Severe Weather Event Attribution

Why values won't go away

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1 Introduction

We start by reviewing the complicated situation in methods of scientific attribution of climate change to extreme weather events. We emphasize the social values involved in using both so-called "storyline" and ordinary probabilistic or "risk-based" methods, noting that one important virtue claimed by the storyline approach is that it features a reduction in false negative results, which has much social and ethical merit, according to its advocates. This merit is critiqued by the probabilistic, risk-based, opponents, who claim the high ground; the usual probabilistic approach is claimed to be more objective and more "scientific", under the grounds that it reduces false positive error. We examine this mostly-implicit debate about error, which apparently mirrors the old Jeffrey-Rudner debate. We also argue that there is an overlooked component to the role of values in science: that of second-order inductive risk, and that it makes the relative role of values in the two methods different from what it first appears to be. In fact, neither method helps us to escape social values, and be more scientifically "objective" in the sense of being removed or detached from human values and interests. The probabilistic approach does not succeed in doing so, contrary to the claims of its proponents. This is important to understand, because neither method is, fundamentally, a successful strategy for climate scientists to avoid making value judgments.

2 The Risk-based approach

Within climate science, the field of Detection and Attribution (D&A) concerns the detection of anthropogenic effects on climate. The attribution part concerns how much or how severe these effects are, that is, how many degrees of temperature, how much extra precipitation or wind or hurricane force, is due to the increase in the presence of anthropogenic "forcing", which is what climate scientists call causal forces on the climate system, generally. D&A is usually done pertaining to either long term trends or to extreme weather events, the latter of which is our topic. In other words, *to what extent is a given extreme event or pattern of extreme events attributable to the increase in greenhouse gases*? Clearly, this is a driving question of the day, especially as it relates to local droughts, floods, and storms.

The methods used to attribute extreme events were first developed and applied in 2003 and 4, in England, by climate scientists at Oxford and the Met Office, specifically Peter Stott, Scientific Strategic Head for the Climate Monitoring and Attribution group at the U.K.'s Hadley Centre in Exeter, part of the Met Office, and Myles Allen and Friederike Otto and their colleagues at Oxford, at the Environmental Change Institute. They developed the probabilistic technique for attributing climate change to extreme events. This is the now-conventional technique for attributing climate change to extreme events. It involves examining the event as one in a class of rare extreme weather or climate events , and using climate models to compare the probability of the extreme event under current factual conditions, represented as (p1), with its probability under counterfactual conditions, represented as (p0), in which the climate, contrary to fact, did not undergo greenhouse warming and anthropogenic changes in general. The relevant probabilities are arrived at through studying climate models as well as empirical data. In this risk based approach, the primary objective is to estimate probabilities and related diagnostics such as the

$$\textbf{Fraction of Attributable Risk} : FAR = 1 - p0/p1$$

and the

$$\textbf{Risk Ratio} : (RR = p1/p0)$$

We can clarify this approach as asking the specific research question: "what is the probability of a specific class of weather event, given our world **with** global climate change, relative to a world **without** global climate change"

3 The Story-line approach

In a set of recent papers, Kevin Trenberth (2011; 2012) and his co-authors, John Fasullo, and Theodore Shepherd (Shepherd, 2016, 2018, 2019; Shepherd *et al.*, 2018; Trenberth *et al.*, 2015), as well as the unrelated group Alexis Hannart and colleagues (Hannart *et al.*, 2016a,b) argued that climate scientists' approach to D&A studies should include a set of *additional, complementary methods*. Shepherd (2016) contrasts the now conventional "risk-based approach" with what he dubbed a "storyline" approach that seeks to explain the origins of *singular events* and the influence of climate change and other causes on those events. He describes the "storyline" approach as "analogous to accident investigation (where multiple contributing factors are generally involved and their roles are assessed in a conditional manner)"(Shepherd, 2016, p.32). This conditioned approach is very general, and can occur at a variety of levels, as Shepherd emphasized. A framework of singular causal events such as that proposed by Cartwright (2017) could be used to formally represent a storyline approach. According to Shepherd, "The most useful level of conditioning will depend on the question being asked." A storyline is a physically self-consistent unfolding of past events, or of plausible future events or pathways. There is no *a priori* probability of the storyline assessed. Instead, the emphasis is on understanding the driving factors involved, and the plausibility of those factors, such as anthropogenic forcings. To illustrate the conditioned, storyline approach, (Trenberth *et al.*, 2015) divide the representation of climate changes into two types.

1. The *dynamics* of the atmosphere. This includes such elements as large scale motions, like cyclonic storms or changes in the jet stream, are responsible for

the placement of a given weather event at a given time; The problems are that these are often difficult to discern, and anthropogenic changes in the dynamics are often small, and therefore hard to attribute (Shepherd, 2014). Shepherd (2014) was very critical of the accuracy of dynamics portions of climate models, and many agree with him.

2. *Thermodynamic changes*. Thermodynamic changes. This includes changes in heat and its effect on moisture content, for example, and are easier to analyze and attribute.

Our understanding of the latter is based on such elements as the basic physical law, the Clausius-Clapeyron relation, that tells us that as the air gets warmer, it will hold more moisture, (7% more water for each degree Celsius), which means that there can be more rain from the storm that is developing. This is an important relation that comes up with regard to hurricanes (Shepherd, 2016; Trenberth *et al.*, 2015). The warmer the sea water is, the more water the hurricane holds. We saw Kevin Trenberth on TV news (MSNBC—in Houston while Harvey was happening.) commenting on the expected floods in Texas based on this physical relation.

For a given severe weather event, the storyline advocates suggest, given a case where we do not have a physically credible model that includes the dynamics, then "under such conditions," it is better for event attribution to focus on thermodynamics of the event (Trenberth *et al.*, 2015, p.729; Shepherd, 2016, p703). In such cases, we should often set aside the question whether climate change altered atmospheric dynamics to make the storm type more likely, for the moment. Instead, the idea is: take the extreme event as a given constraint and ask if thermodynamic factors are involved in such a way as to worsen it.

In essence, they are proposing a conditional format: Given the atmospheric dynamics that brought about the event, how did climate change alter its impacts?

4 Constrasts between the two approaches.

To understand the differences between the two approaches, we like to utilize a framework called the "logic of research questions," which focuses on consideration of the research questions and their possible responsive and appropriate answers (Lloyd, 2015), as we have already hinted. This includes consideration of the relations between the research questions and their possible responsive and appropriate answers, as we've already hinted. Consider the following two examples of a story-line-based, and a risk-based research question, respectively (see Lloyd and Oreskes, 2018):

Box 1: Research Question (Storyline)

"Given the Boulder, CO flood of 2013, How did climate change affect the severity of the flood, all other things being equal?"

Possible and responsive answers:

- It made more water available to the storm (e.g. through Clausius/Clapyron relation) Making the flooding more severe
- It made the storm less severe
- There was no effect of climate change on the severity of the storm (Trenberth et al. 2015)

Note that this research question assumes that the Boulder Flood occurred when and where it did, and also assumes all the climate and weather dynamics associated with its occurrence. In other words, the research question simply assumes these facts, the causes of which are frequently unknown (Shepherd, 2014). The question is simply and narrowly the thermodynamic contribution of climate change and other causes to the severity of the event. On Trenberth et al.'s suggested account, climate change led to increased water in the air, which was funneled into the Boulder Flood from the south, thus increasing the amount of rainfall, and thus the severity of the flood itself (Trenberth *et al.*, 2015). That is a typical answer to a storyline-style extreme event question; it concerns the single event and some of its causes, including climate change and thermodynamics, in the absence of adequate dynamical modeling.

Box 2: Research Question (Risk-based)

"What is the probability or risk of a specific class of event, given our world with global climate change relative to a world without such change?"

Possible and responsive answers:

- The risk of this type/class of extreme events will increase because of climate change
- The risk of this type/class of extreme events will decrease because of climate change
- The risk of this type/class of extreme events will be unaffected by climate change

This is a very different research question than the above. It consists in asking, “what is the probability or risk of a specific class of weather event, given our world *with* global climate change, relative to a world *without* such change?” This question anticipates different possible answers than the question above. All of the possible answers in box 2 involve classes or types of events, rather than singular events, but this logical fact is often forgotten by the users of the risk-based method, who tend also to phrase their answers in terms of singular events (e.g. Lloyd and Oreskes, 2018; Shepherd, 2016, p.32; Stott, 2016; Stott *et al.*, 2017, abstract)

5 When the approaches compete

The Boulder Flood of 2013 was a paradigmatic case of an apparent conflict between the two approaches and the two styles of research question. On the one hand we had Martin Hoerling of the U.S. National Oceanic and Atmospheric Administration, a meteorologist who was responsible for the first study on the Boulder Flood of 2013. He and his colleagues concluded there was no effect from global warming. If anything, they said, climate change may have made the Boulder event less likely (Hoerling *et al.*, 2015).

Trenberth et al.(2015), on the hand, concluded that human effects did have an impact on the storm results. This confirms the point stressed by the National Academy of Sciences. They said that the approach and framing of an attribution study —in our terms, the logic of research questions—may affect its conclusions.

6 Controversy

Trenberth had informally suggested a Bayesian conditional approach in 2011. And in the 2014 IPCC Fifth Assessment Report, the authors of the Working Group II D&A chapter mention the potential utility of the Bayesian approach, noting “uncertainties may in some cases be further reduced if prior expectations about attribution results themselves are incorporated, using a Bayesian approach to attribution, but this [is] not currently the usual practice” (Cramer *et al.*, 2014).

In a paper in *Climatic Change* with Michael Mann as the lead author, Elisabeth Lloyd and Naomi Oreskes presented an argument for the proof of concept for a conditional Bayesian approach to D&A (Mann, Lloyd, and Oreskes, 2017). This suggestion was thought so controversial by this leading journal in climate theory that they commissioned Peter Stott, and his colleagues David Karoly, and Francis Zwiers to write a rebuttal and commentary (Stott *et al.*, 2017). Their commentary does not address a primary issue of ours, which was one of values; we shall address their critique itself in a moment.

What we would to note here, is that work on the storyline approach generated a great deal of controversy (Allen, 2011; Eden *et al.*, 2016; Masters, 2018; McSweeney, 2015; of Sciences and Medicine, 2016; Stott *et al.*, 2013, 2016, 2017; Tollefson, 2015). Why was the proposition to pose and answer different research questions in the science of climate detection and attribution so controversial? Why have scientists reacted so heatedly? We will not address or repeat the more value-laden or personal attacks here. In general, one might expect that community to acknowledge that Trenberth, Shepherd, and colleagues have raised some serious and significant questions and proposals with their new methodology. At minimum, one might have expected a discussion addressing the pros and cons of changing default assumptions, and/or of the feasibility of replacing conventional approaches with conditional ones.

This is not the primary thing that happened. While some scientists responded positively, the dominant response of scientists within the DA community has been strongly negative. Substantive discussions of DA opposed to Trenberth and Shepherd have been offered by a group led by Peter Stott, at the Met Office in the UK, as well as Gabriele Hegerl and Francis Zwiers (2011), and Friederike Otto, Myles Allen, and colleagues (2016), leaders in D&A studies at Oxford. Most centrally—and this appears to be their most forceful objection—these scientists criticize the storyline account suggestion to focus on the thermodynamic aspects of climate change, on the grounds that this would give an incomplete and potentially misleading picture:

> While climate models appear to capture thermodynamic changes well, they may struggle to simulate circulation changes. . . in light of these difficulties, it could be decided to ignore dynamical changes and concentrate instead on how human-induced thermodynamic change have affected extremes. . . However, many event attribution studies consider how the probability of an event is changing. This forces consideration of both dynamical and thermodynamic influences because both can play a role in the changing probability of an event"(Stott *et al.*, 2016)

These authors stress that dynamical effects can work in both directions—potentially making certain kinds of events less likely—so one cannot simply set them aside.

7 Analysis of the controversy

To review the objections given in Lloyd and Oreskes (2018), while these considerations certainly must be taken seriously, there are concerns one might have about the empirical status of the assumption that the counter-examples that they muster are theselves in good standing. This is precisely the assumption about the dynamics we argued can be called into question. Logically, we must consider the following facts. First, the storyline approach is proposed to be applied *when we do not know or do not have confidence in the dynamical effects*. The issues is not one of "ignoring" them; rather, they are not available or adequate in these cases.(Trenberth *et al.*, 2015, p.729; Shepherd, 2016, p703). The challenge from Trenberth et al., Shepherd, and colleagues, however, was whether these dynamics are at all plausible or reliable; That is the point of arguing that we should rely more heavily on thermodynamical modeling. Thus: the claim that there are several dynamical models that go against direction of thermodynamics model, ignores the objection that many dynamical models are inadequate. So offering these dynamical models as counterexamples ignores the basic objections that discredit many dynamical models as inadequate or not credible.

Moreover, why should we treat *these* counterexample dynamical models as more adequate than most? This logical situation has remained unnoticed by the climate scientists involved in this argument and debate, on both sides. But it is a simple logical problem (see Lloyd and Oreskes, 2018). For example, in their critique and rebuttal solicited to comment on the Mann et al. paper in *Climatic Change*, in September, Peter Stott, Karoly, and Zwiers argued that their proposed priors, which allowed for climate change influence, could be wrong in a given case. They demonstrated the wrongness of Mann et al.'s priors through listing a series of four modeling efforts of cases in which the dynamics of the situation went in opposition to the thermodynamics, a situation logically identical to the argument we have just been discussing. They wrote:

> Given that changes locally can be very different to global expectations, as a result for example of *dynamically induced* changes over-coming thermodynamically induced ones, great care must be taken in using prior expectations derived from global considerations.(Stott *et al.*, 2017, p.149)

Again, this argument has the same problematic logic as we've just been describing. If there are likely to be issues with dynamical modeling—and there are —then citing four dynamical modeling cases as counterexamples to the correctness of thermodynamic inferences is begging the question, that is, it assumes what needs to be shown. Yet they want a very strong conclusion:

> . . . such prior expectations [regarding the effects of thermodynamics] might lead to an inappropriate rejection of the alternative null hypothesis proposed by Mann, Lloyd, and Oreskes (2017), namely that there is an anthropogenic influence on the event in question. (Stott *et al.*, 2017, p.148).

Look, moreover, at what happens here:

> However, many event attribution studies consider how the probability of an event is changing. This forces consideration of both dynamical and thermodynamic influences *because both can play a role in the changing probability of an event.* (Stott *et al.*, 2016)

In this block quote, Stott et al. (2016) seem to be focusing on the question of the probability of the event, that is, asking the research question: "what is the probability of a specific class of weather event, given our world with global climate change, relative to a world without such change?"

But this is not the focus of storyline approaches, which ask and answer a *different* type of research question, more specifically, what is the detailed "autopsy" of the extreme event and its causes? The question concerning the probability of the type or class of event itself and how it might have changed from climate change, *is a different agenda*. Thus, when Stott et al. object that dynamics can play a role in the changing probability of an event, they are *simply reiterating their preferred research question regarding risk*. This is an inappropriate objection, given the independent validity of the storyline approach (see discussion in Lloyd and Oreskes, 2018). Either storyline accounts are legitimate or they are not, without getting into issues of risk or probabilities. People want to know what the causes are for their particular storms and floods and droughts, and indeed, hurricanes and wildfires. Thus, they are invested in storyline accounts or autopsies. Yet the risk-based research question is often taken to be more important, more legitimate, or useful than the research questions asked in the storyline approach (e.g. Eden *et al.*, 2016). The storyline approach omits the dynamics when they are not available; if they are, then researchers are free to perform either the risk-based or storyline analyses, or both, as complementary approaches.

The storyline approach is interested in looking at the problem of the causation of an extreme event from a different angle, or using distinct tools. This may seem to be a rejection of the type of modeling that Stott et al. do, but it should be seen not as a rejection but as complementary to their D&A modeling and projects. We think that further progress could be made by the D&A community if the two approaches were accepted as complementary and usable in distinct contexts, as appropriate.

To be sure, Shepherd frames a serious problem with the type of preferred errors that appears using the risk-based approach, writing that if

> an extreme event was caused in part by extreme dynamical conditions, then any risk based analysis using a climate model also has to address the question of whether the simulated change in the likelihood or severity of such conditions is credible. . . And if plausible uncertainties are placed on those changes, then the result is likely to be 'no effect detected'. . . But absence of evidence is not evidence of absence." (2016, p. 32).

8 Inductive risk.

Philosophers of science familiar with the literature on inductive risk[1] will immediately recognize what is going on here. Shepherd is concerned that the risk-based approach will falsely fail to attribute the extreme event to climate change. He is concerned that

[1]See (Rudner, 1953) (Douglas, 2009) and references therein.

approach has a propensity to underestimate harm. (See also (Anderegg *et al.*, 2014) and (Lloyd and Oreskes, 2018))

And this brings us to an arguably very important point, which is that the risk-based folks are concerned about the opposite kind of error. They are concerned about the risk of overstatement of human effects-– which is the kind of error risked by the storyline account. The risk of over-detection is perceived by the D&A community of scientists to be worse than understatement risked by the their own account. Stott et al, for example, write

> By always finding a role for human-induced effects, attribution assessments that only consider thermodynamics **could overstate** the role of anthropogenic climate change, when its role may be small in comparison with that of natural variability, and do not say anything about how the risk of such events has changed (2016, p.33).

Note the concern about making too many false positive errors, or overstating the role of climate change. And here again, we can see the imposition of the risk-based research question, when Stott et al. insist at the end of this quote that an analysis must say something about the changing risk of such events. That is the same as insisting on taking a probabilistic approach, since the storyline approach does not calculate risk ratios and so on.

Stott and colleagues stress that "mistakenly attributing an increased risk of an extreme event to climate change could. . . lead to poor adaptation decisions;" time and money might be spent preparing for events that will not occur. They also warn against the "danger of premature attribution" (2013). This is all true, but the argument is asymmetrical as Lloyd and Oreskes emphasize (2018).

The risk of spending money needlessly or assigning blame prematurely is clearly articulated and warned against, but the risk of understating the threat, and therefore taking insufficient action or failing to hold responsible parties accountable, is not. Myles Allen takes an importantly asymmetrical approach to error. He suggests that if the scientific community misses effects that are actually there, thus understating the harm as a whole, this does

> no particular harm to climate scientists as a group. An individual might miss out on a high-profile paper, but that would be a small price compared to the reputational harm of claiming a positive result that subsequently turns out to be false. (Allen, 2011)

The biggest problem here, as Lloyd and Oreskes point out, is: ". . .the argument is framed in term of risks to scientists and their reputations, but the group most at risk here is not scientists, but society, or more specifically, members of society who may be hurt by disruptive climate change and extreme weather events" (Lloyd and Oreskes, 2018, p.7) Although Allen states that false negatives "can still do harm," that idea is not pursued. (Lloyd and Oreskes, 2018)

9 Against Pluralism?

It is worth noting the degree to which these sorts of consideration fail to be decisive. It is not hard to think of examples where a focus on the thermodynamics of a physical setup provides better predictive power than a focus on dynamics, but it is also not hard to think of examples where the dynamics are more determinative. We are unaware of any good arguments that either approach is intrinsically better–in general and for all cases.

Indeed, there do not really seem to be well-grounded scientific reasons for preferring either approach. According to the Logic of Research Questions analysis, the different methods would be appropriate for answering distinct and different questions, depending on the interests of the questioner. and the context of the investigation. Under such circumstance, one might expect participants to at least be tolerant of a plurality of approaches. But this has not been what we have observed. So what is going on? Some reflection on some quotations from proponents of both the risk-based and storyline approaches can shed some light on this.

> Mistakenly attributing an increased risk of an extreme event to climate change could . . . lead to poor adaptation decisions There is a . . . danger of premature attribution (Stott *et al.*, 2013).

> . . . whether the simulated change in the likelihood or severity of such conditions is credible And if plausible uncertainties are placed on those changes, then the result is likely to be 'no effect detected' . . . But absence of evidence is not evidence of absence. (Shepherd, 2016, p.32).

> always finding a role for human-induced effects . . . could overstate the role of anthropogenic climate change, when its role may be small in comparison with that of natural variability (Stott *et al.*, 2016, p.33).

In combination, these quotations, alongside the absence of good scientific reasons for preferring risk-based approaches to storyline approaches, make it clear that opponents of the storyline approach are primarily motivated by *considerations of inductive risk*.

10 Climbing the Values Ladder

"Inductive risk" was a term coined by Carl Hempel (1965) to refer to the situation, first highlighted by Richard Rudner (1953), in which a scientist needs needs to balance the risk of accepting a false hypothesis against the risk of falsely rejecting a true one. We can easily formulate Rudner's lesson using an example from climate science. Should we, for example, accept or reject the hypothesis that, given future emissions trends, a certain regional climate outcome will occur? Should we accept the hypothesis, let's say, that a particular glacial lake dam will burst in the next 50 years? Rudner's point was that if we accept the hypothesis, we will surely replace the moraine with a concrete dam, but whether we want to do this will depend both on our degree of evidence for the hypothesis, but also on how we would measure the severity of the consequences of building the dam, and having the glacier not melt, vs. not building the dam, and have the glacier melt.

The situation of inductive risk is: as long as the evidence for a hypothesis is not 100% conclusive then we cannot accept or reject the hypothesis without balancing the risk of falsely rejecting the hypothesis against the risk of falsely accepting it. Opponents of the storyline approach such as Allen and Stott et al. seem to be convinced that the negative consequences of mistakenly attributing a severe weather event to climate change when that attribution was not in fact warranted are more serious than the negative consequences of doing the opposite: mistakenly failing to attribute a severe weather event to climate change when such an attribution is in fact warranted. Why do they think this?

It is hard to say for sure what the sum total of reasons they have for reaching this conclusion about the balance of risks, but one consideration that they seem to weigh heavily is on behalf of the risks scientists would face regarding their credibility if they were to mistakenly attribute a severe weather event to climate change.

> Mistakenly attributing an increased risk of an extreme event to climate change could . . . lead to poor adaptation decisions . . . [It poses a] danger of premature attribution. (Stott, 2013)

Making the opposite kind of mistake, on the other hand, poses

> no particular harm to climate scientists as a group. An individual might miss out on a high-profile paper, but that would be a small price compared to the reputational harm [faced by the community of climate scientists as a whole] of claiming a positive result that subsequently turns out to be false. (Allen 2011).[2]

But this does not include the sum total of risks. Stott and Allen only seem to be weighing the risks born by the scientists themselves—the risk of missing out on a potential discovery versus the risk of harming one's reputation. But inductive risk considerations, if they are going to avoid being hopelessly solipsistic, need to also weigh the risks born by consumers of the scientific findings, and not merely their producers. In the case of climate science, those principal risks, as everyone knows, are the risks of being inadequately prepared for, or inadequately mitigating, the increased damages and lives lost produced by severe weather events, on the one hand, and the risk of over-preparing or over-mitigating on the other.

[2]Lloyd and Oreskes (2018) argue that Allen's claim here is empirically false, that in fact, there is reputation harm to scientists who fail to predict important events, but that is not crucial to our argument here.

As Rudner made clear, when scientists accept or reject claims or hypotheses, they need to balance the harms to society as a whole, of accepting the hypothesis should it turn out to be wrong, against the harms, to society as a whole, of rejecting the hypothesis should it turn out to be true. In short, the principal risks that need to be weighed in situations of inductive risk are not, first and foremost, the risk born by the scientists qua scientists. The principal risk are those born by the consumers of the scientists' findings, which in the case of climate comprises society as a whole.

To be charitable, one would have to assume that Stott and Allen both understand this. No one could work in a scientific discipline that is as socially and politically fraught as climate science and continue to be so solipsistic as to believe that only scientists qua scientists bear inductive risk. So what could we imagine is going on that might explain the above passages while exhibiting at least this much charity?

To offer a first pass at an answer to this question, it is worth reviewing the response that Richard Jeffrey (1956) famously authored shortly after Richard Rudner's work appeared in the early 1950's. Jeffrey's famous reply to Rudner was that scientists could avoid having to weigh inductive risks by simply abstaining from accepting or rejecting hypotheses. If, said Jeffrey, scientists simply offer estimates of their degrees of belief in the scientific hypotheses that they evaluate, rather than accepting or rejecting them, they can avoid having to make value judgements about the balance of risks associated with acceptance and rejection of hypotheses. If degrees of belief are passed on to consumers of scientific knowledge, then those consumers can make their own determinations of what actions should be performed, and those determinations can reflect the consumers' own values regarding the harms of performing each (potentially) wrong action.

Looked at in this way, one can see the debate between storyline proponents and risk-based approaches as more or less straight-forwardly recapitulating the Rudner Jeffrey debate. The storyline proponents want to make determinate proclamations regarding how much anthropogenic climate change contributed to the damage produced by this or that severe weather event. The risk-based proponents prefer to offer probabilities. This strategy, they seem to be saying, can help us avoid 'mucking around' in the values.

The upshot of this might seem clear: insofar as you think that Jeffrey's reply to Rudner was a cogent one, it appears to follow that the risk-based folks have the correct view here. Or at least– that the risk-based folks have the view that best approaches the value-free ideal that Jeffrey defended: scientists should abstain, whenever possible, from making value-laden balance-of-inductive-risk decisions by offering probabilistic assessments rather than accepting or rejecting hypotheses. Looked at in this light, it can appear that the dispute between proponents of story-line approaches and proponents of risk-based approach is a dispute about whether or not to aim as well as possible at the value-free ideal of science.

Is this correct? The answer to this question depends, to a large extent, on the degree to which one thinks that the value-free ideal is achievable in climate science. The degree to which one can achieve the value-free ideal, in turn, depends on how well one can assign precise probabilities to climate hypotheses in a value-free way. As has been argued elsewhere, unfortunately, this is especially difficult in sciences, like climate science, that depend on highly complex models constructed by diverse, interdisciplinary groups. (see Biddle and Winsberg, 2009; Parker and Winsberg, 2018; Winsberg, 2012, 2018) In climate science, it is often difficult to find experts who will offer the precise credences they they assign to hypotheses. In practice, expert groups like the IPCC often offer us much coarser depiction of their uncertainty. Ask the IPCC, for example, whether the net feedback strength of clouds is positive or negative, and they will tell you that the probability that net cloud feedback strength is positive is 66% or higher, and that their confidence in this probability claim is "low". (Stocker *et al.*, 2013)

In one respect, when a group of experts offers us a very coarse representation of their uncertainty, they are adhering as closely as possible to the value-free ideal. A simple example can illustrate this. Suppose you are a weather expert, and a client asks you for your expert opinion about the probability of rain tomorrow. But suppose that, for whatever reason, you are only confident that the probability of rain is somewhere between .5 and .75. Suppose, furthermore, that you do not know the utilities that your client assigns either to getting wet, or to carrying an umbrella to work unnecessarily. If your client forces you to choose an exact probability of rain, he or she is in effect forcing you to make a decision on their behalf about the severity of getting wet relative to the severity of needlessly carrying an umbrella. If you chose .5 as your probability, you are implicitly assigning low weight to the severity of getting wet. If you chose .75, you are implicitly assigning it high weight. And any value in between implicitly assigns it some corresponding middle weight. Thus, refraining from picking a precise probability, and rather reporting to the client that the probability falls in the range between .5 and .75 enables you to refrain from having to choose between erring on the side of high severity or low severity.

In fact, in disciplines like climate science, where precise probability distributions are hard to arrive at in value free ways, the knowledge expert can choose from a continuum of possible deliverables. At one end of the spectrum, the expert can declare hypotheses to be true or false. At the other end of the spectrum, she can report very coarse probability ranges for the hypotheses she evaluates. And in the middle, of course, she can report fine-grained probabilities. In one clear respect, this continuum corresponds, respectively, to a continuum of degrees of meeting the value-free ideal. By offering a very coarse probability range (say, all the way from .5 to .75) the weather expert can completely refrain from imposing her values regarding the seriousness of getting wet and the seriousness of unnecessarily carrying an umbrella, on her client. Simply telling her client that it will or won't rain does exactly the opposite. And reporting a precise probability of, say, .65, is in the middle. In this respect, it appears as though the proponents of the risk-based approach have the upper hand: they adhere more closely to the value-free ideal than the story-line advocates do. Behind all of the above quotations from Allen and Stott, we can hear the whisper: 'you storyline proponents are getting down and dirty in the values. When you don't take proper care to avoid premature attribution, you are advocating on behalf of climate alarmists, rather than dispassionately reporting what you know.'

This analysis is correct, but it is only half the picture. There is indeed a spectrum one can travel along, from offering only coarse grained probabilistic forecasts, to making precise ones, all the way to accepting and rejecting hypotheses. And it is certainly correct that when scientists stick their necks out, by moving further along that spectrum toward accepting and rejecting hypothesis, they are moving away from *a* value-free ideal: the ideal that the scientists should refrain from imposing their values regarding whether it is worse to accept a false hypothesis or reject a true one. But it is not true *in general* that as scientists move along the spectrum in the opposite direction, they are thereby abstaining from making value judgements at all. Suppose our weather expert chooses to give a moderately coarse estimate, informing her clients that the probability of rain is "at least 60%". In so doing, she has made a decision, on behalf of her client, that the risk he bears by ignoring the possibility that the probability might be lower than that is insufficiently important. That has to reflect, at least in part, a value judgment on her part that the harm of needlessly carrying an umbrella is lower than the one she would have implicitly been making had she communicated, instead, that the probability was "at least 50%". By the reasoning of the previous paragraph, she has not adhered as closely as she could have to the value-free ideal.

But there are values involved here other than the utilities associated with getting wet and with needlessly carrying an umbrella. Just as you might value staying dry more than I do, you might value getting a more informative forecast from your experts than I do, and conversely, I might value getting a forecast from my experts in which they are maximally confident. And when our expert choses to leave off the probability tail between 50% and 60%, she has given us a more informative forecast than the one that includes the whole range . But a fortiriori, this is also a judgment in which she is less confident. She has favored your values over mine.

If this is right, there is no value-free fulcrum from which to provide expert judgment when it comes to climate science and expert policy advice related to climate change. While it is understandable the Allen, Stott, et al are trying to keep from mucking around in the values, and it is understandable that they view this as a fundamental norm governing scientific behavior, they are somewhat naive in thinking they are able to follow it. While it is not incorrect to understand the story-line folks as being engaged in something akin to advocacy, it is incorrect to understand the opposite stance as the value-free one. The Allen stance is one that values epistemic confidence over informativeness, and Shepherd's stance is the opposite one. But Allen thinks his stance is in fact objective and value free. It is not. Allen's value assessment is not shared by all of the consumers of climate expertise. Many such consumers want to know the causes of their storm, their flood, their drought, and whether climate change is among them. And many climate mitigation advocates would like to use information about event attribution to raise awareness about the need to take action on the path to mitigation. **If this is right, then it would seem that the ideal situation is one in which both risk-based and storyline-based approaches are allowed to co-exist**, and where consumers of climate expertise understand the values underpinning the judgements of their experts.

11 Conclusion

We have reviewed the two most basic approaches to scientific attribution of climate change to extreme events, the storyline and risk-based or probabilistic methods. Both engage social values, although the values appealed to by the distinct approaches are different, as are the types of errors that they seek to avoid. One key strength of the storyline approach is that it reduces false negative results, an advantage that is claimed to have ethical and social virtues for the good of society, protecting it against nasty surprises and unexpectedly severe events. This social protectiveness and informedness is claimed by the advocates of the risk-based approach to be a vulnerability, leading as it may to an excess of false positive errors, a traditional scientific vice; this attitude is one that is frequently taken to indicate greater scientific "objectivity", or closeness to representation of independent reality. In reality, however, we should understand the position of the advocates of the storyline approach as one that places more value on avoiding "harmful reassurance" than do those who favor the risk-based approach.[3]

While the two "sides" of this debate about methods do sometimes argue directly about which is the preferred error, especially in terms of preparedness and informativeness on the side of the storyline approach, and in terms of overpreparedness on the side of the risk-based approach, the debate is often indirect and implicit. While the debate in climate methodology strikingly appears to mirror the old Jeffrey-Rudner debate about values in philosophy of science, on our analysis, there is a second-order feature to the debate that has been under-appreciated.

Lloyd (1995) describes one key meaning of scientific objectivity as "detached" or "disinterested": "'Objective means detached, disinterested, unbiased, impersonal, invested in no particular point of view (or not having a point of view)". In 2013, she made a separate meaning for "unbiased", in her paper with Vanessa Schweizer, and detached was more narrowly described as: "disinterested, independent from will or wishes, or impersonal", where the will or wishes at stake can be either personal or political.[4] But second-order inductive risk shows that neither method helps us to escape social values,

[3] The term "harmful reassurance" comes from the literature on how to offer balanced reporting regarding research on chronic traumatic encephalopathy (Casper *et al.*, 2019; Finkel *et al.*, 2019) which bears some structural similarity to the present topic.

[4] These are all distinguished from "independently existing", "Really real", unbiased, public, procedurally objective, and interactively or structurally objective (having to do with interactions among participants in producing knowledge; Lloyd and Schweizer, 2014).

and be more scientifically "objective" in the sense of being removed or detached from human values and interests. Thus, while the risk-based approach claims to be more "objective" in the sense of "detached", we conclude that it cannot actually succeed in being so, because valuing confidence more than informativeness, as it does, itself invokes social values of various kinds. Thus, there is a stand-off between the views concerning the level of social values involved, and choices simply must be made about which values are to be preferred in particular cases on a case by case basis.

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