Эпистемология и философия науки
2019. Т. 56. № 1. С. 185–201
УДК 165.15

Epistemology & Philosophy of Science
2019, vol. 56, no. 1, pp. 185–201
DOI: 10.5840/eps201956115

Climate change attribution: when does it make sense to add methods?

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A specific form of research question, for instance, “What is the probability of a certain class of weather events, given global climate change, relative to a world without?” could be answered with the use of FAR or RR (Fraction of Attributable Risk or Risk Ratio) as the most common approaches to discover and ascribe extreme weather events. Kevin Trenberth et al. (2015) and Theodore Shepherd (2016) have expressed doubts in their latest works whether it is the most appropriate explanatory tool or the way of public outreach concerning climate events and extremes. As an alternative, these researchers focus on complementary questions, for example, “How much did climate change affect the severity of a given storm?” advocating a “storyline” approach. New methods and new research questions are neither foreign, nor controversial from the standpoint of history and philosophy of science, especially those, related to public interest. Nevertheless, the new proposal has got a tepid reception from the majority of professionals of the given field. They argued that this approach can cause weakening of standards and neglecting of scientific method. The following paper attempts to find the roots of the supposed controversy. We claim inefficiency of uncompromising approach to D&A in absolute sense and assert that errors of a particular type may have a different level of concern in society, depending on the variety of contexts. Therefore, context defines the risk of over-estimation vs. under-estimation of harm.

Keywords: climate change, method, attribution, epistemology, interdisciplinary research

Обоснование климатических изменений: о роли метода*

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Специфические исследовательские вопросы (вроде вопроса о том, какова возможность определенного типа погодных явлений в условиях глобальных климатических изменений) могут быть разрешены при условии метода рационализации обоснования рисков. Между тем, Кевин Тренберт (2015) и Теодор Шепард (2016) в своих последних работах выразили сомнения по поводу того, является ли этот метод наиболее подходящим инструментом объяснения или способом информирования общества о климатических событиях и экстремальных погодных явлениях. Напротив, эти исследователи сосредоточились на побочных вопросах (например, на вопросе о том, как климатические изменения повлияли на силу определенного шторма), используя «сюжетный» подход. С позиции истории и философии науки, новые методы и подходы не являются ни чуждыми, ни противоречивыми. Тем не менее, предложенный подход не получил одобрения у большинства исследова-

* Данный текст лег в основу доклада, зачитанного профессором Элизабет Ллойд в рамках пленарного заседания на Первом Конгрессе Русского общества истории и философии науки (14 сентября 2018 г., Институт философии РАН).

телей в этой области. Исследователи полагают, что он может расшатать принятые стандарты и даже привести к отрицанию научного метода. В этой статье авторы пытаются осмыслить корни возникших противоречий. Они утверждают, что выявление и обоснование рисков с позиции абсолютистского подхода является бескомпромиссным и неэффективным. Авторы заявляют, что относительный риск переоценки и недооценки прогнозируемого ущерба в значительной мере зависим от социального контекста.

Ключевые слова: климатические изменения, метод, обоснование, эпистемология, междисциплинарные исследования

ʽWe'd like to report from the front lines of a recent fight within climate science and modeling, centering around two themes. On our analysis:

The recent resistance from the detection and attribution community in climate science to new methods introduced from others over the past six years has been unproductive, and the objections have missed their targets. Moreover,

More discussion of the societal risks of under-attribution of human effects on climate and the dangers of under reporting of climate change are needed.

In public outreach, NASA discusses the question of observed human-caused climate change under the category of "Facts"—a fact is something known to be true. And all sides in this debate assume this fact. It might seem logical, then, to conclude that if the climate has changed, then at least some of the recent extreme weather events are at least in part caused by climate change. Climate scientists, however, know that the situation is more complex. There have always been extreme events. So as our climate changes there will be extreme events that would have happened without anthropogenic forcing, and by that I mean, without "the aggregate effect of anthropogenic greenhouse gases, aerosols, etc. on the radiative balance."

Within climate science, the field of Detection and Attribution or (D&A)—that's D AND A not "DNA"—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—and I won't be talking about that today—or to extreme weather events, 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 2004, 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 Fredi Otto and their colleagues at Oxford, at the Environmental Change Institute. They developed the probabilistic technique for attributing climate change to extreme events.

I took a trip to England last May to talk with them about these matters, and found especially Peter Stott to be very congenial. Here are their photos. That's Peter Stott in the middle, Frederike Otto on the left, and Myles Allen on the right. Stott is the most open-minded in this group. They developed the now-conventional technique for attributing climate change to extreme events. It involves examining the event as one in a class of events (e.g., heat waves, floods, droughts), and using climate models to compare the probability of the 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 Fraction of Attributable Risk FAR = 1-p0/p1, and the 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?" For the rest of the talk I will refer to this as the "risk-based" or the probabilistic approach.

Sophie Lewis and David Karoly in 2013, for example, use the risk-based approach to conclude that the risk of a record hot Australian summer, such as occurred in 2013, increased 5x in the period since 2005 and projected up to 2020. They conclude that the human contribution to the increased odds of an extreme event like the record hot 2013 summer was "substantial."

In a set of recent papers, Kevin Trenberth and his co-authors, John Fasullo, and Theodore Shepherd, as well as the unrelated group Alexis Hannart and colleagues argued that climate scientists' approach to D&A studies should include a set of additional, complementary methods. And here are Ted Shepherd on the left bottom row, and Kevin Trenberth on bottom right. 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)." This conditioned approach is very general, and can occur at a variety of levels, as Shepherd emphasized.

He said that "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 and colleagues (2015) divide the representation of climate changes into two types: The dynamics of the atmosphere—such 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 published a paper in 2014 in which he was very critical of the accuracy of dynamics portions of climate models, and many agree with him.

Thermodynamic changes, on the other hand—changes in heat and its affect on moisture content, for example—are easier to analyze and attribute. They're based on, for example, 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. The warmer the sea water is, the more water the hurricane holds. We saw Kevin Trenberth on TV news in the US, 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. In such cases, we should often set aside the question whether climate change altered the 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?

One way to understand the differences between the risk-based and storyline approaches is to do a little bit of philosophy of science, and focus on what I call the "logic of research questions." This includes consideration of the relations between the research questions and their possible responsive and appropriate answers, as I've already hinted.

Consider the following setup: 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 answers:

A: it made more water available to the storm (e.g. through Clausius/ Clapyron relation)

Making the flooding more severe

A: It made the storm less severe

A: There was no effect of climate change on the severity of the storm

For example, this research question under the storyline approach might be: "Given that the Boulder, Colorado flood of 2013 happened, how did climate change affect the severity of the flood, all other things being equal?"

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 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 down south, thus increasing the amount of rainfall, and thus the severity of the flood itself (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.

This is a very different research question from 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, all of which 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 answer in terms of singular events (e.g. Stott et al. 2016 Abstract; Shepherd 2016, p. 32).

Here is the alternative setup:

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 answers:

A: The risk of this type/class of extreme events will increase because of climate change

A: The risk of this type/class of extreme events will decrease because of climate change

A: The risk of this type/class of extreme events will be unaffected by climate change

One scientist who is unconvinced by the argument to reconsider the scientific approaches to analyzing extreme events is 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., 2014].

The fact that Trenberth et al. (2015), concluded that human effects *did* have an impact on the storm results confirms the point stressed by the National Academy of Sciences. They said that the approach and framing—in my terms, the logic of research questions—of an attribution study may affect its conclusions. (In fact, on my view, this is true of any scientific study.)

In addition, Trenberth et al. criticized the dynamical portion of Hoerling's models, saying that they did not have a high enough resolution to make judgements about the amount of precipitation in the Boulder flood or model where it came from.

This point was later taken up in a 2017 paper by Pardeep Pall et al, who use a combination of risk-based and high resolution storyline autopsy methods to give a mechanistic model for the flooding in Boulder, They conclude that greenhouse gas drivers increased the magnitude of heavy Colorado rainfall for the week of flooding by 30%. Using both risk-based and storyline methods. This is a lovely and very contemporary paper, because it demonstrates what Shepherd argues about the *complementarity* of the storyline and risk-based accounts, using both to strengthen its results.

So, from the vantage of history and philosophy of science, as well as, very recently, a couple of the scientists, the idea that under conditions of secure knowledge it is appropriate to attempt to explain the causal pathways and mechanisms of individual events, especially when that the dynamics of the situation may seem unavailable, seems entirely noncontroversial.

I first learned of the storyline method from Kevin Trenberth when I was working as an Affiliate Scientist at NCAR, and I thought it sounded reasonable. I wondered why the proposal had triggered such a negative and emotional response, one using language of blame and failure in public discussions of the method online and in newspaper interviews and journals like Nature. And I learned that many scientists have not viewed it as a good thing: in fact, the proposal to adopt the storyline approach when knowledge of the dynamics is lacking has proved highly controversial. Clearly there was a clash of large personalities, but there was also much more at stake. So I set out to investigate this, and meanwhile, developed with my colleagues some ideas supporting an alternative approach compatible with Trenberth and Shepherd's.

One of the ideas that interested me was a Bayesian conditional approach, as suggested informally by Trenberth in 2011. 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, but this [is] not currently the usual practice" [Cramer et al., 2014].

In a new paper that came out in September, with climate scientist and hockey-stick originator Michael Mann as the lead author [Mann, Lloyd, & Oreskes, 2017], we presented an argument for the proof of concept for an alternative Bayesian approach—that is, a conditional Bayesian approach—to D&A.

This suggestion was thought so controversial by a 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, Karoly, Zwiers, 2017]. We were thrilled to receive such a conciliatory piece of commentary to move forward, although not without its critique, but with a view of the complementary nature of the two main approaches foremost in their minds. They do, however, continue to make one error that I'll discuss in a minute, and have yet to address a primary issue of values.

2. The Argument Against an Altered Approach: Scientific Issues

And today, I focus on a related matter: Why was the proposition to add a new approach to D & A studies—to pose and answer different research questions—so controversial? Why have scientists reacted so heatedly?

When a new method is introduced into a scientific field, we would ordinarily expect lively discussion, but in fact, in this case, the response before now has generally been strongly negative, and value laden. I will not address or repeat the more value-laden or personal attacks today. In general, one might expect that community to acknowledge that Trenberth, Shepherd, and colleagues have raised some serious and significant questions and proposals. 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 D&A community has been strongly negative. Substantive discussions of D&A opposed to Trenberth and Shepheard have been offered by a group led by Peter Stott, at the Met Office in the UK, Gabriele Hegerl and Francis Zwiers (2011), and Friederike Otto, Myles Allen, and colleagues [Otto et al. 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 [Stott et al., 2016].

"However," they continue, "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.

But logically, I think, 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*, so no one is "ignoring" them; rather, they are not available or adequate in these cases. [Trenberth et al., 2015, p. 729; Shepherd, 2016, p. 703].

And I'm concerned about the logic of the second line of argument. Stott (2016), Otto et al. (2016) and others have offered a set of *counterexamples* to the storyline methods that supposedly pose a serious challenge [Stott et al., 2016; Pall et al., 2011; Schaller et al. 2016; Vautard et al., 2016; Otto, 2015; Otto et al., 2016, p. 815].

These D&A scientists recite case after case after case, in which some dynamical models are available in that case that seem to mitigate or go against the direction of a thermodynamic effect. These are, indeed, cases in which dynamics seems to make a difference to a thermodynamic outcome.

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 the direction of the thermodynamics models, ignores the foundational 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 [Shepherd 2014; Bindoff et al. 2013; National Research Council 2016].

Why should we treat *these counterexample* dynamical models as *more adequate* than most? Has this question even been addressed by the opponents? Without showing that the dynamical models they are proffering are ***more credible*** than the dynamical models against which the objections have been lodged, they are assuming what needs to be shown.

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. Take the number of models that include the dynamics essentially, all opposing the thermodynamics; logically, without establishing the veracity and empirical support of these models they cite, *which are under challenge*, the D&A authors cannot use them as evidence against the storyline authors, Trenberth and Shepherd et al.

In fact, defenders of the risk-based approach never confront the basic complaint about the unreliability of dynamical models directly, even though this point lies at the foundation of the storyline approach. (Moreover, the D&A authors have not shown that these models are not the exceptions, and in fact, represent a fair sample of cases, such that they can cause trouble for the thermodynamic approach.)

But in their critique and rebuttal solicited to comment on our Mann et al. paper in *Climatic Change*, Peter Stott, Karoly, and Zwiers (2017) argued that our proposed priors, which allowed for climate change influence, could be wrong in a given case. They demonstrated the wrongness of our 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, [and there you have it] great care must be taken in using prior expectations derived from global considerations" [Stott, Karoly, & Zwiers, 2017, p. 149].

Again, this argument has the same problematic logic as I'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 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" [2017, p. 148].

But assuming what needs to be shown is hardly sufficient to demonstrate a systematic deficiency with a Bayesian approach, as is suggested here.

And there is a further problem with the risk-based objections to the storyline approach. Returning to Stott et al's previous objections, they wrote:

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

Notice that 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.

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. 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. Oreskes and I 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.

3. The Argument Against an Altered Approach Part 2: Emerging Social Issues: Risky Methods: False Negative and False Positive Errors

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 [Shepherd, 2016, p. 32].

This is a classic set up of the propensity of risk-based methods towards statistical errors of underestimating harm. And this brings us to an arguably very important point, which is that overstatement of human effects, a false positive—which is the kind of error risked by the storyline account—is perceived by the D&A community of scientists to be *worse* than understatement, a false negative—risked by the their own account.

Stott et al. (2016), 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” [Stott et al. 2016, p. 33; Stott et al. 2013].

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.

And Stott et al. also write that “carefully designed operational attribution systems should help societies understand how they are being affected by climate change and how to avoid the *worst outcomes*” [Scott et.al., 2016, p. 35]. Here, one relevant question is, what specific research questions would optimize such information? To avoid the *worst* outcomes, the risk-based approach *might* make precisely the error we are afraid of, possibly missing the worst outcomes, or a false negative.

The idea that overstatement is worse than understatement is a common one in climate science [Brysse et al., 2013], and is recapitulated in discussion of the use of attribution studies to guide adaptation—and here, for biologists, adaptation is not by natural selection, it’s by human beings—it’s the activities, usually by policy makers, to change our environment in ways to cope with climate change impacts.

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. 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 as a whole is too conservative—missing effects that are actually there—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]. Allen implies that there is no reputational harm to missing effects, but this is clearly incorrect.

Significant reputational harms can accrue to experts who fail to predict important events (think Pearl Harbor and 9/11), or who fail to recognize and warn against adverse effects in a timely manner (e.g., Fukishima, Japan, L'Aquila, Italy, or Flint, Michigan) [Oreskes, 2015].

More important, Allen's 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. Although Allen accepts that false negatives "can still do harm," he pursues that idea no further.

In essence, detection and attribution scientists are maintaining the conventional scientific view that, in research, a false positive is **worse** than a false negative, and declining to consider the suggestion that in planning for climate change the opposite might be the case.

However, if we examine the reaction when scientists took up the challenge to consider the idea that conventional practice might usefully be changed or augmented, it suggests that more than scientific convention is at stake. Contrary to what is implied by many in this debate, it is not true that *scientists and those working in a scientific context* always assume a default of no effect.

The clearest demonstration of this comes from the arena of pharmaceuticals, where it must be demonstrated that a proposed new drug is both effective and safe. To show effectiveness, scientists assume a null hypothesis of no effect—that the drug is not effective and it must be shown that it is (or at least that it is more effective than a placebo or an existing treatment). But for the drug safety, scientists must prove that the drug does not have adverse effects. In this case, the null hypothesis is "(adverse) effect," and scientists must demonstrate that this is not the case.

[New Drug is effective:Null = no effect

New Drug is safe:Null = adverse effect]

The reason for this practice is clear: society is attempting to protect itself from two types of potential harm: one the harm of drugs that do not work, the other the harm of drugs that have damaging side effects. The preferable harm is different with respect to these two concerns.

This demonstrates that the choice of the preferred error, in a case where societal harm is relevant, may depend upon what particular harm we most wish to avoid.

I would note that contrary to what Myles Allen has argued, the precautionary principle of proactive protection against harm cannot be dismissed as easily as he does, as any moral philosopher can tell you, and it is the foundation of some of our established medical and pharmaceutical practices.

For example, cancer screening is another area where the default hypothesis is not necessarily negative: Screening tests are designed to be sensitive, resulting in a high rate of false positive results. False positives lead to unnecessary anxiety and costly and sometimes dangerous follow-up tests [Gigerenzer & Edwards, 2003], but in medicine, unlike in current D&A practices in climate science, we generally prefer the false positive to the false negative errors: A serious problem could be missed and the patient suffers unnecessarily or even dies. There may be, in addition, costs to the reputation and finances of the practitioner and his/her institution.

The crucial point here is that neither approach is intrinsically "correct" or "incorrect." Neither approach is a priori "scientific" or "unscientific." Rather, we judge one risk to be more severe than the other in its particular context, and in light of a particular set of concerns, and frame our analysis accordingly. Thus, we may prefer the false positives to the false negatives, just as proposed by the storyline approach to climate attribution. Given this, we might consider that the suggestion here is to make climate science sometimes more like medicine in its assessments of risk.

Currently in climate, the testing is insensitive, with a low rate of false positives errors, and little over-reacting, and a high rate of false negative errors with high risks to society of under-reporting. There are far fewer cases than in medicine, and if we are wrong in our attribution it is not easy to find that out by doing a second test like in medicine. The severity of false negative vs. false positive errors depends on the consequences of each of the two; in this case, for example, think of weather warnings and the cost of warning when nothing happens vs. the cost of failing to warn when it does.

In addition, in the case of false positives, the money spend for adaptation may be in the billions. Equally, costs will be in the trillions and in lives if we fail to attribute human influence and fail to adapt. So it may be the decision between being bankrupt from adaptation or dead from climate impacts, and for a city or region that makes one decision the usual concepts of Receiver operating characteristics, or ROC, that can be used for many problems involving risks, are not easy adopted. Thus we conclude that to suggest that climate scientists should alter their approach is not scientifically invalid or inappropriate. But it does move us into extra-scientific considerations.

We suggest that this helps to explain why the [Trenberth et al., 2015 & Shepherd, 2016] proposal to change the standards for accepting a positive claim about climate change, including the standard of preferring false positive, over false negative errors, triggered a heated response from the D&A researchers accustomed to using the risk-based approach: Scientists may feel that being informed that they should change or add to their practice is, in effect, being told that they have been doing something wrong.

However, consideration of the role of social context—and particularly, the recognition that in different contexts society may have a greater or lesser concern with errors of a particular type—demonstrates that this is not a matter of "right" or "wrong" in any absolute sense. It is rather that how we view the *relative* risk of over-estimation vs. under-estimation of harm is context-dependent. When investigated by Rosner et al., in coastal flood management decisions, for example, they found that "to ensure a very low probability of overinvestment, [or false positive errors]one must accept a fairly high probability of under-investment", or false negative errors [Rosner et al., 2014, p. 7]. But under-preparation in events of coastal flooding, are quite socially and financially undesirable in many contexts.

We're now in a situation in which about 75% of the moderate hot extremes and about 18% of the moderate precipitation extremes that we see around the world are attributable to warming, most of which is likely to be anthropogenic [Fischer & Knutti, 2015]. With every degree of warming, the rarest and most extreme events, which are the most damaging, have the largest fraction due to greenhouse gases.

What is the value of the different research question offered in the storyline approach? In fact, the storyline approach adds value to the adequate modeling of the system, through its confrontation with data and its physically based causal narrative, and contributes to good forecasting of extreme events. The risk-based approach and the storyline approach, as complementary, can be represented in the same probabilistic framework, outlined in [Shepherd, 2016]. The one approach thus supports the other approach, as one hand interlaces fingers with the other hand. They are *not only* complementary, but mutually supporting.

Luke Harrington just this past year acknowledges that "[f]rom the perspective of an in-depth attribution analysis, multiple analyses using varying levels of conditioning may therefore be complementary" [Harrington, 2017, p. 651]. What Oreskes, Mann, and I have been arguing for is a complementary use of multiple methods, when appropriate to the issue at hand, and when data are available.

Conventional practice today tends to lead to conservative conclusions—where conservative is defined as the tendency to understate rather than over-state the human influence. you might note that from the standpoint of protecting people from harm, the conservative approach might actually be the opposite. This conservativism might also be characterized as due diligence to avoid over-interpreting a relatively short instrumental climate record.

And finally, assuming a particular method and its research questions may create a challenge in science communication, one that may lead to the impression that climate science is less secure knowledge than it actually is. Climate scientists have been repeatedly asked in real-time interviews to deliver expert opinions on whether a particular extreme event—a drought, a flood, a hurricane—was attributable to anthropogenic climate change. In the near future responses to this situation may change, as scientists are increasingly working towards real-time or near-real time attribution, but at present, without doing an attribution study, scientists are essentially obliged to say that they cannot answer this question, and this is, in fact, what scientists generally do say.

However, this may create the impression that there was no relationship between climate change and the event, even where there may have been one. This can lead to harmful under-preparation for extremes in the future. Because no event can be attributed to climate change without an attribution study, this effectively means that scientists following community norms will nearly always convey the message that individual events are not related to climate change—or at least, that we cannot say if they are. In short, it conveys the impression that we just don't know, which feeds into contrarian claims that climate science is in a stage of high uncertainty, doubt, or incompleteness.

In conclusion, we have found that the resistance from the D&A community in climate science to considering new methods has been unproductive. The assumption made by many researchers that the preference for a risk-based approach is justified by societal needs is not well-supported by evidence. On the other hand, some promoters of the storyline approach have overstated their case against the risk-based approach, preventing our desired cooperation and complementary utilization of both approaches when performing attribution. Progress in science involves new methods and approaches as well as new theories and information. "Storyline" methods in attribution in climate science pose different risks than probabilistic attribution methods, but neither one is right or wrong scientifically. We suggest that they should both be treated as useful and available tools in the toolkit of detection and attribution scientists.

Which approach would be preferable in a given case depends in part on which risks we consider more concerning. The relative risks and benefits of the two approaches—including both the risks of over-reaction and under-reaction—deserve a fuller, and more evidentially based discussion. We especially suggest the need for an enlarged conversation about the real-life risks and dangers of under-reaction, in which the roles of values and assumptions in our methodological preferences are more adequately addressed. Whatever the ultimate outcome, the issues at stake deserve a better and fairer discussion than they have to date received.

Acknowledgements:

Elisabeth A. Lloyd was funded by the Arnold and Maxine Tanis Chair of History and Philosophy of Science and the National Center for Atmospheric Research's Affiliate Scientist program. Naomi Oreskes was funded by Harvard University Faculty Development Funds. The authors would like to thank Michael Mann, Chris ChoGlueck, Angie Clayton, Samuel Ryan Ketcham, Reto Knutti, Greg Lusk, Friederike Otto, Theodore Shepherd, Peter Stott, Kevin Trenberth, Francis Zwiers and anonymous referees for *Climatic Change* and *Earth's Future*.

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