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Climate change prediction: Erring on the side of least drama?

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ABSTRACT

Over the past two decades, skeptics of the reality and significance of anthropogenic climate change have frequently accused climate scientists of "alarmism": of over-interpreting or overreacting to evidence of human impacts on the climate system. However, the available evidence suggests that scientists have in fact been conservative in their projections of the impacts of climate change. In particular, we discuss recent studies showing that at least some of the key attributes of global warming from increased atmospheric greenhouse gases have been under-predicted, particularly in IPCC assessments of the physical science, by Working Group I. We also note the less frequent manifestation of over-prediction of key characteristics of climate in such assessments. We suggest, therefore, that scientists are biased not toward alarmism but rather the reverse: toward cautious estimates, where we define caution as erring on the side of less rather than more alarming predictions. We call this tendency "erring on the side of least drama (ESLD)." We explore some cases of ESLD at work, including predictions of Arctic ozone depletion and the possible disintegration of the West Antarctic ice sheet, and suggest some possible causes of this directional bias, including adherence to the scientific norms of restraint, objectivity, skepticism, rationality, dispassion, and moderation. We conclude with suggestions for further work to identify and explore ESLD.

1. Introduction

Over the past two decades, skeptics of the reality and significance of anthropogenic climate change have frequently accused climate scientists of "alarmism": of over-interpreting or overreacting to evidence of human impacts on the climate system (e.g., Singer, 1989, 2000, 2008; Singer and Idso, 2009; Bradley, 1993). Often it is alleged that the motivation for such exaggeration is to gain media attention and funding for research, suggesting that scientists' human desire for attention and practical need for funding biases them toward exaggerating threats (Michaels, 2009, 2010). Some extreme skeptics have gone so far as to declare global warming a "deception" and even a "hoax" (Inhofe, 2003; Ismail, 2010; Bell, 2011; Jeffrey, 2011). Paradoxically, since the release of the Fourth Assessment Report (AR4) of the Intergovernmental Panel on Climate Change (IPCC) such claims have become more frequent, even as the quantity, quality, and diversity of relevant scientific information supporting anthropogenic climate change has vastly increased (Singer and Avery, 2007; Johnson, 2008, 2009; Singer and Idso, 2009; Glover and Economides, 2010; Ismail, 2010; MacRae, 2010; Michaels and Balling, 2009; Surhone et al., 2010; Bell, 2011; Jeffrey, 2011).

Given these gains in knowledge, and that scientists have been making specific projections regarding the likely outcomes of increased atmospheric concentrations of greenhouse gases since the late 1980s, it is possible to begin to assess whether scientists have over- or under-predicted such outcomes. That is to say, it is possible to evaluate claims of exaggeration and alarmism, and to ask whether the available empirical evidence supports such claims or not. If not, it would be timely to consider factors, including social and cultural ones, which might lead scientists to the opposite behavior: not to exaggerate threats and over-interpret their data, but rather to minimize threats and interpret their data in a conservative way.

In this paper, we suggest that such a factor may exist, and that scientists are biased not toward alarmism but rather the reverse: toward cautious estimates, where we define caution as erring on the side of less rather than more alarming predictions. We argue that the scientific values of rationality, dispassion, and self-restraint tend to lead scientists to demand greater levels of evidence in support of surprising, dramatic, or alarming conclusions than in support of

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conclusions that are less surprising, less alarming, or more consistent with the scientific status quo. Restraint is a community norm in science, and it tends to lead many scientists (ceteris paribus and with some individual exceptions) to be cautious rather than alarmist, dispassionate rather then emotional, understated rather than overstated, restrained rather than excessive, and above all, moderate rather than dramatic (on community norms, see Bernard, 1927; Conant, 1953; Merton, 1979; Keller, 1985; Harding, 1986; Haraway, 1989). We call this tendency "erring on the side of least drama (ESLD)."

We begin by summarizing available evidence that scientists have been conservative in their projections of the impacts of climate change. In particular, we discuss recent studies showing that at least some of the key attributes of global warming from increased atmospheric greenhouse gases have been under-predicted, particularly in IPCC assessments of the physical science, by Working Group I (Rahmstorf et al., 2007; Pielke, 2008; NRC, 2009; Allison et al., 2009; Garnaut, 2011; Mabey et al., 2011). We also note the less frequent manifestation of over-prediction of key characteristics of climate in such assessments. We then analyze these results in light of our hypothesis of the tendency to err on the side of least drama, and suggest some avenues for future research.

2. IPCC predictions vs. actual outcomes

2.1. Previous analysis: Rahmstorf and colleagues (2007)

In a 2007 article, Rahmstorf and colleagues compared projections of global mean temperature change, sea level rise, and atmospheric carbon dioxide concentration from IPCC's Third Assessment Report (TAR) with observations made since 1973 and concluded: "Overall, these observational data underscore the concerns about global climate change. Previous projections, as summarized by IPCC, have not exaggerated but may in some respects even have underestimated the change, in particular for sea level" (p. 709). In the TAR, released in 2001, the IPCC predicted an average sea level rise of less than 2 mm/yr, but from 1993 to 2006, sea level actually rose 3.3 mm/yr—more than 50% above the IPCC prediction (Houghton et al., 2001). Furthermore, the temperature change over the period "is 0.33 °C for the 16 years since 1990, which is in the upper part of the range projected by the IPCC (in the TAR)." The underestimate in sea level rise can be traced in part to under-projection of ice loss from Antarctica and Greenland, as discussed in detail later in this paper.

2.2. Previous analysis: Pielke (2008)

In a 2008 paper, Roger Pielke, Jr., expanded this analysis to include the predictions offered by scientists in earlier IPCC assessments (Pielke, 2008). Pielke observed that for sea level rise, actual changes have been greater than forecast in two of three prior IPCC reports, while falling below the median prediction in the First Assessment Report (FAR). Predicted temperature changes, also higher in the FAR than subsequently observed, were in line with observations for the three subsequent assessments, taken as a whole.

Pielke noted that "A comprehensive and longer-term perspective on IPCC predictions, such as this, suggests that more recent predictions are not obviously superior [to older ones] in capturing climate evolution" (2008, p. 206). This is of course true: More observations, model runs, and even greater understanding of individual aspects of a complex system do not necessarily lead to convergence on truth (Oppenheimer et al., 2008). But the relevant question is how the projections have stood up to empirical evidence of what actually has occurred in the natural world during the time period under discussion. Pielke concluded that "Once published, projections should not be forgotten but should be rigorously compared with evolving observations" (2008, p. 206). We agree. When one does this, as both the Rahmstorf and Pielke analyses do, one finds an overall tendency in the most recent three assessments toward either no bias or toward underestimation.

2.3. Previous analysis: NRC (2009)

These conclusions are also supported in a report prepared by the Committee on Strategic Advice on the U.S. Climate Change Science Program, for the titular purpose of *Restructuring Federal Climate Research to Meet the Challenges of Climate Change* (NRC, 2009). The results of the three-year study, commissioned by the U.S. Climate Change Science Program (CCSP) and published in 2009, were consistent with the conclusion that IPCC projections have systematically underestimated key climate change drivers and impacts. This committee found that "The Intergovernmental Panel on Climate Change (IPCC) projections may have been too conservative" in several areas, including CO_2 emissions by various countries, increases in surface temperatures, and sea level rise (p. 12). The key climate metrics of global mean temperature and sea level rise are biased toward underestimation, so far as the evidence in this analysis shows.

2.4. Previous analysis: Copenhagen Diagnosis (2009)

The NRC findings are also consistent with the analysis of an international group of scientists who summarized advances in climate science since the 2007 IPCC Fourth Assessment Report. This analysis, *The Copenhagen Diagnosis* (Allison et al., 2009), reviewed "hundreds of papers ... on a suite of topics related to human-induced climate change" since the drafting of AR4, and, like the NRC report, found that key changes were happening either at the same rate as, or more quickly than, anticipated (p. 5). Among their key findings were that global temperature increases over the past 25 years have been consistent with model predictions (0.19 °C per decade, virtually the same rate as for the 16 years mentioned in Rahmstorf et al., 2007), while other important impacts are proceeding faster than expected, including CO_2 emissions, increased rainfall in already rainy areas, continental ice-sheet melting, arctic sea-ice decline, and sea level rise. The data examined here overlap substantially with those analyzed by the Rahmstorf team, and it is noteworthy that an independent analysis by a different group of scientists comes to much the same judgment.

Among the key findings:

- Rainfall has become more intense in already rainy areas, and "recent changes have occurred faster than predicted" (Allison et al., 2009, p. 15; see also Wentz et al., 2007; Allan and Soden, 2008; Liu et al., 2009).
- Sea level rise has far exceeded predictions: "satellites show recent global average sea level rise (3.4 mm/yr over the past 15 years)—to be ~80% above past IPCC predictions" (Allison et al., 2009, p. 7).
- Surface ocean heat uptake between 1963 and 2003 was 50% higher than expected based on previous calculations. This difference helps explain why sea level rise (from thermal expansion) is also greater than expected (Allison et al., 2009, p. 35; see also Domingues et al., 2008; Bindoff et al., 2007). Studies also show that deep ocean warming is more widespread than previously thought (Allison et al., 2009, p. 35; see also Johnson et al., 2008a,b).
- Summertime melting of Arctic sea-ice has "accelerated far beyond the expectations of climate models" (Allison et al., 2009, p. 7; see also Stroeve et al., 2007). Indeed, using unusually vivid

language, the authors note that the record for previous Arctic sea ice summer minimum extent was "shattered" in 2007, "something not predicted by climate models ... This dramatic retreat has been much faster than simulated by any of the climate models assessed in the IPCC AR4"—with summer sea ice now well below the IPCC worst case scenario (Allison et al., 2009, pp. 29–30). Summer minimum sea ice was higher in subsequent years, but still fell near or below the long-term observed downward trend (which, as just noted, declines faster than the model predictions). Then, in 2012, another record minimum was set (Stroeve et al., 2007).

- CO_2 emissions were also tracking the high-end scenarios developed in 1999 and applied in AR4, showing that scientists' "worst-case scenario" has in fact been realized (Allison et al., 2009, p. 9; see also Nakicenovic et al., 2000), for the decade before the global financial disruption. Some people have pointed out that the emissions projections were not meant to be reliable in the short term, but it is interesting to note that, so far as these data may be relevant, they fit the pattern of underestimation.

2.5. Predictions of hurricane intensity and frequency

Other predictions have not necessarily been underestimates, but have not been alarmist, either. The debate over hurricanes provides an example, which we discuss below, drawing on *The Copenhagen Diagnosis* as well as subsequent literature. A key question is whether there has been a trend in hurricane intensity or frequency, particularly for the most intense (categories 4 and 5) storms, and if so, whether it could be attributed to human-induced warming. In AR4, IPCC concluded that it was "likely" that "intense tropical cyclone activity" has increased since 1970 in some regions, and that a human contribution to the observed trend was "more likely than not" (one of IPCC's weakest categories of affirmation of a claim; in a more recent special report, IPCC used another weak category to describe this attribution, "low confidence"; IPCC, 2012). Furthermore, AR4 concluded that a continuation of this trend into the future was "likely".

In the aftermath of the record-breaking Atlantic hurricane season of 2005, there was much discussion of whether one could reasonably say that hurricane intensity would increase, or was already increasing, in tandem with global warming. These findings were based in part on two studies (Emanuel, 2005; Webster et al., 2005) published the same year that Hurricane Katrina and its secondary effects destroyed much of New Orleans and following a period that saw increasingly powerful hurricanes particularly in the Atlantic basin. These events triggered considerable controversy (Michaels et al., 2005; Anthes et al., 2006; Emanuel, 2006; Trenberth and Shea, 2006; Landsea, 2007; Mann and Emanuel, 2006; Mann et al., 2007; Kossin et al., 2007; Elsner et al., 2008), with some arguing that no trend could be reliably identified and others that any attribution of recent hurricane changes to human-induced warming was at best premature (Pielke et al., 2005), and at worst simply false (Watts, 2011).

Different views remain today. A recent evaluation by a group of ten experts, spanning a wide range of scientific opinion on tropical storms, arguing that observations "support an increase globally in the intensities of the strongest tropical cyclones" but that "it remains uncertain whether past changes in tropical cyclone activity have exceeded the variability expected from natural causes" (Knutson et al., 2010, pp. 159–160). Furthermore, this relatively cautious assessment agrees with IPCC that "future projections based on theory and high-resolution dynamical models consistently indicate that greenhouse warming will cause the globally averaged intensity of tropical cyclones to shift toward stronger storms" and "We judge that a substantial increase in the frequency of the most intense storms is more likely than not globally, although this may not occur in all tropical regions" (Knutson et al., 2010, pp. 157–161). The latter claim is somewhat weaker than IPCC's.

Thus we find that with regard to one of the most potentially alarming conclusions of climate science—that deadly hurricanes eventually would get stronger—IPCC's claims do not diverge widely from at least one other attempt at assessment by the relevant expert community. Taken together, the comparison of IPCC's judgments and those of Knutson and colleagues shows that the range of plausible judgments from the expert community on this topic is rather narrow, providing little support for the argument that IPCC exaggerates risk.

2.6. Predictions of permafrost melting

One more topic will help to underscore the point. It is well accepted that certain feedbacks in the climate system, such as increased cloud cover or the Arctic ice-albedo feedback, could work to accelerate or decelerate global warming. One potentially large, positive feedback involves permafrost melting, which could release increasing amounts of greenhouse gases. The total carbon contained in permafrost has been estimated at 1672 gigatons, more than twice the amount of carbon in the atmosphere (Tarnocai et al., 2009). This means that the potential amplifying effect of greenhouse gas release from permafrost melting is enormous. Yet this feedback "has not been accounted for in any of the IPCC projections" (Allison et al., 2009, p. 21). This omission introduces a potentially profound bias in the climate projections—not toward overestimation of climate change, but toward its underestimation.

2.7. Previous analysis: Risbey (2008)

In 2008, climatologist James Risbey conducted an analysis of qualitative terms used in recent climate change literature, examining the use of potentially alarmist words such as "catastrophic", "urgent", "irreversible", and "rapid" (Risbey, 2008). When compared to the scientific claims and observations those terms were used to characterize, he found that their use appeared to be both reasonable and consistent with the current science. Thus, in accordance with the other sources discussed here, Risbey found that current climate change discourse is not unjustifiably alarmist, but rather, where alarmed, it was for good cause.

2.8. Summary: systematic conservativism in climate predictions

The studies we have examined here find no evidence that the IPCC has made exaggerated claims in its climate change predictions; indeed, in many cases IPCC predictions seem to have underestimated actual outcomes. *The Copenhagen Diagnosis* finds several areas in which scientists were largely correct, or conservative, in their forecasts, and they find no areas in which climate scientists were overzealous. The hurricane controversy, which has continued to evolve since the Copenhagen report was published, reveals that IPCC's views were somewhat more forceful than but not greatly different from a recent broad-based attempt to reassess the matter. Of course, the studies reported on here represent a small total of the entire universe of scientific projections related to climate change, but they involve climate characteristics which are particularly salient in the public discussion. No doubt the issues addressed in these studies are complex, and there are several possible reasons that could explain the outcomes in any one particular case (e.g., the inherent nonlinearity of the climate system may cause current predictions to lag systematically behind the behavior of the continuously forced system). Still, it is striking that, from the available cases where scientists have done post hoc analyses of prior IPCC

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projections, a pattern emerges: one of under- rather than over-prediction. Obviously, there are several conceivable explanations for this pattern and probably several different contributing factors in each case; here we propose one that we believe is plausible and may apply more or less broadly, consistent with the results we have discussed, and consistent with what we know about the culture of science, more generally. But first we consider why skeptics of anthropogenic climate change have alleged bias in the other direction.

3. A systematic bias?

Our analysis of the available studies suggests that if a bias is operative in the work of climate scientists, it is in the direction of under-predicting, rather than over-predicting, the rate and extent of anthropogenic climate change. So what are skeptical claims to the contrary based on? The late Stephen Schneider is often quoted as having said that climate scientists "have to offer up scary scenarios, make simplified dramatic statements, and make little mention of any doubts we might have" (e.g., Greenberg, 2010). In other words, Schneider is alleged to have openly advocated exaggerating the real dangers of climate change in order to effect more, and more rapid, policy change.

In actuality, the full quotation, taken in context, contained a rather different message:

> On the one hand, as scientists we are ethically bound to the scientific method, in effect promising to tell the truth, the whole truth, and nothing but—which means that we must include all the doubts, the caveats, the ifs, ands, and buts. On the other hand, we are not just scientists but human beings as well. And like most people we'd like to see the world a better place, which in this context translates into our working to reduce the risk of potentially disastrous climatic change. To do that we need to get some broadbased support, to capture the public's imagination. That, of course, entails getting loads of media coverage. So we have to offer up scary scenarios, make simplified, dramatic statements, and make little mention of any doubts we might have. This 'double ethical bind' we frequently find ourselves in cannot be solved by any formula. Each of us has to decide what the right balance is between being effective and being honest. I hope that means being both (Schneider, 1996, p. 5).

Schneider "decried sound-bite science journalism by pointing out that nobody gets enough time in the media either to cover all the caveats in depth, (i.e., 'being honest') or to present all the plausible threats (i.e., 'being effective')" (Schneider, 1996, p. 5). He recognized that sound-bite journalism *could* pressure scientists into over-simplification and exaggeration, and he urged scientists to be aware of this pitfall. But whatever Schneider meant in that one line out of his prodigious output of peer-reviewed and popular writing, it is clear that in their peer-reviewed works, climate scientists as a community have not exaggerated the threat of global warming. If anything, they have downplayed it.

The frequent attacks on Stephen Schneider—as well as attacks on other climate scientists such as Benjamin Santer and Michael Mann—suggests that one possible reason why scientists may have underestimated the threat of anthropogenic warming is the fear that if they don't, they will be accused by contrarians (as was Schneider) of being alarmist fear-mongers. That is to say, pressure from skeptics and contrarians and the risk of being accused of alarmism may have caused scientists to understate their results. This may well be a factor affecting some scientists, particularly those who feel uncomfortable in the public sphere, and particularly in the United States where climate scientists have been subjected to extraordinary pressures (Oreskes and Conway, 2010; Mann, 2012). Certainly, a modest claim is less likely to call attention to itself than an immodest one. But it is important to note that the pattern we are documenting is not restricted to the United States: climate science is highly international, and the IPCC makes a point of seeking broad involvement and full participation from scientists across the globe. The particular politics of the United States cannot be used to explain what appears to be a broad pattern. Similarly, recent events in the United Kingdom—such as the theft of emails from scientists at the East Anglia Climate Research Unit—cannot explain a pattern that clearly predates these events (this point is expanded in Section 5).

Schneider's comment also highlights an important but often overlooked fact: that the norms of scientific communication are different from the norms of popular communication (see also Olsen, 2009). In the latter, drama is entirely acceptable; indeed, it may be necessary in order to get on the evening news or to maintain the attention of your undergraduates. Dramatic tension and dramatic license are familiar concepts from literature, theater, and daily life; in daily life, no one faults a storyteller for making her story dramatic. On the contrary: she wouldn't be a storyteller were she unable to do so. Science, as Schneider noted, is different. Scientists are expected to eschew drama. They are expected to lay out the facts in quantitative terms, stripped of emotional valence. They are expected in their presentations of their work to be modest, even self-effacing, to stick closely to the facts in any interpretation, and not to go "beyond" them—whatever that might precisely mean.

Climate modeler James Hansen has argued just this point. He has suggested that "scientific reticence" is preventing scientists from effectively communicating the true danger of the potential disintegration of the Greenland and West Antarctic ice sheets (GIS and WAIS) (Hansen, 2007). Hansen argues that scientific reticence involves "a tendency for 'gradualism' as new evidence comes to light," and a "pressure on scientists to be conservative," to submit scientific papers that "do not push too far and are larded with caveats" (Hansen, 2007, p. 2). Scientific reticence also influences assessments like the IPCC reports, he argues, which "produce a consensus" among thousands of scientists from most of the world's nations, who are collectively "extremely careful about making attributions" (Hansen, 2007, p. 5).

Hansen attributes this tendency to the desire or choice of many scientists to "stay within a comfort zone, not needing to worry that they say something that proves to be slightly wrong" (Hansen, 2007, p. 5). However, we suggest that there is more at work here than simply a desire to avoid being wrong (or being shown to be wrong), because such a desire does not explain the strong *directionalism* of the bias. After all, one could argue that being wrong by under-predicting is just as problematic as being wrong by over-predicting; indeed, where public health and safety are at stake, one could argue that it is *more* problematic. Rather, it seems that more than wishing to avoid being wrong, scientists wish to avoid a particular manner of being wrong: the wrong of over-prediction. Why, then, in the cases reviewed in the literature, do we find climate scientists consistently under-predicting or hitting the mark, but rarely over-predicting?

Ross Garnaut discusses a similar concept in an update to his 2011 review of Australia's role in the global response to climate change (Garnaut, 2011). In a concluding section entitled "Reflections on scholarly reticence," Garnaut describes a social dynamic that causes scientists to not want to stray too far from the community mean—in essence a kind of anchoring effect. However, this type of systematic bias, which should make estimates fall close to the norm but to either side depending on expert judgment and other factors, would not explain the systematic *underestimation* we have identified. ESLD does.

In his recent autobiographical book, Hansen takes up the concept of scientific reticence in more detail, and suggests that an additional factor beyond the fear of being wrong is 'behavioral discounting'—namely, that some ways of being wrong are considered more problematic than others. In particular, scientists' fear of 'crying wolf' is more immediate than their fear of 'fiddling while Rome burns' (Hansen, 2009, p. 87). We believe that the evidence supports this idea. Below, we offer some additional examples, and then return to our hypothesis to help explain why.

4. Two examples of ESLD: ozone depletion and WAIS disintegration

Between 2008 and 2011, we conducted a series of broad-based, open-ended ethnographic interviews, in person or by telephone or email, with numerous ozone scientists and assessment authors (Brysse) and scientists studying and assessing the West Antarctic Ice Sheet (WAIS) (O'Reilly). We draw on these interviews, as well as the published scientific literature and the popular press, to examine ESLD at work in scientific predictions of Arctic ozone depletion, and of the potential for rapid disintegration of WAIS (see the Supplementary Online Material for more details, and for some sample interview questions.)

4.1. 'Crying wolf' about ozone depletion

The history of ozone depletion research offers an example of how scientists *have* been attacked when they over-predicted a potentially alarming outcome. In February of 1992, NASA scientists studying the Arctic stratosphere issued a press release warning that a major Arctic ozone hole, like the one over Antarctica, could develop that spring (see e.g., Perlman, 1992). NASA's second Airborne Arctic Stratospheric Experiment (AASE-II) had found greatly elevated levels of chlorine monoxide in the Arctic stratosphere in January 1992, indicating the potential for severe ozone loss with the return of sunlight in the spring. It was on the basis of this information that scientists warned of potentially severe Arctic ozone depletion in the coming months. However, while the science behind this prediction was not incorrect, the late winter months turned out to be warmer than expected, preventing the formation of the polar stratospheric cloud particles that provide the surfaces on which some of the key chemical reactions necessary for polar ozone depletion would typically occur. As a result, Arctic ozone depletion in the spring of 1992 was less than scientists had feared, and no Arctic ozone 'hole' appeared that year (Lambright, 2005; Conway, 2008; Oreskes and Conway, 2010). More severe Arctic ozone depletion occurred in other years, however. In the winter of 2010/2011, Arctic ozone levels reached their lowest recorded levels, following an unusually prolonged period of extremely low stratospheric temperatures, and creating an ozone hole "comparable to that seen in some years in the Antarctic" (NASA, 2011).

In the aftermath of the unrealized 1992 Arctic ozone hole prediction, NASA scientists were severely criticized in the conservative press for crying wolf, causing unnecessary panic, and acting according to emotional imperatives or an environmental agenda instead of according to the dictates of scientific objectivity. An editorial in the *Washington Times*, for example, denigrated the original 3 February 1992 NASA warning of a potential Arctic ozone hole, saying "This is not the disinterested, objective, just-the-facts tone one ordinarily expects from scientists. Nor is it the stuff of peer-reviewed science, the consensus-setting standard that helps establish what is or is not 'science.' This is the cry of the apocalyptic, laying the groundwork for a decidedly non-scientific end: public policy." The article concluded that "some people in the agency [NASA] apparently were eager to create the panic of February 3, for reasons having nothing to do with science and everything to do with their ideological environmentalism. As it is, it would be nice if the next time NASA cries 'wolf,' fewer journalists, politicians and citizens heed the warning like sheep" (Anonymous, 1992, p. G2).

Bob Davis, writing for the *Wall Street Journal*, expressed a similar opinion, calling the Arctic incident "another blow to the credibility of the space agency," and gloomily predicting that "[b]y sounding the alarm, NASA may have strengthened the hand of those in the White House who have urged the government to do little about the ozone until scientists can sort out the dangers involved in global climate change." Davis concluded: "Environmentalists warn that by that time, the problems could be so severe that combating them will be extraordinarily costly" (Davis, 1992, p. B2). Even other scientists joined in the backlash: NOAA's Melvyn Shapiro, for example, "impl[ied] that the arctic [sic] ozone affair was a case of 'Chicken Little research'" (Lambright, 2005, p. 33). Clearly, political pressure from the right had an impact on scientists' own views of what would constitute appropriate behavior, and we know of no comparable example of pressure from environmentalists or the political left criticizing scientists for not being dramatic enough.

NASA's response to the ozone incident, according to an unnamed NASA spokesman, was a renewed level of scientific caution: "We aren't going to put out [another] press release until we have a complete story to tell" (quoted in Lambright, 2005, p. 34). While NASA's wish to avoid inaccurate predictions and embarrassment is understandable, waiting for a 'complete' story before warning of potential environmental danger risks a different sort of harm: the harm of failing to alert the public to a potential significant threat.

Many scientists who worked on stratospheric ozone depletion have since become involved with the IPCC, and they recognize that, as in ozone depletion assessments, scientists assessing climate change are vulnerable to accusations of over-dramatization. Jonathan Shanklin was one of the authors of the 1985 paper that first announced the discovery of the Antarctic ozone hole. In an interview in 2009, he suggested that scientific assessments operate according to the "crying wolf principle: if you cry wolf too often, then nobody believes you anymore, and the sky does fall in" (Shanklin, 2009). Shanklin also suggested that in the case of recent IPCC predictions of future climate change, scientists' "best guess for many of these [scenarios] were actually worse than those in the report." In other words, Shanklin argues that scientists put less dramatic conclusions into their published assessments than they actually thought likely, in order to preserve their reputations and credibility—or those of their process.

Shanklin's mixed metaphor is revealing, because the two stories, while superficially similar, are different when interpreted in light of the challenges facing climate scientists. The Boy Who Cried Wolf lost credibility, and so when the wolf finally came, the boy's cries were ignored, and he was eaten. Chicken Little, in contrast, was not ignored when she panicked over a falling acorn, but was joined by many others, who were then eaten by the fox who took advantage of their panic. Both stories provide cautionary tales about the importance of staying calm and reading situations correctly; but in the first case, because of lost credibility, a real hazard is discounted, whereas in the second, an imagined hazard leads to panic. What does this mean for scientists? Shanklin focuses on the need to preserve credibility, suggesting that this is a guiding norm in scientific assessments. But in mixing his metaphor he illuminates the problem that scientists face: of what value is credibility if it isn't used to alert people to hazards that actually exist? What use is the preservation of scientific credibility if it comes at the cost of a persuasive alert? What is credibility being preserved *for*?

Neil Harris, an ozone chemist and contributing author to the IPCC second and third assessment reports, agrees with Shanklin that scientists have downplayed worst-case scenarios. In his opinion "the more extreme possibilities have been underemphasized in each [IPCC] report" (Harris, 2009). He suggested that this phenomenon could be explained at least partially by sociological reasons, citing the reaction of the fossil fuel industry to the 1995 IPCC report which was the first assessment to definitively attribute global warming to anthropogenic activity. The lead author of the relevant IPCC chapter, climate scientist Ben Santer, was "absolutely vilified" by industry (documented in Oreskes and Conway, 2010). This example, he suggests, has made other scientists involved with later assessments much more cautious with their own statements.

What is perhaps most important about this story is that this forcing function would tend to operate in the same direction as the internal values of scientists themselves. After all, many scientists are courageous, and history provides many examples of scientists willing to stand up for their findings in the face of external pressure. But if the external pressures tend to track in the same direction as scientists' own instincts, that is a different matter. We argue here that these pressures often do align. Scientists' desire to avoid external attack, and not be accused of crying wolf, is reinforced by their internal value system in which objectivity is often interpreted to mean downplaying potentially dramatic results. Indeed, sometimes the desire to be objective leads scientists to refuse to provide estimates at all, which might be considered the ultimate version of under-prediction. Consider the treatment of the dynamical contributions to ice sheet loss in AR4.

4.2. Evaluating the risk of rapid disintegration of the West Antarctic ice sheet

One of the most important—and controversial—questions facing climate scientists and policy-makers is the prospect of a rapid disintegration of the West Antarctic ice sheet (WAIS), which could cause global sea level rise of 3–6 m over a period of a few centuries to a few millennia—a potentially catastrophic outcome from a social, political, economic and ecological standpoint. The IPCC has traditionally relied upon the available continental-scale ice sheet models to make predictions of ice sheet contributions to sea level rise for the short term (the 21st century) as well as the long term (beyond the 21st century). There have always been concerns about the reliability of these models for timescales of less than tens of thousands of years, which are usually addressed in caveats to the predictions.

In the decade before the publication of AR4, new observations indicated that model predictions were unreliable, at least for multi-decadal periods: sections of WAIS were observed to be changing much more quickly than model simulations indicted. Some experts argued that these observations reflected temporary changes—just decade-long noise in comparison to the longer timescale behavior of true concern to policy makers—but the observations were sufficiently compelling to convince many WAIS scientists to re-evaluate their views about the future of the ice sheet.

The IPCC authors then had at least four choices for representing the WAIS contribution (as well as that of the rest of the Antarctic ice sheet) to sea level projections in AR4. First, they could use the available ice sheet models, despite their limitations, but frame them with caveats explaining the recently discovered limitations. This route was not chosen, as the authors decided to omit changes in dynamical ice flow (the unreliable aspect of the models) from the numerical estimates. Second, the authors could try to estimate the ice sheet contribution using a variety of simplified approaches and present all of the results, with discussion of the credibility of the various estimates. This option was considered, but the authors did not select it, partly because the literature on such estimates was just emerging and not yet peer reviewed. Third, they could represent their estimates as a probable *lower bound* for sea level rise projections (Rahmstorf, 2010), since these did not include potential changes in dynamical ice sheet processes, an alternative they also considered (IPCC, 2007). Fourth, (the option which was selected): for the 21st century, the authors could present sea level rise estimates based on contributions from melting of mountain glaciers and the ice sheet surfaces, and thermal expansion of sea water, all of which could be calculated reasonably well. The authors augmented these estimates with a small numerical adjustment that assumed ice sheet dynamical behavior would remain essentially constant over this century. That is, the scientists made no attempt to forecast the highly uncertain future changes in the ice sheet contribution that might occur. Thus the AR4 authors chose to leave out potential near-term (by 2100), rapid, dynamical changes in the polar ice sheets' contribution (for both Antarctica and Greenland). This resulted in the published (and caveated) sea level rise range of 0.18–0.59 m by 2100. In addition, citing the deficiencies of the ice sheet models and lack of other methods on which to base predictions, the authors explained that there was "no consensus" on the long-term future of Antarctica as a whole, including WAIS.

This decision has been criticized by some in the scientific community (Oppenheimer et al., 2007; Overpeck, 2009; Vaughan, 2009; Rahmstorf, 2010) as setting a poor precedent, inadequately representing the available scientific material, refusing to give sufficient weight to non-model-based research as a means for providing a numerical estimate, and giving the impression that expected sea level rise is more modest than is likely to be the case. At about the same time, research on methods for roughly estimating this contribution even absent full-scale models (Rahmstorf, 2007; Pfeffer et al., 2008) was underway. While these publications did not meet the deadline for inclusion in AR4, some of the resulting methods and findings were made available to the IPCC writing team. So why did the authors decide to omit an estimate for the dynamic ice loss contribution?

In interviews, several authors have stated that their decision was the best judgment possible at that time, given the information available (Alley, 2009; Gregory, 2009; Solomon, 2010). This may well be the case; dealing with emerging science is clearly a difficult issue for the IPCC (InterAcademy Council, 2010). However, in retrospect, and particularly given the new research published since 2007, the view that it was an overly cautious approach is equally plausible (although only time will tell whether it will result in under-prediction). Two factors interacted to determine IPCC's approach in this case: (1) the way IPCC generally manages uncertainty, and (2) the social composition and interactions of the assessment group in this particular case, which influence the outcome of its deliberations (O'Reilly et al., 2011, 2012). With regard to uncertainty management, structural, model-based uncertainties that dominate the WAIS case remain inadequately represented (or altogether omitted), despite continuing refinement by IPCC of its method for judging uncertainty. The general issue of uncertainty has been the subject of intense study in recent years (see e.g., O'Reilly et al., 2011 and references therein). Here we focus only on the question of whether IPCC's and other recent assessments have a tendency to over- or underestimate uncertain outcomes, where retrospective comparison is feasible. Subsequent research could examine the question of whether such a bias arises under particular conditions of uncertainty, i.e., the evaluation of fat tails of outcome distributions.

With regard to social interactions, while based on recent research, the overall group judgment was inevitably a socially determined decision (see O'Reilly et al., 2011 for a detailed discussion of the social processes involved). The net result was a rather modest prediction of sea level rise on the order of tens of centimeters, lower than estimates produced by several individual scientists soon after AR4, which ran as high as two meters. Moreover, it was an estimate inevitably influenced by the status quo: sea level would continue to rise via processes already underway—thermal expansion and mountain glacier melting—albeit with increasing rates, plus an additional term representing persistence of the poorly understood but already observed pattern of dynamic ice sheet loss. In short, it was a choice, for lack of good evidence, to go with an option that was inherently less alarming than the available alternatives. This choice derived from the particular cohort of assessors working on this problem, the IPCC's institutional exigencies, i.e., bureaucratic and organizational guidelines, as well as the state of WAIS science at the time of writing of AR4 (O'Reilly et al., 2012). Clearly, a projection of sea level rise on the order of a meter or more—potentially affecting huge tracts of land, hundreds of millions of people, and billions (or trillions) of dollars of low-lying infrastructure—would have been much more dramatic than the one that was actually offered. This example suggests not only that ESLD was in play, but also that it may operate more strongly among groups of scientists—such as those coming together to formulate assessments—than it does in the work of individual scientists (for a discussion of group dynamics in peer review panels, see Lamont, 2010). More research on this aspect of ESLD could prove highly informative.

The assessments whose outcomes we report here are inevitably structured to reach consensus. Other authors have noted the tendency of consensus processes to obscure or avoid reporting likelihoods of extreme outcomes and outlying views (van der Sluijs et al., 2010; Patt, 1999; Polson and Curtis, 2010). Our additional observation is that this process is not symmetric, but is biased to avoid emphasizing dramatic outcomes.

5. What leads to ESLD?

Given the challenging political environment in which climate scientists operate, and the fact that climate scientists have been repeatedly accused of fear-mongering and alarmism, we might conclude that scientific reticence with respect to global warming is a consequence of the charged political context in which climate scientists operate. Freudenberg and Muselli (2010) have suggested that the asymmetry of political pressure, particularly in the United States, has contributed to a conservative bias in IPCC assessments. These authors emphasize that most analyses of scientific communication focus on the flow (and impact) of information from scientists to the larger public, paying far less attention to the reverse flow—in this case, the strongly stated criticism of scientists by contrarians and skeptics, widely repeated in the North American press, and then spread more widely on the internet. They suggest that this reverse flow has contributed to a bias in which scientists not only bend over backward to ensure that their results are absolutely warranted by the evidence, but actually take positions that are more conservative than warranted by the evidence to disprove contrarian accusations of scientific ''alarmism.''

As already noted, this may be a contributing factor, particularly for scientists in the United States who have been subject to extraordinary extra-scientific pressures for nearly 20 years, and perhaps more recently for scientists in Canada, the United Kingdom, and Australia, where attacks have recently spread. However, as already noted, the IPCC is highly international, and the pattern of underestimation appears to be a long-established one. Moreover, and perhaps more important, scientists have often proven courageous in resisting external pressures—both historically and in the particular case of climate change. Scientists generically may be somewhat contrarian, viewing science as a creative and individualistic activity designed to resist conventional wisdom. For at least some scientists, external pressure might backfire, leading the scientist consciously or subconsciously to oppose that pressure.

The key point here is that to the extent that the external politics of climate change may be acting as a forcing function, that function tends to act *in the same direction as the internal values of the scientific community*. We base this claim on the observation of similar patterns in debates that had, in and of themselves, no obvious political, social, or economic ramifications, and where there was no relevant industry to pressure scientists.

Consider the debate in the 1980s and 90s over the idea that dinosaurs (and other organisms) did not go extinct gradually at the end of the Cretaceous Period, as they failed to adapt to changed environmental circumstances, but were wiped out more or less instantaneously when a giant meteorite struck the Earth. This claim was controversial for many reasons, but not least of all because it seemed unduly dramatic. Geologists and other earth scientists used to invoking gradualist arguments about evolution and earth processes found it hard to accept such a dramatic—indeed, to some minds, frankly astonishing—idea.

In a 1994 interview, Stephen Jay Gould, one of the few paleontologists to immediately embrace the impact hypothesis, lamented ''the false notion that gradualism is preferable a priori as part of the definition of science,'' which he felt was preventing many of his colleagues from giving the catastrophic impact scenario due consideration. Gould concluded that ''there was no a priori reason whatsoever to accept gradual change. [Geologist Charles] Lyell had pulled the wool over the whole profession's eyes in [applying] uniformity'' (Gould, 1994). That some earth scientists were indeed rejecting the impact hypothesis because of its dramatic implications and catastrophic undertones is made clear in the following passage written by Anthony Hallam, an outspoken opponent of the impact hypothesis:

> Environmental changes on this planet as recorded by the facies should be thoroughly explored before invoking the *deus ex machina* of strange happenings in outer space, for which independent evidence is much harder to find... it is intuitively more satisfying to seek causes from amongst those phenomena which are comparatively familiar to our experience (Hallam, 1981, p. 36).

Dale Russell, a paleontologist involved in the K–T mass extinction debate, argued that the sheer drama and unfamiliarity of the impact scenario was a major factor in resistance to it:

> [W]hen a paleontologist is approached with a catastrophic solution to a biostratigraphic problem, he [sic] is more apt to react with polite reserve than with enthusiasm. He would probably recommend that his friend formulate a series of working hypotheses and select, as the most favored, one that is both congruent with existing data and satisfies the principle of minimum astonishment (Russell, 1982, p. 402).

Geologists have long invoked the ''principle of least astonishment'', or what philosophers of science call the ''no miracles'' argument. They typically interpret this as a version of Ockham's razor: Given a choice between two or more hypotheses to explain observed phenomena, choose the simplest one. Russell suggested that in the K–T boundary debate, what most of his colleagues identified as the simplest or most parsimonious cause of extinction was the one that conformed best to preexisting theory (i.e., gradual, endogenous extinction mechanisms) and provided the smallest shock to the current belief system or overarching paradigm (in this case, uniformitarianism). In this case, Russell has identified a directional bias among paleontologists in favor of the ''least astonishing'' or, in our lexicon, the least dramatic, solution. Thus we label this larger phenomenon "erring on the side of least drama".

In the earth sciences, the tendency toward least drama has a pedigree related to the history of uniformitarianism, and the perception that the alternative—catastrophism—was tainted by clerical and miraculous associations. While geologists in the early 19th century debated the empirical evidence for and against rapid and cataclysmic change—with many observers arguing that Earth history did include some natural drama, and nature did sometimes make leaps—in the mid-19th century, the success of Darwin's theory of the origin of species by natural selection, with its roots in Lyell's principle of uniformitarianism, was widely seen as vindicating the gradualist approach. From that time on, dramatic events—meteorite impacts, comets, and especially floods—were for the most part dismissed as unlikely, their invocation eschewed as unscientific (Oreskes, 1999; Rudwick, 1970, 2007, 2010; Gould, 1965; Doel, 1997).

6. Conservativism: what exactly is going on?

Erring on the side of least drama may be viewed as related to scientific conservatism: an inherent bias in favor of existing knowledge and presumptions, and the avoidance of conclusions that seem excessively dramatic. The history of science is certainly consistent with this broad picture. In science, generally, the burden of proof is on those who wish to change prevailing views and approaches, be they theoretical, explanatory, or methodological. Philosopher of science Thomas S. Kuhn described this phenomenon in the 1960s; he called it (in uncharacteristically prosaic terms) "resistance to change" (Kuhn, 1962, pp. 151–152). Established knowledge is the default position, until sufficient evidence is developed to dislodge it. Thus, anyone with a new claim—such as the idea that there could be an ozone hole or that human activities are changing the climate system—faces the burden of proof. Indeed, any newly discovered phenomenon—whether it be the relativity of time and space, the motion of continents, or the human impact on the global climate system—will face an uphill battle.

In science, the null hypothesis is that existing knowledge is correct; the burden of proof is on the man or woman who wishes to dislodge the status quo (Kuhn, 1962). Overall, this is probably a good thing, helping to protect science from fashions and fads. If the issue at stake has no particular public policy implications, then scientists are no doubt right to proceed cautiously, taking their time to make sure the evidence is beyond reproach before casting off hard-won prior knowledge. But if there is a policy consequence to the scientific results, and particularly if there is a negative consequence associated with inaction or delay, then scientific conservatism may have negative social consequences.

A version of erring on the side of least drama can be found in what statisticians call Type 1 and Type 2 errors. As most scientists know, a Type 1 error involves thinking an effect is real when it is not; a Type 2 error means missing effects that are actually there. Making a Type 1 error can be thought of as being naïve, credulous, or gullible; making a Type 2 error can be interpreted as being excessively skeptical or overly cautious. Interestingly, conventional statistics is set up to be deeply skeptical and to avoid Type 1 errors, by placing a very high statistical bar on claims for statistical significance. The use of a 95% or even 99% confidence limit in many scientific experiments reflects a scientific worldview in which skepticism is a virtue and credulity is not. In fact, some statisticians claim that Type 2 errors aren't really errors at all, just missed opportunities (Lane, 2007; see also Ziliak and McCloskey, 2008; Oreskes and Conway, 2010.)

It is telling that professional statisticians generally regard Type 1 errors as more important to avoid than Type 2; social scientists would argue that each case should be judged on its own merits: which is worse depends upon what kind of damage ensues from the Type 1 versus the Type 2 error in that particular case. That a professional statistician could publicly claim that that a Type 2 error isn't really an error at all is remarkable; it also fits with our hypothesis of ESLD: skepticism is good; credulity is bad. Therefore, scientists often set a very high bar. In these cases, they would willingly err on the side of disbelieving something that is, rather than believing something that is not (IPCC's approach to uncertainty is more nuanced; see descriptions and applications in any of the recent assessment reports).

The tendency to continue to believe what one has believed in the past is not unique to science. In their now-classic work on decision-making heuristics, Tversky and Kahneman (1974) showed that decision-making processes are often "anchored" in prior decisions and beliefs. If early estimates suggest that the climate sensitivity for doubling of CO_2 is three degrees, for example, then this figure will tend to remain accepted unless and until someone has strong evidence to dislodge it (for a detailed discussion of climate sensitivity as an anchoring device and boundary object, see van der Sluijs et al., 1998 and references within). The longer the value has been accepted as correct, the stronger that evidence will need to be.

Assuming such a bias operates, model runs which suggest very different results may tend to be discounted. For example, the outcome of a distributed computing experiment using large ensembles of runs of simplified climate models (Stainforth et al., 2005) yielded a probability of nearly 5% that steady-state climate sensitivity exceeds 8 °C. However, the physical implications of such high sensitivities were not pursued much in AR4. The most comprehensive effort to examine the social and ecological impacts resulting from high sensitivity focused on a warming of 4 °C (New et al., 2011). Yet if the climate system were highly sensitive, a much larger warming would be plausible and the implications would be dramatic—and worrisome (Piani et al., 2005).

One might suppose that this inherent conservatism is even-handed. If no bias were operative, then on the question of climate sensitivity, or sea level rise, we might expect scientists to be equally skeptical of results that are higher *or* lower than conventionally accepted values. However, the empirical evidence presented above suggests that this is not, in fact, the case.

7. Further reasons for ESLD

We have suggested one reason for ESLD that may apply in the realm of earth science and biology: the historic link between uniformitarianism, anti-clericalism, and the rise of modern geology and evolutionary biology. We believe, however, that an even broader pattern may be at play: that the basic, core values of scientific rationality contribute to an unintended bias against dramatic outcomes.

Half a century ago, sociologist Robert Merton attempted to define the norms of science, reducing them to four key ideas: universalism, communism (or communality), disinterestedness, and organized skepticism. While some critics have noted that commercial and national interests may challenge scientific universalism and communism, few have doubted that organized skepticism is a guiding force in science. And this guiding force—organized skepticism—is consistent with the argument we have made here: that scientists are skeptical of all new claims, and ceteris paribus, the more dramatic the claim, the more skeptical they are likely to be, and the greater the evidential bar (this point was made explicitly in the 1920s debates over continental drift: see Oreskes, 1999).

Moreover, to these four oft-cited norms—mostly dealing with the collective social behavior of scientists, acting as a community—we might add a host of others related to the epistemic stance of

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individual scientists: objectivity, dispassion, restraint, moderation, level-headedness, discipline, self-control (which to some degree overlap with those Merton underscored). Conspicuously lacking from this list would be emotionality, which is generally viewed as negative, clouding judgment (see for example Rossiter, 1982; Keller, 1985; Harding, 1986; Tuana, 1993). Scientists strive to be cool-headed, to avoid emotion and drama. Thus a dramatic outcome—very large ozone depletion, very large sea level rise, very rapid disintegration of WAIS, and certainly an instantaneous event that wipes out more than half the species on Earth—inevitably provokes an emotional response, which feels, to most scientists, inappropriate (Barbalet, 2002).

It is not merely that dramatic claims open scientists to criticisms from skeptics and other external opponents; dramatic claims lay scientists open to criticism *from their peers.* Because science operates according to a prestige economy in which reputation is paramount, anything that might incite the distrust of one's peers is to be avoided. Yet, ironically, if our argument is even in part correct, then the desire to preserve one's reputation for objectivity and dispassion may introduce a bias into one's work. What begins as an effort to preserve good judgment may in fact cloud it.

The risk of being accused of being overly dramatic, even hysterical, raises an additional (and worrisome) aspect of this issue: its gender dimension. Feminist scholars including Margaret Rossiter, Sandra Harding, and Donna Haraway have long discussed the strong association of science with supposedly male characteristics, such that 'proper' science is perceived to be "tough, rigorous, rational, impersonal, masculine, competitive, and unemotional" (Rossiter, 1982, p. xv; see also Harding, 1986; Haraway, 1989). Scientists who come across as 'too emotional' or 'too personal' may thus be taken to be 'unscientific' by their peers, and a woman who exhibits these characteristics may be that much more rapidly dismissed. If this is so, then we may find either that women scientists publicizing the dangers of climate change may be more harshly judged for doing so than their male colleagues, or that women scientists may be particularly reticent to do so—to return to Hansen's phrase—for fear of losing hard-won scientific credibility. This poses another question for future research.

8. ESLD vs. the precautionary principle

The directional bias toward erring on the side of least drama may act in opposition to what has become an important guideline in environmental policy in some institutions and governments: the precautionary principle. There are many different formulations and interpretations of the precautionary principle, both weak and strong, but generally speaking, the principle is interpreted in the context of climate change to mean that action in the face of potentially serious and/or irreversible climate risks should not have to wait for complete scientific certainty to be attained (an impossibility in any event). The United Nations Rio Declaration on Environment and Development, for example, defines the precautionary principle this way: "Where there are threats of serious or irreversible damage, lack of full scientific certainty shall not be used as a reason for postponing cost-effective measures to prevent environmental degradation" (UNEP, 1992, p. 3). While the costs of acting in the face of uncertainty must also be taken into account (Gollier, 2002), the precautionary principle suggests that the international climate change community should prepare for the worst while hoping for the best.

If climate scientists and assessors are erring on the side of least drama in their predictions, then they are not preparing policy-makers and the public for the worst, because they are under-predicting what the worst outcomes might be.

9. Conclusion

Evidence from recent analyses suggests that scientists, particularly acting in the context of large assessments, may have underestimated the magnitude and rate of expected impacts of anthropogenic climate change. We suggest that this underestimation reflects a systematic bias, which we label "erring on the side of least drama (ESLD)". ESLD is consistent with a broad pattern in earth science, in play since the mid-19th century, of eschewing catastrophic accounts of natural phenomena. While physicists and chemists do not share this particular history, they do share a broader pattern in science of skepticism toward dramatic explanations of natural phenomena. This stance arises, we suggest, from the core scientific values of objectivity, rationality, and dispassion, which lead scientists to be skeptical of any claim that might evoke an emotional response.

Our hypothesis of ESLD is not meant as a criticism of scientists. The culture of science has in most respects served humanity very well. Rather, ESLD provides a context for interpreting scientists' assessments of risk-laden situations, a challenge faced by the public and policy-makers. In attempting to avoid drama, the scientific community may be biasing its own work—a bias that needs to be appreciated because it could prevent the full recognition, articulation, and acknowledgment of dramatic natural phenomena that may, in fact, be occurring. After all, some phenomena in nature *are* dramatic. If the drama arises primarily from social, political, or economic impacts, then it is crucial that the associated risk be understood fully, and not discounted.

Author contributions statement

All authors contributed extensively to the work presented in this paper.

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Appendix A. Supplementary data

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