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Climate science, truth, and democracy

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ABSTRACT

This essay was written almost ten years ago when the urgency of America's failure as a nation to respond to the threats of climate change first came to preoccupy me. Although the essay was never published in full, I circulated it informally in an attempt to provoke a more public engagement among my colleagues in the history, philosophy, and sociology of science. In particular, it was written in almost direct response to Philip Kitcher's own book, *Science, Truth and Democracy* (2001), in an attempt to clarify what was special about Climate Science in its relation to truth and democracy. Kitcher's response was immensely encouraging, and it led to an extended dialogue that resulted, first, in a course we co-taught at Columbia University, and later, to the book *The Seasons Alter: How to Save Our Planet in Six Acts* (W. W. Norton) published this spring. The book was finished just after the Paris Climate Accord, and it reflects the relative optimism of that moment. Unfortunately events since have begun to evoke, once again, the darker mood of this essay. I am grateful to Greg Radick for suggesting its publication.

1. Introduction

An impasse of credibility currently prevails in the US around the issues of climate change that threatens to paralyze citizens and experts alike. Confidence in the expertise of scientists is at an all time low, with much of the internet, radio talk shows, and popular television deluged with challenges to the credibility and trustworthiness of climate scientists. In an effort to adhere to their traditional ethic of "balance," even our most prestige newspapers and journals have contributed to the confusion by spreading the widespread misimpression that climate scientists are deeply divided about both the extent of the dangers we face and the relevance of human activity to global warming. Not knowing who or what to believe, the natural response for most people is to do nothing.

Meanwhile, evidence of the seriousness of the problem continues to mount, as does the apprehension of so many climate scientists. Yet notwithstanding their concern, most of these have been reluctant to weigh in on (often acrimonious) public debates, instead seeking recourse in the particular authority granted them by "peer review." Their concern is two-fold: first, anxiety about overstepping the traditionally accepted boundary between science and politics, and second (and closely related), fear that going beyond the reach of peer review would undermine their scientific credibility. The consequence is that the debate that rages in the public domain goes largely unchecked for intellectual or scientific reliability, and even the most discerning of non-expert readers are left without any basis for assessing the costs of continuing inaction.

Expert predictions imply costs that few if any of us would be willing to accept, yet a pervasive atmosphere of skepticism drains these warnings of virtually all effective force. What makes the situation particularly dire is that, living as we do in a democratic state, the very possibility of putting the efforts of our scientists to beneficial use depends on the response of a public willing and able to take their warnings seriously. Furthermore, given the critical role of the US in what is unavoidably a global issue, what is an impasse for the US is also an impasse for the world. The questions I want to pose are therefore of two kinds: First, on what basis can lay readers decide who and what to believe? And how does the answer to this question affect their ability to responsibly participate in policy decisions that depend on expertise they do not share? Second, what are the nature and limits of the climate scientist's particular responsibility in this current political and social situation? More specifically, what role ought experts play in the world beyond their particular expertise when their findings have the dramatic social and material consequences for that larger world that follow from the findings of climate scientists?

One might also ask, what is the place of expertise in addressing these issues? And which experts? There exist many different kinds of literature that, directly or indirectly, bear on the questions I raise. They come from philosophers, from political scientists, from students of science and policy, and they are extensive. But they tend

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not to focus on the particular problems of climate change quite so directly as I would wish. I am not an expert in any of these fields, but I will draw on all of them in the effort to bring them to bear more directly on the issues at hand. I do so out of my belief that the magnitude of what is at stake places a moral imperative on all of us to find a way out of this impasse. Even though no expert, I feel compelled to do what I can.

In the end, I will argue that, with regard to the problem of global warming as with so many other issues, there is no escaping our dependence on experts; we have no choice but to call on those (in this case, our climate scientists) who have the necessary expertise. Yet just as in any other endeavor, our confidence in their expertise – our trust – needs to be earned; it requires more than the evidence of a union card. Furthermore, for the particular task of getting beyond our current impasse, I also suggest that climate scientists may be the only ones in a position to take the lead. Finally, given the tacit contract between scientists and the state which supports them on the other, I will also argue that climate scientists are not only in a position to take the lead, but also that they are obliged to do so.

2. Science and politics: some general considerations

2.1. Facts and values: the traditional view

Traditional views about the proper relation between science and politics are founded on the assumption of a strict separation of facts and values, with the former belonging to the realm of science, and the latter, of politics. The task of scientists is to determine (or make positive claims about) what *is*, while the task of politics to decide (or make normative claims about) what *ought* to be – in other words, what follows from what *is* the case that we *ought* to do.

This divide has many ramifications, and has provided the framework for a wide range of discussions about the role of science in a democratic society, about the nature of scientific responsibility, about the proper relation between scientists and the larger public. It under girds the view of science as the domain of truth and objectivity, and of scientific inquiry as untarnished by any moral, political, or religious agenda. Thus, e.g., it is often said (as Andrew Dessler and Andrew Parson write in their recent book on debates about climate change),

> "First, if a positive claim is sufficiently well posed – meaning that all the terms in it are defined clearly and precisely enough – it has right and wrong answers. Similarly, well posed positive claims are either true or false. Second, the answer to a positive question, or the truth or falsity of a positive claim, does not depend on who you are: it does not depend on what you like or value, your culture, your political ideology, or your religious beliefs." (p. 20)

The assumption of such separability of fact from value similarly buttresses the widespread view that the proper role of scientific experts in policy making must be limited to providing the unadorned facts, straight and simple, and this requires, as Roger A. Pielke Jr., former Director of the Center for Science and Technology Policy Research in Boulder, Col., puts it, that we 'put into place mechanisms that somehow ensure the purity of science, so that scientists might deliberate unaffected by external values, pursuing only the truth." (2006: 33). The same view also implies that the appropriate response to conflicts between science and society (should they arise) is more effective dissemination of information, better communication from scientists to the public, increased scientific literacy.

Such clear cut divisions may provide us with an enormous sense of security, but however much comfort they give, they are extremely difficult to defend: First, as most scientists well recognize, few of the questions they actually deal with lend themselves either to answers that are absolutely right or wrong, or to claims that are absolutely true or false (they may, e.g., be true under certain assumptions or circumstances, not under others). Second, even if such answers might in principle be available, in practice their truth can almost never be fully established. Finally, much of the content of scientific claims depends on what, and how, questions are posed, and it is here that tacit assumptions and values are most likely to enter. Indeed, most students of the subject today no longer consider a strict separation between facts and values, or for that matter, between science and politics, to be possible. The record (both historical and contemporary) has clearly shown not only that facts play an important role in the formation of values, but also that values play an important role in the formation of facts. In the real word, it is virtually impossible to avoid the entanglement of fact and value. While the distinction may be of use in some contexts as a rough guide, the expectation that one can draw a clear line between the one and the other is certain to be disappointed.[1]

Nevertheless, the assumption of a clear separation of fact from value persists, and it continues to buttress much of the popular vision of science, perhaps especially in the US. Take, e.g., belief in the purity of science. As a number of historians have demonstrated, the very idea of a "pure science" – separable not only from moral and political values but also from its uses – has often been invoked for purposes that are themselves far from pure – i.e., for conspicuously ideological, political, or practical purposes. Perhaps the most striking example can be found in the particularly vigorous defense of the idea of pure science (alongside that of a value-free science) that came on the heels of the atomic bomb. Vannevar Bush, Director of the Office of Scientific Research and Development, writes in his 1945 report *Science, The Endless Frontier*, "As long as … scientists are free to pursue the truth wherever it may lead, there will be a flow of new scientific knowledge to those who can apply it to practical problems in Government, in industry, or elsewhere."

Examples of more conspicuous breaches of an ideal boundary between science and politics – and bearing considerably closer resemblance to the problem of immediate concern here – also abound. Take, for example, the controversy that has prevailed in the U.S. over the efficacy of mammograms for women in their 40's, and more specifically, the debate that followed the 1993 decision by the National Cancer Institute (NCI) to withdraw its earlier recommendation to begin mammography screening at the age of 40. This decision was based on the report of an international review of breast-cancer screening data (which the NCI had sponsored), from which they concluded, "There is insufficient evidence to make an informed decision regarding efficacy of screening as measured by reduction in breast cancer mortality in women aged 40–49 years" (see Fletcher, 1997). However, the decision was met by widespread controversy, and some three years later, the director of the NCI requested that the NIH convene a "consensus panel" of experts to review the data in an effort to resolve the dispute. The panel issued its report on January 23, 1997, reaffirming the 1993 conclusion that available data did not warrant a universal mammography-screening recommendation for women in their 40's. This time around, the reaction was truly explosive, with accusations against the panel members – of incompetence, of irresponsibility, and even

[1] My focus is on climate science, but I take the arguments I put forth here to be in close sympathy with the more general arguments developed by Heather Douglas in her rich and highly commendable philosophical account of *Science, Policy, and the Value-Free Ideal* (2008).

of fraud – flying fast and furious. Within a matter of weeks, a non-binding resolution supporting mammography for women in their 40's, and urging the NCI to reconsider, was introduced into the U. S. Senate where it was passed by a vote of 98 to 0. On March 27, 1997, the NCI released a new statement in which it now endorsed biannual mammograms for all women in their 40's. In the editorial for an ensuing issue of *Nature Medicine*, Barbara Culliton quipped, "Only in America" (1997: 251).

Maybe so, maybe not. But the main point of these episodes is to show that it is hard experience that has taught us how remote the utopian vision of a strict separation of science from politics is from actual practice. Sheila Jasanoff, one of our foremost scholars on the subject of scientific advising, sums up the situation as follows:

> "Although pleas for maintaining a strict separation between science and politics continue to run like a leitmotif through the policy literature, the artificiality of this position can no longer be doubted. Studies of scientific advising leave in tatters the notion that it is possible, in practice, to restrict the advisory practice to technical issues or that the subjective values of scientists are irrelevant to decision making." (1990: pp. 230-31)

2.2. Can reasonable alternatives be found?

The question is, if the traditional arguments can no longer be defended, what are we going to replace them with? How *should* we think about the place of scientific expertise in a democracy?

In his recent book entitled, *Science, Truth and Democracy* (2001), the philosopher Philip Kitcher has offered us one alternative way of thinking about these issues. Kitcher agrees that values inhabit the scientific as well as the political sphere, just as scientific advances inevitably inhabit both political and scientific spheres. To be sure, the values of science need not be the same values as those of politics, and sometimes they will be in competition. The central point for Kitcher is that the aim of science is not so much truth as it is *significant* truth. He defends what he calls a "modest realism" in which the "facts of the matter" — i.e., the claims that issue from scientific research – are not open to public discussion, but the significance of those facts should be. He argues that the proper aim of a "well-ordered science" in a democratic society is to "serve the collective good" (p. xii), and, given the extent to which scientific research impinges on people's values and interest, such a science requires open and public deliberation both about what research is to be pursued and about how the findings of that research are to be used. In fact, Kitcher's defense of the inviolability of the internal workings of science in this work fits perfectly well within the traditional framework, but his insistence on the unavoidably political dimensions of the judgment of significance (e.g., of the choice of problem to be studied) does not. It recognizes at least one new way (above and beyond questions of the uses of scientific information) in which moral and political values enter into the content of science. In a "well-ordered science", decisions about what to study are to be made through a form of ideal deliberation among mutually respecting individuals with initially different (often uninformed) preferences and recognized experts who can (respectfully) "tutor" these preferences by providing relevant information:

> "In light of the new knowledge, our ideal deliberators revise their collective wish list, the experts update their views about the probabilities of satisfying various wishes, the arbitrator offers a set of options for gaining particular benefits at various levels of cost, and the ideal deliberators pick a policy for making use of the new information. We may think of that policy as reflecting their newly tutored collective wishes." (p. 122)

Kitcher's recommendations merit serious attention, but by invoking expertise to 'tutor' lay participants, he leaves unresolved the fundamental dilemma that many feel expertise poses for a liberal democracy. For example, Stephen Turner, a political philosopher, writes:

> "In the face of expertise, something has to give: either the idea of government by generally intelligible discussion, or the idea that there is a genuine knowledge that is known to a few, but not generally intelligible" (2003, p. 5).

Nor do Kitcher's initial recommendations[2] acknowledge the possibility that expertise itself may harbor collective biases and blindspots. And it is this last problem that Stanford philosopher Helen Longino seeks to address. In her own book, Longino (2002) has argued that the rational justification of a claim (any claim) requires that it meet the criticism of communities with different needs and different epistemological interests. Only in this way can the blind spots of a disciplinary agenda hope to be recognized. But at the same time, she also recognizes the need for criticism to be informed. Just how informed, and by what criteria, she doesn't elaborate.

Others, especially in political science and STS, are more concerned with the essential inequality implied by the very idea of expertise as superior knowledge, opting for a yet more radical integration of expert knowledge and civic concerns. For example, Karin Bäckstrand (a political scientist from MIT and Lund University) takes the assumption of scientific expertise as superior knowledge as being in essential conflict with a "democratic model of public understanding that seeks to establish dialogue, collaboration and deliberation between experts and citizens" (2003: 29). Moreover, she thinks that the particular case of climate science makes demands for an equitable model of the relations between science and society that go significantly beyond the usual demands:

> "In a situation involving large complexity, radical uncertainty and high stakes, new scientific practices to ensure quality control have to be established. This encompasses a re-orientation of science toward incorporating multiple stakeholders. Peer review should include 'extended peer communities' in order to enhance dialogue between stakeholders such as the NGOs, industry, public, and the media" (2003: 32).

But once again, the question of expertise remains. If we accept that there is such a thing as expertise (invoking the definition given by the OED as "one whose special knowledge or skill causes him to be regarded as an authority"), what is its place in deliberations among different stakeholders? How are we to adjudicate, to draw the distinction, either between legitimate and illegitimate criticism, or between knowledge and interests?

These may seem like abstract considerations, and as presented here, they are far too general for my purposes. The problems of expertise in a democratic society depend (and inevitably so) both on the specific kind of expertise entailed and on the implications of that expertise. That they come home to roost with such immediacy and urgency in the arena of climate science derives from the particular character of this science and from the particular implications its claims have for us. Indeed, it is precisely because of the implications of climate research that the distinction between

[2] Kitcher's views have evolved considerably since 2001, and he now recognizes an additional need for public participation in the *certification* of knowledge (see, e.g., Kitcher, 2006).

legitimate and illegitimate criticism of scientific claims about climate change has become so intensely politicized. Yet, at the same time, it is also because of the magnitude of what is at stake that this is just the distinction that we must, somehow, find a way to draw. I take it as obvious that we cannot do so without the help of expert knowledge, without relying on expertise that most of us do not ourselves have. But at the same time, we need to think about how much expertise is required for legitimate criticism, about what ought to count as expertise, and about why it is trustworthy. As Sheila Jasanoff reminds us, "Contemporary democracies depend for their robustness, not to say their very survival, on the wisdom of strangers." How do we know which strangers to trust? Their expertise – i.e., their special knowledge or skill, their experience and training – is surely part of the answer, but Jasanoff also reminds us that "As members of a democracy, we trust experts because they supposedly represent our interests and are accountable to us, but we need to evaluate the basis for that trust from time to time" (quoted by Wheatley, 2008).

To make these arguments more concrete, I need now turn to the specific assertions coming out of climate science and the issues they raise – i. e., to the content and status of the scientific claims, to the various responses to these claims, and to the reasons why they pose such particularly acute challenges to our thinking about science and politics. After that, I will return to the question of the role of expertise in democracy, but this time around, in the particular context of climate science.

3. Climate science: a brief review

Because climate is such an immensely complicated phenomenon, the term climate science refers to a confederation of many different disciplinary distinctions, including, e.g., atmospheric science, earth sciences, chemistry, paleoclimatology, biogeochemistry, oceanography, glaciology, etc. The most far reaching and extensive review of the many kinds of scientific research relevant to climate change, and more specifically, to climate change caused by human activity, was published in 2007 by the Intergovernmental Panel on Climate Change (IPCC), originally established by the World Meteorological Organization and the United Nations in 1988 to undertake just this task. The task was not only huge and formidable, but also potentially fraught with all sorts of political tensions. It is thus worth noting the unusually stringent rules (subject to ongoing oversight) that were imposed on the Panel's *modus operandi* in order to ensure the reliability of its final report. They were as follows: The initial review of the scientific evidence is conducted by a committee of independently selected experts, all working within the framework of peer review. The report resulting from this review is then shared with the larger scientific community, and after that, with government experts. The text is then modified to take into account all commentaries and objections, accepting or rejecting these commentaries on the basis of motivated advice that is also made public. Explicit political influence enters the deliberations in only two ways: First, the entire inquiry is framed by a "road map" defined by a plenary assembly of governmental representatives,[3] and second, the summary statements of the IPCC are subject to approval by a similar body representing all members of the U.N. Finally, the report that is issued to the public contains a record of all these deliberations.[4] This extensive process of checking and

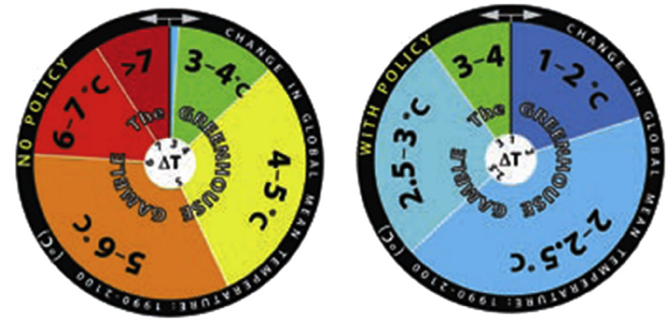

Fig. 1. THE GREENHOUSE GAMBLE: Left, "business as usual"; right: immediate implementation of regulations. The color of each pie slice represents the degree of warming anticipated, while the size of each slice represents the probability of that degree of warming. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

counter-checking does not absolutely eliminate the possibility of errors, but it does work to minimize their occurrence.[5]

The main conclusions of the most recent report (the fourth that the IPCC has so far issued) are based on a comparison of the consequences of four different scenarios, representing four different paths of human behavior, and they are that:

> Warming of the climate system is unequivocal.
>
> Most of the observed increase in globally averaged temperatures since the mid-20th century is very likely due to the observed increase in anthropogenic (human) greenhouse gas concentrations.
>
> Anthropogenic warming and sea level rise would continue for centuries due to the timescales associated with climate processes and feedbacks, even if greenhouse gas concentrations were to be stabilized, although the likely amount of temperature and sea level rise varies greatly depending on the fossil intensity of human activity during the next century.
>
> The probability that this is caused by natural climatic processes alone is less than 5%.
>
> Depending on the particular scenario followed, world temperatures could rise by between 1.1 and 6.4 °C (2.0 and 11.5 °F), during the 21st century and sea levels will probably rise by 18–59 cm (7.08–23.22 in) there is a confidence level >90% that there will be more frequent warm spells, heat waves and heavy rainfall; there is a confidence level >66% that there will be an increase in droughts, tropical cyclones and extreme high tides.
>
> Both past and future anthropogenic carbon dioxide emissions will continue to contribute to warming and sea level rise for more than a millennium.

[3] Thus, e.g., the scenarios to be considered by the scientific panel were explicitly restricted to those that do not include implementation of any additional regulatory initiatives.

[4] See, e.g., Le Treut, 2009, Chapter 3.

[5] The recent flap over evidence predicting the imminent melting of the Himalayan glaciers – evidence that has subsequently been admitted to have been 'poorly substantiated' (see, e.g., the Jan. 20, 2010 report in *The Guardian*, http://www.guardian.co.uk/environment/2010/jan/20/ipcc-himalayan-glaciers-mistake) – might be regarded as a case in point. But given that this 'mistake' did not appear in the IPCC report itself, but only on p. 493 of report submitted to the IPCC by a working group that did not consist of climatologists, what may be most noteworthy about this flap is the degree to which the incident was misrepresented in the American press. As the French climatologist Hervé Le Treut observes, "One must be clear, there is a mistake concerning Himalaya. But the way in which in which this error is publicly represented is suspect: there is a manifestly organized campaign" ("Il faut être clair, il y a eu une faute à propos de l'Himalaya … Mais la manière dont cette erreur est mise en avant est suspecte: il y an une campagne manifestement organisée.") (Quoted in "La pression monte autour des experts du climat", Le Monde, January 28, 2010, http://www.lemonde.fr/planete/article/2010/01/28/la-pression-monte-autour-des-experts-du-climat_1298001_3244.html).

Global atmospheric concentrations of carbon dioxide, methane, and nitrous oxide have increased markedly as a result of human activities since 1750 and now far exceed pre-industrial values over the past 650,000 years.

Since the release of this report, a number of new measurements (re, e.g., the rate of melting of the Greenland ice cap, the rate of melting of the permafrost, the release of methane into the atmosphere) have led a number of scientists to conclude the IPCC estimates were far too cautious. For example, more recent and more comprehensive modeling efforts at MIT that include detailed treatment of possible changes in human activities as well predict at least double the rise in temperature that earlier models had predicted (see Sokolov et al., 2009).

By way of conclusion, the researchers at MIT offer a user-friendly synthesis of their results in a pair of pie-charts (Fig. 1).

Here the probabilities of various temperature increases over the next hundred years under the assumption that we proceed without a significant change in policy are compared with the probabilities that would be associated with a change in policy – specifically, with a policy requiring the implementation of the Kyoto Protocol in 2010 by all countries that agreed to caps in the original protocol, and achievement of the emissions limits described by the U.S. Climate Change Science Program (No-Policy *vs* Policy). Without a change in policy, their models calculate that the probability of an increase in average surface warming of less than 3° C. would be under 1%, while the probability of an increase of temperature of more than 4° C would be 87%.[6] With such a policy in place, however, the odds change dramatically. The probability of an increase of temperature of less than 3° rises to 90%, while the probability estimated for a temperature increase of more than 4° C becomes negligible.

How do these numbers translate into actual human costs? What would an increase in the average temperature of the surface of the earth of even 2° mean for its many inhabitants? In part, the answer depends on how and where one lives, for even though no one is immune, neither temperature increase nor the effects of such increase is evenly distributed around the globe. Drought and desertification are considerably greater worries in the south than in the north, while extreme weather events (e.g., heat waves, floods, and storms) are more likely in the north. Also, drought is more costly to a farmer than to a factory worker, just as extreme heat is more dangerous to someone living in a tenement than to someone living in an air-conditioned condo. The economic and health costs of average temperature increase thus depend critically on the specificities of context. Taking such factors into account is not generally considered to be the climatologists task, but the predictions they are able to make clearly argue that, wherever and however one lives, the effects are likely to be severe.

Just how severe is not known, but most analysts agree that even what may seem like a relatively mild increase of 2° C (a figure widely touted as the maximum increase we can tolerate) would lead to wide-scale environmental disruptions including, among other effects, massive floods and droughts.[7] Indeed palpable evidence of the effect of the even smaller rise in temperature of 0.7° C that we have already experienced can be seen in the melting of mountain glaciers, the Greenland and Antarctic ice sheets and their associated permafrost occurring today. Estimates of the rise in sea level expected by the end of the century have increased dramatically over the last 2–3 years, largely because the observed rates of melting are now much greater than had been anticipated. Current estimates are roughly double those of the IPCC from just two years ago. According to the most recent research, seas are now rising by more than 3 mm a year. From a news conference with four leading glaciologists at a recent climate congress in Copenhagen, the NY Times reports that "the upper range in the rise of sea levels could be approximately 1 m (3.28 feet), 'possibly more,' by 2100. At the lower end of the spectrum, it appears increasingly unlikely, say the study's authors, that sea level rise will be much less than a half-meter by 2100" (http://www.nytimes.com/cwire/2009/03/11/11climatewire-researchers-warn-that-sea-levels-will-rise-m-10080.html).

By far the greatest uncertainty in the physical responses to warming derives from the potential instability of the climate system. Most climate scientists agree that progressive warming exacerbates the kinds of feedback already known and triggers new kinds that haven't yet been thought of, with the potential of eventually leading to runaway global warming of a sort that would make the earth uninhabitable. Evidence of climate instability can be seen from the historical temperature record that has been constructed from experimental paleo-climatic (ice-core) data, but the variations shown in the historical record occurred spontaneously. Today we are introducing an unprecedented perturbation into a climate system that has already been demonstrated to be unstable, and it is very difficult to predict the consequences of such perturbation. Though there is wide agreement that a point of runaway global warming exists, there is little agreement about when, and with what likelihood, such a point would be reached, and hence, extremely difficult if not impossible to agree about how just much CO2 can be tolerated by the atmosphere before triggering such a catastrophe. There are however other senses in which the term "tipping point" has been used, and where consensus may be easier to come by. Sometimes it is used simply to indicate a "point of no return" for some parameter; other times, to indicate discontinuities in the rates of change. In these senses of the term, some tipping points have almost certainly already been reached. For example, many scientists agree that it is already too late to reverse the rise in temperature that has already occurred, no matter what regulations are deployed (at least not over a time span of decades, and perhaps not even over a time span of centuries). Consensus is more difficult to achieve in discussions where the term is used to indicate the limits beyond which a rise in temperature (or CO2 concentration) can no longer be considered tolerable, either because of the human upheaval such a rise would cause in itself, or because it indicates the onset of a new regime of warming in which the rate of warming would be significantly escalated. Thus, e.g., some have argued that a tipping point of this kind has already been reached, and that it is probably already too late – perhaps not for the habitability of the earth, but for human habitation of a kind we are used to. Others fix the limits of safety at a temperature rise of 2C, (or 3.6F) (as Gordon Brown put it, "average global temperatures must rise by no more than 2C" (http://www.timesonline.co.uk/tol/news/world/europe/article6670327.ece)), and this limit has now been reaffirmed at the most recent Copenhagen summit. Whether or not the rise in average temperature *can* be limited to 2C depends on how soon and how drastically greenhouse gas emissions can be reduced in the very near future. For example, a recent study of the consequences of not doing so warns that failure to meet strict targets for cuts in greenhouse gas emissions over the next 40 years could put the goal of limiting planetary warming to 2 °C by 2100 permanently out of reach (see O'Neill, Riahi & Keppo, 2010). And still others, like Freeman Dyson, are confident that our technical ingenuity will enable us to deal effectively with even higher increases. We can

[6] These figures represent an anticipated increase in warming far larger than previous studies. In particular, the median surface warming is now projected to be 5.18C in 2091–2100, in contrast to the 2.48C projected in earlier studies.

[7] Even in the absence of more precise information, more than 100 nations have already agreed to the goal of keeping temperature rise below 2C. The question of course is whether this can be achieved, and if so, how.

however find a kind of consensus by turning the question around: virtually all climate scientists would agree that there are no guarantees of safety anywhere. Even limiting global warming to a maximum of 2C does not altogether eliminate the risk of triggering runaway climate change. Here is a case of uncertainty in which it is not the fact of risk that is in question, but rather its magnitude, and its immediacy. The event to be avoided is not so much hypothetical as singular[8] – not unlike the threat of terrorist attack faced in the U.S. prior to 9/11. It is a threat whose probability cannot be calculated, but against which it is nonetheless possible to take certain preventive steps.

But what may be an even more important point is that the same non-linear feedback effects that lead people to worry about future tipping points are also responsible for irreversible effects of far more immediate concern. Take for example the challenge of limiting global warming to 2C. Not only would such a goal take enormous efforts to achieve, but also, the restrictions that would be required would have to be implemented now. We are already well advanced toward this limit, and, because of the dynamics of feedback, temperatures will continue to rise even if all emissions were abruptly stopped. The task of keeping that increase below 2C thus becomes more demanding with every passing day. In 2012, the Kyoto agreement will expire, and in the absence of a new (and more effective) treaty by that date, it is not at all clear whether it will be possible to sustain a limit of 2C no matter what our efforts. The threats that loom – and that will so dramatically affect our grandchildren – are thus not just in the distant future: they are now, or if not now, in the very immediate future.

Alarming as these prospects may seem, however, they are still relatively abstract – i.e., apart from scenarios so catastrophic as to imply the end of human civilization, the figures produced by climate projections have still not been translated into human costs. The more extreme scenarios are not much discussed by climate scientists, precisely because the risks are so hard to estimate, but even in discussions of less catastrophic scenarios where probabilities *can* be estimated by computer models, it is rare to see such probabilities translated into human costs. What in fact would a 2C (or 4C or 5C) rise in average surface temperature mean for human lives? What would it mean not only for us in temperate climates, but for those living in less temperate climates? Perhaps more to the point, what would it mean for our children and grandchildren? To be sure, answering such questions requires going beyond the expertise of climate scientists, but I will argue later that climate scientists, if they are to be accountable to the public that supports them, must bear a responsibility in seeing that the relevant expertise is recruited. Indeed, I will argue that ensuring the translation of their findings into human costs is a basic part of their responsibility as scientists.

Again, not all of the costs can be anticipated, and new forms of uncertainty enter into the calculations when estimates of human behavior become part of the equation. Yet here too, some costs are relatively easy to predict. For example, we can gain some sense of the consequences of, e.g., a 1 m rise in sea level from the fact 60 million people now live within 1 m of mean sea level, and, as Anthony Ananthaswamy (2009) points out,

> "that number expected to increase to about 130 million by 2100. Much of this population lives in the nine major river deltas in south and southeast Asia. Parts of countries such as Bangladesh, along with some island nations like the Maldives, will simply be submerged.
>
> According to a 2005 report, a one-metre rise in sea level will affect 13 million people in five European countries and destroy property worth $600 billion" (*New Scientist,* July 1, 2009).

How many people would die, how many would flee to other countries, how many simply devastated, we cannot say. Nor can we predict the political consequences of such a major upheaval. Yet as much as sixteen years ago, long before the reality of global warming was officially acknowledged, the Pentagon was already sufficiently concerned to sponsor a study of the possible consequences of such warming. The resulting (and conspicuously "America first") report, not published until 2003, warned that

> "large population movements in this scenario are inevitable. Learning how to manage those populations, border tensions that arise and the resulting refugees will be critical. New forms of security agreements dealing specifically with energy, food and water will also be needed. In short, while the US itself will be relatively better off and with more adaptive capacity, it will find itself in a world where Europe will be struggling internally, large number so [sic] refugees washing up on its shores and Asia in serious crisis over food and water. Disruption and conflict will be endemic features of life." (Schwartz & Randall, 2003).

Since that time, concern about the threat to national security posed by global warming has risen considerably, both in the state department and among political scientists. Thomas Homer- Dixon, Chair of Global Systems at the Balsillie School of International Affairs in Waterloo, Canada, argues that 'climate stress may well represent a challenge to international security just as dangerous – and more intractable – than the arms race between the United States and the Soviet Union during the Cold War or the proliferation of nuclear weapons among rogue states today." (2007, A25) Resource scarcity, and wars over ever diminishing resources, is widely regarded as the prime cause of the political instability feared, and some (e.g., UN Secretary-General Ban Ki Moon) have even suggested that the effects of environmental pressures and resource scarcity can already be seen in current crises like that in Darfur (Ki Moon, 2007).

4. Proof, evidence, and consensus in scientific practic

What is the status of these claims? Have they been proven to be true? Are they accepted by all climate scientists? And if not, why should we believe them? After all, we have all heard warnings of imminent dangers before, most of which never materialized. Why should we believe the experts this time around? As Naomi Oreskes (2007) asks, mightn't they just be wrong?

Of course they might. Science is never infallible. Nor does it claim to offer proof. Contrary to the popular image, uncertainty and doubt are the daily diet of scientific researchers. On what basis then, in the face of so much uncertainty and fallibility, do they warrant our confidence that their efforts to "get it right" are paying off? There is in fact a huge philosophical literature on these issues about which most scientists are not much bothered, and which I will therefore pass over and instead, attempt a brief and quite general description of how, in practice, scientists manage to develop confidence that the claims they put forth are, in Catherine Elgin's felicitous expression, "true enough" (2004).

[8] Here I take issue with Jean-Pierre Dupuy who, together with his colleague Alexei Grinbaum, questions the relevance of the "precautionary principle" on the mistaken grounds that the risk we are dealing with in climate change is not so much a "potential" as "a hypothetical risk, one that is only a matter of conjecture " (2004: 10). Run-away climate change is not a hypothetical risk but a real one, even if we are unable to calculate the odds of its occurrence within a given time period.

In place of philosophical analysis, scientific researchers have evolved various *modi operandi* that have for the most part worked astonishing well, even if they are not immune to criticism. Yet these ways of proceeding bear little resemblance to popular images – either of science as a privileged realm of truth, certainty and proof, or of scientists as the guardians of those achievements. In lieu of philosophical instruction about the nature of truth, of the relation between evidence and proof, or even of the "scientific method," scientists are trained in various practical measures for seeking maximally reliable and maximally useful descriptions of a phenomenon, in criteria (both formal and informal) for the quality and strength of the evidence supporting their claims, and in the ethics of scientific behavior.

Above all, they are schooled in the ethic of truth-telling, for trust in the truthfulness of one's colleagues observational reports is something of a minimal condition for the scientific endeavor to proceed. This is the reason scientists are so vigilant in the detection and prosecution of scientific fraud. But an ethic of truth-telling is no guarantee that mistakes have not been made, or that interpretations are not open to dispute; it only means that it is possible to assume the absence of conscious deception. In other words, truth-telling by itself is no guarantee of truth. Certifying the truth of a claim is a far more ambitious undertaking, and scientists' relation to this commodity is correspondingly more complex. While it is often claimed that scientists "stand on truth", it would be fairer to say that they "reach for truth," standing instead on experience, expertise, and evidence.

How, in practice, is the strength of a claim determined? The standard response is that the strength of theoretical claims is measured primarily by empirical evidence, where empirical evidence rests in the first instance on observation (e.g., on direct measurement of some parameter). In turn, the quality of such observational data rests on their robustness and reproducibility. To be sure, observations are themselves dependent on the quality of instrumentation, and on the theoretical assumptions built into both the construction of these instruments and the calculations required to turn the initial (or direct) measurement into the quantity of interest (e.g., the conversion of measurements of isotopic fractions in ice cores to temperature). When the distance between direct observation and interpretation is large (as it often is in climate science), interpretations (or "theoretical claims") may be particularly open to dispute because of the many levels of uncertainty on which they depend, and it is here that experience and expertise become most critical. Indeed, experience and expertise may be as important as evidence in determining the strength of a claim.

No scientific analysis is immune to uncertainty, but the ways in which uncertainty enters the analytic process multiply dramatically with the complexity of the problem under study. As everyone knows, climate change is an especially complex phenomenon and even after decades of study, our understanding remains far from complete. Climate is the product of a large number of variables interacting over long time periods in extremely complex ways. Efforts to understand this process – to determine what variables are most important and how they interact – depend both on empirical measurements of the relevant variables and on models of the physical dynamics governing their interaction, with constant exchange between the two activities. With recognition of the role that human activity may be playing in these processes, the task has become yet further complicated by the need to also include in these models estimates of both our past and future behavior, along with the effects of this behavior. It goes without saying that the enormous complexity of these models means that they can be analyzed only by very large computers, but once formulated and analyzed, compared with available data and then reformulated and reanalyzed, they can be used to help us to better understand past, present, and future climates. The knowledge thus acquired is far from complete but it is the best that climate scientists have so far been able to achieve; similarly, the predictions this knowledge makes possible are a far cry from certain, but they give us our best shot at anticipating the future. They do not tell us what will happen, but they can provide us with estimates of what, under various scenarios, the future climate is likely to have in store for us, and with what odds.

Lay readers expecting definite answers from science are bound to be disappointed, but not so the scientists who work on such problems. Like most working scientists (mathematicians aside), climate scientists well recognize that they are not in the business of providing either proof or certainty. Even when informally invoking the language of truth to refer to their most robust findings, they by and large recognize that way of speaking as a shortcut for claiming evidence strong enough to warrant the working assumption of truth. Needless to say, judgment of just how strong the evidence needs to be for this purpose will vary, and there is ample room for debate. But the hope is that, as the evidence grows stronger, consensus can be achieved. That is, that there will come a point at which the community as a whole at least provisionally agrees to accept the theories and claims in question, and move on to the questions that follow from those theories and claims. In fact, such a point must exist if the work is to proceed; failing the achievement of such consensus, the research program will eventually just fizzle out. But the main point for the purposes of this discussion is that, while scientists generally recognize that the history of science leaves no room for absolute certainty, they are not bothered by this lack; their confidence in their ability to contribute to the overall progress of scientific knowledge does not depend on total certainty. It suffices to be pretty certain; indeed, in informal discourse, near certainty is sometimes taken to qualify as 'proof', but when it is so taken, it needs to be understood that the notion of proof invoked in such discussions is more accurately a notion of "sufficient proof". Nor, for that matter, do they require complete consensus; for most purposes, almost complete consensus serves well enough. In other words, the ideals of democracy play a crucial role within the boundaries of a scientific community: the practices of open deliberation, collective criticism, and peer review all work toward the resolution of disagreements. These same practices are also called upon to bolster the power and influence of the majority scientific judgment in the world at large.

By contrast to their internal workings, scientific authority is frankly elitist in its relation to outsiders to a disciplinary community: the right to criticize is generally limited to those who have sufficient training and research experience to evaluate the relevant evidence and arguments – i.e., to members of their own communities, their peers. These are the people responsible for reviewing and evaluating papers for publication. Obviously, they share similar viewpoints and make similar assumptions. Indeed, some of these assumptions are required for the endeavor to proceed. For example, members of a community need to assume expertise as a *sine qua non*, and share a common notion of what constitutes expertise; they must also agree to ignore arguments not supported by logic and evidence, and remain suspicious of evidence that has not (or cannot be) reproduced by others. These basic assumptions underlie the process of mutual evaluation that is both an ongoing and a basic part of all research practice, constituting a kind of routine self-policing of the disciplinary community. They similarly guide the extra levels of evaluations that have been added in the particular case of climate science: first and foremost, by the Intergovernmental Panel on Climate Change (IPCC), and second, in the independent reviews undertaken by national committees such as, e.g., the National Academy of Sciences. They might even be said to define what it means to assert a claim as scientific, rather than, say, opinion. Finally, it is precisely because their claims have survived all these levels of scrutiny that (at least the great majority of) climate scientists have as much confidence in their reported measurements, calculations and predictions as they do.

Yet, even so, there is dissent – consensus is by no means absolute. In spite of all this mutual evaluation, the majority conclusions of climate scientists could still be wrong, and we need to ask, what if they are?

5. Truth and consequences: what makes climate science special?

Climate Scientists know their predictions could be wrong. As critics routinely remind them, these predictions are based on complex computer models that are subject to many sources of uncertainty. For one, they can only partially represent the many factors involved in climate change; for another, the dynamics being modeled depend on scenarios of human behavior about which they can only guess. At best, the models provide estimates of the magnitude of the various possible climatic outcomes associated with a given scenario, together with estimates of the probability associated with that outcome. Assessing the economic, political, and health costs associated with a particular outcome – i.e., translating meteorological predictions into human costs – is even more difficult, and for this, climate scientists must turn to other experts (e.g. political scientists, economists, and epidemiologists). But for all their weaknesses, the resulting predictions are the best that they (and hence we) can do; there is no better way to proceed.

What so worries these scientists is the magnitude and seriousness of the implications of their findings should they be right. No one knows how bad things could get – how hot, how inundated, how drought-stricken, how unstable. Nor can anyone say whether the cost in human lives by the end of the century, should we fail to act now, is likely to be in the tens or hundreds of millions (or even billions). We only know that the most likely consequences of inaction would be horrendous. Maybe they will never come to pass, but the question is, how large a risk are we willing to tolerate?

But there is also another issue: The magnitude of the potential consequences of inaction are of an order that requires us to draw a distinction of vital importance between climate science and other familiar clashes between science and politics that are sometimes invoked as parallels –evolutionary theory, e.g.; or theories of planetary motion; or even breast cancer. Or, to put it another way, it is the more general relation between truth and consequences that sets climate science apart from so many past public controversies over scientific claims, and that so acutely exacerbates the tensions between expertise and democracy.

Take evolutionary theory. Today, many scientists are extremely exercised by the fact that, almost a century after the Scopes trial, polls indicate that the percentage of Americans who believe that humans evolved from other species is substantially under 50%,[9] and fewer still believe in evolution by natural selection. These are certainly disturbing figures, and they clearly indicate how little influence scientific authority has in the American heartland, a realization that is undoubtedly disturbing in and of itself. But ultimately, we must ask, what hangs on what the majority of Americans believe about evolution? To be sure, educational policy is one domain that is directly affected, and what is taught in the schools, in turn, affects how Americans think. But the primary effect of changes in educational policy is on the beliefs of the populace, and not on their health or overall welfare. The same can be said about popular belief in a heliocentric universe. But the effects of how people think about climate science are of a different kind – more similar, say, to the effects of how they think about the efficacy of mammograms for women in their 40's where popular views have direct effects on medical policy, and hence on health. Yet even this parallel fails, for while the problem of breast cancer is certainly a serious one, the number of people at risk for adverse effects of one policy or the other is miniscule compared to the number of people put at potential risk by a policy of inaction with respect to climate change. Furthermore, the consequences of people's beliefs here are directly related not only to their own fates, but may prove critical to the future wellbeing of the entire human population – including not only those now living, but perhaps more importantly, those who will inhabit the world we bequeath. The reason is obvious: Most experts are convinced that the climate of the future depends on the collective decisions we make over the next few years, both at home and in our efforts to forge international agreements. In the absence of effective global restraints, we seem to be headed towards a degree of global warming that will not only cause enormous climatological, social and political upheaval, but that can also trigger catastrophic and irreversible environmental effects. It is of course possible that it is already too late, but the best information we have on the subject argues that, if not, our only hope in avoiding such risks lies in a massive reduction in greenhouse gas emissions. Needless to say, that will not happen unless people take the available information seriously. Climate change is of course a global phenomenon, and it is obviously necessary that the scientific predictions be taken seriously everywhere. But given both the political and economic primacy of the US on the world stage, and our disproportionately large contribution to the total quantity of greenhouse gases in the earth's atmosphere, it seems evident that what Americans believe – and on the basis of their beliefs, are willing to do – are of primary importance.

To take an obvious example, any attempt to regulate our own emissions by means of a carbon tax must be submitted to the political process, and cannot begin to be effected without popular support. From where will such support come? Clearly not from those who, for whatever reason, decide to discount the evidence of our experts. Similarly, efforts on the global stage to revise and renew the soon to expire Kyoto accord are likely to be stalled without American support, and that support depends once again on what Americans believe. Here, in other words, is a situation in which what Americans – whatever their level of scientific literacy or respect for scientific authority – believe has direct consequences of a kind that go beyond even their own borders. To be sure, any change in policy that is likely to be effective will also require international support. But one obvious reason for focusing on Americans is the extent to which our own behavior and policies serve as models for the policies and behavior of so many other nations.

Finally, I want to mention one more way in which climate science needs to be distinguished from other scientific issues around which public debate has raged, and that has to do with the relation between the science and risk. In fact, much of the extant literature about expertise and democracy has developed in response to debates, not about climate change, but about appropriate use of the products of biotechnology (e.g., GMO's, or stem cell research). The pace and social impact of recent developments in areas like biotechnology (nanotechnology is another) is such that a number of authors have argued that we are in the midst of a third industrial revolution. Often called an 'information' revolution, it brings with it a transformation of social organization from the older industrial societies to new "knowledge societies" in which knowledge itself has become the primary source of wealth and power. The inevitable

[9] Based on a survey online between February 3 and 5, 2009, the most recent Harris poll puts this figure at 29% (see http://www.harrisinteractive.com/harris_poll/HarrisPollByDate2009.asp).

risks associated with the deployment of these new knowledges have constituted the primary foci for recent discussions about the relation between science and policy, and especially, about the role of citizen participation in evaluation of these risks and in the setting of appropriate policy.

But climate science is not biotechnology. The difference is not due to the inherent uncertainty of its predictions, but to the fact that it neither creates wealth (if anything, it threatens to consume it) nor produces risks. Certainly, climate science is about risk, but the risk with which it is concerned has been produced not by an information revolution, but by the old industrial revolution that is only now beginning to catch up with us. The risk at issue here is not the product of the science, but its subject.

But still the question persists, what if the climate scientists are wrong? What if the investment we would have to make in retooling our economy and reducing emissions turned out not to be necessary after all? These questions need to be juxtaposed with the complementary question, what if the skeptics are wrong? In one case, we have made an expenditure that hindsight proved to have been unnecessary, but in the other case, hindsight shows that we missed the chance of averting a catastrophic degree of human suffering. Perhaps we should think of the costs required by an effective emissions policy as insurance. Most of us do not hesitate to invest in costly insurance policies to protect us from the devastating effects of accident or illness, and yet would nevertheless be immensely relieved should our worst fears never materialize. Why would the same logic not apply here? Part of the answer to this question may lie in the various cognitive biases to which we are all prone, and which may strongly influence the amount we are willing to pay for such insurance – e.g., people seem to systematically underestimate the threat of catastrophic events so rare as to be outside their experience and appear to be correspondingly reluctant to insure against such events, even when their effects are devastating (see, e.g., Kates, 1962). But even so, are there are not limits to the amount of devastation any of us would be willing to risk – once, i.e., as we were convinced that the risk of such devastation was real?

6. Crossing the science/politics divide: scientists who become "activists"

For a large number of climate scientists, there is no doubt that the investment is necessary, and some feel strongly enough to venture out into the public domain, using all their authority and expertise to convince the lay citizenry. The most famous of these is undoubtedly James E. Hansen, Director of NASA's Goddard Institute of Space Studies in New York. As one of the first to predict a substantial increase in temperature over the coming decades, Hansen is often referred to as the 'grandfather of global warming'. And at the same time, he also predicted that anthropogenic contributions to this warming would become evident (i.e., would become distinguishable from noise) by the end of the 20th century. But it was his testimony before the US Senate in 1988 that played the most critical role in bringing both him and the problem of climate change to public attention. On June 23rd, a day of record-breaking heat, Hansen put the following three claims before the Senate:

> "Number one, the earth is warmer in 1988 than at any time in the history of instrumental measurements. Number two, the global warming is now large enough that we can ascribe with a high degree of confidence a cause and effect relationship to the greenhouse effect. And number three, our computer climate simulations indicate that the greenhouse effect is already large enough to begin to effect the probability of extreme events such as summer heat waves." (http://image.guardian.co.uk/sys-files/Environment/documents/2008/06/23/ClimateChangeHearing1988.pdf)

Today, more than 20 years later, improvements in modeling and the acquisition of new data (both historical and current) have only exacerbated Hansen's sense of alarm, and he now spends a good part of his time on efforts to raise public consciousness – speaking, writing, testifying, and even engaging in political actions. The primary advisor to Al Gore in the making of "An Inconvenient Truth," his primary concern today is with our increasing reliance on coal, the "dirtiest" of all fuels, and one that has now become the world's leading source of greenhouse-gas emissions. Hansen is adamant about the need to close down existing coal plants and to place a moratorium on the building of new coal plants until clean coal technology becomes available, and has gone so far as to refer to today's coal-fired power plants as "factories of death." Asked how his involvement with political protests "square" with his science, he says, "I'm trying to make clear what the connection is between science and the policy. Someone has to do it." (Kolbert, 2009, p. 39). And elsewhere, "I'm connecting the dots between the science and the policy. If I don't do that, then the special interests do it and they screw it up" (quoted in Marshall, 2009, http://www.eenews.net/public/climatewire/2009/07/14/1).

Hansen has long been widely regarded as one of the world's most eminent climatologists. He was elected to the National Academy of Sciences in 1996 and has received many prestigious awards. Most recently, he was honored by the Dan David Prize for "achievements having an outstanding scientific, technological, cultural or social impact on our world" in 2007, by the PNC Bank Common Wealth Award of Distinguished Service in 2008, and in 2009, he was awarded the 2009 Carl-Gustaf Rossby Research Medal, the highest honor bestowed by the American Meteorological Society (AMS), for his "outstanding contributions to climate modeling, understanding climate change forcings and sensitivity, and for clear communication of climate science in the public arena" (http://www.ametsoc.org/awards/2009awardrecipients.pdf).

Yet despite all this acclaim, Hansen's blatant transgression of the traditional science-politics divide has made him increasingly controversial in the public arena on the one hand, and increasingly isolated within his own disciplinary community on the other. In response to the AMS award, environmental journalist Andrew Freeman of the Washington Post wrote, "By honoring Hansen, the AMS has raised questions about the proper role of scientists in a world that is facing complex challenges that mix science and politics. A key issue is whether it is appropriate for prominent scientists to serve dual roles as researchers and advocates for political change, or if must there be a clear separation between the two." Freeman quotes meteorologist Joseph D'Aleo as saying that the meteorological society's decision to honor Hansen was "a sad day and embarrassment for a once great society that has lost its way," and Freeman himself concludes with the regret that "The AMS failed to recognize that by giving him the Rossby medal and citing his 'clear communication of climate science in the public arena,' they may have actually sanctioned his political advocacy." (http://voices.washingtonpost.com/capitalweathergang/2009/01/it_normally_does_not_make.html). Even some climate activists are critical: For example, Eileen Claussen, president of the Pew Center on Global Climate Change, is a great admirer of Hansen as a scientist, but, she says, "I wish he would stick to what he really knows. Because I don't think he has a realistic view of what is politically possible, or what the best policies would be to deal with this problem" (quoted in Kolbert, 2009, p. 44). And to many climate scientists, he has become something of an embarrassment. As Naomi Oreskes puts it, "he has started to speak in moral terms, and that always makes scientists uncomfortable" (Kolbert, 2009). Hansen blatantly violates both the traditional boundaries between facts and values, and between science and politics, and many worry that by doing so, he undermines the credibility they, as scientists, have earned precisely by staying on their own side of the fence. But what is undoubtedly the most virulent response comes in the public arena, and it comes from the most militant deniers of the role of human activity in global warming, often referred to as the "contrarians".

7. Skeptics and contrarians fight back: climate science in the public arena

Blogs denouncing Hansen abound, accusing him of everything from 'stagecraft', perpetrating a hoax on an innocent public, to taking more money from the government than skeptics take from the oil industries, and for having supported John Kerry in the 2004 election. But the primary focus of these attacks is two-fold: first, an insistence that Hansen doesn't speak for the scientific community as a whole, nor does he even speak for most climate scientists; and second, the inherent uncertainty of the data he relies upon, and the implication these critics draw that there is no 'proof' to support his claims.

Let's take the question of consensus first. While it might be hard to find a climate scientist who continues to doubt the existence of global warming, there is no denying the existence of some experts who remain unconvinced of human responsibility for these changes. How many? That depends on what you read, and upon who is counted as an expert. There has, e.g., been a great deal of media attention devoted to a few scientists who have come forward to take issue with claims of a "consensus" on this issue. A man who claims to have been ("in effect") Hansen's former supervisor at NASA, John S. Theon, declares himself as one. On January 15, 2009, in a letter to the Minority Office at the Environment and Public Works Committee Theon dismissed climate models as "useless", and wrote "I appreciate the opportunity to add my name to those who disagree that global warming is man made."[10] Theon is not himself a research climatologist, but Richard Lindzen, an atmospheric physicist and Professor of Meteorology at MIT, certainly is. Lindzen has long been a vocal critic of the IPCC's report that human activity has significantly contributed to global warming. And there are others. But far from the deluge claimed in recent publications by the Heartland Institute and disseminated by numerous blogs,[11] they constitute a very small minority among working climate scientists. Public perception however is quite different. Most people — including those who would not themselves rely on the reports of these blogs, nonetheless believe that there is no consensus at all, and that dissent among climate scientists is rampant. Why is this so?

Several authors have pointed to an obvious explanation. People who do not rely on conservative blogs do read newspapers, and much of the responsibility for the widespread misperception about the amount (and kind) of skepticism has been placed on the norm of "balanced" reporting that has traditionally prevailed in journalism, at least in the U.S. For example, in one study of journalistic practices in America's best newspapers entitled "Balance as Bias," Boykoff and Boykoff (2004) point to "the proclivity to tell 'both sides of the story" as a major source of the conspicuous discrepancy between scientific and popular discourse on these issues, and conclude that "adherence to the norm of balanced reporting leads to informationally biased coverage of global warming" (p. 129). Liisa Antilla (2005) goes further: in a follow-up study of 255 American newspapers as well as of the major wire services she published one year later, she reports,

> "Not only were there many examples of journalistic balance that led to bias, but some of the news outlets repeatedly used climate skeptics—with known fossil fuel industry ties—as primary definers. Worse yet, in some instances, such articles originated from wire or news service providers (including newspapers that provide such services or are affiliated with news service agencies)——which caused the exponential spread of misinformation" (p. 350).

These authors take the distinctions between information and disinformation, between bias and impartiality, for granted. But clearly, not everyone — apparently not even most people —shares their faith. Where, they ask, is the proof? In the context of these discussions, it is perhaps unfortunate that scientists themselves do sometimes — especially in in informal exchanges – casually speak so about 'proof.'[12] Although they are well aware that there is no escape from uncertainty, and that actual proof continues to elude them, they often elide distinctions between strong evidence — i.e., strong enough for them to accept (for all practical purposes) a phenomenon as 'true' – and proof. In this, they unwittingly collude with a press that is largely insensitive to such distinctions, and that translates scientific reports of "new evidence", "new observations", or "growing consensus" into claims of "New proof" (www.timesonline.co.uk/tol/news/world/article516033.ece), or even of "The Final Proof" (www.timesonline.co.uk/tol/news/world/article516033.ece). Equally unsurprising is that climate change skeptics are quick to make use of this slippage. Thus, e.g., a number of critics have begun to argue that definitive proof (either of global warming itself, or of the role of human activity) is still lacking, that climate science is riddled with uncertainties, claiming furthermore that political and social action cannot be unjustified until these uncertainties are resolved.

The first part of their critique is of course correct (just as in all scientific claims, some degree of uncertainty inevitably persists), while the last part is patently false, and it is indeed somewhat surprising that it has any credibility at all. As individuals, as corporations, or as governmental agencies, all of us routinely base our behavior on probabilistic predictions. We consult weather forecasts in deciding whether or not to take an umbrella; insurance companies consult mortality data in setting their rates; governments call out troops when armed conflict is deemed likely. Science is no different. Indeed, confusion about the role and meaning of uncertainty in science goes far in explaining both the relative absence of debate in the scientific literature about either the fact of global warming or about the role of human activity in this warming (see, e.g., Oreskes, 2004; 2007; McMichaels et al., 2006)),[13] and the extent to which

[10] Quoted in http://epw.senate.gov/public/index.cfm?FuseAction=Files.View&FileStore_id=83947f5d-d84a-4a84-ad5d-6e2d71db52d9.

[11] See, e.g., the *United States Senate Minority Report on Global Warming* — a report in which a list of almost 700 individuals with implied scientific credentials, and identified as skeptics (citing Heartland Institute publications), is offered as evidence that measures to address climate change are premature. But the Center for Inquiry Office of Public Policy, devoted to assessing the credibility of such claims, examined this list that less than 10% of these individuals "could reasonably be considered to be active climate scientists", and that the views of at least some of those were seriously misrepresented (see www.Senate.20Minority.20Report.20credibility).

[12] It is worth noting however that the IPCC reports explicitly avoided any reference to proof, always presenting their estimates in probabilistic terms.

[13] Analyzing a sample of 928 papers published in refereed climate science journals during the period 1993 to 2003, Oreskes failed to find a single example refuting the claims (a) that global warming is occurring, and (b) that human activity is responsible for at least part of this warming (2007: 71). Which, as she acknowledges, is not to say that no such papers exist (they do), but only that they must be very few.

debate about these issues is conducted in arenas where such confusion is most rampant — i.e., in the popular press, radio and TV, and the Internet.

One feature of such public debates that is especially striking is the degree to which lay critics of climate science have appropriated a popular rhetoric of scientific truth for their own uses, criticizing the scientists for failing to provide the necessary proof, while at the same time claiming precisely that mantle of "proof" for their own arguments. Thus, e.g., a Google search for "anthropogenic global warming" (AGW) and "proof" brings up one site announcing "Proof That AGW Is A Fraud, CO2 Residence Time Too Short" (www.groups.xorte.com/Proof-That-AGW-Is-A-Fraud-CO2-Residence-Time-Too-Short,t,56427,8.html); another, "Global Warming Hoax: Kilimanjaro Glacier Isn't Melting, More Proof That Al Gore Had It Wrong" (www.globalwarminghoax.com/news.php?extend.17); and still another, "Proof that CO2 is not the Cause of the. Current Global Warming" (www.lavoisier.com.au/articles/.../mcclintock-proofnotco2-2009.pdf). Indeed, websites employing the rhetoric of scientific proof to refute the claims of climate science seem to proliferate daily. In some of these, critics employ apparently scientific measurements of their own choosing; in others, they claim to refute the dominant conclusions of climate science by identifying possible flaws in the scientific argument. (A guide instructing climate skeptics on just how to do this – *The Skeptic's Handbook* – has recently been issued and distributed by the Heartland Institute, a nonprofit organization whose stated mission is "to discover and promote free-market solutions to social and economic problems").

Still other websites employ a different strategy: rather than inserting their own "scientific" conclusions, they simply invoke the right of every voice to be heard (see, e.g., "Kids Against Anthropogenic Global Warming || Make Your Voice Heard!" (kidsagainstagw.com). Indeed, the spread of this sort of epistemological populism — what Kitcher describes as "a vulgar form of democracy that "solves" the problem of expertise by denying that there are any experts"[14] – provides a crucial background to one of the more interesting recent developments — namely, the effort of the US Chamber of Commerce to put the claims of climate science to judgment by a public tribunal.

8. The US chamber of commerce

In April 2009, the Environmental Protection Agency (EPA), responding to an earlier ruling of the US Supreme Court authorizing them to regulate greenhouse gas emissions under the Clean Air Act (*Massachusetts v. EPA,* 2007), announced their decision to commence regulation of such emissions. The argument was based on the most recent scientific evidence attesting that emissions of carbon dioxide, methane, nitrous oxide, hydrofluorocarbons, perfluorocarbons and sulfur hexafluoride from new automobiles and their engines endanger public health and welfare by their contribution to air pollution.

American business did not take this announcement lying down. The U.S. Chamber of Commerce (representing 3 million businesses) responded by filing a petition for the "EPA to Conduct Its Endangerment Finding Proceeding On The Record" on June 23, 2009,[15] and a "Supplementary Statement" in support of that petition on August 24. A "Proceeding on the Record", it is asserted, "is necessary to narrow the areas of scientific uncertainty [and] to permit a credible weighing of the scientific evidence"; also, it "is the most efficient and only complete method for testing the competing claims … concerning extreme weather events and disease." In short, the petition demanded a public hearing on climate change science to determine the facts of the matter, "to test the truth of the proponents of an endangerment finding in a public crucible." Charging that "no issue should be more important in deciding whether to make an endangerment finding than the question of whether higher global temperatures will lead to higher death rates in the United States," the chamber argued that a hearing would allow the EPA along with environmental and business groups to engage in a "credible weighing" of the scientific evidence that the emission of green-house gases endangers human health, to be adjudicated by "a neutral decision maker." Should the EPA refuse (as it has in fact done), their plan is to sue the EPA in Federal Court.

The stated purpose of the Chamber is to determine whether human activity is contributing significantly to global warming of a degree that puts the planet and its inhabitants at risk; it is widely understood, however, that, as formal representative of the American business community, its real purpose is to create sufficient controversy and uncertainty in the popular forum to stall or derail any attempts by the EPA (for example) to impose federal greenhouse gas emission standards on U.S. businesses. According to William Kovacs, the chamber's senior vice president for environment, technology and regulatory affairs, freely asserts that climate scientists don't have the science to support the endangerment finding: "We can't just take their word for it."[16] Indeed, he envisions the debate he proposes as "the Scopes monkey trial of the 21st century" – with witnesses, cross-examinations and a judge who would rule on whether humans are or are not warming the planet to harmful effect. "It would be evolution versus creationism"; it would mean putting "the science of climate change on trial."[17]

In fact, the analogy with the Scopes trial obscures what is most novel — and most critically significant – about this petition. Where the stated purpose of the Chamber's trial is to establish the truth of the claims put forth by climate scientists, the purpose of the Scopes trial lay in perfectly familiar jurisprudence territory. It was aimed not at establishing the truth or falsity of Darwin's argument, but rather, at establishing whether or not Scopes was guilty of violating the Butler Act, a Tennessee law enacted in 1925 forbidding public school teachers from denying the Biblical account of man's origin. In conjunction with that determination, the trial was also aimed at testing the constitutionality of the Act itself. In that respect, and contrary to the popular image, it was Scopes rather than Darwin who was the man on trial, and the legality of the Butler Act itself rather than the truth of evolution by natural selection that was the principle on trial. Originally set up (and financed) by the ACLU, the case was aimed at challenging the constitutional legitimacy of that Act on the grounds that it violated both the teacher's individual rights and his academic freedom. Neither Scopes' beliefs nor the solidity of the facts that could support them was meant to be at issue, but simply his right to convey these beliefs as a teacher in a public classroom.

Indeed, it is not easy to find *any* juridical precedent for what the Chamber now proposes. Even the trial of Galileo does not quite qualify, for the offense with which Galileo was charged was not faulty science but heresy — more nearly parallel to the Scopes trial than to what the Chamber of Commerce is demanding. Galileo's crime was his public advocacy of a doctrine that was "explicitly contrary to Holy Scripture", and hence, "formally heretical".[18] In both

[14] Informal communication, January 28, 2010.

[15] EPA Docket ID Number EPA-HQ-OAR-2009-0171.

[16] Burnham, NY Times interview, 8/25.

[17] Tankersley, Jim (2009), LA Times August 25, 2009, http://www.latimes.com/news/nationworld/nation/la-na-climate-trial25-2009aug25,0,901567.story. xxx.

[18] Sentence delivered 22 June 1633, available at http://web.archive.org/web/20040829092858/www.msu.edu/course/lbs/492/stillwell/galileo_trial_docs.html#sentence.

Galileo's and Scopes' trials, the issue to be addressed by the tribunal was whether or not the defendant had been guilty of publicly contravening the Scriptural account.

The novelty of the Chamber's petition is the proposal to delegate judgment of the truth-value of a scientific claim to a public tribunal or jury. One might say that it is not only climate science that is to be put on trial, but the more general (and until now, widely accepted) claim of the scientific community (more specifically, of scientists with the relevant expertise) to the authority for determining the facticity or truth-value of scientific assertions. If the implicit aim of Galileo's trial (and of the prosecution of Scopes) was to maintain the independent (and higher) authority of Scriptural truth, the assumption underlying the Chamber's petition is that the traditional authority of scientists over the status of scientific truth can and should defer to the authority of a public tribunal.

Such a trial will probably never occur, yet the mere fact of its being proposed is surely unsettling. It presupposes a public that might consider such a procedure legitimate and indeed find it congenial; and that, I submit, is where much of both its novelty and significance reside. Further, I suggest that such a presupposition both reflects and trades on a growing tendency in American politics to put truth to popular vote. That tendency goes far beyond questions about climate change, but it may be most conspicuously evident in relation to these issues. Increasingly, questions about whether the earth is warming, and if so, about whether human activity has played a significant role in this warming, have been transformed into matters of debate to be conducted in the public arena, with scientists on one side, pitted against critics, skeptics, and deniers (or "contrarians") on the other side. And if we are to judge by the results of recent polls, it begins to appear that the scientists are losing the public debate.[19]

The prime movers behind the challenge to claims of global warming, and more specifically, to claims of a human influence on the process, are not hard to identify. The U.S. Chamber of Commerce, described as "the nation's largest business lobby"[20], represents the interests of those whose motivation to resist regulation of emissions is most immediately apparent. The Heartland Institute is a libertarian/conservative ("free market") organization funded by foundations well known to be politically conservative (e.g., the Castle Rock Foundation, the Sarah Scaife Foundation, the John M. Olin Foundation, and the Lynde and Harry Bradley Foundation) as well as by ExxonMobil.[21] For this community, what is at stake is, first and foremost, the economic cost threatened by government regulation. But through their publications and blogs, they.

Have rallied support from a far larger population for whom no immediate economic investment is at issue. Many are recruited by their fear of government regulation of any kind; others, by suspicion of all claims to intellectual authority (perhaps especially, to scientific authority), or by appeal to the right of all persons to form and express their own opinions. But what is probably the largest audience affected by these posts and publications, and their greatest effect, is the vast reservoir of non-expert readers who are left mired in doubt, not knowing who or what to believe. However strongly we may argue for the importance of public participation in policy decisions, we cannot forget that participation — in any form – depends on belief. And not knowing who or what to believe, without any way of assessing the risks and costs of either action or inaction, the natural inclination of most people is to do nothing.

9. How should climate scientists respond?

What should climate scientists do? Convinced that they are the only ones with sufficient expertise to distinguish between information and misinformation, they are generally dismayed by the diffusion of so much blatant misinformation. But if their testimony is not credited, if readers do not have confidence in their expertise, what are they to do? Indeed, expertise is their ground zero. It is the reason for their insistence on resolving their own disagreements internally, and for recommending that only claims appearing in articles that have passed through the process of "peer review" be taken seriously. Peer review provides a formal gate-keeping mechanism by which papers have to pass muster with colleagues working in the same area before they can be published, thereby both ensuring a base line of reliability and grounding their claim to scientific authority rests. Critics might of course argue that peer-review is no better a guarantee of reliability than a public tribunal would be, and might even be less reliable precisely because the reviewers all belong to the same community, and hence are likely to share similar viewpoints. But this is to ignore the need for just that expertise that constitutes the foundation of peer review, as indeed of any claim to scientific authority.

The view of scientific authority as resting exclusively on internal evaluation inevitably invites the temptation to refrain from directly confronting the challenge posed by contrarians in the public arena, and the inclination to remain within one's protected domain. But doing so leaves unaddressed the question of what is to be done about the free dissemination of misinformation. Furthermore, such an understanding of scientific authority raises a host of philosophical problems. Here however I want to focus on a conspicuously pragmatic problem: while it is true that the great majority of papers published in peer-reviewed journals support the conclusions of the IPCC, these journals are not read by the audience of readers that climate scientists most need to reach; in particular, they do not reach readers who are not themselves engaged in research on climate change. These audiences may vary greatly (from, e.g., readers of such high-brow journals as the *New York Review of Books* (NYRB) to bloggers and readers of *The Skeptics Handbook*). Despite the great differences among these audiences, they have one important feature in common, and that is their insulation from the professional literature. Indeed, it is precisely this insulation that permits the kind and degree of skepticism we have observed.

I have already discussed audiences at one end of the spectrum, what about the other end? What about the population of sophisticated (often academic) non-expert readers? More specifically, consider the recent intervention in these debates by the

[19] For example, a study conducted by the Yale Project on Climate Change and the George Mason University Center for Climate Change Communication in January 2010 reports that "public concern about global warming has dropped sharply since the fall of 2008:

> The percentage of Americans who think global warming is happening has declined 14 points, to 57 percent.
> The percentage of Americans who think global warming is caused mostly by human activities has dropped 10 points, to 47 percent.
> Only 50 percent of Americans now say they are "somewhat" or "very worried" about global warming, a 13-point decrease.
> In line with these shifting beliefs, there has been an increase in the number of Americans who think global warming will never harm people or other species in the United States or elsewhere.
> The survey also found lower public trust in a variety of institutions and leaders, including scientists" (see http://environment.yale.edu/uploads/AmericansGlobalWarmingBeliefs2010.pdf).

[20] Tankersley (2009), op. cit.

[21] In a statement to the House Science Committee on March 28, 2007, Dr. James McCarthy, Alexander Agassiz Professor of Biological Oceanography at Harvard University, attested that ExxonMobil contributed a total of $560,000 to the Heartland Institute between 1998 and 2005. (Testimony of Dr. James McCarthy, p. 4, House Science Subcommittee on Investigations and Oversight, March 28, 2007).

distinguished physicist Freeman Dyson, a critic who, because of his impeccable reputation as a scientist (he has been described as the "best physicist never to receive a Nobel Prize"), warrants special attention. Over the last several years – in lectures, popular articles, and interviews; in a book entitled *A Many Colored Glass* (2007); and, most recently, in a review published in the *NYRB* on June 12, 2008 – Dyson has weighed in on the question of what to do about global warming. And to the great delight of contrarians, he has come down hard on what he sees as the unnecessary alarmism of so many climate scientists. For example, a 2007 article on the Edge website begins, "My first heresy says that all the fuss about global warming is grossly exaggerated. Here I am opposing the holy brotherhood of climate model experts and the crowd of deluded citizens who believe the numbers predicted by the computer models".[22] In Dyson's view, Al Gore and Jim Hansen are especially to blame. Hansen, he claims, "consistently exaggerates all the dangers." Dyson admits there may be a problem with CO2 in the atmosphere and that the climate may be warming, but, as he said in an interview with Salon. com that same year, "the fact that the climate is getting warmer doesn't scare me at all." Furthermore, ever a visionary, he has no doubt about our ability to invent technical fixes to solve any problem that might arise (one suggestion of his own is to reduce CO2 levels by increasing the amount of biomass in the soil, and this, he thinks, can be achieved by genetically engineering plants to put more biomass into roots (see Dyson, 2007).

In addition to his formidable scientific reputation, Dyson is also reputed to take great pleasure in challenging consensus wherever he finds it, and for proposing wildly imaginative solutions to world problems – solutions that often sound like science fiction. Furthermore, his primary scientific expertise is an area far a field from climate studies, and many experts on climate are frankly appalled at his ignorance and/or his distortion of the facts. They dismiss his claims from serious consideration by arguing, "None of his articles would ever pass peer-review", with the clear implication that the credibility and authority of his claims are accordingly undermined. One should only believe claims that have gone through peer-review. Of course, the flip side of this argument is that they too would lose credibility and authority if they themselves strayed beyond the warranty provided by that process.[23]

This response is unfortunately of little help to readers of the NYRB. By keeping to their own turf, climate scientists leave what is in effect a vacuum, and it is in this vacuum that Dyson finds his audience. Dyson has no need for the warranty of peer-review. Those readers who are persuaded by his arguments are so persuaded not because the arguments have been vetted by disciplinary review, but first, because of Dyson's reputation; second, because of the persuasiveness of his argument; and third, because of the absence of effective rebuttal. To be sure, the NYRB has also published articles on climate change that take a very different position, but the only one of these written by a climate scientist is an article by Hansen himself (2006). What is needed is rebuttal to Dyson's criticism of Hansen by other climate scientists.

Furthermore, as already hinted at, there are other problems as well. Recourse to peer review for final judgment of the technical contents of a paper is a familiar response, but can it in fact be epistemologically justified? Does the internal regulatory system of scientific disciplines suffice to exempt the members of that discipline (or set of disciplines) from *all* external criticism? Can no one from outside raise objections, register doubts, engage in productive criticism? The possibility of collective blind spots shared by a research community can surely not be excluded, and good historical arguments can be made for the value of outside perspectives in identifying such blind spots. As Sheila Jasanoff tells us,

> "What is missing … is a recognition that whoever controls the review process also to a great degree controls the meaning of the available scientific knowledge. Systemic bias—against innovation, for example, or unpopular scientific projects—is well known even in the context of relatively pure science, let alone in the high-stakes, high-controversy world of policy-relevant knowledge. … [C]ontrolling who counts as a peer amounts to controlling the interpretation of complex and ambiguous knowledge." (Messenger I)

But here is a tricky problem indeed, and it brings us back once again to the question of the place of expertise in a democratic society. Even admitting the need for expertise, how are we to define it? Just where is the line between inside and outside to be drawn? And how much expertise must a reader have in order to legitimately engage in critical review?

10. Science and politics redux

Harking back to the political theorist Carl Schmitt, Stephen Turner argues that expert claims are "political" to the extent that they "'affect, combat, refute, and negate' someone or some faction or grouping of persons." By this criterion, claims about the human contribution to climate change are inherently "political" because, as Turner puts it, they "favour the faction that believes in an extensive role of the state in regulating the economy." The word political is in quotes because of his insistence, again invoking Schmitt, that "what is political is a political question. Making something scientific, or true, doesn't mean making it non-political" (2007: 36–37). Fair enough. But we cannot stop here: one also needs to note the converse: Making something political does not mean making it either non-scientific or untrue. Indeed, it is in good part just because politics – in Turner's sense of what affects (advantages or disadvantages) different groups of people – depends so critically on scientific "facts" that the relations between science and politics are as fraught as they are. Again the quotation marks are necessary for the same reason as they were in the case of "political" – i.e., because calling something a "fact" is itself, in part, a political move. Thus, when Turner goes on to say that

> "such policies as global warming are partly based on facts, partly based on uncertain claims, on beliefs about human conduct and on other things, such as ideas about what is and is not natural about weather fluctuation, they are based on complex and uncertain inferences from data, or on guesses",

He is making a claim that is at least partly political. Ditto for his conclusion that climate science is therefore "epistemically different from what is ordinarily understood as science" (p. 39), as well as for his question, "Shouldn't experts just recuse themselves when they don't really have the facts to warrant a policy?" (29–30).

Yet at the same time, he is also making a claim that depends on the facts of the matter quite apart from political values (or apart from their effects on people), and in that sense, a scientific claim (although, insofar as his comments reveal a misunderstanding of the nature of scientific facticity, it might be more accurate to say he

[22] Dyson, 2007, "Heretical Thoughts About Science and Society", http://www.edge.org/documents/archive/edge219.html#dysonf.

[23] Indeed, some of Dyson's conclusions might not even fall under the purview of peer review – for example, it is not clear whether the conclusion that Hansen "exaggerates" the dangers refers to Hansen's estimates of the magnitudes of the dangers or to Hansen's judgment of how worried he thinks we should be by these estimates.

is making an unscientific claim). If what science deals with are not facts in the sense of being absolutely right or wrong, but rather the product of our best efforts at getting it right, and hence always subject to some degree of uncertainty, his parsing of "facts" from "uncertain claims" and "beliefs" not only will not hold, but also is something of a red herring. It's not that scientific reasoning does not depend on beliefs, ideas, guesses and uncertain inferences, but rather that the beliefs, ideas, guesses and inferences are informed; they are constrained by what is known of those facts of the matter that researchers are trying to get at. Moreover, within their own communities, and on the basis of arguments and evidence available to their colleagues, they are subject to ongoing critique and possible refutation. These formulations (beliefs), estimates, inferences and mutual criticism are essential parts of their efforts to 'get it right.' They are the rules of the game in science.

Finally, in the normal course of events, policy is warranted not only by demonstrable facts but also, and far more commonly, by estimates of what is likely to be true, by inferences from available data, and even by informed guesses. Military policy, e.g., cannot afford to wait for 'proof'; it must be formulated on the basis of the best information available. Were the experts on whom policy makers rely to recuse themselves whenever they didn't "really have the facts", policy making would be in a sorry state indeed. (Of course, our recent experience with the use of falsified evidence prior to the war in Iraq suggests that military policy might be a particularly poor example. Yet even here, where the data proved untrustworthy, it was nonetheless the case that policy was formulated on the basis of the best data Congress had before it.) And the same can also be said for the making of policy where we can have greater confidence in the data from which experts make their inferences – e.g., in economic, health, and welfare policy. Why then should the making of climate policy be different?

Like Turner, Roger Pielke is well aware of the impossibility of drawing a clear line between science and politics. As he writes, "Such a quaint, utopian view of science in decision-making has been thoroughly rejected by scholars who study science in politics (2006: 33)." Yet at the same time, he betrays a lingering inclination to do just that. Pielke does not doubt the evidence – he is persuaded of the importance of human activity in global warming – but he is critical of those who, like Rajendra Pachauri, Chairman of the IPCC since 2002, fail to limit themselves to purely scientific issues. Thus, e.g., he complains: "In his capacity as chairman, Pachauri has engaged in a range of political advocacy on climate policy, from calling for support of the Kyoto Protocol on climate change to endorsing a report prepared by a number of environmental advocacy groups calling for specific changes to energy policies." Scientists, Pielke claims, have politicized science, and by doing so have diminished "the constructive role that scientific expertise can play in the policy process" (p. 30). His recommendation? That scientists avoid taking sides and limit their role to that of "honest brokers of policy options," clearly communicating the significance of the scientific results for the various options. Choice of option is then left to policy makers.

I think Pielke may be splitting hairs. If, e.g., the scientific results imply that only one option – the global implementation of strong regulations – is viable, and the scientists attempt to make this clear, are they politicizing science or limiting themselves to the role of "honest broker"? Pachauri's position might be interpreted either way, but I would argue that, given the magnitude of the implications of the scientific claims for the public at large, there is in fact no way to separate these claims from their political implications, just as there is no separating the political arguments from claims about what is or is not the truth of the matter. In other words, where the results of scientific research have a direct impact on the society in which they live, it becomes effectively impossible for scientists to separate their scientific analysis from the likely consequences of that analysis. Perhaps we would better manage the tensions between expertise and democracy if we gave up the effort of disentangling science from politics altogether, and instead recognized our need of special expertise in science, just as we recognize our need of expertise – as Jasanoff puts it, of the "wisdom of strangers" – in all other areas of our lives. And conversely, also recognize that the ability of scientists to acquire their particular expertise depends on the willingness of the rest of us to support them.

11. Scientific authority, accountability and responsibiity

The particular authority that accrues to climate scientists is based on their skill and experience. But authority requires more than the fact of expertise: it also requires legitimation of that expertise, and at least in the United States, that legitimation is normally provided by peer-review. In other words, the authority of climate scientists rests on the approval of their own community – it is legitimated by and for their colleagues – and hence resides in that community. One crucial question that arises is thus how that authority is extended into the public domain. Another concerns the responsibilities that authority unavoidably brings with it – i.e., we need to ask: what particular responsibilities does the scientific authority attributed to climate scientists entail?

Discussions of scientific responsibility often begin with questions about a scientist's responsibility to other scientists, i.e., with issues of scientific integrity and accountability. But when the implications of their research so directly concerns the larger population as they do here, I want to argue that scientific responsibility extends well beyond these basic requirements: it needs also to include the obligations accruing, first, from the trust that the public has invested in them *qua* scientists, and second, as custodians of a public good (i.e., the science they produce). Scientists are of course also citizens, and hence share a general responsibility incurred by what is at stake for all citizens. But it is with their special responsibilities as scientists that I am concerned here, and these go far beyond any concerns they might have about professional authority. I submit that they include, in addition, the responsibilities that derive from their implicit contract with the state that funds them. That contract makes the products of their labor a public good. Because it is a good which, at least initially, is under their own control, they need to bear responsibility for that good, at least for as long as it remains under their control. In other words, they are obliged to ensure safe delivery of the good they have produced into public hands. I am arguing, in short, that their responsibility, *qua* scientists, extends well beyond the issues of professional integrity that first come to mind for most scientists in relation to questions about scientific responsibility – far enough beyond that understanding to encompass the obligation to share the results of their expertise with those likely to be affected by the implications of those results.

How is this obligation to be discharged? And how can it be discharged without undermining the scientist's hard won claim to disinterested objectivity? Many scientists worry that any venturing out into the public domain, beyond the authorization of peer-review, might in itself jeopardize their credibility and authority. To what extent is this a legitimate concern? And to the extent that it is, how might it be addressed? To answer these questions we have to return to the issue of on what a scientist's authority is based. I started out by accepting the prevailing view that it comes from the validation of a disciplinary community. But there is no obvious reason why such validation should automatically endow experts with authority in the world at large – i.e., with the public authority that seems now to be so woefully lacking, at least in the US. I want therefore to suggest that, seen from outside that community, the legitimation of their authority requires rather more. Unless I am willing to accept someone's authority on the basis of third party say-so, I need direct convincing. In the absence of technical expertise of my own, I need to be persuaded both of that person's honesty (or credibility) and of the cogency of his or her argument. Sheila Jasanoff describes the model of expert deliberation that is practiced in Germany as satisfying this need. There, she writes, "It is the expert body's rationale that must be, in German terms, *nachvollziehbar*, or capable of being followed by others. … Reasons are compelling because all members of the public, wherever situated, can follow the expert body's reasoning, even if they do not accept its conclusions" (Messenger 1, p. 24).

Within the scientist's own community, validation and respect is won on the basis of sound arguments and authenticated evidence. Why cannot these same considerations also legitimate authority outside their own community? Is technical expertise really necessary for educated readers to distinguish between good arguments and bad? Between carefully collected and well scrutinized evidence on the one hand and conspicuously flimsy evidence on the other? To improve public understanding, the standard response is increased science literacy. But I would argue that more to the point would be increased articulacy on the part of scientists — i.e., an increased effort on the part of scientists to make their arguments, their doubts, and the reasons for their concern intelligible even in the absence of technical expertise. Doing so is almost never as difficult as is generally assumed: disagreements and uncertainties are already familiar territory to most readers, and examples in which technical arguments are made accessible are similarly not hard to find. But effective communication also requires an ear for what readers need, and one thing they need is to be able to follow the process by which so many climate scientists have reached the conclusions they have, despite the limitations of their knowledge and the uncertainties of their projections. To be sure, the lay reader can generally not make the fine distinctions that a professionally trained reader can, but even so, he or she is quite capable of making the sorts of discrimination needed to establish trustworthiness.

In other words, I am arguing that, wherever the results of their work have direct impact on the public at large, scientists have a responsibility to engage with that public, providing a candid, widely accessible, and meaningful account of both their own findings and the implications of those findings, and responding to whatever concerns persist. Furthermore, I see no reason why this responsibility need in any way conflict with scientists' responsibility to their peers, nor do I see any reason why they cannot draw on the authority they have established among their colleagues in establishing authority with a wider audience. If, e.g., Freeman Dyson complains to the reader of the NYRB that climate scientists tend to exaggerate the dangers, I am saying that these scientists not only can but are obliged to respond, explaining to that same reader just why Dyson is wrong. Similarly, I submit that it the responsibility of climate scientists not only to publicly protest the at least tacit misrepresentations bolstering the recent attacks on the integrity of the IPCC in general, and of Pachouri in particular, but also to explain to readers just why the accounts of "Climategate", "Pachourigate", and "Glaciergate" that have been put forth in both the American and British press are so dangerously misleading.[24]

What I am proposing is far from a solution, and by itself, is almost surely not sufficient. But it is a start. In particular it is a first step in responding to the impasse of credibility with which I began. I argue that a way out of this impasse must be found for the simple reason that non-expert readers cannot do without the assessments of experts. And I am urging climate scientists to take on responsibility for bridging the ever-widening gap between expert and lay reader partly because they are the only ones in a position to take it on – there is no one else– but also, and more importantly, because their responsibility *qua* scientists obliges them to do so. But they cannot do it alone.

12. A final comment: the "heuristic of fear"

Finally, there is one more issue that needs to be addressed, and that is the problem of fear. Our recent experiences with the uses of fear to manipulate public opinion are all too vivid to forget the political uses and abuses to which the generation of fear can be put. Furthermore, fear is not only an unpleasant emotion, but as a motivating force, also notably ambiguous. While it can spur people to action, it can also lead to denial and avoidance. Fear is, after all, frightening. As FDR so famously put it, "the only thing we have to fear is fear itself."

Climate scientists – even those who are themselves alarmed – may be especially wary of evoking fear in their readers. Their aim is to inform, to evoke in the reader what the rational response to what they, as scientists, have learned. Fear, by contrast, is an emotion, and emotions are generally considered counter-productive to the forming of rational responses. Also, given the political abuses to which fear has so recently and so notoriously been put, and the sensitivity of audiences to such abuses, the "fear of fear" has now itself become a political weapon, and it is one that skeptics have not hesitated to deploy. Thus climate scientists (like Hansen) who do not hesitate to elaborate on scenarios that cannot but be frightening are called "alarmists", "fear-mongerers", and accused of creating a "climate of fear", of spreading "climate porn" and "narratives of fear". And no one wants to appear guilty of such charges. Climate scientists especially. So how cautious is it appropriate for them to be in order to avoid alarming people?

Let me first address the issue of emotions and their place in political action. So far my discussion has been confined to the problem of credibility and belief. But there is another problem as well. Belief is only a precursor to action, and certainly not in itself sufficient. Indeed, the gap between belief and action is huge, and a subject of much commentary. For example, analysts of public perceptions of climate change have expressed considerable concern in recent years about a growing gap between intellectual awareness of the problems and a willingness to enact effective precautions. Many students of this phenomenon attribute the gap to a lack of emotional engagement with either the urgency or magnitude of the threat. In other words, if one wants to motivate people to act, it seems to be necessary to engage them on an emotional level. Fear may be the most relevant emotion, but its effect is double-edged: it can spur people to action and it can invite denial. Those in social marketing know this better than most, and they have carefully studied the effectiveness of fear as a motivating force. But climate scientists are not social marketers, and there are powerful constraints inhibiting them from directly seeking such engagement. Theirs' is the domain of the rational, not of the emotional.

I want to suggest however that we may be making a category mistake. In times of danger, fear is not only an appropriate response but also a rational response, and I want to argue that, as an appropriate response, it has a legitimate and indeed important role to play in rational decision making. There is by now a large body of evidence demonstrating the role that different kinds of affect plays in real-time decision making, but less on the positive value such a role can have. The work of the eminent neurologist Antonio Damasio stands out as a notable exception. Together with his colleague Antoine Bechara, he claims that "Emerging neuroscience evidence suggests that sound and rational decision making, in fact, depends on prior accurate emotional processing" (2005: 336). A finding

[24] See footnote #5.

from research in social psychology that may also be of interest concerns the specific effects of fear and anger on risk perception: It seems that fear leads people to overestimate, while anger leads people to underestimate, the degree of risk (Lerner and Keltner, 2001). Why is this interesting? Well, what would an appropriate estimate of the risk of events with low probability and large impact be? There have recently appeared a number of arguments (see, e.g., Taleb & Nicholas, 2007) claiming that conventional decision theory has systematically undervalued the effects of hard-to-predict but high impact events (popularly referred to as "black swans"). In which case, for extreme events with large negative effects, fear might perhaps be said to have the effect of counterbalancing the bias that seems to pervade conventional theorizing about risk. But there is a more basic and more general point to these critiques as well – namely, that what is needed is not to assign new probabilities to inherently unpredictable events (such as the "tipping point" of runaway climate change), but rather to prepare for, or attempt to avoid, such events in whatever ways are possible. Unfortunately– and one of the difficulties with this proposal – such efforts, if successful, will by definition appear in retrospect as having been unnecessary. That is to say, they will be particularly vulnerable to the cognitive bias that comes with hindsight: they will seem not to have been needed just because their effect was to avoid the feared catastrophe.[25]

The other big problem with proposals of this sort is their vagueness. Just how much are we willing to pay in our efforts to prepare for, or attempt to avoid, such events? How are we to weigh the risks if we can't calculate the probabilities?

The short answer is that we can't. Despite strenuous efforts on the part of risk analysis, there is still no generally accepted way of estimating the risk of rare events with high impact. One response to this difficulty is simply to ignore such risks in our calculations, but it is not difficult to perceive the inadequacy of such a response. Another is to focus our efforts on avoidance. The reflections of the continental philosopher Hans Jonas (1984 [1979]) are of such particular relevance to this problem that I will close with a brief discussion of his thoughts on the subject.

Writing over 30 years ago, Jonas was even then already worried about the future of the environment, and he sought to articulate an "ethics for the future" – especially, "an ethics of responsibility for distant contingencies" (p. 26) where "that which is to be feared has never yet happened and has perhaps no analogies in past or present experience" (p. 27). Indeed, he argued that it is precisely when the scientist's knowledge is insufficient for predicting the future that an "ethically required extrapolation" must take over (29). For Jonas, "the mere knowledge of *possibilities*" suffices for such extrapolation, i.e., for the identification of appropriate ethical principles. As he writes, "it is the content, not the certainty, of the 'then' thus offered to the imagination as possible, which can bring to light … principles of morality heretofore unknown" (29).

Central to Jonas' method is what he calls "the heuristics of fear."[26] To Jonas, we learn what it is that we value, what we are committed to preserving, only when that something is under threat. To learn what we cherish, moral philosophy must accordingly "consult our fears prior to our wishes" (27). But for distant and future threats, appropriate fear may not be in evidence. Even though fear is a natural response to present danger, future threats require an effort of reason and imagination to evoke the appropriate fear – i.e., the fear required to guide our responses. Dangers that are imagined and distant arouse fear even less naturally than do imminent dangers. Accordingly, Jonas writes,

> We must educate our soul to a willingness to *let* itself be affected by the mere thought of possible fortunes and calamities to future generations …. Bringing ourselves to this emotional readiness, developing an attitude open to the stirrings of fear in the face of merely conjectural and distant forecasts concerning man's destiny [requires] a new kind of *éducation sentimentale* … (28)

Thirty years ago, the forecasts to which Jonas referred may have been "merely conjectural", but they are no longer so today. Accordingly, the tasks he assigned to us – what he referred to as our "duty" – may be less formidable, even while being more urgent.

[25] Taleb gives the example of the recommendation that might have been made prior to 9/11, and might even have been taken seriously – namely, to impose locks on cockpit doors. That person, Taleb writes, "gets no statues in public squares, not so much as a quick mention of his contribution in his obituary. 'Joe Smith, who helped avoid the disaster of 9/11, died of complications of liver disease.' Seeing how superfluous his measure was, and how it squandered resources, the public, with great help from airline pilots, might well boot him out of office. *Vox clamantis in deserto*. He will retire depressed, with a great sense of failure." (2007, Chap. 1, http://www.nytimes.com/2007/04/22/books/chapters/0422-1st-tale.html?pagewanted=3&_r=1&ei=5070&en=bdae1078f2b4a98c&ex=1178769600).

[26] Jonas' "heuristic of fear" is sometimes conflated with the precautionary principle, adopted by many European countries. But as I read Jonas' work, it is more in the nature of a requisite for formulating any such principle.

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