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Epistemically Detrimental Dissent in Climate Science

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EPISTEMICALLY DETRIMENTAL DISSENT IN CLIMATE SCIENCE

By

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ABSTRACT

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Dissent, criticism and controversy are integral to scientific practice, especially when we consider science as a communal enterprise. However, not every form of dissent is acceptable in science. The aim of this paper is to characterize what constitutes the kind of dissent that impedes the growth of knowledge, in other words epistemically detrimental dissent (EDD), and apply that analysis to climate science. I argue that the intrusion of non-epistemic considerations is inescapable in climate science and other policy-relevant sciences. As such there is the need to look beyond the presence of non-epistemic factors (such as non-epistemic risks and economic interests) when evaluating cases of dissent in policy-relevant science. I make the claim that the stable factors in the production of are the presence of skewed research and the effective dissemination of this 'research' to the public; the intrusion of non-epistemic values and consideration is only a contingent enabling factor.

Table of Contents

INTRODUCTION

In the epistemic context of scientific practice, ‘dissent’ refers to “the act of objecting to a particular conclusion, especially one that is widely held” (Biddle and Leuschner 2015, 262). In that context, dissent is taken to be epistemically valuable on the basis that humans are not infallible knowers, and that an ideal scientific community encourages diverse opinions, interests and research directions in view of arriving at good theories. Dissenting views are valuable since they expand the epistemic reach of the community in terms of covering more ground in the theoretical space of a given domain.

Nevertheless, not all dissenting views actually contribute positively to this. Certain cases of dissent actually impede the growth of knowledge in science; in other words, epistemically detrimental dissent (EDD). The challenge is to characterize what constitutes EDD. In policy-relevant science, such as climate science, where non-epistemic considerations play significant roles in scientific practice, it is often tempting to evaluate cases of dissent on the basis of what non-epistemic factors such as economic agenda or interests played any roles in research.

In recent times, climate science has enjoyed a lot of attention from both scientists and non-scientists as a result of its bearing on the economy and policy. As such, it is not surprising that a lot of the dissenting views generated in the discipline are often considered to be laden with political and economic motives. However, it should be noted that having a political/economic agenda does not necessarily make dissent epistemically detrimental. Many philosophers agree that influence from non-epistemic values is unavoidable in, and integral to, scientific practice. This is an indication that understanding epistemically detrimental dissent in climate science goes beyond the political and economic interests associated with research in the discipline.

In this work, I shall attempt to characterize Epistemically Detrimental Dissent (EDD)[1] and apply it to climate science. Le Bihan & Amadi (forthcoming) provide an analysis of EDD. Our characterization of EDD concedes that non-epistemic considerations could be significant factors in the production of EDD, but that it needs not be the case. I shall present the arguments in a concise form and apply that to the discipline of climate science. I will argue that the presence of non-epistemic values should not be the basis for evaluating a dissenting view in scientific practice. I shall make the claim that the decisive factors are the presence of skewed research and malpractices, as well as the effective dissemination of the skewed research to the public thereby engendering a manufactured controversy.

The first section of the paper will mainly be an expository discussion of the discipline of climate science. I shall try to identify some of the challenges in the discipline that include modeling limitations, parameterization, and uncertainty. Note that this is not an exhaustive list of the epistemic issues that the discipline grapples with today. However, I shall emphasize that these challenges notwithstanding, there is a wide consensus on the validity of the knowledge base of the science among climate scientists. I shall also present the characterization of EDD and outline the main claim – that the stable factors in the causal landscape of EDD are the production of skewed research and the effective dissemination of same to the public.

What follow in the rest of the paper will be a discussion of these two factors and an application of this analysis to the field of climate science. The second section focuses on the presence of non-epistemic factors in science and its relationship to the production of skewed research, with a particular focus on climate science. I shall argue that the history of science shows that the intrusion of non-epistemic considerations is common in science. I shall concede

[1] Le Bihan & Amadi (2016) provides a deeper analysis of EDD. In this work, I shall present the arguments in a concise form and apply that to the discipline of climate science. REDUNDANT

that the role of these factors tends to go deeper in the issues-based sciences such as climate science; however, I think that it is not sufficient to use that as a basis for rejecting dissenting views. Rather, it shows that the presence of non-epistemic considerations seems to be inevitable. As such, it becomes pertinent to look beyond the intrusion of non-epistemic considerations in our evaluation and characterization of EDD. I shall apply this claim to the discipline of climate science. I concede that there are unacceptable cases of intrusion of non-epistemic factors (funding, risks, economic interests, etc.). Nevertheless, the production of skewed research is a stable factor; we do not need to resolve the debate on what roles non-epistemic values play in order to adjudicate that a research study is skewed.

In the third section, I shall go into our discussion of science as a collective enterprise in the bid to highlight that the effective public dissemination of skewed research is a stable factor in the characterization of EDD. The next section deals with the discussion of S Fred Singer's role in the climate change debate as a possible candidate of a case of EDD.

SECTION ONE

1.0 THE STATE OF CLIMATE SCIENCE

In this section, the main focus is on the discipline of climate science; some issues and challenges associated with climate modeling will be discussed, as well as the problem of uncertainty. Then I shall highlight that there is still a large consensus among scientists despite the various challenges that characterize the discipline.

1.1 UNDERSTANDING CLIMATE AND CLIMATE CHANGE

It seems fitting to start this paper with a brief discussion on climate. I shall point out the difficulty in defining the concepts of climate and climate change. It shall be taken for granted that anthropogenic climate change is an issue that demands attention. However, I shall not focus on the debates on the anthropogenic and non-anthropogenic distinction in the attribution of climate change and the implications to our attitude towards global warming and climate change in general.

Climate is different from weather condition in the sense that characterizing climate takes into account the atmospheric conditions over a long period of time. Weather on the other hand usually refers to the condition of the atmosphere within a relatively short period of time, perhaps at a particular point in time or, say, a day. As such, we could say in a loose sense that the climate condition of a place is the statistical mean of the weather condition over an extended period of time. Frigg et al. (2015a) argue that defining climate is nontrivial, and most definitions of climate usually fall under either one of two kinds. The first emphasizes climate as distribution over time while the second definition sees climate as ensemble distribution. By ensemble distribution is meant "the distribution quantifying how likely it is that certain values of the climate variables are expected at a certain point of time in the future given our current uncertainty about the climate variables" (Frigg et al. 2015a, 953).[2] This typology does not take away the fact that defining climate has proven to be a difficult task among scholars.

To characterize climate entails taking into account certain variables including temperature, precipitation, humidity, CO_2 concentrations, atmospheric pressure, etc. of the atmosphere, the ocean, and the cryosphere.[3] These climatic variables are determined by several elements such as topography, vegetation, volcanic eruptions, presence of water bodies, etc. Thus, the variables usually result from the relationship between these elements, and this relationship is often difficult to characterize. All these physical factors taken together are linked in a complex relationship that determines climate, and scientists try to understand and attempt to characterize climatic behavior.

There is a debate on the problem of appropriately characterizing both the *detection* and *attribution* of climate change.[4] The complexity in the composition and the functioning of the climate system has significant consequences for determining the cause(s) of climate change. Studies (Kroll, 1864; Ogg et al., 2004) have shown that the earth has experienced several different climatic epochs in the past. These studies provide climatic records that go back up to millions of years in the past. The transition from one climatic epoch to another is termed climate change. Previous changes in climate conditions are considered to originate in non-anthropogenic factors, spurring debates on how to characterize such climate evolution. As regards anthropogenic climate change, the debate takes a different dimension. There is the issue of

[2] Frigg et al. (2015a) argue that arriving at a precise definition of climate is nontrivial and contentious. After discussing five popular definitions and their inherent problems, they favor the definition given by Werndl (2015) that "the climate is *the finite distribution over time of the climate variables arising under a certain regime of varying external conditions (given the initial states)."* They argue that it is the definition which faces the fewest problems.
[3] The state of the cryosphere is the state of glaciers and ice sheets. That it counts among the relevant climatic variables is shown by the fact that it was included in the IPCC 2013 report.
[4] See Frigg et al. (2015a, 958-959)

whether the anthropogenic and non-anthropogenic distinction is necessary and significant. This, in some sense, has consequences for determining what the appropriate attitude towards anthropogenic climate change should be; such that the argument could be made that if climate change due to a volcanic eruption is natural, then there should be no negative attitude towards anthropogenic climate change since human activity is a part of the earth's dynamics. However, I shall not concern myself with that in this paper. Instead, it is taken for granted here that anthropogenic climate change deserves significant attention. That said, we are faced with the questions of the reality or otherwise of anthropogenic climate change, of how confident we are in the science of climate change and, of what should count as good science in establishing or denying the claim that the current change in climatic conditions are caused by human activity.

1.2 CLIMATE MODELING: COMPLEXITY AND PARAMETERIZATION

The climate system is a complex one and scientists try to understand the interactions of the various components with the help of modeling simulations. This section focuses on the challenges associated with modeling and tries to identify why it has proven difficult for climate models to narrow down their projections of future climate scenarios.

The complexity of the climate system lies in the fact that the complex relationship between the various components of the system is non-linear; there are several variables which interact in numerous possible ways, and it might prove challenging to adequately capture these interactions. In a nonlinear system, unlike a linear system, any modification in the initial state does not necessarily amount to a proportional modification at the latter stage. Rasband (2015, 2) writes that "(N)onlinearity in a system simply means that the measured values of the properties of a system in a later state depend in a *complicated* way on the measured values in an earlier state." By *complicated* Rasband (2015, 2) means something "other than just proportional to, differing by a constant, or some combination of these two.” These (*proportional to* and *differing by a constant*) denote linear relations, i.e. a linear relation is of the type: $x(t)=ax(t_0)+b$, in which x and y are the variables, and a and b are constants.

It is no surprise then that scientifically representing a complex dynamical system such as the climate is nontrivial. In science, complex dynamical systems are usually characterized with the use of models, and models assume a central role in many scientific contexts (Vespignani 2012; Petersen 1999; Frigg and Hartmann 2006). A scientific model could be said to be simplified representation of a physical system which is too complicated to study in detail. The climate system is complex, and very challenging to study in detail. Scientists design models to simulate the climate system in a simplified form in the bid to understand the system. This simplification is an essential aspect of modeling because complex target system (the physical system of interest) often has numerous parameters and variables that cannot all be included in the model design. As such, it is pertinent to simulate the system by simplifying the models in a way that is intended to preserve the essential features of the system of interest (Levins 1966, 421). It is important to state that scientific models should not be confused with the material objects or mathematical objects that constitute their makeup. There are various issues associated with the concept of models and modeling. For instance, there is the ontological problem of properly characterizing what models are, and an epistemological question of how models represent their target objects. However, these discussions are not a concern in this paper. I take it for granted that models are useful in the study of complex phenomena such as the climate system. Modeling has been largely successful in the quest to adequately represent the climate system. For instance, as just mentioned, it is impossible to carry out real experiments on the climate as a whole. But with the help of models, we can fairly represent the complex climate system. This places us in a position in which we can better understand the things we observe or simulate and to test hypotheses (Knutti 2008, 4656).

Since it is agreed that modeling is appropriate for understanding and studying climate, now the question is; how successful has modeling been for climate science? Climate models have been largely successful in simulating past climatic conditions, especially the climate since mid-19th century when scientists started keeping climate records and data (Jansen et al., 2007). Even though in climate science the practice of modeling is also employed in climate prediction, the success recorded in climate retrodiction does not seem to extend as much to the practice of climate prediction (Knutti, 2008). Assessing the validity of the predictions made from climate models has been a contentious issue.

Several reasons could be identified as to why climate predictions from climate modeling are problematic. Firstly, we are dealing with a complex climate system with several variables whose complex relationships could result in varying future climate scenarios. The multiple models in use today largely agree on simulating past climate behavior. But when it comes to projecting future climate scenarios, their predictions do not seem to strictly converge, and there is no non-arbitrary way of favoring a particular prediction since all of these models have proven to be credible, reliable and plausible in climate retrodiction. This is an indication that knowledge of the climate system is still largely limited and also our modeling techniques are equally limited. Secondly, as just pointed out, it cannot be assumed that models will make good predictions just because they have been doing well in simulating and re-presenting the climatic data of the past. The problem, according to Frigg et al. (2015b, 969), is that “climate projections for high-forcing scenarios take the system well outside any previously experienced state, and at least *prima facie* there is no reason to assume that success in low-forcing contexts is a guide to success in high forcing contexts." Climate forcings are external influences on the climate system such as anthropogenic greenhouse gases. IPCC reports (1990, 1992, 1994, and 1995) define a radiative forcing in terms of the change in the net radiation at the troposphere. Clarke et al. (2007) identifies six relevant GHG's in her discussion of radiative forcings viz: carbon dioxide, methane, hydrofluorocarbons, perfluorocarbons, nitrous oxide and sulfur hexafluoride. Climate models have successfully simulated the effects of such external influences on past climate. Climate predictions involve taking into account the projected increase in the intensity of these influences over the coming decades. The challenge is that many of the variables that are reproduced in the models are very sensitive, and the dynamics and relationship between these variables could be described as non-linear and sometimes chaotic. In some systems such as the climate, the *nonlinearity* of the variables often makes them subject to a chaotic dynamic.[5] Rasband (2015, 2) explains chaotic dynamics in terms of deterministic development with chaotic outcomes; that is to say that from moment to moment, the system is *evolving* in a deterministic way. This is contradistinguished from a random system in that the evolution of the system to the current state is dependent on the previous state, whereas there is no causal interaction between the two states in a random system. He asserts that a dynamical chaos is often accompanied by nonlinearity, and some form of nonlinearity is necessary for chaotic dynamics even though nonlinear relations are not sufficient for chaos. Furthermore, "A system exhibiting chaotic dynamics evolves in a deterministic way, but measurements made on the system do not allow the prediction of the state of the system even moderately far into the future" (Rasband 2015, 2).

As such, in climate modeling, it is difficult to constrain the range of possible future scenarios. The future scenario is dependent on the change in the current state. Model simulations

[5] In one of its reports, the IPCC states: "In climate research and modeling, we should recognise that we are dealing with a coupled non-linear chaotic system, and therefore that the long-term prediction of future climate states is not possible." (see http://ipcc.ch/ipccreports/tar/wg1/505.htm)

of future scenario involve tweaking the variables that represent the current state based on observed trends. For instance, simulating an increase in the emission of CO_2 will increase the surface temperature resulting from the heat that is trapped in the atmosphere. However, even though there is a deterministic causal relationship between an increase in CO_2 and an increase in surface temperature, there are other factors difficult to simulate but that could prove significant to what the future state would be (such as the amount of CO_2 that will be absorbed by vegetation). Thus, it is difficult to capture all these in a rigid way in climate modeling. Different models arrive at different projections depending on what variables in the current state get prominence in the simulations and how the values of these variables are measured or arrived at. This accounts for the disagreements among different model projections of future climatic conditions.

In order to understand the nature of this problem, it is crucial to point out that modeling necessarily involves parameterizations that are not theoretically based and to some degree are arbitrary. Winsberg (2015) defines parameterization as "the method of replacing processes—ones that are too small-scale or complex to be physically represented in the model— by a more simple mathematical description. This is as opposed to other processes—e.g., large-scale flow of the atmosphere—that are calculated at the grid level in accordance with the basic theory." Global Climate Models (GCMs) are designed to simulate the climate system as much as possible. Although it is impossible to represent the system entirely in a model no matter how sophisticated the model may be, these models try to simulate many different important variables that help in simulating the dynamics and evolution of the climate system using relevant fundamental equations. However, certain parameters and variables that are crucial to a climate model are difficult to measure or quantify with precision. An example discussed by Randall (1989) involves cloud parameterization. The radiative effects of clouds, studies show, constitute an important forcing function for large-scale atmospheric circulations and deep cumulus convection, and this has been successfully validated by various climate simulations. The amount of radiative flux that is trapped or radiated by the clouds has a significant impact on the thermal circulation in the atmosphere and the mid-range cloud towers. However, the increasing greenhouse gas (GHG) concentrations have led to uncertainties in cloud feedback (the retention and/or radiation dynamics) and this presents serious challenges for making reliable climate predictions. As such, climatic modeling typically involves parameterization of the process of cloud feedback as it is no longer easily represented in forcing contexts.

Also, the method of parameterization comes in handy when representing sub-grid processes. General Circulation Models (GCM) divide the globe into grids and then simulate the climatic dynamics or evolution using the measurements from the observation of the variables of interest such as precipitation, temperature, atmospheric pressure, etc. But there are many crucial aspects of the climate system whose impact is significant to the dynamics of the system yet they occur on a scale that is much too small to be adequately captured on the grid. Such phenomena include convective clouds, carbon sequestration from trees, etc. These variables are difficult to measure or poorly understood, but ignoring them would result in a deeply flawed simulation of the earth's climate system. Climate scientists usually try to solve for these cases by "zooming in" on the global models. Consequently, these variables are parameterized in order to produce a more complete model simulation that captures the relevant geo-physical relationships. However, this approach comes with some serious problems. Schiermeier (2010, 285) succinctly captures the problem thus; "Zooming in from GCMs bears the risk of blowing up any inherent weakness of the 'mother' model. If the model does a poor job of simulating certain atmospheric patterns, those errors will be compounded at the regional level. Most experts are therefore cautious when asked to make regional predictions." This can be understood as modeling uncertainty. I shall discuss the issue of uncertainty later in this section.

Randall (1989, 346) points out that parameterization could be approached inductively or deductively. The inductive approach seeks to "identify intuitively plausible relationships between the unknowns…and the known variables of the problem." General rules are then formulated on the basis of this finite number of particular cases. This approach has been hugely useful. However, on the flip side, the lack of a theoretical foundation for the parameterizations that it yields constitutes a fundamental shortcoming. The deductive approach, on the other hand, "provides a condensed representation of the important physical processes of interest, and so can give physical insight into the phenomenon being parameterized" (Randall 1989, 346). The challenge with this approach, according to Randall, is that its applicability depends on the assumptions being observationally testable.

These issues present a significant challenge for climate projections for two reasons; firstly, they leave room for questioning the reliability of the model. Secondly, one way to evaluate the reliability of the models with parameterized variables will be to find out if it makes valid projections. But climate projections usually span over decades. At the moment, we have to settle for the fact that arriving at a reliable precise model calibration for climate projections presents a significant challenge since predictions of future climate change relate to a state never observed before. The significance of these challenges tends to deepen when we consider that models do not strongly converge in their projections. The reason, as mentioned earlier, partly lies in the differences in the choice of variables and values from parameterization since different models assign relative importance to various components of the climate system. It seems to be an indication that we might not be able to evaluate how much we have advanced in the skill of climate projection by how better our models have become; at least not yet, perhaps until we have enough observational evidence.

1.3 CHARACTERIZING THE PROBLEM OF UNCERTAINTY

One thing we notice from our discussion on parameterization is that some of the values for the various variables and parameters that climate scientists work with are uncertain, and this has significant consequences for modeling practices and climate projections. As pointed out earlier, the complex nature and the dynamics of the climate system make it difficult to assign precise probabilities to the possible climate scenarios that climate models predict. In fact, according to the IPCC (2013, 141), "The complexity of the Earth system means that future climate could follow many different scenarios, yet still be consistent with current understanding and models." Despite the recorded successes of models in climate retrodiction, it does not seem to follow that model climate projections will be equally successful. The wide range of possible climate scenarios tends to make uncertainty (at least in certain forms) an inevitable phenomenon in climate science. As such the issue of uncertainty is central to the climate science debate. In this section, I shall try to characterize what uncertainty in climate science means and the challenges associated with it. I shall also identify and briefly discuss the types of uncertainty. I shall make the claim that scientists are aware of the challenges posed by the issue of uncertainty in climate modeling and keep trying to find ways of managing the issue.

Frigg et al (2015b, 970) highlight that the term 'uncertainty' describes a situation whereby relevant probabilities are unavailable or only partially available, and this is contradistinguished from 'risk' which describes a situation in which we can attach precise probabilities to specific events even though we might be uncertain about their occurrence. This distinction is important in the characterization and understanding of the problem of uncertainty in climate science, especially with regards to the predictive power of climate models.

Several authors have come up with various typologies in their characterization of uncertainty (Frigg et al. 2015b). The IPCC 2013 Assessment Report identifies three sources of uncertainties. First, natural variability which is inherent in the earth system, and cannot be mitigated by the advancement of knowledge or the addition of variables and data. Natural climate variability is caused by various natural factors such as solar and lunar activities, the oceanic-atmospheric interaction, etc. Models are designed and calibrated using past climatic records, but as discussed earlier, the range of possible future climate scenarios is often wide and may not be adequately captured by models. A second source of uncertainty comes from the emission of greenhouse gases (GHGs). This has to do with the uncertainty in the possible emission rates of GHGs and the trends of land use in the future. The rates of emission and future trends of land use depend on various factors such as population, technology, socio-economic and political choices that define the trajectory of the evolution of the human society. It is not easy to estimate or predict this. Scientists only come up with different alternative scenarios and these might not be exhaustive. A third source of uncertainty has to do with our imperfect knowledge of the climatic response to the future emissions and land use trend by humans. This shows that emission uncertainty and climate response uncertainty are closely linked together. Furthermore, these types of uncertainties have serious consequences for climate modeling since different models come up with different empirical estimates for the components that are difficult to quantify precisely or currently not well understood. A fourth source of uncertainty is identifiable and could be referred to as model uncertainty. This has to do with the challenges of “zooming in” on GCM's to understand climate at the regional level as I briefly highlighted earlier. Also, different models often employ different assumptions, methods and equations to arrive at some empirical approximation when solving for these regional resolutions. It is possible that this kind of uncertainty could be overcome with the development of better models in the future. In this regard, Refsgaard et al. (2013) makes a distinction between epistemic uncertainty (resulting from imperfect knowledge) and aleatory uncertainty (due to inherent variability, largely beyond our control). Model uncertainty could be said to fall under epistemic uncertainty. The term "aleatory" is derived from the Latin *alea*, meaning the rolling of dice. Thus, "an aleatoric uncertainty is one that is presumed to be the intrinsic randomness of a phenomenon" (Der Kiureghian and Ditlevsen (2009, 106). Aleatoric uncertainty in climate science is clearly related to the issues of nonlinearity and chaotic dynamic. In this case, the problem of uncertain values or parameters is a result of the fact that these values change every time the model is run because of the nonlinear chaotic relationship between the variables. The problem is not with the model or any shortcoming associated with the model; it is as a result of the inherent nature of the dynamics of the system. Modeling cannot completely resolve the issue of aleatory uncertainty because this type of uncertainty has nothing to do with the current state of knowledge in modeling techniques.

It seems inescapable that scientists should strive to find a way of handling the issue of uncertainty even though it is largely beyond their control. This becomes pertinent when science is needed to inform policy-making. There are efforts at quantifying and reducing uncertainty in climate science. Knutti (2008, 4650) sums this up by saying that "the question asked at the outset of whether we should believe anything that our models predict about future climate is related to how well we can quantify the uncertainty in model projections. To quantify uncertainty, one needs to decide whether a model is 'credible', 'plausible'[6], or consistent with observations (given some known limitations of the model)." In general terms, the credibility or plausibility of a model can be evaluated in terms of how well the models reproduced historical climate conditions and short term forecasts. This backward-looking consideration seems to be the feasible option due to the long period range of climate forecasts. This was the criteria employed by Brekke et al. 2008. It is the case that many models in use today are evaluated and considered credible and plausible in simulating past and present climate, even though their projections of future climate scenario do not converge.

So far, we see that the problem of the issue of uncertainty is at the heart of climate modeling and climate science in general. It might be safe to assume that there is a considerable awareness among climate scientists of the challenges these issues pose, and a better understanding of the problem is certainly a way forward even though the causes of uncertainty are sometimes aleatory. One method that has been employed to deal with the problem of uncertainty in climate modeling is the use of ensemble models. The reason for ensemble models is a result of the acknowledged uncertainties in individual models. Ensemble models seek to produce and identify robust[7] predictions, or provide the estimates of the uncertainty (in terms of the range of scenarios projected by a set of individual models) about future climate scenario in the bid to reduce the effects of the model uncertainties (Frigg et al. 2015b). This is done by identifying the agreements across a collection of models, and then using that to quantify the uncertainties and arrive at 'robust' predictions. For instance, when 17 models out of a collection of 20 make an interesting predictive hypothesis that an increase in surface temperature of 0.3

[7] By robustness in prediction or robust prediction I mean a predictive hypothesis that an ensemble or collection of models agree on when simulating future climate scenarios (cf. Parker, 2015).

Celsius will lead to the loss of 95 percent of the glacier ice sheets. This robust prediction gives us an idea of the level of (un)certainty regarding a phenomenon of interest. Ensemble models come with some significant problems as discussed by Parker (2011) and Winsberg (2012). Still, this is not to say that they are not useful. In fact, ensembles are widely employed in climate science as scientists seek to tackle the issue of uncertainty.

1.4 THE CONSENSUS ON CLIMATE SCIENCE

At this point, we are inclined to ask what implications all these have for climate projections and climate science in general. From the above discussions, we have seen that the issue of uncertainty is largely unavoidable. However, the nature of the field of climate science and its subject matter seem to suggest that the discipline is on the right track, and our models are getting better, but there is only so much information they can give us about a complex climate system. Scientists are confident in the use of models because they are successfully employed in the physical/hard sciences; models reasonably reproduce the distribution of the variables well and continue improving. They also reproduce observed global trends and have been reliable in simulating past climatic conditions. Furthermore, there is increased confidence in climate modeling since multiple models tend to somewhat agree on *large scale simulations*, projections from newer models are consistent with older models, etc. (Knutti 2008, 4656).

Despite the problem of uncertainty and the fact that the values of multiple model predictions often do not converge, there seems to be an overwhelming consensus on anthropogenic climate change among scientists. Oreskes (2004) highlights that the reports of the Intergovernmental Panel on Climate Change (IPCC) clearly express the scientific consensus on climate change. Indeed, IPCC (2013, 17) asserts that “[H]uman influence has been detected in warming of the atmosphere and the ocean, in changes in the global water cycle, in reductions in snow and ice, in global mean sea level rise, and in changes in some climate extremes … It is *extremely likely* that human influence has been the dominant cause of the observed warming since the mid-20th century." The term "extremely likely" expresses a probability of 95-100%[8]. Oreskes also highlights that all major scientific bodies in the United States with relevant experts agree with the IPCC on this; The National Academy of Sciences, The American Meteorological Society, The American Geophysical Union and The American Association for the Advancement of Science, among others. The normal processes of criticism and peer-review were adopted in the drafting of the reports and positions of specific scientific bodies or communities, and this lends credence to the claim that there is a consensus among scientists on the fact of anthropogenic climate change. Were there legitimate dissenting views that were downplayed in the drafting of these reports? Oreskes argues that an analysis of papers published in refereed journals between 1993 and 2003 indicate that this was not the case. "This analysis shows that scientists publishing in the peer-reviewed literature agree with the IPCC, the National Academy of Sciences, and the public statements of their professional societies. Politicians, economists, journalists, and others may have the impression of confusion, disagreement, or discord among climate scientists, but that impression is incorrect" (Oreskes 2004, 1686). A similar position was also expressed by Cook et al. (2013) after a statistical analysis of the author self-ratings and abstract ratings of thousands of papers published in peer-review journals between 1991 and 2011. They found out that among the papers and authors who expressed a position on anthropogenic global warming, the percentage of those that endorsed the consensus position was slightly more than 97 percent

[8] The IPCC 2013 report gives a list of terms used to indicate the assessed likelihood of uncertainties. See Stocker et al. (2013, 36).

(Cook et al 2013, 1). Also, the percentage of papers rejecting the consensus view keeps shrinking to a vanishingly small proportion over the years.[9]

That said, it is erroneous to simply conclude that the scientific consensus cannot be wrong in this case. At least, the history of science warns us against adopting such a disposition towards consensus views. Oreskes (2004, 1686), warning against this, points out that "[M]any details about climate interactions are not well understood, and there are ample grounds for continued research to provide a better basis for understanding climate dynamics." Such is the story of the development and growth of scientific knowledge; consensus does not suggest dogmatism, and there are established norms within the scientific community to ensure that the critical process is effectively deployed without degenerating into epistemic anarchism.

1.5 THE ISSUE OF DISSENT IN SCIENCE

As noted earlier, there is a wide consensus on the issue of climate change among scientists. Despite the consensus, the critical process remains an integral aspect of scientific practice: it is epistemically valuable because it has heuristic power. Scientific knowledge grows as a result of the rigorous testing of hypotheses that are either confirmed as theories or rejected. This rigorous process of testing sometimes involves the challenge posed by growing critical alternative hypotheses. Also, well-established theories are not immune from this critical process since there is the possibility that a novel idea can challenge standard theories. This keeps science from relapsing into a dogmatic form of knowledge**.** I shall argue that even though climate science is immersed in deep non-epistemic considerations and values which inform the production of

[9] It is important to note that a significant percentage of the papers and authors were neutral on the issue. I think it might be an oversimplification to view the neutrality of these papers as indicating the neutrality of the authors. In fact, it is possible that the aim and structure of the papers does not require that the authors take a position.

competing alternative hypotheses, this does not necessarily lead to bad science since non-epistemic factors may play a neutral, or even beneficial, role in scientific practice. I shall attempt to characterize what constitutes epistemically detrimental dissent. Using that analysis, I shall argue that we need to look beyond the sole intrusion of non-epistemic values in understanding the problem of dissent in climate science. More precisely, a dissenting view may not be dismissed on the sole basis of its being informed by non-epistemic considerations.

1.5.1 DISSENT IN CLIMATE SCIENCE

Scientists might hold dissenting views for a variety of reasons, sometimes depending on their value orientation. These reasons might be epistemic such as the case of new evidence; or they might be non-epistemic as in the case of a scientist who is motivated by desire for fame and recognition or other personal interests and agenda. It is important to emphasize that the focus should be on the ideas advanced in the dissenting views of the critical scientists rather than on their agenda or interest. This is because having an agendum, motivation or subscribing to any particular values does not necessarily make the ideas borne out of these bad. This is a point I shall argue later.

There are usually controversies and disagreements on some specifics of a theory within the scientific community. There might be disagreements on the data-theory fit interpretation, the ranking of pieces of evidence, or the manner of experimentation that is expected to yield the best plausible result. These do not necessarily constitute dissent since the disagreements are all geared towards evaluating a hypothesis or theory. However, when there is an alternative view that challenges the basis of the prevailing view of the scientific community such that it denies the plausibility of the view, especially when such alternative view is held by a minority, then there is dissent. We recall that Biddle and Leuschner (2015, 262) define dissent as “the act of objecting to a particular conclusion, especially one that is widely held." Considering the cases of dissent in climate science from the perspective of Biddle & Leuschner's characterization, it is important to highlight a significant point: the dissenting view is usually set against a widely accepted view.

However, this definition of dissent should not be taken to make the sole claim that dissenting views usually deny the plausibility of only the conclusions, theories, or hypothesis. In fact, dissenting views could latch onto disagreements on methods, evidence, and basic assumptions during the process of theory formulation. This is especially the case in climate science where the problem of uncertainty fuels some of the most problematic cases of dissent and controversies. In the words of Harker (2015, 157), "[T]he mere fact that it is always possible to query scientific ideas creates the opportunity for groups to repeatedly and strenuously draw attention to the ways in which a complex set of ideas might be mistaken or incomplete, thereby distracting us from whatever positive evidence and arguments are available. Uncertainty and doubt are magnified and inflated." This captures the prevalent dissenting position on anthropogenic climate change, whereby dissenters argue that the findings of climate science are too uncertain to inform meaningful action or policy.

Thus, when climate scientists arrive at different quantitative predictions of the rate of global warming, or when climate models differ on their assumptions due to their focus on different aspects of the system, then this does not count as dissent; disagreements in this case are not dissent. Moreover, the disagreements notwithstanding, they seem to share one general conclusion: that the climate is warming significantly as a result of human influence. On the other hand, the case of scientists who are climate skeptics is a typical example of dissent, whether in the form of romanticizing the challenge of uncertainties (and climate modeling as discussed earlier) or an outright denial of anthropogenic climate change despite the consensus within the scientific community.

1.5.2 EPISTEMICALLY DETRIMENTAL DISSENT (EDD)

What constitutes epistemically detrimental dissent? Before addressing this question, I want to highlight a crucial point: the problem of uncertainty does not entail that the standard of epistemic merit in the evaluation of dissenting views should not be upheld. Scientific practice involves continuous research and investigation to improve knowledge, and as highlighted in the previous section, there is continued effort at coming up with ways to better manage the issue of uncertainty within the scientific community. Thus, the commitment to coming up with an adequate representation of the world still holds in climate science as much as it did in the historical cases despite their differences. Consequently, every dissenting view is expected to lay a solid claim to epistemic merit no matter what non-epistemic factors are involved.

In a recent paper, Le Bihan & Amadi (forthcoming) tried to lay out the proper causal structure of the factors that lead to what we chose to call Epistemically Detrimental Dissent (EDD). In that paper, we point out that criticism, controversy, and dissent are integral to the advancement of science, especially once one recognizes that science is a collective enterprise. However, we argue that some instances of dissent in the sciences clearly impede the collective advancement of science; in other words, EDD. EDD has to do with the manufacturing of controversies in some scientific fields. Several factors in the causal landscape of EDD were identified which include the presence of severe non-epistemic risks, large financing, intrusion of non-epistemic agendas/interests, skewed research and effective public dissemination of such skewed research. The main claim in the paper is that the intrusion of non-epistemic agenda, and the presence of large financing and severe non-epistemic risks are only enabling causal factors for EDD. We maintain that two stable difference-makers are core to the production of EDD: the production of skewed science and the effective dissemination of that skewed research in the public. This characterization is based on the distinction between contingent enabling factors and stable difference makers. On the one hand, contingent enabling factors are those factors that could well be “necessary” in the sense that the effect would not have occurred without that factor playing a role in the causal process, but are also highly contingent on a specific environment, and as such, they are unstable. By contrast, stable different makers are also possibly necessary, but that is not what matters: what matters is that they hold under a wide range of environmental conditions.

Applying the analysis of EDD to our discussion on climate science, we see that the requirement of epistemic merit is a decisive factor in this analysis. Also, we see that a dissenting view may not be dismissed on the sole basis of its being informed by non-epistemic considerations. This claim is supported by a few reasons;

1. It has been successfully argued that non-epistemic values do play legitimate roles in science. The problem should be when this leads to *skewed research*. Skewed research, in other words bad science, could be said to involve a practice that claims to be scientific without adhering to the established methodology and rules of science. It is possible for there to be a case of bad practice at any stage of the entire scientific process, from observation, hypothesis construction, experimentation, theory formulation, etc. I shall discuss this further later in this paper.

2. Science is a collective enterprise with an established process of transformative criticism and accepted ways of communicating research within the scientific community, and to policy-

makers and the public at large. There is a problem when these established critical process and channels of communication are subverted by *effectively engaging the public directly using the mass media* and sidestepping the peer-review process.

What follows in the rest of this work will be an expansion on these points. The main claim is that there is a case of EDD when a scientist produces skewed research, side-steps the critical and peer-review process and effectively disseminates this skewed science to the public; thereby creating confusion within the public on an issue that enjoy wide consensus among scientists.

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SECTION TWO

2.0 NON-EPISTEMIC VALUES IN SCIENCE AND THE PRODUCTION OF SKEWED RESEARCH

2.1 INTRODUCTION

In the previous section, we discussed the issues of uncertainty, parameterization and the associated challenges in climate projection and how they play out in climate science. Different scientists studying the same phenomenon might have different approaches for ‘resolving’ these issues depending on the relative importance they attach to the various components or variables of which parameters are difficult to constrain. Owing to the present configuration of society and contemporary economic issues associated with the climate, climate science is clearly a policy-relevant science. The knowledge, epistemic benefits, and results of studies and research projects in climate science are often tied to some non-epistemic considerations. Also, it may not seem far-fetched that the approaches individual scientists or research groups adopt to tackle the challenges in the science might depend on ‘why’ they are conducting the study, and this ‘why’ could be fleshed out by some more or less non-epistemic consideration. It is no surprise then that climate science is inextricably caught up with political and economic interests and issues. The findings have enormous consequences on the economy, the business of corporations and the policy direction of the society. It is tempting to accuse only climate skeptics as the only ones with significant bias. The reality however is that both sides of the climate debate accuse each other of bias because the views that define these positions could be easily identified with certain political and economic ideologies. Jacques et al. (2008) reports that skeptics also accuse environmental science as being corrupted by political agendas that has resulted in the unintentional or malicious fabrication or gross exaggeration of the problem of climate change. It seems to be the widely held view that what constitutes bad dissent in climate science has to do with the intrusion of non-epistemic values. However, the thesis that the intrusion of non-epistemic values and considerations is common in scientific practice is increasingly becoming a widely accepted position. As such it is important to characterize the role of non-epistemic values in scientific practice and in relation to the production of EDD.

In this section I start with a slightly historical approach to the discussion of the role of values in science. I shall make the claim that, contrary to popular view on more established sciences of physics, chemistry and biology, a look at the history of science (with particular reference to three major cases) shows that non-epistemic factors and considerations played a significant role in the success of the theories. I shall however concede that the role these factors and considerations play in climate science seems to be of a different and deeper nature. This is followed by a presentation of the discussion of the role of non-epistemic values in science by some philosophers of science in the bid to show that non-epistemic factors have an important place in scientific practice. This discussion is applied to the field of climate science and our discussion of EDD; the claim will be made that there could be cases of intrusion of values which lead to the production of skewed science. Such cases of intrusive values that lead to skewed research in climate science are often illegitimate, but it is also conceivable that error or incompetence could lead to the production of skewed science in a case of EDD.

2.2 NON-EPISTEMIC FACTORS IN THE HISTORY OF SCIENCE

There is a complex entanglement in the relationship between the epistemic and non-epistemic factors in scientific practice. Whether we are talking about the foundational sciences or the issues-based sciences, one thing seems clear from the history of science; these two classes of factors are often present in some form or another and neither one of them is completely suffocated in good science. However, I shall argue here that in the case of climate science, there seems to be a deeper (and somewhat inevitable) entanglement of the science with non-epistemic considerations.

There is the tendency to assume that non-epistemic factors did not play a prominent role in the revolutions recorded in the history of science, especially in the hard sciences. This view subscribes to the value-free ideal of science. The value-free ideal of science holds that non-epistemic values, such as social and ethical considerations and factors, have no place in scientific practice; only epistemic values (such as explanatory power, predictive accuracy, etc.) which are thought to be those that are directly related to knowledge are allowed in science. However, a look at some of the major historical episodes confronts us with the fact that non-epistemic factors played very significant roles in the course of the events. Arguably, the kind of issues that are associated with climate science today and the way contemporary society is configured is remarkably different. But it would be an over-simplification of history to argue that the scientific revolutions in history were mainly hinged on epistemic factors only. Galileo's case involved some politics and threatened the authority of the Church; his discoveries had a significant direct consequence on the socio-political configuration of the society of his day. Duhem (1969, 107) reports the words of Cardinal Bellarmine about the implications of Galileo's insistence that the foundations of astronomical theory conform to reality; "Not only may it irritate all philosophers and scholastic theologians, it may also injure the faith and render the Holy Scripture false…" This was a time when the authority of the Church held sway.

Also Chang (2010 and 2015) highlights that the Chemical Revolution was characterized by the interplay of various factors, epistemic and social included, and it will be overly simplistic to explain the "revolution" in terms of any one factor. For instance, he argues that Lavoisier and his cohorts "did run an effective and well-coordinated campaign for their new chemistry, including the spreading of their new nomenclature and the controlling of institutional spaces such as the Paris *Académie* and the new journal *Annales de chimie*." But at the same time, the success of his theory equally rested on the growing wave of the building-block ontology of chemical composition which was gradually gaining traction at that time (Chang 2010, 69-70). In the case of Darwin, Kitcher (2003, 11) points out that "part of the secret of Darwin's quick success surely lay in his political skill." Also, it is important to emphasize that some of these controversial epochs in the history of science generated significant socio-political concerns; both Galileo's and Darwin's theories challenged the authority of the Church which was dominant at that time and touched on the belief system of the people. Nevertheless, the non-epistemic factors notwithstanding, the epistemic merit of the theories was based on their empirical success and evidential superiority; and that was *also* crucial to their successful deposition of the status quo. As such, both epistemic and non-epistemic factors played significant roles in these cases.

Now let us consider the issues-based sciences, especially climate science. There seems to be something that remarkably distinguishes climate science from the past sciences. Perhaps, this has something to do with the present configuration of society which is remarkably different from the days of the revolutionary instances I discussed. We can identify a few distinguishing factors. Firstly, the debates and discourses took place *mainly* among the community of scientists and in laboratories. In climate science on the other hand, the main actors and stakeholders comprise a much larger group of individuals beyond the class of scientists and political actors. Secondly, the nature of the non-epistemic factors in climate science significantly differs from what was the case with the historical revolutionary epochs. The presence of severe risks on the public and industry financial interests seems to assume a central role in climate science. It might be the case that some kind of relatively large financing was involved in the historical cases, but their economic implications are of a different nature from what is obtainable in the case of climate science.

The implication of these is not far-fetched; despite the presence of non-epistemic factors in both cases, climate science seems to have deeper socio-political and economic dimensions than the past sciences (at least using the historical cases above as reference points). For those historical instances, the effect of the consequences on the everyday economic life of the ordinary citizen for the most part is minimal. For instance, heliocentricism or Darwinism touches on the belief system of the regular individual but might not affect the source of their livelihood very much. On the other hand, the nature of the issues and problems that climate science deals with, and its huge implications for socio-economic and political concerns run deeper. Its findings have enormous implications on the economic configuration of society as well as the everyday economic life of the regular individual. Consequently, it has proven difficult so far to disentangle the disagreements in the science from the socio-economic and political issues that its findings give rise to, and the debates within the science could be said to be policy-based or policy-driven in a sense.

Hence, it seems pertinent to concede that there is no escaping the presence of non-epistemic considerations in climate science.

2.3 CONTEMPORARY VIEWS ON THE ROLE OF NON-EPISTEMIC VALUES IN SCIENCE

Various scholars have contributed to the discussion of the role of non-epistemic values in scientific practice. If values are considered integral to science, then the questions arise of why and how this is the case. There is the further question of characterizing what should be the appropriate roles of these non-epistemic considerations in order to ensure the integrity and objectivity of the scientific enterprise. Here, I will present the attempts of various philosophers of science at addressing these questions.

2.3.1 THE INESCAPABLE ROLE FOR VALUES IN POLICY-RELEVANT RESEARCH: KEVIN ELLIOTT

Kevin Elliott argues that contextual (non-epistemic) values are part and parcel of scientific practice. Elliott (2011a, 59) defines societal values as “qualities or states of affairs that societies or social groups hold to be good or desirable. Typical examples include fairness, justice, diversity, efficiency, liberty, stability, privacy, and community.” Elliott (2011a) argues that societal values have a significant role in policy-relevant research. This argument is based on three major principles, and scientists have the ethical responsibility to appeal to societal values in their response when these principles apply to any cases they are working on (Elliot 2011a, 55):

The ethics principle – Scientists are ethically obliged to consider the major societal consequences of their work, and as such take reasonable steps to mitigate harmful effects that might arise.

The uncertainty principle – It is often the case that the scientific information in policy-relevant research topics is uncertain and incomplete, and the scientists involved in such research have to take a decision on what should count as standard proof before drawing conclusions.

The no-passing-the-buck principle – It is frequently impracticable and socially harmful for scientists to respond to uncertainty by withholding their judgment or providing uninterpreted data to policy makers.

These principles, according to Elliot, come in handy in justifying two prominent strategies for challenging the value-free ideal viz: the gap argument and the error argument. Proponents of the value-free ideal assert that a narrow formulation could effectively justify the exclusion of societal values in research. One narrow interpretation of the value-free ideal would be to argue that contextual values should be excluded from very specific aspects of scientific reasoning. For instance, when a scientist is faced with a particular theory or hypothesis and a particular body of evidence, there is no legitimate role for societal values in evaluating the evidential support for the theory/hypothesis.

The “gap” argument responds to this claim by highlighting that there is always a logical gap and underdetermination between theory and evidence. It is unavoidably the case that this gap is usually “filled” by such considerations as societal values. The “error” argument, on the other hand, emphasizes that researchers, when drawing conclusions, always run the risk of either accepting a claim that turns out to be false (a false positive or Type I error) or rejecting a claim that turns out to be true (a false negative or Type II error). Elliot (2011a, 66-70) argues that the three principles (ethical, uncertainty and no-passing-the-buck) lend credence and justification to the claims of these arguments despite the various possible objections that might be raised against them.

In chapter 2 of *Is a Little Pollution Good for You?* Elliott, using the hormesis research as a case study, adumbrates and discusses what he referred to as “categories of judgments or methodological decisions that impinge on scientific research” (Elliot 2011a, 28). These include judgments involving (1) the choice of research projects and design of studies; (2) developing scientific language; (3) evaluating studies; (4) applying research. He maintains that the case could be made for the legitimate role of societal values in policy-relevant research that involves these categories of judgments, although it is more obvious in some than in others.

The obvious or easy cases are the decisions on (1) and (4). That contextual values play a legitimate role in the choice and design of studies seems to be an uncontroversial claim according to Elliot. And this claim still holds even if a (controversial) distinction is made between “basic” and “applied” science. In terms of “applied” science (e.g. policy-relevant research such as geoengineering), it is intuitive that the consideration of such societal considerations such as what is needed usually plays some role in the choice of what studies to pursue. Even for a study that is considered as basic research, societal values usually play some role in the choice of continuing with the study instead of some other endeavors.

Also it seems uncontroversial that societal values play a legitimate role in the decisions on applying research (4). Societal values come into play when deciding whether new research-driven regulatory policies are justified and also in determining how to address the combined benefits and harms that result from the new policies. Furthermore, societal values play a clear and important role in deciding what ethical approach (deontological or utilitarian) to adopt in addressing these benefits and harms. This could be seen in cases that involve social justice issues such as when there is a disadvantaged group or population. Elliot (2011a, 72) argues that such societal considerations play a clear and uncontroversial role in the application of scientific knowledge to public policy when such issues are involved.

Regarding categories (2) and (3), Elliot (2011a) presents a detailed argument for the legitimate role of societal values using the three principles used to justify the gap argument (ethical, uncertainty, and no-passing-the-buck principles). In the categories of judgment that deals with the development of scientific language (2), scientists have the ethical responsibility to consider the major impacts of their conclusions on society; they have the responsibility to choose their descriptions or how to frame the concepts in their studies from a range of options. Also, this choice is inevitable even if they do not have decisive reasons to accept one terminology rather than another.

In a similar vein, as regards decisions on the evaluation of studies (3), scientists have the ethical responsibility to consider societal factors when interpreting and evaluating evidence. This is especially the case in policy-relevant research because scientists are aware of the societal ramifications of their work. Also, scientists are often faced with uncertainty because judgments that deal with the evaluation and interpretation of evidence are usually underdetermined by epistemic values and they have the responsibility to make practical decisions despite their epistemic uncertainty (Elliot 2011a, 77).

According to Elliott, societal values are relevant to practical judgments about which hypothesis to accept as a basis for action. "Once societal values influence the collection, analysis, and interpretation of data, scientists can find it difficult to keep their epistemic and practical judgments distinct in practice" (2011a, 80). As such non-epistemic values inescapably play crucial roles in scientific research.

2.3.2 THE DIRECT AND INDIRECT ROLES FOR NON-EPISTEMIC VALUES IN SCIENCE: HEATHER DOUGLAS

Douglas (2009) also argues along the lines that social and ethical values are part of an ideal for scientific reasoning. The basis of this claim is similar to Elliott's principles viz: scientists need these values to make judgments about the potential social and ethical consequences of error their work, the importance of those consequences and set the burdens of proof accordingly. However, she has a normative dimension to the discussion. Douglas warns that there must be important limits placed on the role these values play in science. "To find these limits, it is time to explore and map the territory of values in science. This will allow me to articulate a new ideal for values in science, a revised understanding of how values *should* play a role in science and of what the structure of values in science *should* be" (Douglas 2009, 87). This is in reaction against the value-free ideal of science and its claim that non-epistemic values, such as social and ethical considerations and factors, have no place in scientific practice.

Hence, Douglas sets out to argue that values can play *direct* and *indirect* roles in science. She maintains that values can play an indirect role at every stage of the scientific process, but the direct role is acceptable only for certain kinds of decisions in science. In the direct role, “values determine our decisions in and of themselves, acting as stand-alone reasons to motivate our choices. They do this by placing value on some intended option or outcome, whether it is to valorize the choice or condemn it” (Douglas 2009, 96). An example would be if I chose to take green tea often because I value the health benefits of it’s being a good antioxidant. The direct role is needed when scientists make a decision on which research projects to carry out, when allocating funds for research, and at the stage of deciding which methodology to pursue after choosing a research project (2009, 98-99). A direct role is in play when a scientist chooses a particular subject of interest for his research; say for instance, a climate scientist chooses to study carbon sequestration because she hopes to understand how much a reforestation project would be beneficial in the quest to reduce the amount of carbon dioxide in the atmosphere.

There could be cases of conflicting values in a research project. Douglas gives the example of testing pesticides on a selected human population in a controlled situation in view of understanding the effects of the chemical composition of the pesticide on humans. There is a conflict between the epistemic advantages of such tests and the social/ethical concerns involved. In such cases, the social and ethical values trump the cognitive advantages of the test, argues Douglas (2009, 100).

It is usually problematic when values are allowed to play a direct role during the later stages of research (Douglas, 2009, 102). In such cases, a scientist could reject data or evidence, or even interpret it to fit her perspective merely on the basis of non-epistemic consideration. An example would be rejecting the theory of evolution because of Christian doctrine.

In an indirect role, “values instead serve to assess the sufficiency of evidence for our choices. Values here evaluate whether we think the uncertainties concerning our choices are acceptable, by assessing the consequences of error rather than by assessing the choices themselves” (Douglas et al. forthcoming, 7). Douglas et al. demonstrate this with an example; if Jane does not accept the claim that she needs yearly mammograms between the ages of 40 and 50 because she does not consider the currently available evidence strong enough to support the claim, especially given the known risk of cancer generated by the radiation needed to do the mammogram and the value she places on avoiding that risk; that is an indirect role for values in her judgment. Jane would re-evaluate and change her mind accordingly if the evidence becomes stronger. “The value serves only to assess the acceptability of uncertainty, staying in the indirect role” (forthcoming, 7). In this case, a direct role for values would be a total rejection of mammograms no matter the amount of evidence showing the benefits, simply because she avoids x-rays at all costs. On the basis of this distinction, Douglas et al. argue that, in scientific practice, the indirect role is needed and acceptable at every stage considering that science is a “value-saturated process.” The direct role, on the other hand, should be excluded at certain stages.

Douglas, by making a distinction between the direct and indirect roles for values, makes a case for a new normative ideal that seeks to replace the value-free ideal while checkmating the unacceptable intrusion of non-epistemic factors in science. There is the question of how effective this distinction for evaluating research. I shall address that issue later.

2.3.3 LEGITIMATE AND ILLEGITIMATE VALUES IN CLIMATE SCIENCE: KRISTEN INTEMANN

Kristen Intemann (2015) also brings in a normative bent to the discussion on the role of values. However, instead of evaluating non-epistemic values on the basis of the nature of the role (for instance Douglas's *direct* via *indirect*) they play in the scientist's research, Intemann advocates the aims approach in the discussion of the place of non-epistemic values in science. The aims approach maintains that a distinction can be made between legitimate and illegitimate values on the basis of whether or not they promote democratically endorsed epistemological and social aims of research. Scientists are obliged to make value judgments in their choice of methodological and conceptual frameworks, models and tools, as well as strategies for dealing with uncertainties. And these choices must be based on the aims of the research context which "must be justified by democratic mechanisms that secure the representative participation of stakeholders likely to be affected by the research" (Intemann 2015, 219). Intemann further points out that the legitimacy of value judgments on this approach is a matter of degree, depending on how much they could be said to promote the aims of research, and the extent to which they reflect democratically held values.

Applying the aims approach to climate modeling, Intemann identifies various ways in which it is legitimate for scientists to make value judgments and appeal to the ethical and social aims of the particular research context in making modeling decisions. These include:

Judgments about model adequacy

General Circulation Models (GCMs) provide information and make useful predictions on changes in the global average temperature. These projections may not be very helpful for regional adaptive measures. Regional Climate Models (RCMs) come in handy in such cases, and depending on the aims of the research, individual scientists could legitimately make value judgments on what model features would be adequate in their model design. The adequacy of the model in this case hinges on its ability to serve its purposes as long as they reflect the democratically held values or aims for the region in designing the model. These social and ethical values or aims could range from discounting and duration mapping (sustainability concerns) to economic concerns (food production, forced migration resulting from floods, etc.). For instance, if the concern is how to adapt to worst-case scenarios, then it seems legitimate to choose models that capture extreme weather events.

Decisions about epistemic trade-offs

According to Intemann (2015), value judgments could also play a legitimate role when scientists are faced with the task of adjudicating between epistemic or cognitive values in view of promoting particular social and policy aims of research. Precisely in the practice of model tuning or parameterization (when "zooming in" on GCMs), scientists adjust model parameters to match observed variables. "Improving the model in certain respects often means that it is less accurate in other respects" (Intemann 2015, 221). In the aims approach, the justification for such epistemic trade-offs lies in the social and/or policy aims of the research. For instance, models that are tuned to provide a better representation of the tropical precipitation between land and ocean in Maritime Southeast Asia may not perform well in representing tropical intraseasonal variability (Mauritsen et al. 2013).

Intemann also identified and discussed other ways in which value judgments could play legitimate roles in climate modeling. These include: assessing causation, employing normative concepts, dealing with uncertainties, selecting evidential categories, and interpreting data (Intemann 2015, 221-228).

Intemann concedes that much work still needs to be done in developing the aims approach, especially in terms of what constitute "democratically endorsed" research aims. However, she identified and addressed some objections. Firstly, an objection might be that the aims approach might not be able to exclude paradigm cases of illegitimate values; for example a group could decide to promote the aim of stalling regulatory policy. Intemann (2015, 227) responds by highlighting that "value judgments will be illegitimate insofar as they are not relevant to promoting democratically endorsed aims of the research." Thus, aims are not hinged on the whims and caprices of just any group, but are developed by a democratic mechanism that involves all relevant stakeholders. A second objection could be that the aims approach is consistent with the value-free ideal since the social and ethical value judgments that constitute the aims of the research are made by the stakeholders, not the scientists. Individual scientists merely engage in practical reasoning about what means will achieve or promote those aims. Intemann (2015, 227) responds by arguing that scientists, even with a means/ends reasoning, engage in value judgments since they make evaluations on which means *best* serves stakeholder interests from a range of available options.

A final objection that Intemann identified focused on is the challenge of coming up with a "democratic" machinery for developing the aims of research. The objection highlights the challenging issues of identifying who constitutes a stakeholder and how best to represent a stakeholder group and, even if this were possible, how to get all stakeholders to work together to come up with a consensus on what the aims of a particular research context should be, etc. Intemann responds by arguing that it is a mistake to think that the aims approach requires a two-step process: democratically setting research aims and scientific research to achieve those aims.

“Rather, both climate science and stakeholder engagements can proceed (as they do now) as a process of interactive feedback loops” (Intemann 2015, 228).

The merit of these views cannot be over-stated. One theme that runs through all three views is that non-epistemic values permeate every step of the scientific process in some way. However, the different approaches in the normative ideals adumbrated by Douglas and Intemann indicate the complex nature of evaluating how values should (not) be incorporated in scientific practice. Douglas (2009) and Elliott (2011a) outline the arguments and bases for the claim that non-epistemic values play an integral role in science, the moral obligation to consider the consequences of error being one of the major bases of the claim. For Douglas’s account, its success as an ideal for scientific practice lies in the effective distinction between cases of a direct role and indirect role for values. This distinction may not always be an easy one to make. Elliott (2011b) points out that one way to make sense of this distinction would be on the basis of the *motives* of scientists rather than their *actions*. The problem with this is that it is not always the case that humans have a clear grasp of their motivations. Elliott (2011b, 321) demonstrates this with an example, “suppose that a regulatory scientist who hates the chemical industry chooses to use a particular dose-response model that tends to be highly protective of public health (i.e., it tends to overestimate rather than underestimate the harmfulness of toxic chemicals). It seems unlikely that this scientist would be able to reliably distinguish whether he was choosing that particular model in order to avoid falsely exonerating toxic chemicals (an indirect role for values) or in order to cause financial trouble for the industry (a direct role for values).” Now imagine if there is a case whereby different scientists on the same research team have different motives even though they use the same methodology. As such, it might not be a simple task to evaluate a research project based on the nature of the role non-epistemic values played.

For the aims approach, despite Intemann's acknowledgement that some work needs to be done in terms of what the democratic mechanism should be, some other issues seem to demand attention. Precisely, even if it seems feasible that an effective democratic mechanism could be put in place and work in a feedback loop with scientific research, there seems to be no objective way to regulate the aims that are set for research or what should objectively count as an aim worth pursuing or not. Let us demonstrate this with an imperfect example; right from the years after the 1997 Kyoto Protocol, leaders of some of the most powerful nations including the United States, Australia, and the United Kingdom latched onto the prospect of the Carbon Capture and Storage (CCS) technology and funded studies to realize the supposed potentials. CCS technology, it is expected, would be able to capture up to 90 percent of the CO_2 emissions from fossil fuel-powered industrial processes and electricity generation, thereby drastically reducing the amount of CO_2 released into the atmosphere. Governments touted this supposedly promising technology and economists lauded it as the way to go. But the problem is that "from the outset impartial experts argued that the promise of CCS was exaggerated" (Hamilton 2010, 9). For our purposes here, I shall assume that the backing of the government provides a basis for the claim of "democratic endorsement" in a sense; the government is the trustee of the people, and this case did not really incite significant direct public involvement in the issue. This is a typical case of economic considerations with exaggerated promises conflicting with environmental considerations. From Intemann's analysis, it could be argued that if the optimistic results of the pro-CCS studies are based on the aims of the studies, then they are justified. This gives us an idea of how problematic the aims approach could be since there is no guarantee that the democratically endorsed aims of research would be worth pursuing and/or how to make such adjudication.

From the foregoing, we can make the distinction between the normative approaches of Douglas (2009) and Intemann (2015). For Douglas, the legitimacy of non-epistemic values in scientific practice depends on how these values intrude. Whereas for Intemann, legitimacy depends on which kinds of values are intruding. The upshot of the problems we raised for these normative approaches is that there could be cases where it is slightly challenging to evaluate good or bad science *solely* on the basis of the non-epistemic factors involved. However, we do not need to first determine the nature and role of any non-epistemic factors involved in adjudicating the project to be good science or skewed research. Neither do we need to prove that a scientist willingly skewed his/her research (with ill intentions) in order to decide for a case of skewed science. Furthermore, the non-epistemic factors involved may not have necessarily played a problematic role, at least when considered from Douglas's or Intemann's accounts. The crucial fact is that the research does not fulfill the accepted standards of good science. The implication of this to our discussion of EDD is that the production of skewed science is the stable factor, and its characterization does not have to depend on the evaluation of the roles of the non-epistemic factors involved; these factors are contingent. Nevertheless, owing to the way current society is configured and the centrality of the issues that climate science deals with in the socio-economic and political spheres, cases of skewed science in climate science are often anchored on an unwholesome role for non-epistemic values.

2.4 SKEWED SCIENCE IN THE PRODUCTION OF EDD IN CLIMATE SCIENCE

From the foregoing discussion, we could see that non-epistemic values do play legitimate roles in science in general, and especially in policy-relevant research. There is a sense in which non-epistemic factors are inevitable but also benign in climate science as a policy-relevant science. Also, Winsberg's (2012, 124) highlights the inevitability of value judgments in climate modeling. "When a climate modeler is confronted with a choice between two ways of solving a modeling problem, she may be aware that each choice strikes a different balance of inductive risks with respect to a problem that concerns her at the time…when a modeler is confronted with a methodological choice, she will have to decide which metric of success to use when evaluating the likely success of the various possibilities. And it is hard to see how choosing a metric of success will not reflect a social or ethical value judgment, or possibly even a response to a political pressure, about which prediction task is more 'important' (in a not purely epistemic sense)." This is in line with the views of Douglas (2009), Elliott (2011) and Intemann (2015) as discussed above. This sense of inevitability of the intrusion of non-epistemic factors makes it implausible to evaluate a dissenting view mainly on the basis of the value-inclinations since that would lead to the difficulty of trying to adjudicate between values.

This is not a denial of the claim that there are identifiable acceptable and unacceptable roles for values. Rather, the merit of a dissenting theory or view lies in its claim to provide a more adequate explanation of the world than the competing theories. We do not necessarily need to ascertain the merit or otherwise of a dissenting theory on the basis of influence of non-epistemic values. That is why I argue, on the basis of our characterization of EDD, that the major factors are the production of skewed research and the bypassing of the established channels of scientific communication to effectively feed the public with their "research."

At this point, it might be useful to characterize how skewed science as a stable difference maker in the production of EDD plays out in climate science. But before that, it is important to note that applying the EDD analysis to the historical instances discussed earlier also shows that they were not cases of EDD because skewed research and effective public dissemination were not the decisive factors (the effective campaigns and political skills of Darwin and Lavoisier would not constitute effective public dissemination since they mainly engaged their peers, and more importantly, no skewed research had been done).

Characterizing what constitutes skewed or bad science is no easy task. I will be reluctant to attempt coming up with any list of necessary and sufficient conditions that determine what counts as good or bad science. Indeed, philosophy of science over the years have concerned itself with understanding what should count as (good) science and why we should have confidence in scientific theories and knowledge. It is the case that philosophers of science since the 20th century had advanced various and quite different ideas of what qualifies as a good scientific theory. That being said, it is not bogus to make the claim that good science can be distinguished from bad or skewed research.

In order to attempt a discussion of what constitutes bad science, it seems plausible to have a sense of what scientific inquiry is in general. Longino (1991, 17) thinks of scientific inquiry as a social activity and should not be merely reduced to the products of inquiry. From this, a good understanding of science would take at least two important aspects into consideration within a social context: practices (methodology) and products or goals (knowledge, theory, predictions, control, etc.). The importance of highlighting the social nature of scientific inquiry cannot be over-emphasized since it has enormous bearing on the quest for the objectivity of science; hence the methodology and goals of science are not merely arbitrary or subjective.

In terms of methodology, science as a communal enterprise subscribes to certain rules, criteria or standards which govern the way science seeks to achieve its goals. Certain characteristics such as objectivity, reproducibility, or past success are intrinsic to the justification of a scientific methodology (Anderson & Hepburn, 2016). Ideally, these standards apply at, and guide every step of scientific inquiry – from collection/interpretation of data and experimentation, through the formulation of hypothesis and theories. These steps are not immune from the intrusion of non-epistemic values and background assumptions. Longino (1990, 86) identified ways in which values can enter a given research program and shape knowledge being produced therein: practices, questions, data, specific assumptions and global assumptions The essential transformative critical process integral to science as a communal enterprise serves the purpose of enforcing the standards and checkmating the unacceptable intrusion of values and background assumptions. As such, good science could be said to be scientific inquiry or research whose methodology and products subscribe to the standards.

The methodology of scientific inquiry is expected to yield the desired aims and products of science (knowledge, control, prediction, etc.). We could say that the legitimacy of any of the goals of inquiry lies in its being achieved through the application of the established standards and criteria of scientific inquiry. For instance, astrology is considered as pseudoscience since it does not subscribe to these standards, even though it claims to offer explanation and predictions using the positioning of the stars and planets.

A very important requirement for the legitimacy of scientific aims and methodology is that they successfully pass through the communal critical process that strives to ensure objectivity. As such, individual scientists follow established standards and defer authority to the entire community. Kitcher (1992) talks about the idea of consensus practice within the scientific community; "it is constituted by a language; an (impersonal) assessment of significant questions; a set of accepted statements with a (partial) justificatory structure; a set of explanatory schemata; a set of paradigms of authority and criteria for identifying authorities; a set of exemplary statements, observations, and instruments and justificatory criteria; and, finally, a set of methodological exemplars and methodological principles" (Kitcher 1992, 87). When an individual scientist or a group of scientists come up with a novel idea or claim, that claim is critically considered by the community using their shared standards, accepted as valid knowledge, acknowledged as potentially useful and needing further research, or rejected if it fails to satisfy the criteria of scientific inquiry. Without this mechanism, it could be difficult for science to hold onto its claim as a systematized body of knowledge since that could lead to a situation whereby anything goes – literally – in that case. As such, deferring authority to the community does not entail dogmatism. Rather, it ensures that every claim to knowledge meets the established standards that governs scientific practice. When an idea is rejected by the community, it does not mean that it is discarded completely. It could still be considered as promising or accepted if new evidence emerges in its favor. The reproducibility of research means that other scientists could still work on the idea.

From the discussion above, we can identify what could count as skewed research. Firstly, there is skewed research when the research fails to meet the established standards and criteria that govern scientific inquiry, or when a purported scientific explanation, theory, prediction or knowledge did not yield from the scientific method. For instance, when a scientist plays down the role of GHG's or overlooks the importance of melting glaciers when making climate projections. Another example would be when an astrologer makes horoscope predictions that appear to be true. Secondly, there is skewed research when the inquiry advances results that are informed by assumptions that have been considered and rejected by the community, informed by the desire to advance an already held assumption, and challenges the authority of the scientific community in an unwholesome manner. In other words, the scientist(s) go rogue in a bid to advance other interests. As I mentioned earlier, this would not be the case if the scientist(s) defer to the authority of community while striving to improve on their idea if they truly believe it has a legitimate claim to knowledge.

Climate scientists have sometimes been accused of groupthink. "Groupthink" is a concept introduced by Irvin Janis to denote the psychological tendency of members of a group towards conformity, thereby suppressing dissent. The main principle of groupthink, according to Janis (1971, 85) is: *"The more amiability and esprit de corps there is among the members of a policy-making ingroup, the greater the danger that independent critical thinking will be replaced by groupthink, which is likely to result in irrational and dehumanizing actions directed against outgroups."* Scientists whose research and studies validate the claims of anthropogenic climate change have been accused of groupthink. How true is this claim?

In order to address that question, it is useful to highlight two important terms that Janis used: "amiability" and "esprit de corps." These terms suggest a strong social cohesion among the members of the group. This is more evident when consider the symptoms of groupthink as adumbrated by Janis (1971): illusion of invulnerability and unanimity, collectively constructing rationalizations so as to brush off negative feedback, unquestioned belief in the morality of the group, apply direct pressure to any member that expresses any doubts, stereotyped view of "enemy" groups, etc. (Janis, 1971).

Social cohesion is often anchored on shared values, commitments and intentions and "the sorts of groups identified by social psychologists as exhibiting a high level of social cohesion are precisely groups that engage in shared cooperative activity – committees, task groups, sales teams, and scientific research teams" (Tollefsen 2006, 42). This sounds like a damning verdict against climate scientists as a result of the value-ladenness of the results of their studies. However, it would be an extremely difficult claim to make that the scientific community is a highly socially cohesive group. It is possible that various research groups working on climate science have strong social cohesion within the groups; however, it would be sheer oversimplification to make the claim that such social cohesion translates and extends to the entire community of scientists. The claim that climate scientists exhibit the qualities of groupthink seems to overlook the fact that the consensus on anthropogenic climate change is based on numerous studies conducted by scientists on various different research groups. Moreover, the various research groups often compete among themselves; for instance, funding opportunities are limited and research teams have to compete for the available opportunities. In such cases, it is difficult to make the claim that the various research groups and teams share the kind of social cohesion that characterizes groupthink.

Thus, competition is an important aspect of scientific practice. Of course there are shared values and commitments that the entire scientific community subscribes to – the accepted standards of scientific practice. However, it is this commitment to the shared standards and methodology that ensures that the critical process is sustained within and across research groups and leaves room for competing differing views (including dissent). *Well-supported outstanding claims* often generate significant interest within the scientific community. For instance, in the case of climate science, if a scientist or group of scientists could find a *plausible scientific* way to deny the claim of anthropogenic climate change, then that would be an important contribution that could have great benefits such as fame and great funding opportunities. As such, there is enough incentive that encourages intellectual competition within the scientific community. This makes the groupthink claim in climate science implausible.

In climate science, various phases of research are prone to what we might refer to as bad scientific practice. For instance, there seems to be a large consensus among scientists on the data from paleoclimatology, especially the records of the climate from the mid-19th century when scientists started recording climate data. However, how to interpret these data and how much should be attributed to anthropogenic influences are subjects of controversies. As such, skewed science in climate science could manifest in the form of a biased interpretation of climate data. Such unwholesome interpretation could stem from a number of reasons ranging from sheer incompetence to the quest to justify a presupposed conclusion.

The hockey stick controversy is an instance of this whereby interest groups such as the fossil fuel industry funds research to question the reconstruction of the temperature record of the past millennium. Several scientific reconstructions of the temperature record for the past millennium (Bradley & Jones, 1993; Mann, Bradley & Hughes, 1998) have shown an unmatched sharp rise in the record of the past 150 years following a long-term decline for the previous centuries. A graphical representation of this gave rise to the “hockey stick graph;” the outline looks like a hockey stick. There have been some authors (Soon & Ballunas 1998; Holland 2007) disputing the claim of the hockey stick graph. Connor (2013) reports that industry-funded groups and lobbyists have been disputing the data and methods used in this reconstruction. The aim was to argue that the temperature of the planet has not increased as the hockey stick graph shows, and even if there was significant warming, it was not as a result of the increased emission of CO_2 from the use of fossil fuels; hence the hockey stick controversy. However, the US National

Academy of Sciences (NAS) has come out to back the findings of Mann (1998), expressing that they (members of the NAS) “roughly agree with the substance of their work.”[10]

The practice of climate modeling is also another area that could prove to be prone to skewed research. There are several General Circulation Models that are used in climate projection. As we saw in section one, there are no standard measurements for some of the data that are fed into these models. As such, scientists employ parameterization to account for some of these variables in their modeling of projected future climate scenarios. What informs a scientist’s assumptions and choice of parameters and values in this case could be prone to error. And as in the case of data interpretation, incompetence or bias are potential decisive factors for such cases of error or bad science. For instance, Oreskes and Conway (2010, 186) reports that the Marshall institute released a report authored by Robert Jastrow, Russell Seitz, and Bill Nierenberg blaming the increase in global temperatures on the activities of the sun. They cherry-picked data from a study that claimed the causes for the trend in global temperatures from 1880 were CO_2, volcanoes and the increased solar activity, with emphasis on the effects of the increased emission of CO_2. However, the Marshall institute, motivated by the goal of attacking climate science and “environmental alarmists,” released this report by Jastrow, Seitz and Nieremberg which effectively excised CO_2 and argued that the increased activities of the sun was the major cause of global warming. This is a typical case of bias being the decisive factor in scientific research.

These cases of skewed science were clearly motivated by an unwholesome role of non-epistemic factors. But what stands out is that there was skewed research produced. As I tried to

[10] Brumfiel, G. (2006). Academy affirms hockey-stick graph. Retrieved from URL: http://www.nature.com/nature/journal/v441/n7097/full/4411032a.html

highlight earlier, cases of skewed research might be as a result of a role for values in situations that we might not easily adjudicate their legitimacy or illegitimacy. Thus, the intrusion of non-epistemic values might lead to skewed research, and indeed this is often the case in climate science, however this must not be the case. Applying this analysis to our characterization of EDD, the intrusion of non-epistemic values is only enabling factors of EDD. A stable difference maker is the production of skewed research.

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SECTION THREE

3.1 SCIENCE IS A COLLECTIVE ENTERPRISE WITH ESTABLISHED CHANNELS OF COMMUNICATION

It is safe to assume that bad science or skewed research on its own does not constitute what we have characterized as EDD. The rationale behind this claim is that science as a communal endeavor could identify and address skewed research and bad practices (either resulting from incompetence or bias) with an effective peer review mechanism. There is a case of EDD when bad scientific practice or skewed research manages to rig the system by engaging the public directly using an effective medium.

Conceding that values are integral to scientific practice does not entail that anything goes in the sciences; far from it. Longino argues that the social character of science ensures the objectivity of scientific investigations. This social character does not mean an aggregation of individual efforts, rather "scientific knowledge is…produced through a process of critical emendation and modification of those individual products by the rest of the scientific community" (Longino 1990, 68). Individual ideas and hypotheses (subject to value-ladenness) are passed through the crucible of critical analysis by the entire scientific community. Kitcher (1993, 70) buttresses this point: "If no single scientific mind can store all the propositions (say) that are relevant to the further advancement of a field, then the differences among scientists are not accidental but essential to continued growth: the development of the field would be stunted if uniformity were imposed." This highlights the social nature of the process of knowledge production in science. According to Longino, publicity is essential due to the social character of hypothesis acceptance. Hence, theoretical assertions, hypotheses, background assumptions are public in terms of being generally available; and the states of affairs to which theoretical explanations are pegged are public in the sense of being intersubjectively ascertainable. This makes criticism possible in a way different from mystical experiences or emotional expressions.

Thus, scientific claims are criticized, reviewed, evaluated in relation to competing claims in the bid to rid them of all crude subjective elements. Longino outlines two kinds of criticism:

Evidential criticism: this questions the degree to which a given hypothesis is supported by the evidence adduced for it, also questions the accuracy, extent, and conditions of performance of the experiments and observations.

Conceptual criticism: Longino distinguishes three kinds viz; (a) questions the conceptual soundness of a hypothesis; (b) questions the consistency of a hypothesis with accepted theory; (c) questions the relevance of evidence presented in support of a hypothesis (Longino 1990, 72).

She maintains that (c), criticism that questions the relevance of evidence presented in support of a hypothesis, is crucial for the problem of objectivity since it questions the background assumptions in light of which states of affairs become evidence. This is relevant to our interest in the role of values. “As long as background beliefs can be articulated and subjected to criticism from the scientific community, they can be defended, modified, or abandoned in response to such criticism” (Longino, 1990, 72). Background beliefs and assumptions affect the choice of evidence, the interpretation of data and the weight given to successful tests and experiments. It is important that these assumptions be subjected to the critical process among other competing assumptions and beliefs by the entire scientific community. “Criticism is thereby transformative” since it permits the transformation of background assumptions and beliefs. Subjecting assumptions, beliefs and value judgments to this critical process validates them. Intemann (2001) points out that scientists make value judgments on whose testimony is taken to be reliable. These evaluative judgments and assumptions influence which theories, hypotheses or data they decide to pursue, and the choice of what will count as evidence for or against a theory. It is important to note that the justification or otherwise of these value judgments is what helps to support evidence for a theory or otherwise. "(I)f we recognize a case where scientists have implicitly relied on a value judgment in their theorizing, we cannot immediately infer that it is a case of bad science. Rather, we must seek to determine whether the judgment was in fact relevant and, if so, whether it is justified." (Intemann 2001, S517) These value judgments are not taken at face value, they pass through the crucible of the rigorous critical process of the scientific community.

Thus the emergence of dissenting views is an integral part of science, and Intemann tries to characterize what constitutes reasonable dissent in scientific practice as we saw in the previous sub-section. Addressing how the scientific community should function, she writes; "Although it will also be important to allow room for dissent within scientific communities (again because of the epistemic fallibility of individuals), we need a conception of *reasonable dissent.* That is, there should be equality of intellectual authority among those within a scientific community to propose alternative hypotheses, methodologies, and interpretations of data, as well as raise challenges to the work of others within the scientific community, so long as that dissent relies on reasonable, plausible, or empirically viable claims (including value judgments)" (Intemann 2011, 129). Usually, when this is the case, the scientific community lays a stronger claim to objectivity without degenerating into anarchism[11]. This is in line with Longino's discussion of scientific

[11] More precisely, epistemic anarchism. This is summed up in Feyerabend's (1987 & 2010) critique of scientific practice as "anything goes!" Denise Russell (1983, 440) points out that Feyerabend "uses the expression 'anything goes' to sum up the negative claim that there are no rational criteria of theory evaluation and the positive points that fluid value-laden standards are used to evaluate theories, and that a plurality of methodologies, involving a new relation between reason and practice, is most conducive to progress." That should be the overriding principle for Feyerabend. He argues that an abstract analysis of the relation between idea and action and a thorough consideration of historical episodes confront us with the fact that reliance on any specific methodology in science

criticism. She asserts that the condition for objectivity of a scientific community depends on the ability to satisfy these criteria necessary for achieving the transformative dimension of critical discourse: (1) Recognized avenues for criticism; (2) Shared standards; (3) Community response; (4) Equality of intellectual authority (Longino 1990, 76-81).

These criteria are embodied in the established peer-review system. It is through the peer review system that the transformative critical process ensures the objectivity of scientific research, thereby weeding out skewed studies that do not meet the established standards of scientific practice. In policy-relevant research, it seems intuitive that scientists feel the need to inform the public about the outcomes and implications of their studies. For instance, scientists involved in the climate change debate feel the need to inform the public on the dangers of greenhouse gases/impending climate change and economic consequences of climate mitigation efforts depending on which side of the debate they are. Parker (2011, 120) gives a moral argument for communicating research findings; "In many cases, when their research findings indicate a serious threat to humans, scientists ought to communicate those findings (and the associated threat) to the public." But fulfilling this moral obligation is not so simple in climate science as a result of certain factors that give rise to complications. These include uncertainty, multidisciplinarity, complexity and politicization (Parker 2011, 120). Thus, there is always the danger that a lot of conflicting information on the nature of risks and consequences, some of them based on questionable research, is thrown out to the public through the mass media, engendering confusion and unnecessary polarization of views.

One way to avoid such danger would be for scientists to communicate only studies that have been successfully subjected to the crucible of the critical process/peer-review system. When

is wholesomely illusive in principle. For the purposes of this work, we do not agree with the radical position of Feyerabend, neither shall we go into any discussion of his radical ideas.

a researcher side-steps the critical process of the collective scientific enterprise and engages the public directly with findings that conflict with what the general consensus of the scientific community, it engenders a manufactured controversy, confusion in the public and often leads to a situation where the scientific community feels pressured to address “controversies” that are unreal to a large extent. This, in other words, is EDD – the kind of dissent that impedes the growth of knowledge. This component – the effective public dissemination of skewed research – is a stable difference maker in the production, and an integral aspect, of EDD. This claim is hinged on the intuition that the situation would be significantly different and less grave if the skewed research did not find its way out of the scientific community. There would not be confusion and the misleading idea of an existing controversy, and any resources employed towards addressing such controversies (e.g. funding more studies on the claims of the skewed research project) could probably have been channeled to more cogent studies.

3.2 THE EFFECTIVE DISSEMINATION OF SKEWED SCIENCE IN THE PRODUCTION OF EDD IN CLIMATE SCIENCE

From the foregoing, we could assert that the problem with EDD is not merely about correctness or error of the dissenting view, but the role it plays in impeding knowledge and circumventing the way scientific practice and communication works to advance knowledge. What makes EDD problematic is that it discountenances the accepted shared standards and procedure of scientific criticisms of the scientific community, engages the public directly and engenders an erroneous impression that a genuine scientific controversy exists. We should be clear on what makes these instances cases of EDD. Firstly, results and ideas from bad scientific practice and skewed research are effectively disseminated to the public without passing through the accepted channels of science communication and peer review. Secondly, these ideas significantly diverge from the current accepted position in the scientific community, and most likely they have been considered and rejected as lacking merit among scientists. Thus, the public is made to think that there is a controversy on an issue that enjoys consensus among experts. This was the case with the Marshall Institute report mentioned earlier. Oreskes and Conway (2010) write that the report was first circulated as a "white paper" and eventually got the opportunity to present it as a special briefing to members of top government offices and councils. Ceccarelli (2011) refers to this phenomenon as manufactured controversy. "A scientific controversy is 'manufactured' in the public sphere when an arguer announces that there is an ongoing scientific debate in the technical sphere about a matter for which there is actually an overwhelming scientific consensus. The manufactured scientific controversy can be seen as a special type of 'public scientific controversy' in which 'strategically distorted communication' works to corrode the democratic process" (2011, 196). For instance, as I pointed out in section one, about 97 percent of scientists who took a position explicitly or implicitly in their papers agree to the fact of anthropogenic climate change (Cook et al. 2013). It would be a case of EDD when a scientist gives the public the impression that this is not the case.

An Australian science writer and blogger who holds a BS in microbiology and did an honors research on DNA, Joanne Nova, has a blog that entertains views that challenge the climate change consensus. Being a TV personality, coupled with her "science credentials" ensures that her blog has wide readership. One of the papers published on Nova's blog is "*Honey, I have shrunk the consensus*,"[12] written by Christopher Monckton of Brenchley, a science journalist and an "expert" in global warming issues who was a science adviser to Margaret Thatcher, a former British PM. Monckton's paper adopted a subtler form of argument

[12] Monckton, Christopher. (2013, September). "Honey, I shrunk the Consensus." Retrieved from URL http://joannenova.com.au/2013/09/monckton-honey-i-shrunk-the-consensus/

to reject the claim that there is a consensus on climate change, questioning the figures used by Cook et al. (2013) to argue that there was a consensus. Monckton claimed that a significant percentage of the papers that were considered in determining the level of scientific consensus did not take any position on the debate. The problem with this line of reasoning is that it fails to state the (very minimal) percentage of papers that implicitly or explicitly deny that anthropogenic climate change is a fact. Also, it tends to overlook the fact that many technical scientific papers only aim at reporting observations, data and even make hypothesis on climate records without necessarily going into the issue of anthropogenic climate change. These are clear candidates for EDD because we have cases of "experts" effectively disseminating skewed studies to the public.

Thirdly, a case of EDD usually involves a scenario in which the public is made to question integrity of mainstream scientists and the scientific community unnecessarily. Oreskes and Conway (2010) report that Fred Seitz wrote a letter published in the Wall Street Journal on June 12, 1996, in which he made serious and misleading accusations against Ben Sander, who was the convening lead author for the Chapter 8 of the second IPCC assessment, "Detection of Climate Change and Attribution of Causes." In Seitz's words, according to Oreskes and Conway (2010, 208); "In my more than 60 years as a member of the American scientific community, including my services as president of the National Academy of Sciences and the American Physical Society, I have never witnessed a more disturbing corruption of the peer-review process than the events that led to this IPCC report." It is no surprise that Seitz's problem was that the contents of that report refute the anti-anthropogenic climate change claims he supports. Such scenario does not seem far-fetched considering that the public is one of the stakeholders in the climate debate. The reality of climate change has a direct consequence on the current socio-economic configuration of our society today. Apart from the huge impacts that any pro-climate action policy could have on the fossil fuel industries and other large corporations, the people's way of living as we know it today would be significantly affected too. It might seem intuitive that people would be willing to significantly adjust their lifestyle to save the planet, but they would probably want to be certain that there is an imminent danger to the planet. Manufactured controversies create confusion in the public, and pro-climate scientists are likely to be dubbed alarmists. As such scientists are forced to respond in the bid to address this manufactured controversy and redeem the integrity of the scientific community. This could potentially be time and resource consuming and, instead of advancing knowledge, actually impedes it.

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SECTION FOUR

4.1 S. FRED SINGER ON CLIMATE CHANGE: A CASE OF EDD?

At this point, it might seem fitting to attempt to consider a concrete instance of EDD in climate science in a bid to drive home the arguments in the previous section. I will discuss S. Fred Singer, a well-known climate skeptic, and try to argue that his position is a clear candidate for EDD in climate science.

S. Fred Singer is an atmospheric physicist and a well-known climate skeptic. He rejects the scientific consensus on climate change. Singer considers himself a skeptic, contradistinguished from those he refers to as warmistas (pro-climate change/global warming scientists) and deniers (climate change deniers). In an article he published in the *American Thinker*, he wrote; "In principle, every true scientist must be a skeptic. That's how we're trained; we question experiments, and we question theories. We try to repeat or independently derive what we read in publications—just to make sure that no mistakes have been made… many skeptics go along with the general conclusion of the warmistas but simply claim that the human contribution is not as large as indicated by climate models. But at the same time, they join with deniers in opposing drastic efforts to mitigate greenhouse (GH) gas emissions."[13]

Several different (and in some way contradictory) themes seem to run through Singer's views. His arguments are laced with claims that warn of the economic consequences of climate mitigation. He also tends to argue in a book, *Unstoppable Global Warming*, that the global warming recorded is a result of the activity of the sun, and not human influence. (Singer, 2006).

[13] Singer, F. (2012, February). "Climate Deniers are giving Skeptics a bad name." Retrieved from URL www.americanthinker.com/articles/2012/02/climate_deniers_are_giving_us_skeptics_a_bacd_name.htm

Also, he sometimes seemed to claim that the climate has not warmed in the past three decades.[14] I shall not concern myself with an evaluation or analysis of these claims. However, my analysis of his case as a possible candidate of EDD will be based on his self-characterization as a skeptic who questions the validity of climate models and model predictions.

He is the founder and president of the Science and Environmental Policy Project (SEPP), a research group he founded in 1990 to combat the growing consensus among scientists on the issue of global warming. Scheuering (2014, 125) reports that SEPP has received grants from ExxonMobil, and that Singer has been a paid consultant for some of the largest energy corporations including ExxonMobil, ARCO, Shell Oil Company, Sun Oil Company and Unocal Company. This evident affiliation with the fossil fuel industry is an important factor, and arguably could have played a very crucial role in the research Singer produces. That being said, I do not think the presence of industry financing is enough to discredit Singer's research.

My claim is that there is the need to look beyond the intrusion of such non-epistemic factors such as industry financing. Thus, in the case of Singer, I argue that beyond these non-epistemic factors, there are other identifiable factors that clearly make him a clear candidate for EDD. These factors fall under the two difference makers in the production of EDD: *skewed research* and *effective public dissemination*; and what follows is my trying to provide arguments for my claim.

The first factor has to do with the production of skewed research. Singer's research which counters the consensus position of scientists has been shown to be flawed. Scheuering (2014) reports that Singer repeatedly criticized and rejected the computer model which is the major research tool available to climate scientists. And in a bid to drive home his criticism, he used the computer models that scientists use in predicting global warming and ran his own analysis. His

[14] Singer also made this assertion in the article "Climate Deniers are giving us skeptics a bad name."

analysis for the period between 1950 and 1980 came up with temperatures that deviated from the actual temperatures for the same period. Hence he argued that climate models are faulty and that their prediction of global warming is wrong. He published his findings in his regular column for the *Washington Times* on 29 November 1994 in a paper entitled “Climate Claims wither under the Luminous Lights of Science.”

Now the problem with this claim, according to Elrich et al (1996) as reported by Scheuering (2014, 122), is that Singer “had neglected to adjust his numbers to compensate for the cooling effects of aerosols in the atmosphere, and he also failed to include the actual temperatures of the 1980s, collectively the warmest years in recorded history up to that point.” In the words of Elrich (1996, 39), “If the models had been properly represented and the actual record taken into the 1990s, the predictions would have been shown to be reasonably accurate.” From this, it can be argued that Singer cherry-picked his data in a bid to advance a predetermined conclusion; this makes his research questionable.

Also, there is some cherry-picking in Singer’s choice of data collection method. He prefers to use the data set from satellite measurements to analyze global temperature. Criticizing the IPCC report in which 2500 scientists issued a monumental statement that the balance of evidence suggests a discernible human influence on global climate, Singer argued that “(T)he summary does not mention that the satellite data – the only truly global measurements, available since 1979 – show no warming at all, but actually a slight cooling” (Singer 1996, 581). However, Scheuering (2014, 123) reports that Singer’s critics identified some very important flaws in his argument. Firstly, it has been discovered that the differences in measurement from the gradual decay of satellite orbits were not taken into account by satellite readings. When this factor is taken into consideration, the readings indicate the same warming trend as recorded with surface thermometers. Secondly, weather satellite record only goes back to 1979, which is considered too short to show long-term trends. We might consider this to be a case of an unacceptable direct role for values since Singer's interpretation of climate data was aimed at justifying his preconceived conclusion. However, this line of reasoning must be hinged on clearly identifying what non-epistemic factors (such as industry funding) influenced Singer's study and how that was the case. The problem would be that science is not in the business of mind-reading, and it might be difficult to demonstrate undoubtedly that such he was a mere big-money stooge posing as a skeptic. Fortunately, the scientific community does not have to resolve this quagmire before ascertaining that Singer's research was simple bad science.

When it comes to the issue of effective public dissemination of flawed research, we see that Singer is no stranger to the art of reaching out to the public. Scheuering (2014, 121) writes that when the Earth Summit of 1992 in Rio de Janeiro took center stage, "Singer began making *frequent pronouncements in newspapers and magazines and on radio programs*, spreading doubts about its validity. *His Ph.D. credentials carried significant amount of weight, and his arguments generated significant skepticism among a public that has remained relatively uninformed about the issue*. Even people interested in global climate change don't purchase the computer models that predict warming trends, and very few people actually read journals of atmospheric physics or IPCC reports. *Popular magazines, evening news, and radio talk shows therefore become the source of authority for a wide audience*, and ***Singer, who writes and speaks frequently through these media outlets, can reach millions of people***" (emphasis mine).

Singer has published his views in magazines like the *The New York Times* (e.g. September 28, 1993), *Washington Post* (e.g. October 1, 1967), *American Thinker* (e.g. February 19, 2011), *The Daily Telegraph* (interview; November 19, 2009), *Financial Times* (e.g.

November 26, 2003), among others. He has also given interviews on television stations like the PBS. At first blush, it might seem that there is nothing wrong publishing in these media outlets. However, the problem is that Singer's views which he published through these media have been discredited by the scientific community. This is a case of a scientist rigging the system by engaging the public directly instead of publishing his research in peer-reviewed journals. Even though Singer has published a number of books on climate change and global warming, Scheuering (2014, 125) reports that a Representative in the U.S. Congress, Lynn Rivers, "questioned his credibility because he had not been able to publish any work in a peer-reviewed scientific journal (except one technical comment) for the previous fifteen years. Singer did not deny the charges."

In sum, there is no doubt that there are significant non-epistemic factors that are associated with, and played a role in the case of, Fred Singer. However, my claim is that the reason why his case is a candidate for EDD in climate science is that it is an instance of *bad science* that was *effectively disseminated to the public*. It is likely the case that some kind of economic interest and industry funding or financing were involved, and these might have played significant roles in the production of EDD in his case. But, as Le Bihan and Amadi (forthcoming) argue, these factors are not the stable factors that result in the production of EDD. Instead, the presence of flawed research which he effectively publicized via unorthodox means constitutes the relevant criteria for EDD.

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CONCLUSION

Our discussions so far show that it seems plausible to assert that the science-policy interface is now such that non-epistemic considerations tend to have considerable bearing on the outcome of scientific research. The high degree and nature of the uncertainties in policy-relevant science such as climate science leave room for tinkering and maneuvering of the available data and interpretation to suit intended outcome or preconceived prejudice. This problem is made more complicated because the nature of climate science as a policy-relevant science entails that the various stakeholders (including the public) are eager to get answers from scientists. The established closed peer-review system might be effective in weeding out what gets published in scientific journals, but cannot determine what gets out to the public through the mass media.

To combat the problem of EDD, it is necessary to identify and properly characterize the causal structure of the factors involved. The characterization of EDD and how it plays out in climate science is partly an attempt at clarifying the role of non-epistemic considerations in scientific practice. It is my submission that what counts as EDD is when skewed research is effectively disseminated to the public, in other words, well publicized bad science, leading to manufactured controversies and the consequences that follow from this. In climate science, EDD mostly involves romanticizing the challenges in the science in ways that involve a biased collection and use of data, as well as any skewed research or malpractice aimed at advancing a particular view point (in this case, the denial of climate change), and then engaging the public directly using effective mass media platforms to engender a manufactured controversy. However, it is not clear that the non-epistemic factors leading to a case of bad research are necessarily problematic, and also in some cases, it seems not to be an easy task clarifying what roles these values played in the production of skewed science. As such, the role of the intrusion of non-epistemic values in the production of EDD is contingent; there is the need to look beyond them in the evaluation of dissent in climate science. The production of skewed research is a stable factor – the community of scientists can adjudge a research project to be skewed without having to determine the legitimacy or otherwise of the non-epistemic factors involved. Thus, it is possible that non-epistemic considerations play a role in this scenario, but their roles are largely contingent. The stable difference makers are the production of skewed research and the effective dissemination of this bad science to the public.