The Rightful Place of Science:

Science, Values, and Democracy

The 2016 Descartes Lectures

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The 2016 Descartes Lectures

Consortium for Science, Policy & Outcomes
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Commentary on Lecture 2

VALUES AND ACCOUNTABILITY IN SCIENCE ADVICE: THE CASE OF THE IPCC

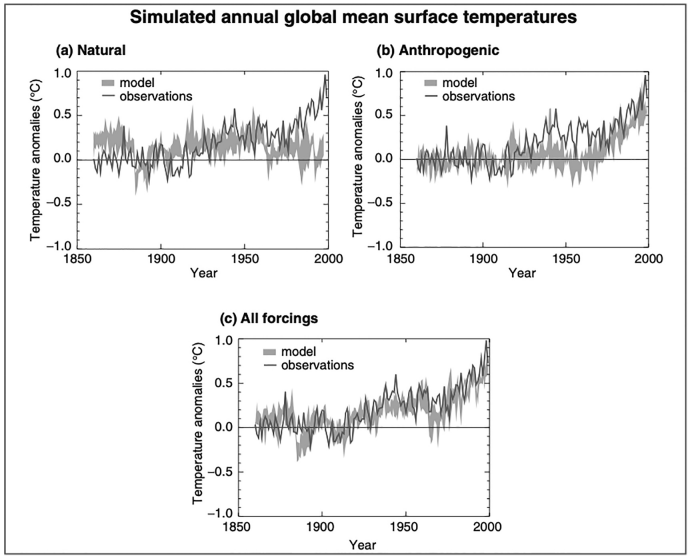
Arthur C. Petersen

In this commentary essay, I would like to delve more deeply into an important case that illustrates very well the concerns raised by Heather Douglas in her lecture on the accountability of expertise. The case I am referring to is an example of the way the Intergovernmental Panel on Climate Change (IPCC) deals with scientific and political values and accountability.[1] I will connect to several of the points that Heather raised in her lecture, especially the question: How assessable is expertise?

I will focus on the example of expertise on the causes of climate change ("attribution"). I will make the argument that if you want to assess expertise, you will have to engage with an "extended" peer community.[2] Reflection on assumptions should lead experts to give an account of the epistemic underpinnings of their expertise. I will argue that IPCC reports do not do that enough.[3] In pushing scientists to give such accounts, one must realize that experts often do not like to hear that; in this sense, their expertise should

be considered to be on tap, but they are not on top in terms of being free to decide how transparent they will be.

Figure 1. Climate-Change Attribution Figure in the Summary for Policymakers of the Third Assessment Report of the IPCC[4]



In the 2001 report of the IPCC, a figure was included that has become iconic at the science-policy interface for attributing climate change to human influences (reproduced here as Figure 1). The figure contains three panels, each showing, on the one hand, the same line with measurements of the global mean surface temperature since 1850 (going up in the beginning of the twentieth century and going up at the end of the twentieth century) and, on the other hand, a different band of model results (the bands representing the "internal" variability of the climate system, that is, the sensitivity to initial conditions): one for only natural external influences on the climate (volcanoes, sun), one for only human external influences on the climate (greenhouse

gases, particles), and one that combines natural and human factors. The latter panel depicts a beautiful match of measurement and model, giving rise to the suggestion that we know everything, that there is no room left for any doubt that humans are causing the recent climate change. In fact, the chair of the IPCC suggested exactly this at a press conference in 2001.[5]

Of course, philosophers of science understand that the number of degrees of freedom in climate models is high. And they will not be surprised to hear that, indeed, virtually all climate-modeling groups in the world are able to present the same final panel with a match. This is not to say that the results are wrong. But how should one communicate that the bands are "just" model results, whose match with the measurements cannot establish reliability? The pertinent questions are: How do we know how reliable the models are? And in which senses can we say that they are reliable?

The IPCC has developed a methodology, through three subsequent guidances, for assessing and communicating the uncertainties in the findings of its assessments. This methodology includes calibrated terms for communicating probabilities. For the example of climate-change attribution to human influences, the IPCC did not communicate in 2001 that it was 100% certain that humans are causing climate change, even though the picture is beautiful and the line and band match. It said, rather, that it was "likely" that most of the warming of the last 50 years has been caused by human greenhouse gases. According to the experts, "likely" here means a 66% chance that the finding is true.

I was sitting at the table at the time (in Shanghai, on 20 January 2001) as an IPCC contact group negotiated what I think became one of the most important statements ever from the IPCC, that most of the warming is likely due to human influences.[6] But I could not understand why they said "likely." If you believed the models, the likelihood was

already estimated to be way higher than 90% (that is, "very likely," the next likelihood category). I had to dig deep (through interviews, reviewing internal emails, etc.) to determine how the lead authors had reached their judgment. The reason they did not choose "very likely" was that they did not trust the models enough. So they picked the next lower likelihood category. Nowhere could this reasoning be found in the IPCC report; there was no traceable account of how they had arrived at this crucial judgment.

Six years later, the IPCC panel assessed the same question. The 2007 report features a similar figure as the 2001 report, but now the graphs are shown for every continent and the authors are willing to say "very likely" (90%). And again I could ask the question: Why not the next likelihood category of 99% or "virtually certain"?[7] The narrative could have been, "Even though we still do not fully trust the models, there have been more warm years, there have been more model runs, there have been different types of model experiments, and there is a belief that the models have become more reliable." I do think that the latter belief is problematic. Again the IPCC featured, in my view, a weak practice of assessing the reliability and the quality of models.

So what I argue has been missing from the Third and Fourth Assessment Reports of the IPCC (2001 and 2007, respectively) is sufficient attention to "methodological reliability" rather than simply "statistical reliability."[8] Assessment of methodological reliability requires a qualitative discussion and a corresponding qualitative assessment of the underpinning of results. Additionally, after "Climategate," the realization has come that "public reliability" needs attention too; how to gain back trust and be publicly relied upon is a difficult question for climate scientists. I do not have simple answers here. In this essay I am really focused on the importance of the second type of reliability: methodological reliability.

Let me give one example from the negotiations on representing methodological reliability in the *Summary for Policymakers* that occurred in Paris in 2007. This is the sentence that was under negotiation:

> *Most of the observed increase in globally averaged temperatures since the mid-20th century is very likely due to the observed increase in anthropogenic greenhouse gas concentrations.*

We have to get a bit into the politics now. Because these IPCC sentences are transferred from the sphere of knowledge assessment to the sphere of political negotiations (in the climate framework convention), there is always a country that does not want a stronger statement than the last time. A stronger statement would highlight that there is more scientific certainty, which would increase the likelihood of international agreements to curb climate change. The IPCC meeting in Paris in January 2007 was less than two years from what turned out to be the failure of the Copenhagen Summit at the end of 2009. In this instance, a country used all kinds of ways to prevent this sentence from being included. There is, however, an order of speech within the IPCC, which is: the chapters have been written—hands off, governments cannot touch those chapters!—but government delegates can comment, making use of a set of criteria (such as clarity and representativeness), on sentences in the Summary for Policymakers. Governments obviously will have different views. And the authors have a veto right on any change that is made to their summaries. One can imagine how hard it sometimes becomes to negotiate the summary line by line, as is the case in the IPCC. But it works.

Still, I argue that it can be done in a more productive way if both parties, authors and governments, would behave more diplomatically toward each other, understand better where they are both coming from, and what their re-

spective rationalities are. One group of actors in these meetings is there on authority of their social, ethical, political, and economic values (their role is to represent their publics), and another group of actors is there on authority of their scientific values (their role is to represent, to the best of their ability, the papers they have assessed), and must provide good "reference."[9]

Back now to the sentence that was under discussion in the final hours of the Paris meeting. After days of negotiations and having entered very deep into the night, finally we are in agreement—all the countries of the world can agree on the sentence by inserting the following footnote: "Consideration of remaining uncertainty is based on current methodologies." Of course we were all tired. But it is interesting: Why would the opposing country agree with this sentence? What is the spin they could give? They might say, "The methodologies used are based on models. It is just models. It is not reality." Indeed models are used, but that does not imply that there is no reference to reality; still, that is typical of the argument they would make. How would another country that tends to dramatize climate change and typically wants to downplay uncertainty spin this sentence? They might say, "Next time the likelihood will go up further; from the original "likely" (66%) it went up to "very likely" (90%), and it will go up again." And yes, indeed, in Stockholm, nearly seven years later in September 2013, it became "extremely likely" (95%).

One issue with the IPCC methodology of likelihood statements has already been addressed: The methodological unreliability of models has been used to "downgrade" likelihood statements without saying so. Another issue, which is related to insufficient transparency of expert judgment in the IPCC, is that there is hardly any reflection on the nature of expert judgment. "Very likely" means more than 90% chance that a particular statement is true. But what does that really mean? What do these probabilities

mean? How reflexive is the IPCC about what is actually happening, and what is behind these statements? The "90%" only means that the few authors who have been selected to do the assessment in a particular chapter have somehow reached this collective expert judgment. Nothing more and nothing less. It carries a lot of weight, because these authors have had the scientific training, acquired the relevant skills, and have a lot of experience in their scientific practices—they bring all these things to the table. These lead authors are the experts. We choose them for that expertise. Then, other experts are asked to thoroughly review their statements. The lead authors, however, in the end, when they write down their conclusions, get rid of any reference to "expert judgment." Suddenly their conclusions are made to flow directly from the underlying science. "It is not us." I find it incredible!

Twice we have had to intervene as the Dutch government delegation asked to make the Summary for Policymakers more explicit about expert judgment. In Paris in 2007, for example, the authors, when defining their uncertain terminology, referred in the final draft to the "assessed likelihood of an outcome or a result." We added "using expert judgment" to that phrase. In Stockholm in 2013, the same problem arose with the definition of "probabilities": "Probabilistic estimates of quantified measures of uncertainty in a finding are based on statistical analysis of observations or model results, or expert judgment." We looked at it and saw that it was going in the wrong direction. We thus changed "or expert judgment" into "and expert judgment." I think this is important. It is worrisome that scientists who act as science advisers are often not able to reflexively say what they are doing.

Questions on how expert judgment can be reflected in the IPCC are intertwined with questions on how science and politics relate in the IPCC. I would like to frame IPCC assessments as social constructs with elements from both

science and politics. Thus both types of values are in play: values both intrinsic and extrinsic to science. How successful is the IPCC? Well, critics would say they are too successful in terms of connecting with policy, and unsuccessful in connecting with science. That issue is what I studied for the Third Assessment Report (published in 2001), to address criticism in the US Senate testimony by Dick Lindzen that the IPCC would not be open enough to skeptics.

In addition to too little reflexivity in the IPCC, I also found that the criticism of lack of openness to skeptics was incorrect. For the report that I studied (I took the chapter on attribution of climate change to human influences), I looked at all the comments there were submitted for that chapter in all the review rounds. I looked at all of the responses to those comments, and all of the review-editor comments to the responses, and discovered that there were a lot of critical comments, many of which had led to improvements in the text in terms of more inclusion of uncertainties and better language.[10] So, I do think that skeptics (taken in a broad sense, i.e., including not only the "typical" climate skeptics but also people who for good reasons are critical of climate modeling) play a constructive role in the IPCC process. The final outcome is a policy-relevant assessment. It is not, however, the scientific consensus with full certainty, and thus it should not be framed in this way. Of course, the IPCC can still further improve its communication of uncertainty, be more transparent, and explain where the expert judgments come from, to connect with what Heather also emphasizes in her lecture. And I think the IPCC could be more reflexive about what is actually happening in these plenaries. They are all closed. Why? Include a webcast, for instance. There is no reason not to do that.

I conclude with four lessons that I took from my 14 years of being a science adviser:[11]

1. **Explicit reflection on uncertainty and values.** Take "normal science" seriously, but also organize reflection on its uncertainties and value-ladenness.

I have bought into the discourse of post-normal science, while I do agree with Heather that there never was a period where there was no post-normal science. So "post-" should perhaps read "extra-": "extra-normal science." With "normal science," I really mean those proceedings where it is the scientific community that is doing whatever they are doing: modeling, publishing, peer reviewing, etc. So when I say that we need to open up look at ways to bring out the different epistemic and nonepistemic values in this discussion, I mean that we need to organize reflection on uncertainty and value-ladenness within normal science as well, without throwing it away. So don't throw away the baby (post-normal science) with the bathwater (a form of scientism that does not sufficiently reflect the presence of uncertainty and ignorance in science)! Hence, I do not buy into very simplistic readings of post-normal science.

2. **Addressing methodological and public reliability.** Alongside the *statistical reliability* of results (expressed in terms of probability), devote due attention to their *methodological reliability* (expressed in terms of strengths and weaknesses) and their *public reliability* (expressed as the degree of public confidence in the scientists who produce them).

As I have already belabored in this essay, do not focus only on statistics; also focus on qualitative dimensions of reliability.

3. **Extended peer review.** Involve a larger group of specialists and nonspecialists who hold different values in monitoring the quality of scientific assessments.

"Extended peer review," which also comes out of this literature of post-normal science, concerns the ways in

which one can engage a wide group of people who can provide comment and are sensible enough so they can be processed and responded to, for instance, in the IPCC. Everybody—on the basis of a very minimal claim to expertise—can sign up to be an expert reviewer of the IPCC and can submit comments. It is very important that not only is a very small group of climate modelers, for instance, providing comments on the climate modeling chapter, but so too are neighboring disciplines and people who work for Greenpeace, for example. They all have a stake, as well as very valuable contributions to bring, because they can highlight particular risks to the climate that may not have become mainstream yet in the scientific community.

4. **Acknowledging social complexity.** Be wary of accepting the conclusions of actors and practitioners at face value; try to delve deeper through the layers of complexity by means of narrative methods.

The final point—looking at deeper dimensions and different things that are happening at the same time—is related to the notion of "social complexity." Scientists often have a self-image (overly rationalized) of what they are doing and the country delegates have a self-image (again overly rationalized) of what they are doing, and these self-images are too simplistic in what they hold, because they do not reflect the complexity of the way different types of values (epistemic and nonepistemic) are interwoven in practices. In terms of how to understand this, it is important to delve deeper. The big question still remains: Are there improvements that we can suggest to this mess? It is a mess, but already a good and interesting mess.

Notes

[1] I have been a Dutch government delegate to the IPCC from 2001 to 2014.

[2] See, e.g, Arthur C. Petersen, Albert Cath, Maria Hage, Eva Kunseler, and Jeroen P. van der Sluijs, "Post-Normal Science in Practice at the Netherlands Environmental Assessment Agency," *Science, Technology, & Human Values* 36, no. 3(2011): 362–388.

[3] Cf. L. A. Meyer and A. C. Petersen, eds., *Assessing an IPCC Assessment: An Analysis of Statements on Projected Regional Impacts in the 2007 Report*, PBL Report 500216002 (The Hague and Bilthoven, Netherlands: PBL Netherlands Environmental Assessment Agency, 2010).

[4] IPCC, *Climate Change 2001: The Scientific Basis. Contribution of Working Group I to the Third Assessment Report of the Intergovernmental Panel on Climate Change* (Cambridge, UK: Cambridge University Press, 2001), 14.

[5] See Arthur C. Petersen, *Simulating Nature: A Philosophical Study of Computer-Model Uncertainties and Their Role in Climate Science and Policy Advice*, 2nd ed. (Boca Raton, FL: CRC Press, 2012), 145.

[6] For a transcript of what transpired at that contact group meeting, see Arthur C. Petersen, *Simulating Nature: A Philosophical Study of Computer-Model Uncertainties and Their Role in Climate Science and Policy Advice*, 2nd ed. (Boca Raton, FL: CRC Press, 2012), 191–197.

[7] Ninety-five percent or "extremely likely" was only added to the methodology in the most recent assessment round.

[8] Arthur C. Petersen, *Simulating Nature: A Philosophical Study of Computer-Model Uncertainties and Their Role in Climate Science and Policy Advice*, 2nd ed. (Boca Raton, FL: CRC Press, 2012); Leonard A. Smith and Arthur C. Petersen, "Variations on Reliability: Connecting Climate Predictions to Climate Policy," in *Error and Uncertainty in Scientific Practice*, Marcel Boumans, Giora Hon, and Arthur C. Petersen, eds. (London, UK: Pickering & Chatto, 2014), 137.

[9] Matthijs Kouw and Arthur Petersen, "Diplomacy in Action: Latourian Politics and the Intergovernmental Panel on Climate Change," *Science & Technology Studies* 31 (2018): 52–68.

[10] Arthur C. Petersen, *Simulating Nature: A Philosophical Study of Computer-Model Uncertainties and Their Role in Climate Science and Policy Advice*, 2nd ed. (Boca Raton, FL: CRC Press, 2012).

[11] Arthur C. Petersen, "The Ethos of Scientific Advice: A Pragmatist Approach to Uncertainty and Ignorance in Science and Public Policy," in *Building Bridges: Connecting Science, Technology and Philosophy – Essays presented to Hans Radder*, Henk de Regt and Chunglin Kwa, eds. (Amsterdam, Netherlands: VU University Press, 2014), 53.