemically flawed, denial-based argument in which they refuse any evidence that might be contrary to their belief. Problematically, however, Washington and Cook situate this type of denial in the East Anglia email scandal: in the "Climategate" scandal, several prominent environmental science researchers' emails were hacked and released to the public, giving evidence that politics might have been informing the science coming out of a prominent environmental research facility. Washington and Cook write about the East Anglia situation: "a few suggestive quotes taken out of context [which] may serve as a distraction for those wishing to avoid the physical realities of climate change, but they change nothing about our scientific understanding of humanity's role in global warming" (45). Pielke and Beck will demonstrate that there is more to be learned from the "Climategate" scandal than simply the status of denial; there are pressing philosophical concerns regarding the politicization of climate science that are beyond the claims that nonexpert deniers make on Twitter, like #climatehoax. The arguments of

[19] There is an interesting discussion regarding the negative attitudes we take towards both conspiracy theories and conspiracy theorists in Coady's chapter "Conspiracy Theories and Conspiracy Theorists" in *What to Believe Now*. In this chapter, Coady rejects the claim that conspiracy theories are in themselves illogical and epistemically irresponsible. Instead he argues that conspiracies happen often, tend to be significant, only are known to the public when they fail, don't tend to be exposed, and they still occur within an open society. Rather than considering conspiracy theories as logically flawed, we ought to consider them epistemically neutral. Importantly he writes: "We cannot stop power for being abused just by investigating and exposing conspiracies. But we also cannot stop power from being abused if we ignore the fact that much of that abuse is, and probably always will be, conspiratorial" (Coady, *What to Believe Now*, 130).

Zimmerman, Pielke, and Beck illustrate that there are epistemological implications of the politicization of climate science (for example, "Climategate") which can negatively affect both public policy and the public's opinion of climate science.

2.2: Expert Critiques of Climate Science

In this section, I will discuss Michael E. Zimmerman's defense of his own (skeptical) climate change stance against what he calls the *demonization* of those who don't conform to the orthodox view of anthropogenic climate change. This demonization he roots in the polemical and polarized nature of the debate regarding climate change. His framing of the argument between "moderns" and the "Greens" shows how simplistically the stereotyping on either side is blending many issues into a false dichotomy of "good" believers and "evil" nonbelievers. This polarization has led both sides of the debate to overly politicize the debate, which "inadvertently invites the public [nonexperts] to conclude that legitimate scientific research is just another tool used by lobbyists to promote special interests" (Zimmerman 1).

Zimmerman's use of the term *Green movement* highlights the problematic nature of polarizing the debate over climate change. He writes that he began to see the limitations of "Green thinking" (5) because of what Stewart Brand calls an "'indifference to human starvation'" (5). That is, Zimmerman's characterization of the Green perspective is a regard for "all life on Earth as equally important" (5), which Zimmerman claims leads the way for some Greens to conclude that a decline in human population would not be a bad thing. The Green perspective is multi-faceted and fractured, and Zimmerman's perspective seems to be a stereotypical view of a large group of people holding similar and different views regarding what ought to be done about climate change.

I take the other side of the debate characterized by Zimmerman to be under the heading *moderns*. He writes that on the other side of the Green perspective is the modern's perspective that tends to "overlook the suffering and destruction that modern economies have brought to non-human beings" (5). The main shortcoming of this worldview is that, according to Zimmerman, it "has been treating nature solely as a resource bank for humans" (5). In other words, Zimmerman believes the nuanced debate regarding what ought to be done about climate change has been fractured into two sides: the Greens (environmentalists) and the moderns (those who might not be as willing to make changes to their behaviour to potentially better the environment).

One of the most glaring instances of climate science politicization is the tendency for those who believe in the orthodox view of anthropogenic climate change to vilify or demonize those who do not agree with their perspective. The valuation of only certain belief sets—for instance, belief in climate change is valued as good and skepticism of climate change is valued as bad or evil—adds a political, value-based element to empirical research which is damaging to the debate. This polarization extends not only to the fringe opinions of which I offered some examples above but includes academic critiques of climate science. Zimmerman, an environmental philosopher, has been subject to this kind of labelling. He is concerned that this overzealous demonization of anyone who does not agree with the Green perspective risks further polarizing the debate.

Zimmerman has been considered by some to be a skeptic for his controversial critique of the Green movement and his claim that the Green movement and the science as overly politicized. In his article, "Clarifying My Views about Anthropogenic Climate Change," he addresses the claim that skeptics might take some solace in his work. He writes, "I have never

intended to give ‘aid and comfort to the enemies’ of rational discourse needed to develop a more sustainable human culture” (Zimmerman 1). Instead, through his work, he is attempting to give when he calls “a nuanced critique of the politically-correct IPCC climate view” (Zimmerman 1). This he does with the goal of moving “public discourse beyond its current polarization” (Zimmerman 1). Zimmerman appears to view the characterization of his work as unfairly negative. He attributes negative perspectives on his work to his controversial unwillingness to “accept at face value the claim that climate science is ‘settled’” (Zimmerman 3) and his rejection of the claims that the *one* cause of climate change is burning fossil fuels. He writes that “This might be enough for some to brand me a ‘skeptic’ about climate change, but such an attribution is misguided in my case” (Zimmerman 3). Zimmerman claims that his work aims to push green movements and environmental scientists to “get serious about admitting what we do know and what we don’t know about climate change” (2).

As discussed earlier, there are two ideas regarding how to respond to scientific uncertainty. Some, such as Oreskes and Cook, believe that the best way to approach the public with scientific uncertainty is to show a unified front: all scientists agree that anthropogenic climate change is happening. This unified front is intended to address and corral public confusion. Others (Pearce, et al. for example) are not concerned with “confusing” the debate. They instead push for honesty in dealing with the public, as Zimmerman is arguing for here. Zimmerman claims that he is not casting doubt on the anthropogenic causes of climate change, but instead pushing against an almost dogmatic acceptance that fossil fuels are the only cause.

The problem Zimmerman sees with current public discourse is that it has become a “vitriolic debate” between “the Greens” and “the modernists” where both sides are demonized by the other (Zimmerman 1). In the chaos of this debate, science is viewed as a politicized weapon

which reduces the public's opinion of science itself. He writes, "By politicizing climate science [using it as a political tool], people from both sides inadvertently invite the public to conclude that all legitimate scientific research is just another tool used by lobbyists to promote special interests" (Zimmerman 1). A reduction in public opinion regarding the integrity and reliability of science, not only further polarizes the debate and fosters misunderstandings on both sides of the debate, it opens the door to ignoring the problems of anthropogenic climate change and our possible moral obligations to mitigating probable threats and our continued impact on the environment.

Further, Zimmerman argues that the Green movement ought to create "*the context needed to respectfully take into account valuable insights from both the Green and the modern perspectives*" (his emphasis, 2). Asking climate science to take seriously the attitudes and perspectives of those who disagree with them is not the approach the Green side tends to take. However, Zimmerman claims that the only way to create a more productive debate is to meet the public who do not believe and address the controversy in different ways. His solution to this polarization is to use integral methodological pluralism and what he calls "Integral Ecology." He proposes that the Green movement can be more effective through an integral perspective which "investigates developmental differences among individuals and cultures to devise effective ways of framing the climate change issue for different audiences" (Zimmerman 2). He pushes further, not limiting the scope of public discourse to those who have yet to decide. The vision that Zimmerman has is that Integral ecologists "have an obligation to speak to moderns in a way that first acknowledges modernity's noble contributions, before directly or indirectly criticizing modernity for its environmentally destructive practices" (6). Meeting moderns halfway is not a path that Greens tend to be willing to take. For instance, Canadian Environment Minister

Catherine McKenna said in an interview with Evan Soloman, on CTV in April 2018, that she has no time for people who don't believe in climate change. This is surprising since recent numbers have shown that as many as forty percent of Canadians don't accept that climate change is happening, is human caused, and is a large threat (Anderson).

Zimmerman does not limit this mediation to only the ecology side of things. He writes that "Integral ecologists also have the opportunity to invite moderns to move beyond their often self-congratulatory stance, and to take into account modernity's dark side, including regarding nature as having value solely as an instrument for human ends" (Zimmerman 6). Both sides ought to meet in the middle and "appreciate the constructive contributions of their respective worldviews, so that pointless antagonisms do not get in the way of pragmatic solutions that respect the need both for social justice and for environmental well being" (Zimmerman 8).

Zimmerman's overall critique of climate science is that it has been "politicized in a way that threatens to undermine the integrity of natural science" (4). The division between the moderns and the Greens is becoming more problematic as the divide widens, and the climate science in the middle is at risk of being undermined by both the unwillingness of the climate scientists to admit what they don't know, and by the Green's unwillingness to consider the modernist perspective, instead vilifying it. The insertion of morality—the valuation of the side we like as good and the other side as evil—is a type of politicking that might be undermining public trust in climate science. I will continue to consider this perspective from the work of Roger A. Pielke, Jr. who is also concerned with the politicizing of science. His critique is that climate science has developed a linear model of expertise in which the science side of things has morphed into a political debate regarding policy issues.

2.3: The Linear Model of Expertise

In "When Scientists Politicize Science: Making Sense of Controversy Over *the Skeptical Environmentalist*," Pielke argues that the politicization of climate science is a threat to creating effective public policy. By *politicized*, Pielke means that science is being used by scientists "as a means of negotiating for desired political outcomes" (405) and he argues that "scientists increasingly seem to be equating particular scientific findings with political and ideological perspectives" (405). Rather than demonizing those in disagreement, Pielke sees scientists becoming integral players in politically charged games of public policy and interference with global governance. The risk of allowing scientists to operate from their own political and ideological perspectives is that, "If scientists evaluate the research findings of their peers on the basis of political perspectives, then 'scientific' debate among academics risks morphing into political debates" (Pielke 405). This morphing risks both the academic integrity of the science and the public's view of climate science.

I present Pielke and Beck's arguments here to provide evidence for the claim that politicization is bad, and juxtapose this important consideration to my general concluding argument that values can and do hold an appropriate role in climate science. In other words, there are inappropriate roles for values in science—understood through Pielke's concerns about politicization, Pielke and Beck's concern with the linear model of expertise, and Douglas' warning against the inappropriate role that values can play within scientific enquiry (which will be discussed in Chapter 3)—but this does not preclude the appropriate and necessary role for values in science.

An example Pielke provides of politicization in science is the stacking of health advisory panels by President George W. Bush in 2002. Pielke writes that it appeared that President Bush

was choosing scientists "more for their political views than their scientific credentials" (406). In other words, a scientific panel, supposedly formed to advise based on their expertise, was formed also based on political partisanship. The charge is that Bush chose scientists based on their willingness to agree with his political stance. In this case, "rather than seeking to understand the significance of science in the context of specific policy alternatives, these committees would instead focus on the political challenge of bolstering support for decisions already made, presumably based on factors other than science, e.g., ideology" (Pielke 406). As already discussed in Coady's assessment of which experts to believe, sometimes vested interests result in outright lying by the experts, and sometimes vested interests result in implicit biases directly affecting the process of knowledge directly.

This is already the case, according to Pielke, in a great deal of climate research. He writes that "From the perspective of the public or policy makers, scientific debate and political debate on many environmental issues already have become indistinguishable" (Pielke 405). The results of this conflation are disastrous, according to Pielke, for those who want science to have a meaningful and supportive impact on both the public's understanding of anthropogenic climate change and the role of good science in policy creation. Pielke has three main concerns regarding the politicization of climate scientists: the conflation of politics and science limit science's creative uses, the implementation of political aims in climate research can damage both the science and the policy produced, and the public and policy makers have come to view politicized science as a marketing tool rather than empirically-based research.

Pielke claims that science reaches this point of politicization through a misinterpretation of the role and ability of science to determine public policy through the *linear model of expertise*. Pielke writes, "The politicization of science by scientists is rooted in a 'linear model'—get the

fact right, then act—of science's relation to society" (406). But he argues that science and policy are not that simple. Effective policy does not simply emerge from scientific data; "the task of political advocacy necessarily involves considerations that go well beyond science" (Pielke 406). This "get-the-facts-then-act model" (Pielke 406) ignores many of the considerations that other forms of expertise, like political, economic, social experts, might be more attuned to than science. Pielke claims that this view of science is wrong because it "assumes that science can and should compel political outcomes" (408). He claims that it is "an undesirable approach to the relation of science and decision-making because of the ample evidence showing that policy does not simply emerge from scientific understanding" (Pielke 409). One policy does not simply emerge as the end result of scientific research.

If we consider the orthodox view of anthropogenic climate change, the linear model of expertise allows the facts of the matter to be established [claims 1 and 2] and then moves directly into values and politics [claims 3 and 4]. For Pielke, "Science thus becomes a convenient and necessary means for removing certain options from a debate without explicitly dealing with disputes over values" (409). The move from fact to values eliminates debate regarding the important nuances in the space between. This is an error, according to Pielke, "because the linear model in fact fails to accurately describe the relationship between science and political outcomes, it may simply mask normative disputes in the language of science, to the possible detriment of both science and policy" (409). The lines between empirical and political are collapsed in the linear model. The move from establishing empirical fact to policy implementations is automatic. The concern Pielke raises is that the implicit link between fact and policy results in debating fact rather than policy, when determining what ought to be done.

Pielke explains that the relationship between empirical and political claims is not this simple; they are not two sides of the same coin. Contra to the linear model of expertise, Pielke claims that science does not equal politics. In other words, debates within the science do not determine the state of the policy, and the consensus on the science does not mean consensus on policy. Pielke writes, "Disagreement on science does not preclude consensus on action, and general agreement on science does not preclude opposing views on action" (Pielke 410). For example, in the case of the orthodox view of anthropogenic climate change, simply because someone might believe that climate change is happening [claim 1], it does not follow that the same person would support a carbon tax in Ontario [a possibility stemming from claim 4]. Equally he might believe that something ought to be done to mitigate climate change, but might not believe that the science is complete in terms of determining what the leading human cause of increased global temperatures is. For instance, Pielke is skeptical that the only major cause of increased global temperatures is burning fossil fuels. This does not mean he intends to debate whether global temperatures are rising. For Pielke, the linear model assumes that, from the empirical data, political implications immediately following; however, Pielke insists that this is not the clear relation between science and policy.

In Silke Beck's chapter from *Environmental Governance: The Challenge of Legitimacy and Effectiveness*, titled "From Truth to Trust: Lessons from 'Climategate,'" he expands upon this notion of the "linear model of expertise" (220). He writes that the linear model of expertise frames science as compelling action (Beck 222) but rejects that we ought to treat science as a trigger for good and effective public policy. For Beck, it is not true that if science first gets the facts right, the right policy will be revealed. Beck writes that "the influence of science on policy

is assumed to be strong and deterministic" (222) and is driven by the idea that sound science ought to have "an immediate, direct impact on policy" (223).

However, Beck argues that viewing science as a "trigger" for policy decision-making is far too simple; he writes, "Policy does not emerge in a simple, straightforward way from scientific understandings" (223). He claims that the role of the IPCC[20] is not to determine public policy decision-making on a global scale; its role is not to compel action. Instead he sees the IPCC's role as a body intended "to develop and spread heuristic knowledge about global change and to inform policy-makers about the general extent and structure of the problem" (Beck 224). The linear model, according to Beck, assumes that good and sound science ought to have more immediate and direct impacts on public policy. The problems with the linear model are that it situates science as the centre of policy-making, it ignores other important factors such as justice and economics, and its own fallibility. Beck writes that 'Given that climate is usually only one factor among many other factors in decision-making, the linear model of expertise is significantly flawed" (232). When making policy decisions, other considerations must be made: for instance, economics, justice, social stability, and so forth. Effective policy does not *only* emerge from the hard sciences done well.

For example, Beck claims that there are working groups within the IPCC, namely Working Group III, who have been "accused of systemically portraying climate policy as more straightforward and cheaper than can be responsibly concluded on the basis of academic research" (229). In other words, the IPCC are being overly forceful with their political prescription "based on the unstated assumption that science compels action on climate change" (Beck 229). They are no longer simply improving the way the world is understood, but also

[20] Understood here as a body of the foremost expert climate scientists who have strong impacts on international policies on climate change.

attempting to compel action. The effect of this linear model of understanding expertise, as coming out of the IPCC, is that "scientific and political statements become inseparable" (Beck 229), and thus, according to Beck, the statements coming out of the IPCC are undesirable (Beck 229).

Pielke makes similar conclusions about the errors of the linear model of expertise but is more concerned about public perceptions of climate science. He writes that, when policy outcomes are presented as if they are compelled by the science, this both diminishes the quality of the science and the public's view of the science. Pielke's concern with the use of science as a political tool is that "when scientists seek political outcomes through science, it can arguably limit the positive contributions that science undoubtedly can and should make to policy development" (412). For instance, Pielke claims that the implementation of the linear model of expertise in the debate over climate change limits our possible courses of action. He writes, "Action is typically narrowly defined as the Kyoto Protocol and the political stakes are victory in either securing or denying its implementation. Under the linear model both sides argue about science as a proxy for actually discussing the worth and practicality of possible alternative courses of action, of which the Kyoto Protocol[21] is but one of many" (Pielke 409). The

[21] The Kyoto Protocol, as outlined on the United Nations Framework Convention on Climate Change webpage, is an international agreement to follow international guidelines for reducing emissions to mitigate climate change. The intention of the protocol is to stabilize GHG emissions and "provide the architecture for further international agreement on climate change" (UNFCCC). The first commitment was limited to reducing emissions of carbon dioxide, methane, nitrous oxide, hydrofluorocarbons, perfluorocarbons, and sulphur hexafluoride (UNFCCC). These reductions are met through both national measures and three "market-based mechanisms": International Emissions Trading, the Clean Development Mechanism, and Joint Implementation (UNFCCC). International Emissions Trading allowed countries who didn't meet their maximum emissions to sell their units to other countries. The Clean Development Mechanism allowed committed nations to "earn saleable certified emission reduction (CER) credits" (UNFCCC) by implementing emission reduction projects in developing countries, which are counted for meeting the Kyoto targets. The Joint Implementation mechanism allows one nation committed to the Kyoto Protocol to earn emissions credits by assisting another nation also committed to the protocol in emission reducing projects.

assumption that the science points directly to the actions outlined in the Kyoto Protocol limits the other contributions available.

Pielke concludes that "There is no magic bullet or panacea for the challenges presented by the politicization of science by scientists. And perhaps worst of all would be a withdrawal of the scientific community from involvement in contested political issues, as was historically the case when scientists sought to be 'value free' and removed from practical concern" (Pielke 415). He appears to understand the integral values of science in politics but warns that going too far would be dangerous. If the science and the politics become too intertwined, Pielke writes, the public and the policy makers will no longer consider the work of science to be objectively reliable. Politics and science, both Pielke and Beck claim, should not be too conflated that they are indistinguishable because it would reduce and has reduced the public's understanding of science and their trust in scientists and accompanying expert testimony.

For the purposes of this thesis, Pielke and Beck's concern regarding the politicization of climate science, that is, climate science being used as a political tool, is an example of the inappropriate role values can play in science and the inappropriate way that climate science can be used to argue for normative claims. While some critics of climate science have been labelled skeptics, I think that the concerns regarding the politicization make this labelling inaccurate.

2.4: The Scandal of Climategate

One of the most prominent debates regarding the politicization of climate science is the email scandal at the University of East Anglia commonly referred to as "Climategate." A closer look at what happened at East Anglia might illustrate better the effects of the public seeing climate science as a manipulated political tool.

In “Climategate: The Role of the Social Sciences,” Myanna Lahsen provides what seems to be a balanced account of the University of East Anglia “Climategate’ incident. She writes that, in 2009, thousands of emails from the Climatic Research Unit at the University of East Anglia were either leaked or stolen. Lahsen writes that these emails offered “a rare, uncensored glimpse into the fractious world of climate science” (546). The more concerning emails were between what she calls scientific leaders from within the IPCC, relaying what appear to be “possible holes in key evidence upholding concern about ACC [anthropogenic climate change], and expressing desires to deny countervailing science legitimacy, weight, and public visibility, as well as strategies to ensure that goal” (Lahsen 546). These emails have been considered evidence for various types of claims, ranging from the perspective that climate science is overly politicized to outright claims that the science is being doctored and the public is being lied to.

In *Climate Change Denial: Heads in the Sand*, Washington and Cook[22] provide a selection of excerpts from the emails hacked at East Anglia. The first email was sent from Phil Jones’, which reads as follows: “‘I’ve just completed Mike’s Nature trick of adding in the real tempts to each series for the last 20 years (i.e. from 1981 onwards) and from 1961 for Keith’s to hide the decline’” (qtd. In Washington and Cook 44). The other email that Washington and Cook use as an example is Kevin Trenberth’s email. He wrote: “The fact is that we can’t account for the lack of warming at the moment and it is a travesty that we can’t” (qtd. in Washington and Cook 45). Both seem to provide evidence that climate scientists are using tricks to manipulate their data, and find it difficult to account for (or perhaps mask) a stall in global warming, although there are ongoing arguments regarding what exactly was meant in these emails.

[22] Washington and Cook provide an interesting defense of these emails, which ought to be considered when trying to determine what to believe about the East Anglia scandal.

Lahsen writes that "there was a well-organized PR campaign ready to go at the time the emails were released, aimed at shaping public perceptions of ACC and undermine efforts to reduce global emissions of greenhouse gases" (546). She also writes that the "charges of corruption and conspiracy did not hold up to close analysis" (Lahsen 546) and that these emails failed to disprove the long-understood effect of greenhouse gases on global temperatures. There were numerous problems found within the emails, although the investigations into their behaviour resulted in no findings of scientific malpractice. The problematic behaviour that was identified was a lack of transparency, the exclusion of the work of some scientists based on "extra-scientific considerations," as well as a failure to "comply with the UK Freedom of Information Act" (Lahsen 547). Despite not finding any direct examples of malpractice following Climategate, it appeared through public surveys that "the controversy weakened public faith in climate science", as well as weakened belief in anthropogenic climate change, its severity, and the need for intervention (Lahsen 547). Lahsen writes that the incident "set off an international dispute over the quality and trustworthiness of research on ACC and the scientists who [produce] it" (547). Those looking for a reason to distrust the science felt justified in their (arguably) wrongly held beliefs.

There have been many discussions regarding Climategate. For present purposes, the important role that "Climategate" played in the debate regarding climate change is that it might have been a significant factor in reducing public belief in climate change. Belief in climate change, specifically belief in the dangers of anthropogenic climate change, is firmly rooted in the public's view of the trustworthiness of experts. If the public cannot trust that the experts will be either accurate or truthful, the public will not believe in anthropogenic climate change.

In an empirical study published by Anthony A. Leiserowitz, Edward W. Maibach, Connie Roser-Renouf, Nicholas Smith, and Erica Dawson based on the collected empirical data from two nationally representative surveys of the American public, there is significant statistical evidence leading to the claim that public opinion regarding climate scientists has decreased in view of the "Climategate" scandal. The authors conclude that "a significant decline in the American public's beliefs that climate change is happening, human-caused, and a serious threat, along with declines in public trust in climate science and scientists" (Leiserowitz 821) and they use these studies to examine the specific impacts of "Climategate" on this rising skepticism. The authors note that "In short, the impact of 'Climategate' on those who followed the story varied considerably and mainly affected the views of those who were ideologically predisposed to be skeptical of global warming to begin with" (Leiserowitz 826).

From the sample size, 25% of those surveyed followed the news stories about "Climategate" and 12-13% of those surveyed said that "the stories had led them to become more certain that global warming is not happening and to have less trust in scientists" (Leiserowitz 826). Further, 17% surveyed said that "the scientists involved in the scandal had either falsified their results or conspired to suppress contrary research" (Leiserowitz 826) and 16% believed that "the emails undermined the conclusion that global warming is happening" (Leiserowitz 826). The authors conclude that their research strongly suggested that the "Climategate" scandal "solidified the observed declines in public beliefs that global warming is happening, human-caused, and of serious concern. It also helps to explain the erosion of public trust in scientists as sources of information on global warming" (Leiserowitz 828). As discussed in the previous chapter, belief in expert testimony is strongly affected by trust.

The authors also note that while decreased public perception in climate scientists does follow party lines: those with more conservative political worldviews experienced a great loss of trust in scientists than those with more liberal worldviews. The results of their surveys indicated that "cultural worldviews, political ideology, and motivated reasoning play [an important role] in mediating public interpretations of and responses to global warming" (Leiserowitz 828). In other words, individualists and those with more conservative political leanings seem to be predisposed to doubting anthropogenic climate change in many different forms. Those who were previously motivated to doubt the existence of anthropogenic climate change seem to have been given new reasons for their skepticism in the "Climategate" scandal.

2.5: Theorizing A New Role for Environmental Scientists

To illustrate some of the backlash of the view of science following claims of politicization, I will reiterate the argument made by Beck in "From Truth to Trust: Lessons Learned from 'Climategate.'" This article outlines the affect that "Climategate" had on the public's view of the IPCC and provides some recommendations coming from critics in how to improve the image of the environmental organization of climate scientists. Responding to the East Anglia email scandal, Beck writes that the scandal offered new approaches for the IPCC and climate science to take that are beyond the linear model of expertise. He highlights that climate scientists ought to correct their own approach to both the public and policy relevant decisionmaking. Beck also suggests that the IPCC, in its role of increasing the public's understanding of science, ought to include an enforcement of truth and trustworthiness in its organization structure, as well as focusing not only on outcomes and products, but also on social processes. The main elements Beck suggests the IPCC ought to address are transparency regarding their limitations, openness with regards to their informed professional judgment of their scientific findings, self-reflection

regarding trustworthiness in the eyes of the public, and honesty regarding its own shortcomings.

Beck's main critique of the IPCC is that the IPCC doesn't have an accurate understanding of their role regarding climate change policy and advocacy. He suggests that perhaps the IPCC should recognize that there are a multitude of factors that enter the debate regarding the best policy measures to address climate change. Further, he writes that the IPCC ought to acknowledge the epistemological limitations of climate science and that there are many "corresponding ambiguities in the available knowledge" (Beck 232). This humility ought to "make transparent the complexity and limits of their expertise and the extent of their uncertainty" (Beck 232). De-centering the IPCC's role in determining effective policy on climate change would also remove scientific disputes from policy disputes. Beck suggests that "opening up these decision-making processes [in ways which de-centre and alleviate science] would render their primary nature more honestly political and economic, while giving proper weight to scientific reason and evidence" (Beck 232). Beck claims that the IPCC ought to get their facts correct, and deliver the facts to policymakers, rather than insisting on a policy-making role for the IPCC. His position is that science ought to be about the science, and then it can be used to make informed suggestions to policymakers. In chapter three, I will provide an alternative view of the relationship between science and politics from Latour. That is, the epistemological distinction made by Beck's assertion, that the IPCC ought to stick to their facts and only inform policymakers about these facts, inadequately divides nature and culture; science and politics.

Trust is an integral factor in influencing what we know and believe about climate change and action on climate change. Beck writes that errors in the IPCC reports and the East Anglia email scandal "appear to have contributed to the public mood of suspicion" (234). The various mistakes coming from the IPCC have raised questions in the public eye, namely, "could politics

and society still rely on the IPCC for an assessment of the scientific knowledge on climate change and could policy-makers and the public at large still trust the IPCC's key messages?" (Beck 234). According to Beck, public confidence in climate science has been waning and the effects of "Climategate" have been that public confidence in the science is significantly affected; the numerous failures of the IPCC to deliver honest, politically neutral information regarding climate change means the public does not trust the experts any longer simply because they are experts. He also suggests that the IPCC ought to address the two most significant challenges it faces: "to produce the highest quality information about climate changes, and to build public trust" (234). To have effective action on climate change, the public must believe that scientists are not trying to trick them. According to Beck, after Climategate, the major challenge to climate science and the IPCC is "to rebuild public faith in the credibility of climate science" (235).

One way that Beck suggests for the IPCC to rebuild public trust is to "commit itself to rigorous self-examination and to implement organizational learning" (236). Some of the backlash, Beck notes, comes from a lack of formal rules which results in both public misunderstandings and leaves the possibility of corruption (Beck 236). "Climategate" both has opened the doors to understanding that organizational failures of expert bodies like the IPCC and has enabled an understanding of how these expert organizations ought to work to improve their organizational structures. To increase the accountability of these bodies, Beck suggest that, first, we must understand that the robust rules of science are rarely enough to establish trust with non-expert bodies (in other words, the public). Second, these expert bodies "should be held to the norms of transparency, accountability and deliberative adequacy" (Beck 237) despite that the IPCC "is subject to none of the legal political requirements that constrain, but also legitimate, national expert committees" (Beck 237). Lastly, Beck notes that "Climategate" highlighted the

difficulties arising out of the international, multicultural elements that are necessarily entangled with intergovernmental concerns. Overall, the IPCC and other expert bodies ought to be accessible to the public so that public trust and confidence can be retained (238).

Concerns over the politicization of science are not to be undermined (both under the definition provided by Pielke and the definition provided by Douglas); as I have argued, public trust is directly related to the effectiveness of climate scientist's believability and the public's willingness to both listen and act. However, the implications of the critiques of the politicization of climate science in cases such as "Climategate" further reduces the public's feelings of justified true belief stemming from expert testimony. In the next chapter, I will look at the concession that Pielke makes: "Because science, politics, and policy are inextricably intertwined, a challenge exists for developing practical strategies for decision makers to use science effectively. Utopian views of clearly separating science from politics, facts from values are not helpful" (Pielke 406).

Going forward, I will agree mostly with Zimmerman, Pielke, and Beck: the politicization of science is detrimental to the integrity of the research, to the policies emerging from the data, and to public opinion regarding specific scientific enquiries and regarding science in general. However, it is not possible to extradite values from knowledge production, nor science from politics. Considering Latour's argument that when it comes to climate disaster, political obligations emerge immediately from empirical data, Lorraine Code's work on situated knowing and the emergence of a new epistemology, and Douglas' framing of an understanding of objectivity which does not rely on a value-free ideal of science, I will argue that concerns over the politicization of climate science are legitimate and do not preclude the necessary role of values in climate science; it is preferable that these values have an *explicit* role rather than an *implicit* role in the production of knowledge, for this belief in this knowledge to be justified.

CHAPTER THREE

3. Clarifying the Role of Values in Climate Science

In *Science, Policy, and the Value-Free Ideal*, Heather Douglas rejects the long-standing belief that science and politics can exist as separable realms of enquiry. She writes, "Science is a value-laden process" (Douglas, *Science*, 112) and it is unavoidable that values have a role in the whole process of scientific enquiry. Values— ethical, social, and cognitive values—help with knowledge productions in that they "help weigh the importance of uncertainty and to evaluate the consequences of error" (Douglas, *Science*, 112). This does not mean that there ought not to be concerns about the politicization of science; however, even accepting the arguments coming from Pielke and Beck—that climate science ought not to be politicized—I will maintain that there will always be values within scientific enquiry and we must restrict the role values are permitted to play by making them explicit. The role of values in scientific enquiry must be made explicit to restrict them and to account for them. The problem of the politicization of climate science does not extend to an elimination of all values from all scientific enquiry, and the presence of any value in climate science ought not to diminish public trust in anthropogenic climate change.

It is important to understand the concerns that critics like Zimmerman, Pielke, and Beck present: politicized science is irresponsible and untrustworthy science. Politicized science is unacceptable science both because of the inappropriate role that values have taken in the process of scientific enquiry (Douglas) and when it is used as a means of negotiating for partisan views

(Pielke). Douglas provides two examples of the unacceptable role that values can play in science: "when scientists suppress, or are asked to suppress, researching findings because the results are unpalatable or unwelcome" and "when scientists alter, or as forced to alter, the interpretations of their results because they are unwelcomed by their funders or their overseers, values again are playing an unacceptable, direct role" (Douglas, *Science*, 112). There is a significant difference in the acceptable and needed roles of values and the unacceptable and harmful role of values in scientific enquiry. That is, in appropriate roles, values help "us grapple with uncertainties" and perhaps determine the causal effects of scientific research; in other inappropriate roles, values direct the outcome of science (Douglas, *Science*, 112). As Douglas writes, "All science is value laden. But unacceptable, politicized science occurs when values are allowed to direct the empirical claims made by scientists" (*Science*, 113). This is a different stance than Pielke takes, however it is similar and will be considered as such. Pielke warns of the politicization of science in its use as a political tool; Douglas frames politicized science as science affected by the inappropriate and direct role that values can play in the doing of science.

To situate this chapter, I want to make clear what I take to be the expert critiques of climate science, which has led to some being labelled climate change skeptics. I take the arguments discussed in Chapter Two to be critiques of value-laden *politicized* science applied specifically to climate science. This is initially discussed in the first pages of Douglas' book. She writes, "Central to the concerns over the use of science in policy-making is the degree of reliability we can expect for scientific claims" (Douglas, *Science*, 1). Tracing the history of what she calls the value-free ideal of science, her view of the standard epistemology adopted by science is that "the best way to preserve the reliability of science is to keep it as far from policy as possible" (Douglas, *Science*, 3). It was long thought that ideal scientific enquiry operated in a

separate realm from policy decision-making. The pristine pursuit of empirical knowledge ought to leave behind the incompatible, "daily political squabbles" (Douglas, *Science*, 3). Any involvement in politics would threaten the sanctity of empirical enquiry. It would threaten science's objectivity. Once the boundary between objectivity and subjectivity is crossed, research findings are no longer reliable.

I am concerned however with the assumed epistemological implications of the arguments coming from Pielke, Beck and Douglas. These authors agree that policy depends on accurate scientific reason. However, to accept any relationship between policy and science, we must consider the correct role for values to play in the doing of science and in relating science to policy, which is missing from Pielke and Beck's framing of the adequate epistemology of science that is removed from politics. Pielke and Beck acknowledge that policy needs sound scientific advice; however, my concern is that efforts to mitigate the relationship between science and policy away from the linear model of expertise could move towards a removal of what Code and others call the "interestedness" of scientists. Keeping science away from policy, as suggested by Douglas in the quote above, might be a rejection of the reality of the human element of the scientists doing the work. In other words, Pielke and Beck's account of the relation between science and politics embraces the traditional epistemological stance of a removed and disinterested knower. I will argue that the important epistemic model rising out of social epistemology must be taken into account—that of knowers who are bodies situated in the world—and that their values must be accounted for, monitored, and maintained in explicit ways for scientific production to be reliable.

While Pielke and Beck may be correct—that when "science compels their favored perspective" (Pielke 405) and when "scientists increasingly seem to be equating particular

scientific findings with political and ideological perspectives" (Pielke 405) there is cause for concern—I will argue that this is not true of all values in all knowledge production. The presence of values in empirical work is unavoidable and significantly different than the claim that we ought not allow science to become politicized. Nonexperts ought to retain trust in expert opinion despite the fact that scientists are involved in policy decisionmaking and that their judgments will always involve the invocation of value-based decisions.

In this chapter, I will consider five perspectives regarding the right role of values in science to enhance our understanding of the necessary and restricted place of values in science, the risk of the politicization of climate science, and the detriment of eliminating value-based considerations from all science. First, Bruno Latour (indirectly) rejects Pielke and Beck's rejection of the linear model of expertise. He writes that once the facts of the matter are known about the causes of climate change, our ethical responsibility to act emerges. I will illustrate Latour's position, that from climate science closely follows a policy prescription which ought to be accepted, and argue that his position is potentially eliminatory of the distinctions that must be made about the relationship between empirical and normative claims. I will give consideration of his position that what is needed to address climate change is a hybrid style of enquiry that encompasses both science and values; however, I ultimately reject that we ought to take seriously his consideration that policy necessarily follow from climate science because this merges empirical and normative claims in damaging ways.

Second, the standard epistemological approach of a value-free enquiry, which climate science fails to meet, is not sufficient. Using Lorraine Code's feminist social epistemology, I will demonstrate that the *humanness* of scientists is always important and that their situatedness will be implicit in the doing of research. That is, scientists' values, beliefs, positionality, and

subjectivity does have a role (either implicit or explicit) in the doing of their science. Rather than rejecting the place of values in research, scientists must be aware of their own positioning and those of us receiving testimony from these experts must also be asking the question: where is the scientist positioned in the normative, social and political world? Code suggests that the scientist [the knower] will always be positioned within the world. Her perspective on the positionality of knowers will lead into a discussion of Douglas' stance on values. Because values will always be involved in the process of scientific enquiry, we *must* be upfront about their position rather than assume they can be removed. Values in science need to be explicitly known to be accounted for.

Third, the standard epistemological view of the value-free ideal is undesirable. Building off Code's critique of mainstream epistemology and her suggestion for a feminist social epistemology, I will use Douglas' work in *Science, Policy, and the Value-Free Ideal* and in "Rejecting the Ideal of Value-Free Science" to critique the value-free ideal as an inadequate approach to the relationship between values, politics, and scientific enquiry. In other words, value judgments and considerations must be made for scientists to produce both reliable and just science. An epistemology striving towards a value-free ideal does not and cannot account for the subjective nature of human enquiry that is always implicit in the doing of scientific research.

Fourth, Douglas argues that there are acceptable and unacceptable roles for values to take in science. These values are ethical, social, and cognitive values which must be considered in some doings of science, and must be restricted in others. The distinction between an acceptable indirect role and an unacceptable direct role of values in scientific enquiry is key to understanding that values and politics are always involved in scientific enquiry—most obviously in policy-relevant science and more implicitly in science that rarely affects policy development.

Lastly, the final consideration for the role of values in scientific enquiry is the insistence that Douglas makes regarding the definition of objectivity: rather than relying on a value-free objectivity, there are seven other ways to assess the objectivity of a scientific enquiry. These seven distinct understandings of objectivity do not require a value-free ideal. These understandings of objectivity do not eliminate the subjectivity of scientists nor do they ignore the appropriate indirect roles that values ought to hold within scientific enquiry.

3.1: The Complexity of Relating Nature to Culture and Culture to Nature

Pielke and Beck are specifically concerned about the effects of the linear model of expertise and its effect on the relationship between climate science and climate mitigation policy. They reject this "get-the-facts-then-act model" (Pielke 406) of science's role in producing or effecting politics and policy. However, Bruno Latour's lecture, "On the Instability of the (Notion of) Nature," offers a differing perspective on the relationship between science and policy. His argument frames the epistemology of policy-making and its relationship with science as a significant problem for climate scientists when the two are considered distinct realms of enquiry. Latour describes and rejects a similar epistemology to the epistemology advocated for by Pielke and Beck: an epistemology emerging from our view of the natural world as *distinct* from our creation of culture—"the 'natural world,' as everyone seems to agree, cannot dictate to humans what they must do. Between what is and what must be, there must exist a gulf that cannot be crossed?" (Latour 22). This problematic view of the natural world he calls the *default* position of *ordinary* epistemology. While Latour's view is an extreme example of alternative perspectives, his rejection of ordinary epistemology leads into the arguments presented by Code and Douglas. That is, the humanness of scientists must be accounted for when measuring what to believe.

The standard epistemological view that Latour is dismissing is one in which states of affairs must "speak 'for themselves' and one has to take endless precautions not to draw any moral conclusions from them" (Latour 22). Within this epistemology is "a very powerful moral requirement: the one according to which one must abstain completely from moral judgement if one wants to take the full measure of the reality of what is" (Latour 22). Latour argues that, under the guidelines of this ordinary epistemology, climate scientists must come to know the world as it is, and nothing more.

Latour doesn't accept this distinct "relationship" between the world as it is known and what humans ought to do. Accordingly, he argues that this epistemology has been harmful in the debate between climate scientists and climate change skeptics, and this epistemology is a misinterpretation of nature-culture relations. The idea that scientists must investigate the state of the natural world without involving *culture* fails to acknowledge how nature and culture operate as one. When we speak of our "relation to the world"—that is thinking about our situatedness in nature and our epistemological relationship in coming to know the natural world—we "presuppose two sorts of domains, that of nature and that of culture, domains that are at once distinct and impossible to separate completely" (Latour 15). We remove ourselves from nature and instead assume that we have a relationship *with* nature. However, Latour writes that this separation is impossible. He writes, "We are not dealing with domains but rather with one and the same concept divided into two parts, which turn out to be bound together, as it were, by a sturdy rubber band" (Latour 15).

The unity of culture and nature is complicated by the implementation of this difficult epistemology of sticking to the facts and removing values and politics. As Latour framed it, it is impossible to move beyond the culture-nature divide; we must understand that this divide is

inaccurate. Attempting to remove ideology from nature—allowing "states of affairs speak 'for themselves,' and [ensuring] one has to take endless precautions not to draw away any more conclusions from them" (Latour 22)—largely prevents scientists from engaging with the world as it really is: a melding of nature and culture. The division between culture and nature is particularly problematic for scientists specifically in domains of policy-relevant science, like climate science.

This division was adopted by scientists to protect the objectivity of their work, and thus protect their image as trustworthy, but it has also become their weakness within the climate change debate; that is, it has weakened their integrity as reliable experts. While climate scientists were occupied holding to the "facts" alone, those who have been accused of organizing public disbelief in climate change could easily see that from the description of climate change would immediately follow the policies that needed to be put into place. Latour writes that "Despite the distinction between facts and values that is so dear to philosophers and ethicists alike, the heads of major companies[23] under threat identified the stakes right away" (Latour 25). Latour writes that those who would suffer from climate change policy realized that "if the facts were known (CO_2 emissions are the principal source of climate change), politicians, pressed by the anxiety of the public, would immediately demand that measures be taken" (Latour 25). Implementation of policy that limit CO_2 emissions would be eminent. It would be an ethical imperative, according to Latour. This is complicated because political claims are not empirical facts. Often political solutions must mitigate many issues, not all environmental. Latour's position does not seem to engage with multiple political obstacles such as economic costs and issues of justice.

[23] For an interesting assessment on orchestrated and organized climate change denial, see *Merchants of Doubt* by Naomi Oreskes and Erik M. Conway, and "Organized Climate Change Denial" by Ryley E. Dunlap and Aaron M. McCright.

Environmental issues are not the only political issues which policies must account for and work with.

Latour attributes what he calls the best formulation of this "profound philosophy" to Republican strategist Frank Luntz, who wrote: "'the *description* of the facts is so dangerously close to the *prescription* of a policy that, to put a stop to the challenges addressed to the industrial way of life, one has to cast doubt on the facts themselves'" (Latour 25).[24] In other words, Luntz argued that because description and prescription of climate change were so intertwined, the only possible way to prevent the prescription was to call into doubt the facts of the description. Luntz wrote, "'Should the public come to believe that the scientific issues are settled...their views about global warming will change accordingly. Therefore, you need to continue to make the *lack of scientific certainty* a primary issue'" (qtd. in Latour 25).[25] In other words, once the facts about climate change emerge—claims 1 and 2 of the orthodox view of climate change—it immediately follows that something ought to be done to mitigate this change—claims 3 and 4.

Luntz advises that, "'The prescriptive charge of scientific certainties is so powerful that these [certainties] are what must be attacked first'" (qtd. in Latour 25). Those who want to maintain the status quo can only do so by calling into question the science *because* the obligation to act immediately emerges from the facts. Luntz recognized that science and policy were operating under the linear model of expertise. This is the concern that Pielke addresses in "When Scientists Politicize Science": if we logically tie policy to scientific enquiry, the only way to

[24] Latour is using Luntz in an advocacy position that Luntz himself would not have adopted: Luntz is advocating for putting a "stop to the challenges to the industrial way of life," (Latour 25) rather than advocating for a fair consideration of the science.

[25] Luntz here is invoking consensus uncertainty; the idea being, if all scientists agree, the facts are solidified. To prevent policy implementation, Luntz calls into question scientific consensus on climate change.

debate policy (or stall policy) is to debate the facts of the matter. In cases where one does not want to alter their way of life, change policy related to emissions, or curb automobile use, because policy is seen to be guided only by science, the only tool at hand is to question the validity of the science on these facts. If the prescriptive charge of science is such that it compels only one policy, the only way for Luntz to undermine the policy was to undermine the science.

Latour argues that this is the state of nature and culture: they are so intertwined that the relationship between nature and culture is not a relationship at all. Instead, it is a unity. It is the world. And we must allow scientists to operate in ways that establish the world as it is, and order the world as they are able. The belief that science is only concerned with the natural world and that politics is only concerned with "the moral, ideological, and political consequences that must be drawn—or not—from the first" (Latour 34) misconstrues the way that science and politics interact. We end up ignoring that, fundamentally, "What is *just there* is...also always what is *just*" (Latour 34). As Latour puts it more finely: "to *order* (in the sense of ordering the world) is to *order* (in the sense of giving orders)" (Latour 34).

According to Latour, the incongruous division between science (nature) and politics (culture) was adopted by scientists. The epistemological constraints of adhering to science alone, which was long intended to protect scientists and the science, Latour writes, was in fact "protecting them badly" (Latour 33, fn. 63). The long standing epistemological tradition of knowledge production has barred scientists from considering their "moral and political charge," and now the experts don't know how to handle them, "even though the implications were quite obvious" (Latour 28). Climate scientists are trapped between the important implications of their work, in the form of values, politics, judgements, and policy-making, and their epistemological framework which enforces a "view from nowhere" perspective in which scientists believe they

must remain detached from the social world in which they are conducting their work. Latour asks: "What is to be done, indeed, in the face of 'inconvenient truths' if you possess only the right of uttering them with a mechanical voice and without adding any recommendations to them?" (Latour 28). He answers: they are paralyzed.

Latour makes a specific call for scientists to accept the political and ethical dimensions that is integral to their work by explicitly engaging their responsibilities. He writes, "There is no reason for them to keep claiming that they are not in the game, as if they were speaking from nowhere and behaving as if they didn't belong to any earthbound population" (Latour 32). The fear is that this humanistic element, that is always locatable within science, undermines the research, undermines the data produced, and casts doubt upon "the quality, the objectivity, [and] the solidity of the scientific disciplines" (Latour 33). It goes against the standard tradition of epistemology. As I will explore in both Code's work in social epistemology and in Douglas' critique of the value-free ideal, this "speaking from nowhere" ideal is not ideal at all. As Latour notes, "Objectivity is neither a state of the world nor a state of mind; it is the result of a well-maintained public life" (Latour 47, fn. 14). The notion of objectivity ought not rely on value-free facts; it ought to be maintained by social, expert knowledge production.

Simply put, the idea that scientists ought to remove themselves and their politics from their work is impossible in practice. According to Latour, by enforcing a robotic role for the scientist[26] and ignoring the political implications which arise out of climate science, this has "only reinforced the confusion, since we now encounter combinations of what is and what must be at every level" (34-5). To address that situatedness of scientists is a necessary element of

[26] Latour relates this pressure to conform to the role of Mr. Spock, in his removed, unaffected mentality.

knowledge production that cannot be ignored any further in the climate change debate, I turn to Lorraine Code and her work in feminist social epistemology.

3.2: An Incomplete View from Nowhere

The claim that science ought not to invoke values or politics in its knowledge production has been the long-standing scientific ideal in both the philosophy of science and mainstream epistemology used in scientific enquiry (Douglas, *Science*). There is a staunch adherence in the tradition of mainstream epistemology to the idea that, when knowledge is produced, the knower's identity is inconsequential and where the knower is standing ought to not impact on how knowledge is gained or produced. Code's work shows that this "view from nowhere"—the removed and disinterested perspective of "real" scientists—is impossible, irrelevant, and ignorant.[27] In "Ignorance, Injustice, and the Politics of Knowledge," Code writes of traditional epistemology: "a conviction prevailed that *knowledge* worthy of the name stood uncontested and apart from such situation-specific areas of theory and practice. To suppose otherwise would endorse a pernicious descent into a relativism from which no knowledge claims could hold fast or purport to achieve truth" ("Ignorance," 149).[28] Mainstream epistemology is committed to an analysis of knowledge that is "infinitely replicable" for anyone, at anytime, anywhere (Code, "Ignorance," 149). The consequences of these assumptions about knowledge production results in a view of knowledge that limits itself to the "'generic knower'...knowing everyday medium-sized objects...abstracted from how they figure in human lives and pared down in their complexity" (Code, "Ignorance," 149). Code claims that objective knowledge is seen in mainstream epistemology as "having been produced by reason alone, uncontaminated by

[27] There are important conversations being had regarding situated knowing, originating from Donna Haraway's text "Situated Knowledges: The Science Question in Feminism and the Privilege of Partial Perspective."
[28] The question of achieving truth without a value-free ideal will be discussed with Douglas in 3.5.

emotions or particular interests" ("Ignorance," 151). In other words, traditional epistemology strikes a firm distinction between nature/facts/science and culture/values/politics.

This configuration of objectivity holds knowledge apart from emotion and values, but also "from such 'personal,' subjective features as sex, gender, race, age, class, physical ability, cultural location and other idiosyncratic details" (Code, "Ignorance," 151). These intertwining features, however, will always be implicitly affective in knowledge production. The assumption underlying this enforcement of traditional epistemology's "knowing subject" is of a fundamental assumption of neutrality, in which "power and privilege, specific interests and values must not be permitted to taint the knowledge-seeking project" (Code, "Ignorance," 150). In this framework, knowledge is simply knowledge: there is no question of how it is made, who made it, or what implications it holds. According to Code, this value-free knowledge produced nowhere by an unsituated and unaffected body is an illusion that endorses the *false* removal of values and politics from knowledge production. The consequences of a view of knowledge production coming from a disembodied and dislocated knower is a discounting of the affective values, emotions, and politics that are imbued in all knowledge production. Simply put, objective truth ("science" or "empirical fact") is not produced by disembodied, dislocated knowers, despite the claims of traditional epistemology. Assuming it is possible for knowers to be disembodied and dislocated, as assumed by the traditional framework, both discounts the ethical equalities of scientific enquiry—the moral requirements of scientists—and ignores the valuations that are implicit in many forms of knowledge production. Considering this situatedness, scientists must be aware of their political and social prejudices to begin to abstract from them. Values can be present in many different ways, and the epistemological framework that is taken up will

determine whether these values are implicit—and largely ignored—or explicit—and thus capable of being restricted.

Expecting climate science to produce knowledge that is free from values and politics is impossible and not ideal, just as Code writes that expecting knowledge to be produced from a "view from nowhere" is both impossible and not ideal. All knowledge production is already tied up in the values and politics of the knower, and there are appropriate and inappropriate roles these normative elements can play. As explored in Coady's work, the concern is that bias or vested interests not only can affect a knower's likelihood to tell the truth, they can also implicitly impact their research. Denying that values impact research is to turn a blind eye to the ways in which our *situatedness* affects the knowledge we produce. Justified belief in climate change should not suffer because the politics that emerge from the science are more visible. Accepting the visibility of the values and politics allows for a more transparent, humanistic form of scientific enquiry that is more accountable, reliable, and trustworthy.

In "Testimony, Advocacy, Ignorance," Code reflects that her theory of ecological thinking[29] "generates a revisioned engagement with knowledge, subjectivity, politics, ethics, science, citizenship, and agency that pervades and reconfigures theory and practice in multiple, intersecting projects" ("Testimony", 8). The work of ecological thinking does not shield itself from thinking *in* the world. Ecological thinking engages with the world, *both* the natural *and* the cultural world:

> "It engages with events and situations across the human and other-than-human world, in project where the dividing line between the (usually quantitative) *Naturwissenschaften* (natural sciences) and the (commonly…interpretive)

[29] Code proposed this theory in her book *Ecological Thinking*.

> *Geisteswissenschaften* (human sciences) blurs, and epistemic and ethical-political matters are reciprocally informative." (Code, "Testimony", 8)

Climate science must occur within a process of knowledge production that is jointly scientific *and* cultural, humanistic and natural. Code juxtaposes this ecological thinking against the more scientific approaches which both science and mainstream epistemology are accustomed. She writes that her theory of ecological thinking is concerned with abandoning the previous epistemological assumptions made in knowledge production.

Code describes her work as a rejection of both positivism and Quinean naturalism.[30] She rejects these epistemological frameworks in which "interchangeable observers leave their subjectivity and agency, interests and enthusiams, actions and interactions, prejudices and hopes outside the laboratory door, to isolate the disinterested epistemic practices from what Latour (2004) aptly calls 'matters of concern'" (*Testimony*, 9). This *disinterestedness* is not possible. Scientists are concerned; they ought to acknowledge their own subjective, prejudiced, enthusiastic state of being. Code describes a new perspective for *knowers* that is, "politically engaged, accountable for the knowledge it produces, often explicitly and unabashedly conducting its enquiries in the service of values, commitments, agendas, and political programmes that have themselves to be kept open to critical, deliberative evaluation" (*Testimony*, 9). In these cases, knowers are *active* participants in the production and testing of knowledge, and they are engaged with culturally, ethically, and politically significant matters. They cannot be isolated from the research or the output, "even when they appeal to laboratory-derived and other forms of empirical/experimental evidence for pieces of it" (Code, *Testimony*,

[30] The details regarding her critique of ordinary epistemology's traditions—for instance, positivism and naturalism—are largely unimportant here. What is important is Code's rejection of the requirement for scientists or any type of knowledge producers to be unsituated and disinterested.

9). Code has proposed an ecological naturalism where nature and culture produce "good" objective knowledge.

The lingering concern is that this acceptance of a value-infused, political entity threatens objective knowledge production, sometimes justifiably. The threat is that to allow scientists to be situated in the world "would endorse a pernicious descent into a relativism from which no knowledge claims could hold fast or purport to achieve truth" (Code, "Ignorance," 149). However, this is not the case. In the coming sections I will reject this concern and maintain that objective truth is still possible without an insistence on traditional epistemological divisions between facts and values (Douglas), between scientist and society (Code), and between nature and culture (Latour). Using Douglas' description of the value-free ideal and subsequent rejection of its place within scientific enquiry, I will argue that value-laden science can produce objective knowledge that is reliable; thus, public trust in the experts regarding anthropogenic climate change is still justified.

3.3: The Value-Free Ideal of Science

In line with Code's rejection of the "view from nowhere" ideology in traditional epistemology, Douglas rejects a similar epistemological standpoint as "the isolated view" of scientists as the groundwork for the *value-free ideal* (Douglas 61). In *Science, Policy and the Value-Free Ideal*, Douglas critiques the value-free ideal as an inadequate approach to the nuances of scientific enquiry relating to values and politics in science: value judgments and considerations which must be made in order for scientists to produce both reliable and just science. Douglas gives a thorough and considered perspective on the value of values in science and rejects the longstanding ideal of a value-free science. She writes that, concerning values, scientists ought to be held to the same moral accountability as the general population; that they are doing empirical

research does not excuse them from the responsibility to consider the "potential social and ethical consequences of error in their work" (Douglas, *Science*, 87). To make these considerations, scientists *must* invoke ethical, social and cognitive values, but the role that these values play must be limited in order to maintain scientific integrity.

In Douglas' epilogue to *Science, Policy, and the Value-Free Ideal*, she writes, "Even in rejecting the value-free ideal, we would be foolish to allow values to serve in the same role as evidence throughout the scientific process" (*Science*, 175). Permitting values to take the place of evidence would undermine the integrity of the science and no longer would trust in science be justified. However, she maintains that it is possible to differentiate "between the roles values play in science, and [restrict] values to the indirect role at key epistemic moments in science" (Douglas, *Science*, 175). In fact, we must make these differentiations because "We will always need scientists to interpret their data, to make clear statements about uncertainties, and to clarify what is at stake in our subsequent decisions" (Douglas, *Science*, 175). In this section, I will consider what the value-free ideal is and why Douglas rejects it. I will illustrate the specific roles which values ought to play in science and I will also provide reasons for constricting and evaluating values in science to prevent misuse. Useful for this thesis, Douglas argues that scientists have a moral requirement to use values in their work. Additionally, values can have an integral, direct, and indirect role to play in knowledge production which requires examination of "how values *should* play a role in science and of what the structure of values in science *should* be" (Douglas, *Science*, 87).

Douglas traces the origins of the value-free ideal from Francis Bacon in the seventeenth century—in his discussion of the connection between science and the state (Douglas, *Science*, 46)—through the first traces of these ideals in Robert Merton's "The Normative Structure of

Science" from 1942—which limited science to "universalism, organized skepticism, communalism, and disinterestedness" (Douglas, *Science*, 46)—to philosophers of the 1940s, 1950s, and 1960s, who debated the role of values in science, many arguing that for science to be considered good science, it must avoid all value judgments not specifically related to knowledge production. For instance, in 1951, Hans Reichenbach concluded that "'knowledge does not include any normative parts and therefore does not lend itself to an interpretation of ethics'" (Douglas, *Science*, 48). Others argued that there were specific needs for science to invoke value judgments and even play a role in advising and overseeing policy decision-making. The battle between those who accepted the value-free ideal and those who rejected the ideal revolved around the concept of the *science advisor*, in which the scientist would play an integral role in advising politics. That is, the scientist would be positioned as both a "public advisor and decisionmaker" (Douglas, *Science*, 49). In this role, ethical reasoning is integral to the scientific process. Douglas writes that, "one might imagine that philosophers of science would illuminate [the role of science advisor], examining the place of expertise in a democracy and helping to shape public discussion of the proper relationship between science and society" (*Science*, 44). However, this has not been the case; rather than accept the role of values in science, the position of science advisor was rejected. She writes that questions regarding "how to understand science in society, the role of social values in science, and the responsibilities of scientists" (Douglas, *Science*, 44) were largely excluded from epistemological enquiry.

Following decades of debate regarding the scientist's role in society and the role of values in science, the value-free ideal took hold in the 1960s (Douglas, *Science*, 49), with arguments like the arguments of Isaac Levi who argued that "scientists should utilize only 'epistemic values' in their judgments of whether there is sufficient evidence for accepting a

hypothesis" (cited in Douglas, *Science*, 55). To be more specific, Levi's perspective was, "If the sole goal of science is 'to replace doubt with true belief,' then 'epistemic values' (such as simplicity, explanatory power, and scope) are sufficient for setting decision criteria for scientists, and scientists should not go beyond those values" (Levi, qtd. in Douglas, *Science*, 55). The implication of this statement is that, if other non-epistemic values were to be included in scientific decision-making, *true belief* would be replaced with *doubt*—we would lose our certainty about scientific findings. This ideal was embraced by philosophers of science because, according to Douglas, "they could more carefully demarcate the boundaries of the newly forming discipline of philosophy of science while insulating their nascent discipline from political pressures" (*Science*, 55). There were numerous philosophers who rejected this ideal or were more ambivalent about it than others (Douglas, *Science*); however, the ideal was mostly adopted within the scientific enterprise. The crucial step into this ideal, Douglas writes, was to view "the scientific community as demarcated and isolated from the surrounding society" (*Science*, 60). The value-free ideal is based on the assumption that "scientists are not involved in public life, that they provide no crucial advisory functions, and that they provide no assistance for decision-making" (Douglas, *Science*, 45). Scientists are assumed, under the value-free ideal, to be isolated from society.

Similar to Code, Douglas rejects that this ideal is possible. She writes, "It does not hold that science is a completely value-free enterprise, acknowledging that social and ethical values help to direct the particular projects scientist undertake, and that scientists as humans cannot completely eliminate other value judgements" (*Science*, 45). The value-free ideal is not possible to achieve. If we insist that all non-epistemic values be removed from science, scientists will not be able to make moral judgments or be held morally responsible for ignoring value-based

considerations under the ideal of value-free science; and, if we insist that scientists are fully morally responsible for the possible errors within their world or moral issuing stemming from their work, we must allow for value-judgments to be included in the considerations that scientists are making. We must be reflective of our values in the doing of science.

3.4: The Moral Responsibilities of Scientists

For Douglas, scientists should not be held to a different moral standard than the public. Every person, the scientist included, has a moral responsibility to consider the consequences of their work. These considerations necessarily involve value-based judgments; values must be consulted when beginning a scientific pursuit. While we ought not expect scientists to be able to foresee every difficulty arising from their work, "we should expect reasonable foresight and care from our scientists" (Douglas, *Science*, 66) and further still, "scientific isolation from moral responsibility [is] unwarranted and undesirable" (Douglas, *Science*, 90). Going forward in this section, I will examine the value judgments that come into play in scientific enquiry and the roles they can play, both acceptable and unacceptable, in producing scientific outcomes.

There is generally a division drawn between acceptable values in science and unacceptable values in science which is often framed as the division between epistemic values and non-epistemic values. Douglas rejects that this division ought to carry weight; she instead places weight on the division between the appropriate roles values in science ought to play and the inappropriate roles that values might take in scientific enquiry. She argues that the effect of non-epistemic values on epistemic values means the divide is an illusion. She pushes further, claiming that what have commonly been called epistemic values are instead scientific criteria for doing "good science," and should not be considered values at all. Instead there are three types of values which play a role in scientific enquiry: *ethical*, *social*, and *cognitive* values. These three

types, while neither acceptable nor unacceptable intrinsically, can hold both acceptable and unacceptable roles in scientific enquiry. It is possible to misuse values in science, as explored earlier with Pielke and Beck. There is a threat to the reliability, accuracy, and the dependability of research *if* values can be shown to have played an inappropriate role. For instance, Douglas writes, "values are not evidence" (*Science*, 87) and there must be constraints on the ways in which values are used within science. However, there are acceptable and necessary roles for values to play, as well. The necessity of these values and how they shape scientific enquiry cannot be ignored.

The first appropriate role for values in science is in the initial stages; the initial impetus to begin *doing science* starts with a value-based decision or interest. Generally, the decision is that the enquiry is meaningful. Douglas writes that these judgments are often made "under constraints of ethical acceptability, resource limitations, and skill sets" (*Science*, 88). These judgments are based in values, and must be made *before* the enquiry begins. Additionally, these values all can "serve different goals and thus perform a different function in science, providing guidance at points of judgment when *doing* science" (Douglas, *Science*, 94). The important term here is *guidance*, not to be confused with guide or direct. As Douglas writes, values cannot serve as evidence; instead they shape the ways in which the evidence is viewed.

According to Douglas, there are three kinds of values in science: ethical, social, and cognitive. Ethical values are determinations about what is good and what is right. She writes that, "Ethical values help us weigh whether potential benefits are worth potential harms, whether some harms are worth no price, and whether some harms are more egregious than others" (Douglas, *Science*, 91-2). The decision to begin a scientific study must begin by weighing various elements and possible outcomes and determining if these are good and just enough to

pursue the study. Social values are culturally based, within the society and emerge through concepts such as "justice, privacy, freedom, social stability, or innovation" (Douglas, *Science*, 92). She writes that, at times, the ethical and the social contrast each other: that is, to do what is ethical, one must cross what is socially acceptable. Her example is the social value of stability, which was "antithetical to ethical values underlying the push for desegregation and the civil rights movement" (Douglas, *Science*, 92). In other words, the ethical need for desegregation had to disrupt the social value of stability in the push for what is right and just. She writes that, social "values [sometimes] run directly counter to ethical values focused on the rights and qualities of individuals" (Douglas, *Science*, 92).

Cognitive values, which Douglas insists are not epistemic values or epistemic criteria for science, are values which impact the ways in which scientists "think through the evidential and inferential aspects of one's theories and data" (Douglas, *Science*, 92). Cognitive values are invoked as a way of thinking through the doing of science. Examples of these cognitive values are simplicity, explanatory power, scope, consistency, predictive precision, and fruitfulness (Douglas, *Science*, 92-3). The concept of cognitive values is further complicated by the fact that these values do not necessarily make one claim truer than another: just because one claim is simpler does not make it truer. These are influencing values, not determining values. They help the scientist think through their work, they do not determine what the scientist thinks. Cognitive, ethical, and social values provide guidance "at points of judgment when *doing* science" (her emphasis, Douglas, *Science*, 94); they do not and ought not determine the scientific outcome. This is the fine line that scientists must consider between when their values are acceptable in their scientific research and when they are unacceptable. That is, the roles values play within scientific enquiry can have either a positive or a negative impact.

Scientific enquiry must begin with "a value-laden judgment"; that is, scientific enquiry must be considered a "worthwhile pursuit" (Douglas, *Science*, 95). The value of knowledge must be consistent throughout the enquiry. This value-laden judgment of the *worthwhile-ness* of the scientific enquiry is both an ethical valuation of the good of truth, and a social valuation from a society that views scientific enquiry as valuable and thus worth funding (Douglas, *Science*, 95). The role of values in science must begin with an understanding that science itself is valued "as the source for reliable empirical knowledge" (Douglas, *Science*, 95). Douglas proposes that there are two roles that values can play in science: direct roles and indirect roles. Values are acceptable throughout the entire scientific process when playing an indirect role; however, she warns that values ought to be restricted to only certain decisions in a direct role. That is, she understands the direct role that values can take is when "Values [are permitted to] determine our decisions in and of themselves, acting as stand-alone reasons to motivate our choices" (Douglas, *Science*, 96). If values are not restricted and monitored in this role, the integrity of the science will not hold. According to Douglas, an indirect role for values "can completely saturate science, without threat to the integrity of science" (*Science*, 96). These roles help the scientist come to determinations about the evidence, based in the evidence.

Douglas provides strict guidelines for how scientists should avoid using values in a direct role in their research. Trust would be justifiably undermined should science take up values in direct roles. She writes that scientists cannot maintain the integrity of their enquiry and use values to either "direct the selection of a problem [or] a formulation of a methodology that in combination predetermines (or substantially restricts) the outcomes of a study" (Douglas, *Science*, 99-100). If we consider the core value of science to be the production of reliable knowledge, as Douglas does, the possibility of allowing one's values to affect the outcome of the

study seriously undermines this value and thus seriously undermine the nonexperts' trust in expert opinion. Rather than allowing the evidence to dictate the outcome of the study, allowing values to dictate the selection of a problem or the methodology undermines this foundational value of science. This is not a foolproof rule, and Douglas acknowledges that "The best we can do is to acknowledge that values should not direct our choices in the early stages of science in such a pernicious way" (*Science*, 101). The role of values should to be restricted to providing a motivation for believing a claim, but it should never be that values are considered instead of the evidence (Douglas, *Science*, 97).

Values must be considered when selecting a study; for example, considering which projects are worth funding because they are both feasible and interesting. Douglas writes that, "ethical, social, and cognitive values help scientists decide where to direct their efforts and are reflected in both funding decisions and the scientists' own choices" (*Science*, 99). In the initial stages, these values can still maintain scientific integrity while shaping the choices made regarding the enquiry. These values must also shape the methodology decisions that scientists make: specifically in judgments regarding ethics, social acceptability, and cognitive possibility. Methodological choices must be ethical, they ought to (in most cases) conform to societal norms, and they must be cognitively possible. If the methodology is not ethical, it should not be performed in that way. If the study does not conform to societal norms, it might not get funding. If the study is not interesting or is not cognitively possible, the study won't be pursued. These direct roles of values are necessary consideration which must be made at the beginning of a study. However, this role must be limited. Douglas writes that, "Once the study is under way, any direct role for values must be restricted to unusual circumstances when the scientist suddenly realizes that additional direct value considerations need to be addressed" (*Science*, 101).

Later stages of scientific enquiry ought not have direct value-based judgments involved. Douglas writes that there are three problematic stages of enquiry which ought not directly involve values: characterization of data, interpretation of evidence, and acceptance of theories (*Science*, 101). Allowing values to take the place of evidence is an inappropriate intrusion of values in science. The valuation of these elements might alter the outcome of the study: the conclusions of the study could be slated towards the scientist's own values rather than towards the evidence. This direct role for values in later stages of the scientific enquiry could "allow values to have equal or more weight than the evidence itself, and scientists could select an interpretation of the evidence because they preferred it cognitively or socially, even if the evidence did not support such an interpretation" (Douglas, *Science*, 102). The consequences of allowing values to play a direct role in inappropriate ways would "undermine science's ability to tell us anything about the world. Instead, science would be merely reflecting our wishes, our blinders, and our desires" (Douglas, *Science*, 102). The role of science is not to reflect our values, but rather invoke important values in pursuit of an objective outcome of empirical research. This is not to say that the value-free ideal is preferable. Douglas writes that "The value-free ideal is too strict, excluding the needed ethical and social values from a legitimate indirect role, and thus preventing scientists from fulfilling their moral responsibilities to fully consider the consequences of error" (*Science*, 102). Scientists must fulfill the same moral requirements as the general public through these value-based judgments and they can do so without undermining the integrity of scientific enquiry.

Because values have no direct impact on the world as it is, and scientific enquiry is aimed at an accurate understanding of the way the world actually is, Douglas claims that "when deciding upon which empirical claims to make on the basis of the available data or evidence,

values should play only an indirect role" (*Science*, 102-3). Douglas proposes that values should never take the frontline place in decision-making in scientific enquiry, they ought to "provide a motivation to believe a claim, [but] values should not be construed as providing epistemic support for a claim" (*Science*, 97). It is only when these values play a direct role within the scientific enquiry when serious problems arise. Douglas writes that "Science is a value-laden process. From the decision to do science, to the decision to pursue a particular project, to the choice of methods, to the characterization and interpretation of data, to the final results drawn from the research, values have a role to play throughout the scientific process" (*Science*, 112). These values must be considered when weighing important judgments regarding uncertainty and assessment of the moral requirements of scientists. Douglas' distinction between the direct role of values and the indirect role of values helps in determining the good and bad influences of values in science. The value-free ideal, to which science is often held, does not stand as a legitimate ideal. *Value-saturated science* can still produce objective, empirical knowledge claims (Douglas, *Science*, 113).

In "Rejecting the Ideal of Value-Free Science," Douglas writes that there are two objections to her position against the value-free ideal: [1] "Scientists shouldn't make choices involving value judgments—they should do their science concerned with epistemic values only and leave determining the implications of that work to the policy makers, and [2] we should shield scientists from having to think about the consequences of error in their work in order to protect the 'value neutrality' of the scientific process" ("Rejecting the Ideal," 8).

The first objection insists that scientists ought to stick to their research and not consult their values because "they are simply reporting their data" (Douglas, "Rejecting the Ideal," 8). Decision-makers ought to be concerned with values when they are using science to make

important decisions (as argued in Latour, the role of science in making policy determinations cannot be ignored). However, insisting that scientists stick to their *raw data* only ignores that even simple raw data can and does include "judgements of characterization that require values in the process" (Douglas, "Rejecting the Ideal," 8). Douglas contends that it is mostly not possible for decision-makers to have the type of expert insight needed to make accurate judgments about complex material, particularly in instances of complex theoretical science regarding the warming global temperature. Scientists must be called upon to interpret their raw data, and this requires value judgments. Their interpretation will always "involve selection of background assumptions, among other things, with which to interpret the data" (Douglas, "Rejecting the Ideal," 8). To summarize, Douglas argues that scientists must create interpretations of their work, and these interpretations are subject to and influenced by their own values.

The second objection to Douglas' argument is that in order to maintain scientific objectivity through the value-free idea, scientists must be shielded from "considering the consequences of scientific error" (Douglas, "Rejecting the Ideal," 9). This however would starkly mark a difference in expectations of the work of scientists and everyone else. Douglas notes that "in other areas of modern life, we are required to consider unintended consequences of actions and to weigh benefits against risks" ("Rejecting the Ideal," 9). To separate scientists from the potential consequences of their errors is to allow their continued pursuit of truth, accepting "any errors they make along the way" " (Douglas, "Rejecting the Ideal," 9). Thus, "scientists may make dubious choices with severe consequences of error" (Douglas, "Rejecting the Ideal," 9) without holding them responsible.

According to Douglas, there are only two reasons why we might accept this absolution of scientists: one, we consider epistemic values above all other values, or two, "someone else could

take up the burden of oversight" ("Rejecting the Ideal," 9). She argues that since neither can be true, that is social and moral values are not considered less valuable than epistemic ones, as evidenced by the restrictions on human and animal subjects in scientific experiments. As it stands, epistemic values do not surpass social and moral values. Additionally, we cannot have an external party maintaining moral oversight over scientific enquiry because "the costs of non-epistemic research oversight by outsiders...outweigh the potential benefits" (Douglas, "Rejecting the Ideal," 10). She writes that the consideration of consequences must be an integral part of research, not merely an introductory hurdle, once crossed. Those examining the consequences of error in the scientific research process must "be kept abreast with the research program at every stage" (Douglas, "Rejecting the Ideal," 10). The risk of having such oversight is an extreme loss of autonomy for scientists and significant chance for human error. Having a distinct body overseeing the work of scientists would additionally lack the technical, scientific expertise needed to manage the chances of error: only scientists will have the adequate expertise to manage the possibilities of error. Therefore, she concludes, "the responsibility to consider the social and ethical consequences of one's actions and potential error cannot be sloughed off by scientists to someone else without a severe loss of autonomy in research" (Douglas, "Rejecting the Ideal," 11). Scientists must grapple with and make considerations because of the role that non-epistemic values must have in the pursuit of good science.[31]

[31] There is a long-standing argument over "good science" versus "junk science," which wages throughout the climate change debate. Both sides, the problematic categories of deniers and believers, claim the other is doing "junk science." See: Ryley E. Dunlap and Aaron McCright, "Organized Climate Change Denial" and William E. Tucker, "Deceitful Tongues: Is Climate Change Denial a Crime?" Tucker explains the phenomenon as follows: "One of the earliest of the campaign's 'front groups' was The Advancement of Sound Science Coalition (TASSC)…. TASSC originally coined the deceptive terms 'junk science' (to refer to legitimate mainstream, peer-reviewed science) and 'sound science' (to mean industry-funded pseudo-science)" (Tucker 846).

3.5: Maintaining Value-Laden Objectivity

Those who maintain that science ought not to consider non-epistemic values propose that objectivity in science is at risk when we disavow the value-free ideal. Douglas provides a strong argument for why this is not the case. Douglas insists that there is a distinction between objectivity and the value-free ideal in science. She writes that "there is nothing necessary about the link between the two concepts" (Douglas, "Rejecting the Ideal," 12). As evidence for this, she provides seven meanings for objectivity which do not rely on a value-free ideal. I will present each of her definitions and explore what that means for climate change research. This should not be considered a definitive list for producing objective knowledge; instead, it should be considered partial evidence for the dependability of scientific enquiry which does not rely on the value-free ideal.

One of the main concerns regarding the role of politics and values in scientific enquiry is that objectivity cannot be achieved when normative elements are permitted to take a role. In "The Irreducible Complexity of Objectivity," Douglas previously explored the *irreducible complexity* of the concept of objectivity. The complexity of the notion is further compounded by the simplistic nature it has been reduced to through its use as a rhetorical device which Douglas characterizes as "'I endorse this and you should too'" ("Irreducible Complexity," 453). Objectivity however is not this simple. Douglas argues that there are a multitude of conceptual connections that provide a clear concept of objectivity and thus "no one concept emerges as core" ("Irreducible Complexity," 454). In other words, there is no one sense of objectivity can be used to argue that any values and politics in science demolishes objectivity. In this section, I will explicate Douglas' seven alternative ways that objectivity does not rely on a value-free ideal to show that values in science do not necessarily threaten scientific integrity in climate science.

First, Douglas defines two kinds of objectivity rooted in the interaction between humans and the world: manipulable and convergent objectivity. *Manipulable objectivity* [1] derives its objectivity from the repeated and consistent malleability of some other aspect of the world (Douglas, "Rejecting the Ideal," 13). In other words, the theory in question, when implemented, changes other aspects of the world. The weight of a body moves the bus seat when the bus seat is sat upon, for example. The realness of the body is proven through its effect on the pleather. Douglas notes that this is how complex theories are repeated to the point of probability: "scientists don't doubt the objective existence of electrons when they can use them to reliably produce images of entirely different things with an electron-scanning microscope" (Douglas, "Rejecting the Ideal," 13). In terms of climate change predictions, Douglas notes that it is not possible to achieve an objective truth in the sense of manipulable objectivity because it would take decades, it would be too risky for vulnerable populations, and "it still would not be conclusive" ("Rejecting the Ideal," 13). An example of the manipulability of climate change can be seen in its side effects: such as melting ice caps and the increase in extreme weather patterns.

The other kind of objectivity based in human interactions with the world is *convergent objectivity* [2], where an object is consistently present through a variety of different independent methods (Douglas, "Rejecting the Ideal," 13). She writes that "If we can approach an object through different and hopefully independent methods and if the same object continues to appear, we have increasing confidence in the object's existence" (Douglas, "Rejecting the Ideal," 13). A variety of research methods ought to result in objectively formed knowledge, but Douglas concludes that "objectivity is no *guarantee* of accuracy" (her emphasis, "Rejecting the Ideal," 14). We can confirm through several different tests that the increase in CO_2 corresponds with the

rising global atmospheric temperatures, but correlation does not mean causation. Yet the consistent results in a variety of tests can still be considered objective results.

The next category of objectivity Douglas defines is rooted in individual thought processes which she warns can be confused or conflated with a value-free ideal ("Rejecting the Ideal," 14). *Detached objectivity* [3] rejects that values can take the place of evidence in knowledge production. This meaning of objectivity is informed by the principle that "values cannot act in place of evidence; they can only help determine how much evidence we require before acceptance of a claim" (Douglas, "Rejecting the Ideal," 14). This does not mean, however, that science must banish values from its processes to meet the quality of detached objectivity. Instead, evidence and values must operate together. This idea is grounded in the understanding that, although values must not replace evidence, values shape the ways in which scientists make use of the available evidence. It is evident how easily detached objectivity might be confused for a value-free objectivity. Detached objectivity is met by considering empirical evidence in making scientific determinations, not by removing all values from the research. In climate change research, enquiry begins with empirical data, for example, rising climate temperature measures over a period of time. The values involved in this type of research impact whether these scientists thought it was important to monitor atmospheric temperatures in the first place. This does not mean values were not consulted in this process or involved in the ongoing research; merely, evidence drives the conclusions and values are part of this process.

Value-neutral objectivity [4] (not value-free objectivity) is also a type of objectivity stemming from individual thought processes. Here, value-neutrality is sought by putting aside unresolved value debates, perhaps even pursuing certain research with the assumption that value *x* is correct even if that debate is unresolved. Douglas writes: "many conflicts involving science

and society reflect unsettled debates [and] … taking a reflectively balanced value position, can be usefully objective" ("Rejecting the Ideal," 15). This is an issue encountered often in the pursuit of knowledge: when there is an ongoing debate about values, to continue the research, one must take a side and continue the research rather than being weighed down by a continuing debate outside the direct field of study. In climate change research, researchers must put aside debates regarding things like consensus among their peers, or whether one can choose one's own beliefs or not. The research is not dependent on whether there are definitive answers to these debates. Or in other cases, the research must be done under the assumption that the solution to these questions is already there: for instance, when conducting research into climate change, scientists need to assume that something can be done regarding climate change, whether the effectiveness of that "doing" is known.

The final three meanings of objectivity Douglas locates in social processes: procedural, concordant, and interactive ("Rejecting the Ideal"). *Procedural objectivity* [5] is achieved when the process for determining data is consistent regardless of who is doing the testing. That is, results that are consistent throughout time, place, or researcher.[32] Having a plurality of researchers from different backgrounds, bodies, and locations improves the data's objectivity. In the scientific process, results will be confirmed by continuous, separate, and consistent testing by many different researchers. Diversity within the community becomes even more important. For example, if only researchers from Sudbury, Ontario are examining the effects of rising sea levels, they might not consider the effects of rising sea levels in other parts of the world, for example, coastal towns where the threat is more pressing.

[32] This might seem contra to Code, however I would not say that this is the case. Douglas writes that science can be considered objective if the results can be found regardless of situatedness. This does not imply that findings are only objective if they are reproducible by everyone.

Concordant objectivity [6] is achieved when a consensus is obtained from a group of people. This can be considered an intersubjective objectivity, when "nine out of ten dentists" recommend a specific kind type of toothpaste. In the climate change debate, this can be understood as the 97.5% of climate researchers agree[33] that anthropogenic climate change is a significant risk to current and future populations. Objectivity here is achieved through most experts agreeing on the same conclusions.

Lastly, *interactive objectivity* [7] is achieved when active and intense debate results in conclusive findings (Douglas, "Rejecting the Ideal," 15). A good scientific community has checks and balances for confirming results. Experts critique each other's research and dig deep enough to find errors which results in the production of "good" knowledge. The benefit of the framework of social epistemology is that a great deal of emphasis is placed on the epistemic value of community. It is the "checks and balances" within the climate science community that maintains its integrity, its trustworthiness, and its honesty. Errors do occur. Corruption is a possibility. Collusion and conspiracy have been a reality immemorial. However, arguably, the larger the community, the more difficult it becomes for moral rot to foster, for errors to go unchecked, and for dishonesty to maintain itself. As Latour writes, "Objectivity is neither a state of the world nor a state of mind; it is the result of a well-maintained public life" (47, fn. 14).

In line with Douglas' seventh meaning of interactive objectivity, a strong scientific community has in place various measures to ensure that what is "known" are objective facts about the world; these systems are there specifically to test, analyze, and judge the quality to research output. These forms of objectivity are complex and, as Douglas notes, come with many

[33] There are many important debates regarding consensus in the scientific community on climate change, some of which were discussed in chapter one. Putting aside those debates for this argument can be considered an example of value-neutral objectivity.

difficult questions regarding implementation and construction of these kinds of objectivity. These seven different meanings of objectivity are intended to illustrate that value-free and objectivity are not one and the same, and that doing away with the value-free ideal of science does not demolish its claim to strong objectivity.

These seven alternative senses of objectivity require further discussion. Their role in this argument is to make clear that the value-free ideal is neither necessary nor sufficient for trustworthy knowledge production. The critique that accepting a role for values in knowledge production reduces the objectivity of the enquiry fails to account for the variety of senses of objectivity which do not require a value-free ideal. Ultimately when it comes to climate science, there are appropriate roles for values and politics to play within scientific enquiry. These roles must be accounted for and monitored without requiring a value-free ideal to maintain objectivity. Even without the value-free ideal, the conclusions of climate scientists can be trusted. Without abiding by traditional epistemological standards of the value-free ideal, their work can still be trustworthy science.

Conclusion: The Weight of Community

Determining what to believe about the world is a complicated matter. The time and ability needed to learn everything there is to know about the world would take many lifetimes. Fortunately, there are ways to form beliefs about the world to which we can hold with relative certainty. The most common way to determine what to believe, about matters of which we know very little, is to turn to experts. However, when we rely on experts, we are vulnerable. We depend on their ability to form true beliefs themselves and we depend on their willingness to tell us the truth.. Relying on the testimony of others requires several elements: the one element I have been concerned with here is trust. Trusting the experts might be difficult. In situations where we are not experts, it is difficult to tell if we are being manipulated, lied to, or fooled.

In this thesis, I have argued that the increasing public distrust in climate change experts is related to claims that the science is being politicized. Some climate skeptics and some concerned experts within environmental studies argue that values and politics are infiltrating climate science. Skeptics claim that our belief in anthropogenic climate change is no longer justified because of the politicization of climate science. Experts who critique the level of politics in climate science argue that public trust in climate scientists is waning and thus some members of the public feel justified in their disbelief and their inaction. I have argued here that, given the research of Latour, Code, and Douglas, the risk of politicization of climate science does not necessarily preclude the role that value-based judgments must play in scientific pursuits.

Beyond the claims made by Latour about the need to realize political obligations stemming from the facts of the matter regarding climate change, and beyond the claims made by Code about the reality of the situatedness and interestness of scientists, Douglas leaves us with important considerations about the values of scientists. She writes that "We all share a general responsibility to consider the consequences of our choices, including those consequences of error" (Douglas, *Science*, 84). Scientists are no different. Insisting that scientists maintain a value-free ideal isolates them from the value-based considerations they must make about their own research. The human responsibility for making value based judgments about the impacts of knowledge production is one that extends to climate scientists. It is not the case that there are moral guidelines for the general public and moral guidelines for scientists. Value-based assessments are important in all realms of enquiry (Douglas, *Science*, 84).

Latour's work illustrates the integral part scientists must play in policy development for climate change mitigation and adaptation; Code's work shows the ways in which scientists and all knowers bring their own human-situatedness to knowledge production; and Douglas' work develops new understandings of objectivity that do not require a value-free ideal of science or a value-free scientist.

There are many elements within the climate change debate which were not included in this argument. The nature of epistemological enquiry is that, if you are looking in one direction, you are not looking in at least three others. Various concerns which I hope will be spurred from this work include an examination of the *ethics of belief*: for example, what are we morally obligated to believe? There are also important considerations to be made regarding the workings of power and bias in scientific enquiry: particularly questions regarding who is being believed

and who is being systemically ignored both in political realms of advocacy and in scientific realms of research?

Overall, I have argued that there are significant threats to allowing politics to enter into scientific enquiry, but as I have shown, there are numerous reasons not to assume that [1] values and politics can ever be removed from knowledge production and [2] removing values from science is the only way of ensuring objectivity within science. Although it is true that the infusion of values in science can undermine science and lead to the politicization of science, this is not necessarily so.

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