Weaponising Controversy: A Demarcation of Illegitimate Dissent

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Submitted to the faculty of

the department of History & Philosophy of Science

of the requirements for the degree of

Bachelor of Philosophy

University of Pittsburgh

2019

UNIVERSITY OF PITTSBURGH

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Wednesday the 8th of May, 2019

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Climate change research has faced increased public scrutiny over the last several years. The scientific consensus on anthropogenic climate change has significant public policy implications, and this, combined with the resulting public scrutiny, has had material consequence to how science in this field is done. Dissenting research, as well as dissent more generally, is often made in a way that is damaging to the pursuit of knowledge, instead of beneficial to it.

While historical accounts of 'epistemically detrimental dissent' (EDD) have captured some of the ways these views are created and spread, establishing strict criteria for these without implicating the normal dissent that is so crucial to scientific practice, remains difficult. After discussing the background of EDD in existing philosophical literature, an alternative – the communications account of epistemically detrimental dissent – is proposed.

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1.0 Introduction

Scientific research on anthropogenic climate change has faced increasing public scrutiny over the last several decades. What began as an observation on the chemical properties of carbon dioxide in the atmosphere rapidly evolved into scientists coming to increased certainty over a worrying trend: not only are average global temperatures rising annually, they are doing so as a result of human activity (Held and Soden 2006).

The policy ramifications of climate science have had several pertinent consequences for the study itself. For instance, there is quite a bit of emphasis on consensus in the field of climate science (E. Winsberg 2018), and as a result, those that participate in it err overly to the side of conservativism both when it comes to constructing models for research, and when it comes to reporting the data generated from these (Brysse, et al. 2012) (Hansen 2007). This is because anthropogenic climate change has deeply significant economic implications, due to the measures that must be taken to limit its effects. Fossil fuel combustion is one of the leading causes of carbon dioxide emissions globally, and policies designed to reduce consumption of these fuels pose a very real threat to the financial viability of the companies that produce these.

As a result, climate research has been subject to dissenting views that are unlike the sort usually seen, particularly when compared areas of science that are further removed from political implications. While criticism is a normal part of science, much of what we see in climate science goes well beyond the norm, in a way that has been shown as harmful to research endeavours. As well

as erring on the side of conservatism (Rolin 2017), this dissent has made researchers hesitant to publish their findings (Biddle and Leuschner 2015) due to fear of public backlash. More and more, we are beginning to see the pressure from this dissent dictate and direct not just the topics and methods of inquiry chosen by scientists, but what they publish, and by extension, what is accepted as fact. So not only is this dissent abnormal, it is having a material damaging effect on climate science as an epistemic pursuit.

This essay considers where, how, and why some of this criticism deviates so far from the norm in science. In doing so, it joins the efforts of others, most notably Biddle and Leuschner, in attempting to demarcate between this kind of dissent and others, which, as mentioned, are a normal and important part of scientific research. Without this effort, it becomes difficult, if not impossible, to call out the bad actors perpetuating this damage, and limits our ability to mitigate its effect in the future.

It will begin by providing the philosophical background that these issues are rooted in, particularly, values and inductive reasoning in science. Then, it will describe other perspectives on dissent in science, and scientific norms. Then it will introduce a case example to show the confluence of factors that can be used to describe what will be called epistemically detrimental dissent. Wider concerns about the implications of this will be discussed.

2.0 Values, Disputes, Controversies

Before discussing cases of epistemically detrimental dissent, it would be useful to begin by defining some key terms. Despite most of the literature on dissent being written from a shared philosophical perspective, the use of central terminology varies quite often, making a disambiguation of the terms worthwhile.

2.1 Values

A value, as used in philosophy of science literature, is an attitude or belief that directs, dictates, or defines the priorities of a scientist (Rudner 1953). **Epistemic values** are values that dictate, define and guide (decisions that are made in) the production of knowledge. They allow us to know that our best theories have "some empirical grip on the world" (Douglas, The Value of Cognitive Values 2013, p.80). In other words, they give us epistemic assurance, which allows us to hold and test our best theories in a reliable manner. Epistemic values, also sometimes called cognitive values, include explanatory power, empirical adequacy and internal consistency; a theory has to explain a phenomenon, as well as agree with itself and agree with the data in some measurable way. Internal consistency of a theory is the least controversial epistemic value. It demands that a theory is logically sound according to the principles it establishes. Empirical adequacy, sometimes called empirical success, is the demand that a theory have enough evidence to support the claims that it makes. The value of explanatory power is that a theory should account well for phenomena it is attempting to describe, and account better than preceding theories.

Other values such as simplicity, fruitfulness, and scope of applicability are also often included in this category, (Lacey, 1995) but they are better grouped under constitutive values for reasons I will explain.

Constitutive values are "the source of the rules determining what constitutes acceptable scientific practice or scientific method" (Longino 1990, p.4) and these would contain within them a) epistemic values, such as those listed above, as well as b) other values, such as simplicity. In other words, epistemic values are a necessary subset of constitutive values. Constitutive values acknowledge the desires of researchers in the context of knowledge-seeking practices, and as such, aren't considered epistemic values proper. For instance, a simpler theory is preferable to its less simple alternative *ceteris paribus*, because that makes it easier for scientists to use and share. The simplicity does not itself make the theory any more likely to be true. Epistemic values are identified as such because the most important characteristic of a scientific theory is that it is epistemically good, sound, or proper. Contextual values nevertheless remain important, but because they're focused on accessibility of researchers, they are less prioritised, and therefore more flexible in their use. A theory that sacrifices scope of applicability can be redeemed in other ways, but a theory that sacrifices internal consistency absolutely cannot.

Contextual values are sometimes referred to as non-epistemic or non-cognitive values. These are values that are of a personal, social or cultural nature that guide scientific research and can include, for example, the wish to avoid harm to human populations in risk assessment (Wandall 2004). While this is a specific example of a contextual value that is regarded as valid and acceptable

to have present in scientific research, the kind of values that are called out as inappropriate most often fall under this category.

The **value free ideal**, then, refers to the common belief that science should not allow for the involvement of these contextual values in crucial parts of the scientific process. While "social and ethical values [can] help to direct the particular projects scientists undertake," the judgements that are made in the context of justification should not include any kind of value motivation whatsoever (Douglas 2009, p.45). This ideal fundamentally rests on conceiving of science as something that is done in isolation from wider society, rejecting the idea that the internal standards of science should take broader societal problems into consideration at all. The reasoning behind supporting this view is the idea that knowledge is a descriptive enterprise, while values are normative, making attempts to reconcile the two misguided. This view was pervasive in the philosophy of science until the 1980s.

Elliott and Richards describe in detail the origins of this view, along with the noted individuals that still defend it (Elliott and Richards 2017). While the more popular view is to reject the value free ideal, these individuals argue that the value free ideal can be maintained through avoiding altogether the argument from inductive risk.

2.2 Disputes

Scientific disputes occur when researchers disagree about a given evidentiary conclusion, and are a core and even necessary feature of any progressing scientific field. They have happened over theory and hypothesis acceptance, or lack thereof. In more concrete terms, they occur over scientists

who believe a theory is strong enough or good enough to use as a foundation for their work, as opposed to scientists who either do not believe this, or believe that an alternate competing hypothesis is better. These theories are often not just related to the external subjects of the field, they can also relate to internal practices, such as disputes over whether a given instrument is adequate for its purported use (Kitcher, Patterns of Scientific Controversies 2000). When a technical dispute arises, it can call attention to problems in the presently accepted literature. Researching around these problems can help scientists to develop a more concrete understanding of their object of study, as objections call attention to areas that have been taken for granted, or not sufficiently considered.

2.3 Controversies

A scientific controversy, however, has come to mean significantly more than a dispute. Controversies have been well documented by historians of science, (Oreskes and Conway 2010) and are, in essence, disputes that have been extended over time (Engelhardt and Caplan 1987). Where disputes have a feedback effect that can be epistemic and therefore positive in nature, controversies, per Winsberg, create a non-epistemic and therefore destructive effect on the scientific enterprise. (E. B. Winsberg 2018).

3.0 Current Philosophical Views on Dissent

One of the main perspectives on dissent in the current literature is the **Inductive Risk Account of Epistemically Detrimental Dissent.** It describes four criteria that are meant to be

jointly sufficient for identifying the kind of epistemic dissent that is "objecting to a particular conclusion," in a manner that "not only fail[s] to contribute to scientific progress, but impede[s] it" (Biddle and Leuschner 2015). This dynamic – both the lack of progress, and the consequential impediment of progress – is one that I also believe is central to understanding EDD. I am labelling it 'epistemic vacillation.'

Epistemic vacillation takes the idea of transformative criticism that was first introduced in Helen Longino's seminal 1990 book, Science as Social Knowledge: Values and Objectivity in Scientific Inquiry. Given that "effective criticism that advances understanding" has been identified as a vital part of the objective scientific process, then focusing on its opposite allows us to identify what's non-essential, and indeed damaging to that process. The opposite, therefore, is something that when it's fully engaged and explored, advances no novel understanding of the field. This describes the first important aspect of vacillation – normally, when an idea fails to contribute in this way, attention moves away from it, onto other ideas that may better aid our understanding of the target of investigation. We begin to see vacillation occur when that 'moving away' is stifled, and instead of pursuing new ventures, scientists go back to those same topics repeatedly due to factors that are outside of their control.

This has a crucial secondary effect: though going back to well understood topics does not destroy the existing understanding that scientists have worked to build, preventing their exploration of new ideas is detrimental, because it limits the combined efforts that we would have seen to advance understanding of it. To wit, the end result of epistemic vacillation is stalling scientific

advancement and research, so that there's no additional evidential support for any claim you would make about the subject, whether it aligns with the consensus position or not.

Biddle and Leuschner therefore believe that EDD operates via this mechanism of vacillation. Though it isn't explicitly stated by them, I believe that epistemic vacillation acts as a precursor necessary condition, from which point their four named criteria begin to apply.

Justin Biddle and Anna Leuschner outline criteria that allow us to identify when scientific dissent or controversy is epistemically detrimental. According to them,

Dissent from a hypothesis H is epistemically detrimental if each of the following obtains:

1) The non-epistemic consequences of wrongly rejecting H are likely to be severe.

2) The dissenting research that constitutes the objection violates established conventional standards

3) The dissenting research involves intolerance for the risk of accepting H.

4) The risks of accepting H and being wrong and the risk of rejecting H and being wrong fall largely upon different parties.

In order to fully address Biddle & Leuschner's inductive risk account of epistemically detrimental dissent (Biddle and Leuschner 2015), we need to review the role of inductive risk in our understanding of values in science.

The argument from inductive risk – otherwise called the error argument – describes a type of epistemic risk (J. B. Biddle 2018) that is involved in theory and hypothesis appraisal when they are

influenced by social values (Elliott and Richards 2017). The preoccupation with the influence of values on the scientific process began to be raised in 1953, with Richard Rudner's influential essay 'The Scientist qua Scientist Makes Value Judgements.' (Rudner 1953). It was revived and revised by Heather Douglas (Science, Policy, and the Value-Free Ideal 2009) at the start of this century. Rudner's standard account made the claim that value judgements exist in science because, owing to the inductive reasoning used in all sciences, there must be something that arbitrates the evaluation of evidence as sufficient or insufficient for accepting a hypothesis. This rests on the idea that there will always be limited or finite evidence for a theory, and so there must be a collective standard for acceptable evidence thresholds when assessing different theories. Understanding that there are two different types of error (false positives and false negatives,) and understanding that these standards of evidence can vary based on our perception of the possible errors, the crux of this argument rests upon the idea that the evidence standards ought to be chosen in consideration of social, moral, and ethical values (ChoGlueck 2018).

As a consequence of the Rudnerian thesis, one must accept that social values exist within the scientific process. This is where the inductive risk account gets its name – the acceptance or rejection of hypotheses based on how willing we are to be wrong can, first, depend on our values. So, epistemically risky inductions are mediated by social values.

Heather Douglas expands the original claim to say that value judgements of the sort that Rudner detailed actually take place throughout the entire scientific process. Any sort of a decision made during research depends on our values. Instead of the inductive risk being focused only at the

end point, science is a series of value judgements, each coming with their own inductive risk and requiring some input from social values.

Many philosophers that reject the strong value-free ideal of science, Biddle & Leuschner included, accept this modified claim. Because they have adopted this idea, Douglas, Biddle and Leuschner aim to set out ideas about the correct and proper use of these values in science. Despite their rejection of the value-free ideal of science, they believe that there is a right way to do good science, and that this is something that happens in fact – even with the inclusion of values.

Douglas outlines the role of values and their proper place in science from a normative perspective, differentiating between direct and indirect roles, proper to epistemic and non-epistemic values, respectively. Whereas indirect values influence the kind of evidence standards that are appropriate for given circumstances, direct values are values which are used in considering how worthy a given theory or knowledge claim is (Elliott 2011, p.304). We can identify improper interference of values when the wrong 'kind' is playing the wrong role in any given research procedure. A similar idea can be seen from Phillip Kitcher, who introduced the concept of grubby and depraved motivations of scientists in *Science Without Legend* in 1993.

These all capture a general theme: although values are pervasive within the scientific process, there must be rules as to when they can be manifest in the work, by whom, and under what capacity. This is one of the main things that keeps science understood as something that can be counted on to produce reliable knowledge.

4.0 Inductive Risk Account of Epistemically Detrimental Dissent

Let's now return more closely to the Inductive Risk Account of Epistemically Detrimental Dissent (IRAEDD). Recall that Biddle and Leuschner begin by asserting that

1) *The non-epistemic consequences of wrongly rejecting H are likely to be severe.*

They start with this criteria because, as is demonstrated in the paper, widely held social values manifested through political pressures can be one of the largest forces behind EDD. Politics only gets involved when there are some sort of non-epistemic consequences at play, such as the ecological disaster that will obtain due to climate change. Anything that 'raises the stakes' in a manner that draws the vested self-interests of people outside of the scientific community focused on studying that topic is susceptible to EDD under this rule.

Here, we can say that something has non-epistemic consequences if its intimate effects are not merely limited to the body-knowledge. These kind of pressures have the potential to change the natural distribution of labour that would normally exist in a scientific field for the worse via a) researcher intimidation and b) the creation of busywork (p.10) and this criterion was designed to capture those social epistemic changes that directly obtain due to political pressure. Hence, a researcher *should* choose the direction of her research based on epistemic considerations, like that she believes a particular avenue to be the most intellectually engaging, or most promising for explanatory power, rather than any external pressures that would compel her to do something different.

Next, Biddle and Leuschner state,

2) *The dissenting research that constitutes the objection violates established conventional standards*

This seeks to capture the idea that the detrimental dissenting research is in itself qualitatively bad. Biddle & Leuschner describe, for example, research that was "deeply flawed and quickly exposed as such" (2015). Repeating this sort of research is what makes the creation of busywork mentioned actually possible. The idea is that these reports don't provide particularly novel challenges to the established orthodoxy, so responding to them is not epistemically fruitful but, in fact, a waste of time, capturing the notion of epistemic vacillation also discussed previously.

The latter two criteria,

3) *The dissenting research involves intolerance for the risk of accepting H,*

and

4) The risks of accepting H and being wrong and the risk of rejecting H and being wrong fall largely upon different parties

entail each other to an extent. They want to capture the idea that, because the interested party has some stake in the outcome, they will manufacture circumstances so that the structure of the experiment itself makes the less desirable outcome less likely. Together they provide the concept that somebody is 'stacking the deck' in an unfair way in the process of experimentation. 3 shows that they are doing it because they or the individuals they represent have something to gain, while 4 demonstrates that, in addition to this, they have nothing to lose.

To summarise, Biddle & Leuschner want to show that when given research is mired in political dispute, of poor quality, and with experiments rigged to diminish producer risk, it is epistemically detrimental dissenting research.

4.1 Feedback and Criticism for the Inductive Risk Account

We'll consider each of these criteria in more detail now. The claim to political pressure as a sufficient condition is possibly misplaced on Biddle & Leuschner's account. Because they aim to give criteria that are only jointly sufficient, it doesn't seem necessary to specifically focus on the kinds of political pressures that make up their first condition. That is to say, while it may be more probable you will find instances of EDD in areas of science that have demonstrable non-epistemic consequences, focusing on these consequences may make us overlook EDD in areas that have none, or similarly, wrongly make us believe that these cases are 'weaker' versions of EDD. Additionally, it would seem like wrongly *accepting* H must also have some significant consequence for the effects of the dissent to obtain. That is to say, the reason that there is a controversy is that people (legitimately or otherwise) fear what might happen if we accept climate change, and in doing so, pass legislature that would do irreparable damage to the economy. So, at the very least, this criterion must read

The non-epistemic consequences of wrongly rejecting ***or accepting*** *H are likely to be severe.*

It's also worth interrogating whether it really is the case that these consequences need be 'severe' to adequately become part of a sufficient conditions criteria set. While many of our paradigm examples of EDD come from the field of climate science, where the consequences affect everybody

inhabiting planet Earth, there are many ways in which political disputes of this sort could obtain *without* severely affecting the lives of anyone outside of a fringe minority. To put a slight twist on a historical example, we might look to big bang theorists vs creationists.

One of the most hotly disputed scientific claims in existence is the one that places the age of the Earth at approximately 4,500,000,000 years. This is at odds with the claims of the Bible, which places the age of the Earth at several thousand years. The palaeontologists and the Christians were using entirely different epistemic values to assess the evidence they each presented, and so the dispute persists to a controversy. The way that this incident ended up happening meant that the controversy played out very differently compared to the one we have seen regarding climate science. The epistemic standards of the Christians who publicly denounced these areas of science were viewed as opinions that had no bearing on the standards used by the scientists themselves. So while there were definite effects on policy – many school districts voted not to teach this science to students – the public arena did not change the manner that scientists conducted their research. While the epistemic state of science was not changed because of this public controversy, the state in climate science very much has been.

Despite the two episodes being somewhat different in nature, it is more than feasible that a devout fundamental Christian with millions of dollars on hand would have personally set out to fund research that would prove the consensus scientific view on the age of the earth to be wrong, as lobbyists successfully did with tobacco and climate science (Oreskes and Conway 2010). Wrongly rejecting or accepting this hypothesis may have some epistemic consequences, but generally

speaking, it doesn't appear that society at large would change the way it would live its life. On the other hand, the millionaire Christian's life might be fundamentally changed by the theory being rejected or not. People taking the science seriously, in his view, risk eternal damnation – that being the ultimate consequence.

This example illustrates that the ambiguity of the argument as it's laid out here might unnecessarily misdirect people seeking to identify EDD. Avoiding this is important; while some believe that EDD can only obtain in areas of science that have policy implications, I believe that EDD can occur anywhere there is a vested, non-epistemic interest in the research at hand. This could mean something as trivial as research behind the mating habits of spiders, so long as there is at least one interested party at hand.

I have taken 'severe' to mean something along the lines of life-altering enough to will an individual to take significant action, making it worth bringing the creationist example forward. That this controversy didn't in fact play out with countervailing research thought of as equally legitimate, does not mean that these aren't the kinds of situations that would draw the kind of dissent we're attempting to capture. So I think my case shows that the first criterion would even better read:

1) The non-epistemic consequences of wrongly rejecting or accepting H are likely to be severe to some interested, powerful, influential party.*

Where powerful indicates possession or command of resources that would let the social epistemic landscape of a field be altered.

Turning to the second criterion, Winsberg remarks that the invocation of the "conventional standards" named in 2) is an empty one, for two reasons. First, while most of these standards are understood implicitly, there is no conventional set of standards that is detailed enough to resolutely decide if a given research paper has broken these standards, or adhered to them (E. B. Winsberg 2018). Second, closely related to the first - it's often difficult or impossible to ascertain whether or not the standards have been adhered to in any given research paper, due to the technical difficulties and limitations that go into the assessment of experimental procedures themselves. Briefly, this lack of clarity leads to assessments having to be made about whether some research has adhered to the conventional standards *enough*, and it's not clear that these assessments could be made thoroughly or uniformly.

He supports the inclusion of a sufficient condition advanced by Phillip Kitcher that would identify researchers having "depraved" motivations as being epistemically detrimental forces – Kitcher differentiates between these and mere "grubby" researchers, whose motivations include non-epistemic goals as well as epistemic ones – the desire for new, legitimate knowledge, for example (Kitcher 2011). Arguments like these are closely related to the discourse of values that is so primary in this discussion. Contextual values playing a role in science means that the direction of any given research can depend, at least in part, on the natural dispositions, attitudes, and motivations of a scientist. This is what Winsberg is appealing to when showing support for Phillip Kitcher's conception of 'depraved' vs 'grubby' motivations come in, for example – scientists who are motivated to do good science, and to get famous for it, vs scientists that are only interested in getting famous (2011). It is also why Biddle & Leuschner include criteria relating to the non-epistemic

consequences of rejecting hypotheses or the different risks that fall on different parties. The idea is that knowing about the scientist's disposition is the key to unlocking further understanding about the nature of her dissent, like whether or not it is detrimental.

While there is often concern expressed about the ability of outside observers to access the true motivations of an individual's dissent, e.g. (de Melo Martín and Intemann 2018), this risks distracting from the issue of whether or not the initial assumption is true – that there is a need to know her disposition at all. At the least, an account like this risks excluding the well-intentioned actor whose stances nevertheless cause epistemic vacillation. Take, for instance, a climate sceptic who genuinely believes for whatever reason, that the existing research in climate change is ambiguous. The sceptic's position paves the path for vacillation, which in turn allows EDD to occur. Furthermore, it is worth wondering whether the equivocation between depraved motivation in Kitcher's sense, and motivations based in contextual values, is made too hastily here. Our contextual values also address aspects of research that are regarded as positive, like research ethics, so forming an account of EDD that depends on motives may risk inadvertently implicating these.

Furthermore, despite there often being technological limitations on the feasibility of understanding some violations, I would nevertheless argue that while defining explicitly the conventional standards at work in any scientific field can be difficult, identifying when they are violated is in fact easy, because of their general uniformity within fields of research. Much of the literature describes science as a process where these standards (or, shared values) are constantly in

play, e.g. (Longino 1990); (Kitcher, Patterns of Scientific Controversies 2000) so that they constantly underpin the scientists' activity, despite often being unspecified or unspoken.

4.2 Place and Time of the Motivations Account and its Criticisms

IRAEDD, and others of its kind, are largely founded upon something I will call the motivations account. The 'argument from motivations' literature is founded on the idea that, in order to be able to find out the nature of a researcher's dissent, we need to have access to her attitudes and desires as they relate to her work. The mindset of individual scientists is something that is appealed to or alluded to in making claims about types of dissent, e.g. (de Melo Martín and Intemann 2018, p.33-34). We have previously mentioned Kitcher's delineation of *grubby* and *depraved* researchers – terms used to describe when a scientist is motivated by epistemic and selfish ends, as opposed to selfish ends only (Kitcher 1993). Successfully making a claim about the motivation of a scientist would therefore be the key sufficient criterion needed to identify cases of EDD. Responses to this are varied. Some say that it is a worthless pursuit from the onset, as it depends on claiming a level of 'mind-reading' that is impossible (Lloyd and Winsberg 2018). Others say that attempting to single out certain types of dissent is something we ought not to pursue as it risks discouraging legitimate dissent, and is not necessary for the goal of creating sound public policy (de Melo Martín and Intemann 2018)

Heather Douglas says that we ought to keep track of how individual scientists have historically weighed evidence, in order to see whether or not our contextual values (and therefore

tendencies in weighing evidence) align. This can help us consider to what extent we agree with a scientist's conclusions; the contextual values have some epistemic worth (Douglas 2015).

On a related view, Winsberg is unconvinced by the claims of Biddle and Leuschner that we cannot know the motivations of organisations and individuals, and instead argues that these can be accessed empirically, "in various kinds of historical records, communications, funding sources, and the like." His discussion of the depravity criterion rightly calls for historical investigation, but he does not make explicit one of the more important aspects of the idea: we can establish depraved intent by the absence of a thing, namely, of meaningful response to critique, or of meaningful evolution of an original viewpoint. This is no better exemplified by a case in 2011 with two researchers that published and publicised epistemically detrimental dissenting research: John Christy and David Douglass.

John Christy and David Douglass were two scientists whose climate research using data from radiosondes (Douglass, et al. 2008) was discredited almost immediately by several climate scientists. As told by Lloyd and Winsberg (2018),

> "In March 2011, a world expert in satellite climate data, John Christy, from University of Alabama, Hunstville, testified to a Congressional Committee that global climate models were contradicted and undermined by those data, and that global warming and the greenhouse effect were not occurring, contrary to what the models said. He placed a published paper into the Congressional Record to support these claims: Douglass et al. (2008). This paper had been thoroughly discredited in the scientific literature by other climate scientists, including those who handled satellite data-sets, as well as statisticians and climate modelers (Santer et al. 2008; see below). But this apparently made no difference to Christy, who simply repeated his earlier claims.

> Each scientist that set out to disprove the conclusions of their work **successfully adopted different means of theoretical exploration** to show why using radiosonde data in the way that it was used, was seriously flawed (Santer, et al. 2008). Despite this discreditation, Christy would go on to testify in front of the United States Congress – not to offer a justification of the research in light of the comments that had been made about it, but to directly repeat the science that his peers had already negatively assessed. Their testimony was not informed by the best judgements of the entire scientific community, but rather their own, which had since been shown to be inadequate." (Lloyd and Winsberg 2018)

The details of this situation leads us to a point that may satisfy Winsberg's concern with the second criterion of the inductive risk account. He argues that it can be difficult to point out when appealing to a "conventional standard" when it's vaguely characterised in this way. To add further to this, any attempts to do so will eventually encounter instances rightly considered 'good science' that for whatever reason *did* violate these conventional standards.

However, these problems can be sidestepped if we choose to focus instead on communication standards, rather than research standards. This gives us a far more concrete picture of exactly the manner of standard being discussed, thus avoiding the issue of vagueness; there are universal and unambiguous laws governing the ways you are allowed to share your research if you wish for it to be considered science. That's because communication standards are one of the ways that conventional standards are able to be enforced. It furthermore provide a means of avoiding a related problem discussed in the literature, e.g. by de Melo Martin and Intemann (2018) and Winsberg (2018) – that we need access to the researcher's motivations in order to assess the nature of her dissent. Hence, rather than focusing on violations of conventional research standards, we might fare better by taking violations of conventional research *communication* standards into consideration instead. So, criterion 2 would read:

2) The dissenting research that constitutes the objection violates established conventional communication standards.*

Entering standards of communication into consideration further strengthens the link, also invoked by the latter criteria of Biddle & Leuschner, to ideas of **scientific community**. Such standards are a requirement of objectivity in science as outlined by Longino (1990), so dissenting events like these may additionally be interpreted as actions that worked to diminish the objectivity of scientific practice. Therefore, it would be prudent to discuss scientific community in a way that might help us further understand that.

This might best be done using an understanding of the scientific community as expressed by Kristin Rolin. She provides detail that would clarify the sort of doubts expressed by Winsberg, namely, in that we can understand conventional standards of research as a representation of shared **epistemic responsibility**. *Pace* Rolin, "A scientist is epistemically responsible in making a knowledge claim when she provides sufficient evidence in its support or adopts a defence commitment with respect to the claim" to whoever the "main audience" for her research is (2017). There are two things that should be focused on from this. First, the concept of 'defence commitment' and second, that of a scientist's 'main audience.' Rolin defines the defence commitment as "A duty to defend or revise the [knowledge] claim whenever it is challenged," and this has important implications for what we have discussed so far. Note that the defence commitment is a communication standard, in addition to an epistemic principle. If it is the case, as I have said, that Biddle & Leuschner aimed in their latter criteria to reflect the importance of community standards in understanding detrimental

dissent, then characterising Chrissy and Douglass' testimony to congress as an absence of defence commitment makes a strong argument for the idea that they were acting with epistemic irresponsibility, in a way that violates the communication standards of science.

Winsberg and Douglas, among others, are right to call attention to the ancillary residual details of scientists' activities in the manner that they have. They begin to touch on the idea that there is some historical evidence we can appeal to in order to assess the nature of dissent. Though it becomes quickly apparent that merely identifying the motivation of a scientist is not enough to say whether or not she is producing qualitatively bad science, exploring features related to motivations proves a fruitful way into the discussion.

I propose that the most relevant 'related feature' is in the research itself. The manner of research dissemination is enough to show that a scientist is violating the communication standards of science in a way that is indicative of epistemically detrimental dissent. The following is an outline of the framework that scientific research is to be understood for my **communications account of epistemically detrimental dissent** (CAEDD.)

I will introduce the account more fully in the following section. However, a clearer understanding of 'main audiences' is needed to see whether it would in fact hold up – as, of course, Rolin's argument centres around the idea that these concepts are about certain actions taken towards particular groups. So then what is the 'main audience' of a scientist that is producing epistemically fruitful, rather than detrimental, dissent? The idea that good scientists will have other experts within their field as the main or intended audience of their research, is one that is disputed by de Melo

Martin and Intemann, who point out that using non-expert address is an option used by people who desire expert attention, but who are unable to get it due to the bottle-necking effect of orthodox research dissemination structures (2018, p.38-39). If people are trying to get their research looked at because they genuinely believe it to be superior to its widely accepted competitor, they will use other or additional means to guarantee people see it. So, the main audience does not *have* to be other scientists, because the current system of research dissemination is not viable for most heterodox research. If there was an ideal state, in which all research was able to be looked at and thoroughly assessed by the proper means, though, I believe it would then be right to say that the only proper audience for research would be the scientific community. A picture of how such an ideal system would operate may be worth exploring further.

It's important to point out that while this may account for original research, it does not fully address epistemically detrimental dissent like that of our case study in Christy & Douglass. Recall that their research on radiosonde data was responded to extensively by the scientific community, and that the pair of them went to testify about their findings in Congress instead of defending their research in light of what their peers were saying, thus violating epistemic responsibility. While it can be legitimate to address non-expert audiences to garner attention for an idea, a researcher must bear the burden of epistemic responsibility when it comes to subsequent responses to a successfully disseminated hypothesis. 'Defend or revise' is something that scientists must be held to if they are to continue being considered expert voices in a particular scientific field, and Christy & Douglass' testimony was representative of the fact that they intended to do neither, even though they presented themselves to Congress as experts of that very field.

5.0 The Communications Account of Epistemically Detrimental Dissent

To properly introduce CAEDD, we must first describe the manner in which science functions as a communicative process. A scientific researcher is an individual who seeks to further understanding of a given topic by gathering information (data) through experiment or observation. She gathers this information, usually with a small group of her peers, in order to share it with the wider scientific community through mutually recognised avenues such as journals or academic conferences.

The community, in turn, engages her work. This engagement is minimally done through the process of peer review, and then further when a scientist outside of her group decides to cite the research. The understanding of the wider scientific community can then grow because of this broader engagement. Errors can be overlooked by one group of researchers, but they would quickly catch the attention of another group that can write about these mistakes through the same mutually recognised avenues. accessible to all in that scientific community. This process repeats itself constantly throughout a researcher's career-span.

First, responses must be given in light of critical comments made against it. That is to say that they must address the critical topics raised in the response, in the terms that the responder has made them – a manner of epistemic responsibility. This must be done whether or not the scientist believes those terms to be epistemically good ways of thinking about the phenomena; her response is indeed substantively the reasoning behind why her peers ought not to consider them epistemically good

either. Failure to respond in like terms would be a violation of the standards of communication of science.

To be clear, I take non-adherence to communication standards to be indicative of epistemically detrimental dissent because science functions, by its nature, through the kind of communication engendered by epistemic responsibility. Therefore, anything that works to frustrate the proper communication of scientific research among researchers is working to frustrate the functioning of science itself.

5.1 The Framework

Earlier we described the way that science proceeds through research dissemination. To wit, the scientist qua scientist must go through this process. Being held to this process is what enables us to mark scientists as **epistemic agents** in the strong sense. This is what makes them accountable to epistemic responsibility as stated previously by Rolin (2017), but more crucially, it's what separates them out from lobbyists, politicians and other laypeople. It can be necessary to discuss the actions of the latter kind of people to consider what moral consequences are at stake, but I believe we can't do this too much when trying to assess what is going on in an epistemic sense. So, while the people around these issues that wield lots of power (such as U.S. president Donald Trump) can appear to be the most crucial to understanding issues of epistemically detrimental dissent, their status as non-epistemic agents makes them, at the very least, fall outside of the purview of this particular project.

5.2 The Argument

I have made the case for how normal science occurs. I mentioned that the nature of research dissemination and peer review/critique is repetitive in nature, occurring many times throughout the career-span of a researcher. This, combined with the non-private nature of scientific communication, is what makes me believe that the publication history of a given scientist can enable us to identify legitimate instances of epistemically detrimental dissent. While others that have touched on this topic, e.g. (de Melo Martín and Intemann 2018) have focused on the difficulty of assessing the *contents* of such publications, I instead place emphasis on the timing and location (avenues) of publication as most important for identifying EDD.

The idea is to resolve one of the biggest issues with making the distinction between a possible case of EDD, and a scientific dispute that has fallen by the wayside for some less sinister reason. A researcher may publish a number of times on a controversial topic through "recognised avenues" (scientific/epistemic means) in addition to popular venues, such as magazines and newspapers. (Though I use the term 'publication' here, I do intend to apply this to all other public forums such as academic conferences, and television appearances.)

If she loses interest in the topic for some personal or professional reason, one would naturally expect the number of publications related to that topic to fall significantly, and even drop off, in both scientific and popular publications. A falling number of only popular publications is not problematic, as it may show that she's beginning to find public engagement of her field tiresome. Public engagement, though preferable for a number of social reasons, is not necessary for advancing

scientific knowledge. Though much of philosophy, notably (Douglas 2009), rightly discusses the material epistemic benefits of public engagement, by themselves it's never been a sufficient means in itself of advancing scientific knowledge. On the other hand, scientific engagement, as I've described in the previous framework and description of epistemic agents, *is* necessary for such advancement.

An absence of scientific publications, combined with a high amount of popular publications, is a necessary criterion for EDD on my view. Ascertaining whether this has been reached requires case-by-case interrogation of the content and time factors that I have discussed. To illustrate, let us look back to the paradigm example given by our previous case study. Chrissy and Douglas did not present a total absence of scientific publications, as both had published in journals on the topic of climate change prior to this case example. But at the time of their testimony before U.S. Congress – a public engagement, or 'popular publication' – their work had been the specific target of scientific publications responding to the evidence and reasoning given in their paper. The same paper, centered on a refuted result, that they then used as the basis of their testimony to Congress.

Defenders of this view, e.g. (de Melo Martín and Intemann 2018), claim that a lack of shared evidentiary standards mean that different scientists can justifiably not see eye to eye on issues such as these, without it having to mean there's bad faith or EDD at play. Contrary to this, I believe our case example shows that 'something has got to give' when it comes to cases like these. Consider the important benefits that are at stake when considering whether or not dissenting views are cases of EDD. Firstly, there's the benefit of epistemic diversity: dissent can help scientists see the epistemic value (or lack thereof) of assumptions and methodological processes that are regularly used during research. Challenging these compels people to defend them, and through trying to create this

defence, we make explicit each of the qualities that makes them worthy and legitimate, or not. Additionally, dissent prompts, or is accompanied by, alternate hypotheses in these instances, leading to epistemic diversity in the area. This leads to a more pronounced understanding, and therefore advancement, of the foundations of science.

I characterise timelines like these as instances of epistemic silence because of the lack of scientific discourse in response to things that should be considered pressing scientific manners (such as varied papers showing glaring errors in the reasoning behind your work). Such epistemic silences are suspicious when combined with generous activity in popular publication, where scientific publication is warranted. Epistemic silence stands in contrast with the behaviour of a scientist holding a position that's merely unusual; in the interest of furthering understanding about a subject, a person in this position would either maintain total silence (with neither popular or scientific activity,) or total communication, expressing in *both* popular and scientific publications why the newest reasoning against his view is in fact misguided. In this case, total silence could have been maintained until Chrissy and Douglass had time to gather the reasoning or evidence that would allow them to advance to total communication, something that would have made their congressional testimony permissible. But as an unambiguous instance of epistemic irresponsibility, epistemic silence should be considered one of the key aspects of EDD.

If I am right in making this characterisation, then it is hard to see how EDD can represent both a welcome injection of epistemic diversity and possible source of scientific progress, as well as something that has no obligation to engage the orthodox body of knowledge it would allegedly transform. While lack of shared evidentiary standards have been present in what's considered

progress in the history of science (see e.g. Lavoisier's rejection of qualitative evaluation standards taken as normal in the phlogiston theory,) it is far more difficult to find old transformative theories that have come to be known as such, without engagement with the competing theories of its time. The worry of conflating legitimate dissent with EDD is a very important one, but it has brought a too-strong dose of cynicism to the demarcation effort. The result has been overlooking conflicting characteristics like these, that make the work of accurately describing EDD far easier.

In this regard, the worries that have been appealed to in order to discourage demarcation attempts haven't really been well directed.

Our current technologies afford us the ability to unambiguously track what scientists put out on the public record. I believe that taking advantage of this, by looking at the type and timing of both popular and scientific publications of scientists, is one of the key ways to understanding a significant portion of epistemically detrimental dissent. When these are improperly matched, it can be a useful warning signal for EDD. An individual whose scientific publications on a topic fall to nothing, while popular publications on the same remain high, would be deserving of closer scrutiny than scientists who maintain the inverse, or an otherwise equal balance. Furthermore, timing can also be tracked as a fruitful way of helping identify EDD, as in the case of epistemic silence, where both popular and scientific publications were seen, but in the wrong order.

As epistemic agents, scientists must publish academic research, and an absence of scientific publication of a topic during a hotly debated scientific topic, shows the individual at hand is no longer willing to contribute to understanding this subject matter. On its own, this may be

insignificant. It is only through combining this with active popular publication on the same topic, that the incidence of EDD truly becomes apparent.

Some may see the framework I have used as too much of an oversimplification. It is certainly the case, for example, that the same researchers often don't engage in a dispute even past publishing original research that is later critiqued. It's perfectly legitimate to lose interest in a particular topic for whatever reason, and not go through the work of publishing a response to someone that has criticised your work, even if you have good reasons to disagree with the criticism. You do not owe epistemic responsibility to a field you are no longer interested in. This critique is spelled out further in (de Melo Martín and Intemann 2018, p.46-48). Attempting to claim that a certain work did not genuinely "engage" the critical response made against it requires an extreme amount of effort, especially because the genuineness of these engagements are in themselves assessed by the epistemic standards that the different groups may not share.

The idea that de Melo Martín and Intemann's view can account for *all* instances of research is unpersuasive when you take a scientist's overall public communication history into consideration, however. Recall that in our case study, Chrissy and Douglass testified about their research findings to congress despite the fact that they had never acknowledged or addressed the widespread criticism of it. Simply not sharing the same constitutive values as the orthodox researchers cannot legitimise non-communication in an enterprise that relies on it. Their coming out to testify in front of Congress would indicate that they were at the time still interested in being (or appearing to be) experts in the fields of climate science, but their epistemic irresponsibility shows that they did not want anything to do with the science itself.

5.3 Relevant Available Science

One of the key problems that is raised with attempts at demarcating EDD, is that the demarcation rests upon the dissenter sharing epistemic standards with his target. For instance, somebody may not agree with consensus regarding climate change because they do not believe that our current modelling methods provide sufficient evidence for the subsequent claim that Earth's climate is experiencing a warming trend. Such differences are merely a consequence of epistemic diversity. Epistemic diversity is valuable to the scientific endeavour for a number of reasons, see e.g. Longino 1990, and so, making (wrong) assertions about certain forms of dissent is a threat to this diversity, and hence, to its very essential benefits.

This claim rests on the belief that all dissent is potentially transformative. Any scientific claim has the potential to change the beliefs and/or practices of other scientists. While this is true, we can see from (Brysse, et al. 2012) (Oreskes and Conway 2010) and (Nash 2018) that this is true, but not rightly so. These show that the transformative effect of dissenting research in climate science has at the very least been disproportional to the research put out on it, strongly suggesting that there was an influence of popular communication on their research practices.

On the other hand, I believe that not all dissent should have the ability to transform. Scientists should only transform their beliefs or practices by research that's informed by the Relevant Available Science, and similarly, criticism ought to only be transformative when it's informed by RAS. By this I mean that people attempting to criticise a particular theory, hypothesis, or school of thought, ought to be aware of the most rigorous evidence according to those who support it, as well

as the reasoning behind it. This is what would enable their critique to be novel, and so, rightly potentially transformative. Being aware and merely disagreeing with the RAS you are criticising can be a consequence of non-shared standards.

However, that the RAS has been understood and well reasoned with, has been taken for granted somewhat. Indeed, the most relevant reasoning behind the strength of a theory would be its greatest downfall in the eyes of an opponent to that theory.

The link between avoidance of RAS and EDD is not, however, immediate.

A researcher can write dissent to a theory without engaging RAS if she is indeed ignorant or otherwise unaware of it. It's a great deal to ask of any human being to be up-to-date on the cutting edge research of an entire scientific field, and levelling claims of EDD towards dissent that is borne of this confusion is not what I intend.

Meanwhile, as incidents like these occur, and the critiques and challenges to the heterodox publications go unanswered, external influences use the prominence of the dissent, and the appearance of dissidents as experts, to justify directing additional research efforts towards answering questions considered already well known in the scientific community – in other words, towards epistemic vacillation. This is how EDD and epistemic vacillation are inextricably linked. Groups or individuals that renounce engagement of scientific critique in favour of popular communication end up guiding the direction of science, but through outside, indirect means.

The conclusion of this argument would seem to drive a wrench in the normal functioning of science when directed towards legitimate dissent. If these 'rules of minimal engagement' should be

applied to potentially detrimental dissent, then they must surely be applied to their counterparts. If researchers had to engage every frivolous critique or counter-critique of their original research, we would end up with something that looked very much like the epistemic vacillation we were initially seeking to avoid.

In response to this, I would appeal to the epistemic superiority of the scientific enterprise as I've described it. Of the groups involved in matters of EDD, we can be certain that the aim of the scientist, as epistemic agent, is to find empirically supported claims about nature. As such, they have adopted a method of communication between researchers that has to date allowed them to advance a great deal of knowledge. This does not mean that they are without issues of unwarranted assumptions or non-diverse value inclusivity, but rather that, as a matter of course, they will endeavour to overcome these obstacles, which means that vacillation should be of no concern because it can only obtain once a theory is genuinely well accepted. If it's well accepted for good reason, frivolous critique would be exposed as such, and of course, critiques of science that do indeed lead to improvement, would not be frivolous at all.

6.0 Conclusion

Epistemically detrimental dissent is one of the greatest threats to the ideal distribution of epistemic labour in modern science. The blueprint to stalling or disturbing research efforts in fields that threaten vested interests is being created in front of our eyes, and it is important that we understand what makes the differences between these, and the dissent that is so central to our

scientific method. There are concerns raised that existing efforts to do so have not been comprehensive – that they do not successfully describe paradigm examples of EDD, or indeed only describe a select number of cases that may be part of the relevant group. My attempt is certainly one such effort. Rather than considering this a negative, I believe it draws attention to the true heterogeneity of EDD. A true and thorough accounting of EDD may take a series of narrow, singular arguments, but it is a worthwhile endeavour that should be pursued further in the philosophy of science.

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