



Characterizing uncertainty in expert assessments: ozone depletion and the West Antarctic ice sheet

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Large-scale assessments have become an important vehicle for organizing, interpreting, and presenting scientific information relevant to environmental policy. At the same time, identifying and evaluating scientific uncertainty with respect to the very questions these assessments were designed to address has become more difficult, as ever more complex problems involving greater portions of the Earth system and longer timescales have emerged at the science–policy interface. In this article, we explore expert judgments about uncertainty in two recent cases: the assessment of stratospheric ozone depletion, and the assessment of the response of the West Antarctic ice sheet (WAIS) to global warming. These assessments were fairly adept at characterizing one type of uncertainty in models (parameter uncertainty), but faced much greater difficulty in dealing with structural model uncertainty, sometimes entirely avoiding grappling with it. In the absence of viable models, innovative approaches were developed in the ozone case for consolidating information about highly uncertain future outcomes, whereas little such progress has been made thus far in the case of WAIS. Both cases illustrate the problem of expert disagreement, suggesting that future assessments need to develop improved approaches to representing internal conflicts of judgment, in order to produce a more complete evaluation of uncertainty. © 2011 John Wiley & Sons, Ltd. *WIREs Clim Change* 2011 2 728–743 DOI: 10.1002/wcc.135

INTRODUCTION

In the second half of the 20th century, a new phenomenon emerged in science: large-scale, organized and formalized assessments of the state of scientific knowledge for the purposes of public policy. Scientists have long relied on particular mechanisms for summarizing the state of their art for each other—textbooks, review papers, annual and

ad hoc professional meetings—and various forms of assessment for external audiences have existed for some time. In the 19th century, commissions were established to answer specific policy-relevant questions, such as the Royal Commission on Vaccination.^{1,2} In the 20th and 21st centuries, various national and royal academies have produced thousands of reports on myriad subjects; the US National Academy of Sciences alone produces over 200 studies each year.³ Still, the more recent assessments have a distinct character, intended not merely to address a specific matter of fact or policy, but to summarize and make judgments about the state of a broad body of policy-relevant knowledge. Assessments of science for policy have become particularly prominent in the earth and environmental sciences, where formal assessments have played a major role in political and social debates over acid rain, ozone depletion, and global warming.⁴

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High-impact, politically significant issues like global warming and ozone depletion have become subjects of scrutiny in part because the science surrounding them was, in important ways, uncertain or incomplete. Yet because the potential ramifications of ozone depletion and global warming are so great, and involve lags between causes and consequences, policies have been planned or implemented under a great deal of scientific uncertainty. A number of scholars have studied assessment processes from policy and social perspectives,^{4–15} and there is a copious literature on risk assessment, perception, and communication.^{16–26} This literature is primarily concerned with how uncertainty is communicated to the public and policy makers. However, a major, prior, issue—largely unaddressed in the literature—is how these assessments identify and evaluate scientific uncertainty in specific cases.

By uncertainty, we mean ‘imprecision in the characterization of any outcome’ (Ref 27, p. 158). Scientists (and other experts) are well aware that their characterizations of potential outcomes may be imprecise, and have grappled with the uncertainty question, recommending a variety of techniques for uncertainty estimation and articulation.^{23,28–32} For the class of cases where observations or physical understanding are particularly lacking, there is disagreement about the wisdom of making such judgments at all.^{33–36} Recently, some scientists have also begun to consider how well earlier assessments have predicted actual environmental change.^{37–39} Studies about how uncertainty is identified and evaluated by scientists within the framework of formal assessments are in an emergent stage.^{13,40}

This paper undertakes such a study. First, we provide a brief overview of categories of uncertainty. Then, we analyze two cases to illuminate some approaches to uncertainty in scientific assessments relating to global change. Our cases are: (1) stratospheric ozone depletion as discussed in international ozone assessments and (2) the response of the West Antarctic ice sheet (WAIS) to warming, as discussed by the Intergovernmental Panel on Climate Change. The ozone case, on the one hand, is an example in which the science has been more or less agreed upon, even to the point of being considered finished or complete from a policy perspective,^{41–43} and has had a strong influence on environmental policy. The WAIS, on the other hand, is an example of highly uncertain science with which researchers and assessment authors are currently grappling.

We view a case study approach as necessary to ground the hypotheses we propose, and situate the questions we raise, in real examples. The juxtaposition

of these two case studies in particular, one where the science and policy have reached relative closure and one where accord has stalled far short of closure, allowing real-time analysis as the issues unfold, provides a sociohistorical perspective on the development of uncertainty regimes in environmental assessments. Following the argument articulated by Morgan et al.⁴⁴—that judging and communicating scientific uncertainty may be described as distinct categories (which are, however, not always distinct in practice)—we focus on the identification, evaluation, and characterization of scientific uncertainty within the assessment process. In this paper, we do not address the rationale, means, and effectiveness of communication of uncertainty to the public and policy makers.^{29,31,45} We encourage other investigators to extend our analysis to other case studies beyond the ozone and WAIS examples that serve as reasonable starting points here.

CHARACTERIZING UNCERTAINTY

Characterizing uncertainty serves various aims. It may serve to (1) articulate what you do not know; (2) indicate the imprecision of what you believe you do know, and/or (3) quantify how much not knowing something (or only knowing it within a certain range of precision) matters to a given audience or for a given purpose. Webster^{46,47} identified four types of uncertainty, as analytical categories: *empirical* (gaps in data), *methodological* (different models with different approaches), *institutional* (arising from the social dynamics of consensus and/or expert judgments), and *philosophical* (arising from contrasting views of probability, risk, and the future itself). In identifying types of uncertainty, Patt¹³ distinguished between *model* uncertainty (e.g., arising from distinct model representations) and *conflict* uncertainty (arising from disagreement among experts in their judgment of the matters at hand, such as how to weigh differences in models; Table 1). In their 2011 uncertainty guidelines for the upcoming Fifth Assessment Report, the IPCC describes the division of model and conflict uncertainty as quantifiable uncertainty and confidence in their assessments, respectively.⁴⁸

Model uncertainty is part and parcel to the proliferation of simulations of climate aimed at prediction, and is generally depicted by reporting a range of possible outcomes (Ref 13, p. 38). This may be done by running multiple simulations (or ‘realizations’) of a single model while varying initial or boundary conditions, or by presenting outcomes for an ensemble of models using a single set of initial conditions. Recent studies indicate that the

TABLE 1 | Types of Uncertainty

Type of Uncertainty	Definition	Subcategories
Model Uncertainty (Patt, ¹³ Funtowicz and Ravetz, ⁴⁹ and Draper ⁵⁰)	Uncertainty generated through the production and running of models, due to their inherent nature as approximations.	1. Structural: generated by the code used to build the model; includes 'known unknowns' and 'unknown unknowns'. 2. Parameter: generated by the inputs the modelers select for their model runs.
Conflict Uncertainty (Patt ¹³)	Uncertainty generated by disagreements by assessors over how to interpret the information at hand.	
Judgment Uncertainty (O'Reilly et al.)	Even in the absence of explicit disagreement, assessors generate uncertainty simply through the act of making judgments about the research they are assessing. An extension of conflict uncertainty.	

performance of ensembles of models exploring the effect of the same range of conditions (e.g., greenhouse gas emissions scenarios) on physical outcomes may improve with an increase in ensemble size, as a smaller ensemble size may not be able to fully describe the full range of uncertainty—as judged by the match between model outcome and observational data—and that Bayesian approaches applied to ensembles may yield additional improvement.^{52,53} On the other hand, if ensembles explore a great range of conditions, the net uncertainty regarding future outcomes may increase with ensemble size.^{50,54,55}

Model uncertainty arises from both 'structural uncertainty' and 'parameter uncertainty'.^{13,56} *Structural uncertainty* refers to questions about the way in which the model construction reflects the processes—be they physical, chemical, biological, or social—represented by the model. By definition, a model represents but does not replicate the natural world, and there are always uncertainties introduced when describing the essentially infinite complexity of natural systems by simplified representations.^{49,57} These uncertainties include both 'known unknowns'—processes that are known or suspected to exist, but which we have no satisfactory way to describe mathematically, or for which we lack measurements—and 'unknown unknowns'—processes and variables that might be pertinent but of which we are (as yet) unaware. Both of these categories challenge the prediction-making reliability of models.⁵⁸ Statistical approaches are being applied in an attempt to reduce structural uncertainty, but the verdict on their efficacy is not yet in.^{59,60} *Parameter uncertainty* refers to the questions and approximations surrounding the values of parameters used as model input. These can be explored through

sensitivity testing, which examines the impact of variations in model parameter input on model output, in some cases using probabilistic representations of parameters combined with Monte-Carlo analysis.^{61,62}

Conflict uncertainty refers to the fact that, even when presented with the same information, experts may come to different conclusions about its significance, placing experts into conflict with each other. (Experts may also find themselves in conflict with their own prior expressed judgments or published opinions.) Formalized expert elicitation has been used to determine the range of expert opinion of topics for which general agreement is lacking.^{23,63} Expert elicitation can be used to illuminate the origins of such differences, and in some cases to narrow them, by making the assumptions on which they are based explicit and inviting experts to reconsider their views in light of this additional information.^{23,46} On the other hand, there is the potential risk that such 'invitations' can effectively act as pressure on outliers to bring their views into concordance with those of their colleagues.⁶⁴

Our two case studies provide the opportunity to explore how these diverse potential challenges were faced and addressed in practice. The division of model uncertainty into parameter and structural uncertainty is useful in describing these cases. We conclude by suggesting that, while the assessments studied did a great deal to consider and address parameter uncertainty, structural uncertainty was often treated only in passing. Furthermore, we found no systematic approach was employed to articulate and, if possible, explain or resolve disagreement among experts. At the same time, we acknowledge that to some extent, the ability of assessments to evaluate and characterize uncertainty is limited by the

degree to which uncertainty is explored in the basic research literature.

THE ASSESSMENT OF STRATOSPHERIC OZONE DEPLETION

Structural Uncertainty in Ozone Assessments

The Antarctic ozone hole, a region characterized by massive depletion of ozone in the lower stratosphere, was first reported in 1985.⁶⁵ It came as a surprise, because elaborations of the original hypothesis that emissions of chlorofluorocarbon compounds used as coolants, solvents, and foaming agents would eventually deplete stratospheric ozone, causing large increases in ultraviolet radiation at Earth's surface,⁶⁶ predicted much smaller depletion for the late 20th century (about 1%) and relatively modest (about 5%) depletion of global ozone decades later (assuming constant emissions of ozone-depleting chemicals).^{27,67} Furthermore, depletion was expected to occur largely in the upper stratosphere at middle and lower latitudes. Instead, the ozone hole was a geographically limited circumpolar area of major ozone depletion (up to 60% in total column ozone in 1987), occurring only in the austral spring.⁶⁸

NASA scientists mounted three expeditions to study the Antarctic ozone hole: the National Ozone Expedition (NOZE) in August/September 1986, and NOZE-2 and the Airborne Antarctic Ozone Experiment (AAOE) in August/September 1987. Three main competing theories had been proposed to explain the ozone hole: a dynamic theory (ozone-poor air is drawn up from the Antarctic troposphere into the stratosphere), a solar theory (increased solar activity produces large quantities of high-energy particles, which react with oxygen and nitrogen to stimulate formation of ozone-depleting NO_x), and a chemical theory (involving reactions of anthropogenic chlorine and/or bromine). Although the final results of these expeditions, in the form of peer reviewed scientific papers, were not published until 1989,⁶⁹ it was apparent when the initial findings of the AAOE were released on September 30, 1987 that they provided compelling evidence for the proposition that the ozone hole was caused by perturbed chlorine chemistry, involving anthropogenic chlorine, facilitated by the unusual meteorological conditions of the Antarctic polar vortex.^{70–72}

Thus, by 1988 scientists had demonstrated that a major cause of the ozone hole was heterogeneous (i.e., multiphase) chemical reactions involving anthropogenic chlorine. Reactions on the

liquid or solid surfaces of polar stratospheric cloud (PSC) particles in the Antarctic atmosphere (and, when conditions were cold enough, in the Arctic as well) catalyzed large ozone depletion every polar spring. However, before the ozone hole discovery led scientists to rethink their conceptual models, ozone assessments had not considered such multiphase reactions to be important. At the time, gas-phase atmospheric chemistry was much better understood than multiphase chemistry, and heterogeneous reactions were seen as arcane and generally unimportant to stratospheric processes.

The earliest national and international ozone assessments mentioned heterogeneous reactions only to dismiss their possible significance.⁴ For example, a 1976 US National Academy of Sciences report discussed heterogeneous reactions only in an appendix called 'Detailed Discussion of Removal Processes'. There, they suggested 'that inactive removal of CFMs [a class of CFCs] from the stratosphere by heterogeneous processes is not at all significant', and also concluded unequivocally that '[i]t is therefore clear that heterogeneous removal of ClO is negligible when compared with the homogeneous ozone catalysis processes' (Ref 73, p. 226). Why were the National Academy authors so quick to dismiss a set of processes that would later come to be seen as crucial to explaining the ozone hole? One reason is that, in 1976, the authors were only considering heterogeneous *removal* of CFCs and chlorine (process that would protect ozone), not heterogeneous reactions that could directly cause ozone depletion.

Elsewhere in the report, the authors offered the caveat that their predicted ozone depletion reaction rates would be accurate 'unless there is some presently unknown process that quickly returns ClONO_2 into active ClO_x and NO_x species' (Ref 73, p. 154). However, nowhere did they suggest that heterogeneous reactions might enable such a process; yet this turned out to be precisely the case. The possibility that heterogeneous reactions might directly contribute to ozone depletion was not raised explicitly in ozone assessments until after the Antarctic ozone hole had been discovered.^b

The absence of discussion of heterogeneous chemistry from ozone assessments of the late 1970s and early 1980s did not reflect a complete absence of basic scientific research on the subject, however. If some scientists were studying heterogeneous reactions, why did this work not percolate into the assessments? The answer to this may be that these studies produced ambiguous or negative results, which seemed to suggest that heterogeneous chemistry was

not important in ozone depletion. A number of studies, involving both laboratory experiments and computer modeling, were conducted to evaluate the potential occurrence and rapidity of ozone relevant reactions on sulfate aerosols of the Junge layer (the only commonly known location of a persistent concentration of stratospheric particles, before the discovery of the Antarctic ozone hole called attention to PSC particles). All of the reactions investigated in these experiments and models were apparently too slow, either inherently or due to the small numbers of aerosol particles present, to contribute significantly to ozone depletion.^{74–77}

Heterogeneous reactions were also extremely hard to measure. Reaction rates depend on the presence of surfaces, as well as their particular type.⁷⁸ There are always surfaces in contained laboratory experiments, which may not in any way resemble stratospheric aerosol, yet may catalyze reactions and produce misleading outcomes. These difficulties meant that no laboratory data yet existed that would allow these reactions to be incorporated in a realistic fashion. In a 1989 retrospective on his ozone depletion work for *American Scientist*, Rowland explained (somewhat too pessimistically, as it turned out):

The problems inherent in modeling of heterogeneous reactions are intractable, especially when the surfaces involved are of uncertain composition and structure, as with the various kinds of stratospheric aerosols and clouds. Any consensus that heterogeneous reactions are significant in any part of the stratosphere almost automatically precludes modeling future ozone depletions, at least in that portion of the stratosphere (Ref 68, p. 42).

Therefore, because scientific knowledge of heterogeneous chemistry was both highly uncertain and apparently unimportant, and because as a measurement problem it seemed intractable, it was left out of early assessments virtually entirely. Even once it became clear that heterogeneous reactions were important, measurement difficulties inhibited experiments aimed at characterizing reactions so that scientists could incorporate them into models, which might be able to replicate the ozone hole (or mid-latitude ozone depletion). Despite considerable progress, this remains a challenge today.

One might argue that there were no significant consequences to this omission; once the Antarctic ozone hole was discovered and linked to heterogeneous chemistry, scientists and assessors acted quickly to understand the problem and bring its significance to the attention of policy makers. However, as Ha-Duong and colleagues have argued, if ozone-depleting

substances (ODS) had been phased out 10 years earlier than they actually were, the ozone hole might never have developed at all.⁷⁹ It is quite possible that if scientists had had a deeper insight into the potential magnitude of the risk posed by heterogeneous reactions, this knowledge might have been used to support a more aggressive policy, at an earlier date—a not insignificant outcome.⁸⁰ Moreover, this is not an isolated example. Oreskes and Belitz⁴⁹ have shown that, lacking good information about known or suspected processes, and being understandably reluctant to use bad information, modelers may simply leave out those processes entirely. But this effectively amounts to assigning a value of zero to those processes, which may lead to significant error.^{49,81} The heterogeneous chemistry example reinforces the idea that there may be consequences when scientists, or assessors, choose not to emphasize particular uncertainties that are perceived as unimportant. Both scientific discovery, and scientific non-discovery (for instance, due to the lack of observations), may have significant consequences.

Despite the absence of reliable models including the relevant heterogeneous chemistry, policy makers had been making judgments about ozone depletion based on scientific information. How did they manage to do so? Political scientist Edward Parson suggests that ozone assessors responded to the scientific difficulties by applying a series of metrics for ozone depletion that in effect served as surrogates for predictive photochemical/dynamical models (Table 2). Parson ascribes this role to the chlorine loading potential (CLP) in particular, writing: ‘Stopping the analysis [of projected ozone depletion by a given substance] at the intermediate step of chlorine loading avoided all the complexity, uncertainty, and controversy associated with the actual ozone-loss processes’ (Ref 82, p. 160). Though these ozone metrics were primarily proposed to give ozone scientists and policy makers a way to gauge the relative risk associated with various ODS (relative to each other in terms of their chemical properties, and also relative to their respective emissions profiles), these metrics (or proxies) were also, as Parson suggests, useful work-arounds when it became necessary (yet remained difficult) to model heterogeneous chemistry.

The first ozone metric was the Ozone Depletion Potential (ODP), proposed by atmospheric modeler Don Wuebbles in 1983, and first appearing in an ozone assessment in 1989.^{83,84} The ODP incorporated information about the composition and atmospheric lifetime of a given ozone-depleting substance; results were presented relative to the action of an equal mass of CFC-11 (i.e., CFC-11 was arbitrarily assigned an ODP of 1). With the recognition of the importance of

TABLE 2 | Metrics for Estimating Ozone Depletion

Ozone Metric	Calculated By	Formula	Interpretation of Results
Ozone Depletion Potential (ODP)	How much ozone depletion a fixed quantity of a given substance will cause, over its entire atmospheric lifetime, relative to the same amount of CFC-11	$\text{ODP} = \frac{\text{Global } \Delta\text{O}_3 \text{ due to } X}{\text{Global } \Delta\text{O}_3 \text{ due to CFC-11}}$	Need to combine calculated ODP with actual quantity released to estimate actual ozone depletion from this chemical
Chlorine Loading Potential (CLP) or Bromine Loading Potential (BLP)	How much chlorine (or bromine) a fixed quantity of a given substance will deliver from the troposphere to the stratosphere relative to the amount of chlorine delivered by the same quantity of CFC-11	$\text{CLP} = \frac{T_x}{T_{\text{CFC-11}}} \times \frac{M_{\text{CFC-11}}}{M_x} \times \frac{n_x}{3}$	Need to combine CLPs for all ozone-depleting substances (ODS) with actual quantities of all ODS released to figure out actual ozone depletion
Equivalent Effective Stratospheric Chlorine (EESC)	Estimates the total number of chlorine (and bromine) atoms delivered to the stratosphere, over a given time period (<i>t</i>)	$\text{EESC} = n_x \times \text{Cl}^{t-3} \times \frac{\text{FC}_x}{\text{FC}_{\text{CFC-11}}}$	EESC proportional to amount of chlorine (and bromine) available in stratosphere from all ODS; allows easy calculation of indirect (cooling) contribution to global warming potential (GWP) of these ODS

T, lifetime (in years); *M*, molecular weight; *n_x*, number of chlorine (or bromine) atoms in halocarbon *X*; *Cl^{t-3}*, strat halocarbon mixing ratio at time *t* (with 3-year lag for transport); *FC*, fractional release of Cl (or Br) in stratosphere (when calculating with bromine, there is also a constant that accounts for bromine's greater ability to destroy ozone relative to chlorine's ability).

additional ozone-depleting compounds (particularly those containing bromine), as well as the role of heterogeneous chemistry, ODPs were supplemented by another proxy metric: the CLP. As Parson noted, use of the CLP circumvented, for the purposes of developing policy-relevant scenarios, the need to calculate actual ozone depletion.

The CLP, which first appeared in a July 1988 report of the US EPA and was also first used in the 1989 ozone assessment, represented how much chlorine (or equivalent ODS such as bromine) from a given halocarbon would be delivered to the stratosphere.⁸⁵ In 1994, another shift occurred in the ozone assessment reports with the introduction of a third metric: the equivalent effective stratospheric chlorine (EESC). The EESC provides an estimate of the total amount of chlorine and chlorine equivalent delivered to the stratosphere that would participate in ozone depletion at a particular time. In addition to serving as an easily-interpreted measure of a particular chemical's contribution to halogen loading in the stratosphere, the EESC has another purpose: it provides a means to incorporate the indirect radiative (cooling) effect of ozone depletion caused by a chemical into the global warming potential (GWP) for

that substance.⁸⁶ With this new metric, policy makers could establish the net contribution of a given ozone-depleting chemical to climate change, a concern which had risen to prominence and which the assessment panelists began to consider alongside ozone depletion at the behest of governments, beginning in the early 1990s.^c

Since 1998, most of the assessments' graphical predictions of stratospheric chlorine levels under various emissions scenarios have been presented in terms of EESC. Even without a sophisticated knowledge of the relevant chemistry, it is easy for anyone comparing these graphs to each other to see that the higher the EESC, the greater the expected ozone depletion. The gray bar also clearly indicates when the ozone layer might be 'healed' by emissions decreases (Figure 1).

Conflict and Parameter Uncertainty in Ozone Assessments

In contrast to the complexities associated with structural uncertainties described above, ozone assessors seem to have had an easier time dealing with parameter and conflict uncertainty. For example,

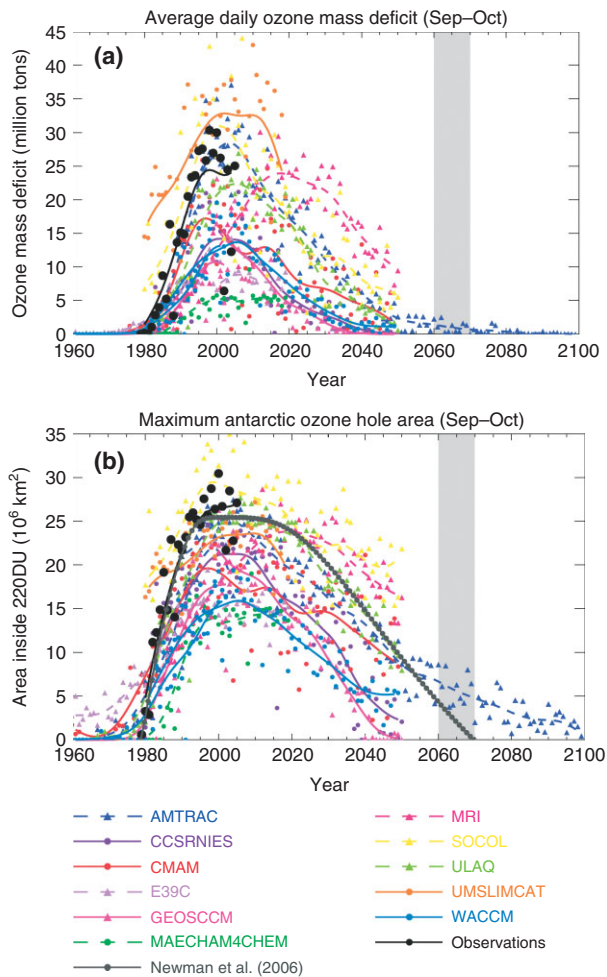


FIGURE 1 | The gray bar in both (a) and (b) shows when equivalent effective stratospheric chlorine (EESC) is predicted to return to 1980 levels. (Reprinted with permission from Ref 87, p. 6.35. Copyright 2006 World Meteorological Organization)

while scientists were grappling with the unexpected discovery of the Antarctic ozone hole, another discovery had precipitated a dispute among ozone experts.

In 1981, NASA scientist Don Heath announced the detection of significant global ozone depletion, detected by solar backscatter ultraviolet (SBUV) instruments on a NASA satellite.⁸⁸ The results were leaked to newspapers before official publication, and some scientists discounted them, arguing that Heath's attempted corrections for known instrument degradation had not been done properly. In response to this difference of interpretation, NASA (along with the US National Oceanic and Atmospheric Administration, the Federal Aviation Administration, the World Meteorological Organization, and the United Nations Environment Programme) formed the Ozone Trends Panel. As its name suggests, the Ozone

Trends Panel was charged with analyzing these and other reports of recent trends in ozone levels (they eventually also took on the task of analyzing the reports of severe Antarctic ozone loss), and producing a detailed report about them.

The executive summary was released in March 1988; the complete report followed a couple of years later.⁸⁹ This report found that while Heath's results overestimated observed ozone depletion, other studies had underestimated it. Taken together, the studies indicated that a small but significant downward trend in global ozone levels was indeed observable. One reason this trend had been difficult to detect was that scientists were averaging their data, annually and globally, and giving more weight to the less variable (and therefore presumably more reliable) data from the summer months. However, it turned out that most significant nonpolar ozone depletion was happening in winter, and at high latitudes, a trend that was not revealed until the data were analyzed on a month-by-month and station-by-station basis. The Ozone Trends Panel, created in response to a disagreement over the way to interpret ozone satellite data, employed a new, in-depth analysis and comparison of both ground-based and satellite ozone readings, and in the end produced a consensus that global as well as polar ozone depletion was occurring due to anthropogenic influences.^{82,89} In effect, the creation of the Ozone Trends Panel was a successful means to manage, and resolve, conflict uncertainty.⁸⁹

The international ozone assessments also found successful means to address parameter uncertainty. This can be seen, for example, in the adoption of strategies developed by Stolarski, Douglass, and collaborators in a series of papers beginning in the late 1970s.^{90,91} They advocated the use of Monte-Carlo simulations to determine the sensitivity of atmospheric computer model simulations to variations in different input parameters. The Monte-Carlo method, first adopted in the 1985 assessment, allowed the assessment authors to combine the uncertainty in several parameters—in this case, different rates for several different chemical reactions—and determine which ones were contributing the most to the overall uncertainty. Subsequent assessments of ozone depletion have continued to apply and refine this approach.^d

The international ozone assessments also adopted Bayesian approaches, most notably in the selection and consideration of a handful of discrete CFC emissions scenarios. While the first report presented the results of several model runs with different chemical reaction rates but under a single, steady-state, emissions scenario,⁹² the 1985

assessment presented, for the first time, scenarios depicting exponential growth of CFCs.⁶⁷ Beginning with the 1991 report, the assessments presented the predicted ozone depletion associated with several different emissions scenarios explicitly corresponding to varying levels of global compliance with the Montreal Protocol (Ref 93, p. 8.33).

Discussion

The history of international ozone assessments suggests that ozone assessors had difficulty managing structural uncertainty, in the form of heterogeneous processes that were just too hard to measure, until they were motivated to reexamine their assumptions when an unexpected observation (the Antarctic ozone hole) emerged. However, a detailed reading of the scientific literature contemporaneous with these assessments shows that ozone scientists did not simply ignore these hard-to-measure processes: they tested them to the best of their abilities and to the extent they felt was warranted by the evidence. The available observations and laboratory data suggested heterogeneous reactions were unimportant, so scientists understandably did not focus on them. However, when an unexpected phenomenon emerged in the form of the Antarctic ozone hole, suggesting that some crucial piece of the puzzle was missing, they went back to work, and found fairly quickly that heterogeneous processes were the missing piece. Even knowing this, however, it was not immediately possible to model the relevant chemistry adequately, or to run the more complex two- and three-dimensional models repeatedly. Scientists therefore developed surrogates (CLP and EESC), consistent with the known science, for the benefit of policy makers.

The history of ozone assessments thus suggests that well-understood empirical surrogates, like the EESC, can be an important tool for scientists to express what is known, even while acknowledging the complexity of the systems under investigation, and without burying the uncertainties. This suggests that, even lacking full mechanistic understanding, scientists can perhaps offer empirically adequate descriptions of a given phenomenon that can usefully guide policy. On the other hand, we also emphasize that in describing the *uncertainty* in projected ozone depletion before the ozone hole was explained, the *ozone assessments* (as opposed to individual scientific studies) could have accorded greater attention to the indications of a possible role for heterogeneous reactions.

In contrast to their handling of structural uncertainty, ozone assessments seem to have grappled relatively successfully with parameter and conflict uncertainty, by, for example, adopting Monte-Carlo

methods to test the sensitivity of their outcomes to changes in parameters, and creating the Ozone Trends Panel to resolve the dispute over mid-latitude ozone depletion. However, we are still left with some key questions: First, how complete are the results of Monte-Carlo analysis? The ozone assessments have heavily favored these in their handling of parameter uncertainties, but as a recent NRC report emphasizes, they are no panacea—not even for parameter uncertainty alone.⁹⁴

Second, what happens when the creation of an assessment panel fails to resolve the conflict uncertainties it was created to address? The Ozone Trends Panel was created to resolve conflict uncertainty arising from divergent views of the data, which it succeeded in doing. This leaves open the question of what might happen when experts disagree within a panel—an issue that arose in the most recent attempt of the Intergovernmental Panel on Climate Change to address the potential for the rapid disintegration of the WAIS.

ASSESSING THE POTENTIAL FOR RAPID DISINTEGRATION OF THE WAIS

The West Antarctic Ice Sheet

Since the 1960s, scientific experts have been concerned about the potential for rapid disintegration of the WAIS and a resulting rise in global sea level.^{95–100} WAIS contains about 10% of all Antarctic ice and is considered to be less stable than the larger East Antarctic ice sheet. If WAIS disintegrated entirely, global average sea level rise would increase between 3 and 6 m, a high-impact event. But experts are unclear as to the details of the mechanisms by which the ice sheet might disintegrate, how rapidly this might occur, how soon the process might begin, and whether it may have already begun. Because of important knowledge gaps, reliably modeling future ice sheet behavior is currently not possible. Many experts have spent their careers working on this topic without achieving a satisfactory resolution of many questions, and on some aspects, making little progress at all. Yet clearly, this is an area in which policy makers have a pressing need for answers, as they are responsible for planning responses to sea level rise.

It seems logical that more research will lead to less uncertainty, but the question of WAIS' contribution to sea level rise has become more uncertain, rather than less, over time—at least in the IPCC representation of it. In the Summary for Policymakers (SPM) of the Third Assessment Report (TAR), the IPCC's Working Group I included a contribution from WAIS in its projections of

long term (multi-century) global sea level rise. While acknowledging in the Technical Summary that the results were ‘strongly dependent on model assumptions regarding climate change scenarios, ice dynamics and other factors’, the TAR authors nevertheless provided the following estimate: ‘Current ice dynamic models project that the WAIS will contribute no more than 3 mm/yr to sea level rise over the next thousand years’ (Ref 101, p. 77).

In the SPM of the Fourth Assessment Report, however, WGI not only specifically excluded any estimate of the long-term WAIS contribution but also omitted potential increases in ‘dynamical ice loss’ from either the Greenland or Antarctic ice sheets from its tabulated projections of sea level rise in the 21st century.^e The authors stated that ‘dynamical processes related to ice flow not included in current models but suggested by recent observations could increase the vulnerability of the ice sheets to warming, increasing future sea level rise. Understanding of these processes is limited and there is no consensus on their magnitude’ (Ref 102, p.17). Numerical estimates of future sea level rise based only on thermal expansion of ocean water, melting of mountain glaciers, and current ice movement were presented in the charts in the SPM, although the authors did insert the caveat that ‘larger values cannot be excluded, but understanding of these effects is too limited to assess their likelihood or provide a best estimate or an upper bound for sea level rise’ (Ref 102, p. 14). That is to say, increased discharge of ice from Antarctica and Greenland could raise sea level dramatically and relatively quickly, but no quantitative estimates of how dramatically or how quickly were offered. Thus, it appears at first glance that the science surrounding WAIS became more uncertain, rather than less, from the Third to the Fourth Assessments.

However, it is doubtful that any scientists involved with WAIS actually hold this view. Even if modeling capability remains limited (due to structural uncertainty), information from observations has increased rapidly. Still, there has been an acknowledged lack of progress in the ability to assess the new information and make coherent judgments based upon it: the available models are simply not up to the task of providing a framework for interpreting it. Scientists are unable to make meaningful projections for the ice sheets based on the available information.

Many people involved therefore consider the IPCC’s treatment appropriate, asserting that it would have been wrong to offer an estimate that might have been incorrect.⁵¹ Some have even argued that the AR4 authors were brave in deciding to leave potential changes in the dynamic ice sheet contribution out of

numerical sea level rise tabulations for the 21st century in AR4, resisting the pressure to assess something they were not honestly able to assess.⁵¹ However, others consider this a serious omission, insofar as the scientists’ declining to provide an estimate of this factor, or the range of expert judgments on its potential magnitude, meant the knowledge that did exist was not fully characterized.⁵¹ The result, they feel, is that the sea level rise estimates that were offered are virtually useless—in that they leave out a contributory factor that is not only significant, but that could overwhelm the other factors.

Several explanations have been offered for the impasse that resulted.⁵¹ One, invoked by some of the AR4 authors, is that given the data long at hand, the immense number of new publications that had recently come to light revealing the importance of ice dynamics, and the limited time to assimilate them, experts simply could not come to an agreement. New remote sensing data, in particular, revealed both the East and West Antarctic ice sheets to be far more dynamic than previously thought: not just a chunk of solid ice, but rife with cracks, streams, and lakes—in effect, a complex subglacial system that provided many more potential mechanisms for rapid ice loss than were previously known. As a result, the physical picture of WAIS became more complex, and the understanding of which processes are important to its dynamics remained in flux. The new data suggested that modelers have substantial work still to do to make the models represent the ice sheet adequately enough to make credible predictions. The model estimates for AR4 were based on simulations that did not take the new information into account—due to time constraints—and therefore, the authors could not claim that those estimates realistically approximated WAIS conditions and behavior.

Social and institutional factors may also have contributed to increased uncertainty in AR4. For example, IPCC chapters have a (largely) new writing team for each assessment. For sea level rise, new estimates must be made in light of the new data sets, methods, scales, and so on, based on hundreds of sources assessed. Writing teams are composed of scientists who carry not only professional credentials but also beliefs and opinions on how to manage the massive project of writing an IPCC chapter, and diverse subjective approaches to risk and uncertainty. While the SPM represents a consensus of a large group of scientists, and receives careful scrutiny for bias and eventual approval by governments, the underlying technical material upon which the SPMs are based represents the product of much smaller groups of scientists. In the role of assessors, the writing team

must make judgments about how to organize and convey the available material, so personal experience and bias inevitably play a role.

Another explanation focuses on the organization of the reports: bureaucratic organization can contribute to propagating as well as mitigating judgment uncertainty. In the Fourth Assessment Report, the treatment of WAIS became more fragmented than it had previously been, as a result of a top-down chapter reorganization. In the first, second, and third IPCC assessment reports, the WAIS question was primarily confined to a single chapter on sea level rise. However, in the Fourth Assessment Report, there was no single chapter on sea level rise. Instead the WAIS issue was splintered among three chapters in Working Group I: Chapter 4—Observations: Changes in Snow, Ice, and Frozen Ground; Chapter 6—Paleoclimate; and Chapter 10—Global Climate Projections. Two different groups in Working Group II also had responsibility for assessing WAIS (Chapter 15—Polar Regions and Chapter 19—Assessing Key Vulnerabilities and the Risk from Climate Change). With different writing groups focusing on different aspects of the WAIS question, it was difficult to achieve consistency among the chapters (see Ref 51 for a discussion of how differing views of uncertainty and risk were rationalized between the Working Groups). Authors informally worked with one another to tell a consistent story about ice sheets across the chapters, but with more people involved and the cohesion of several chapters at stake, decisions become more difficult to manage.⁵¹ The bureaucratic decision to reorganize assessment report chapters resulted in a reorganization of scientific content and a less unified approach. Writing teams for chapters meet separately, have massive workloads, and would have to make extra meeting arrangements to collaborate across chapter groups.¹⁰³ Finally, with more people, there are simply more views to consider. The organizational structure of AR4 may not have created a social environment that maximally facilitated collaborative decision making.

Discussion

It is clear that a good deal of the explanation for what happened between the Third and Fourth assessment reports involves the impact of new data, which made scientists understand that the ice sheet system was more complex than earlier models allowed, underscoring the importance of structural model uncertainty. Increased knowledge led to a deeper appreciation of this natural complexity. Increase in uncertainty estimates was in part a reflection of a deepened understanding of a complex natural system. On the other

hand, it also suggests that the presumption that more research will lead to less uncertainty is not necessarily correct,²⁷ which has implications for the overall value of assessments—and indeed, even for science, writ large—as a policy-relevant tool.

Data alone, however, are not the whole story, and indeed the question remains as to whether AR4 could have presented a more complete assessment of the uncertainties arising from structural model uncertainty affecting ice sheets and sea level rise in general and the WAIS contribution in particular. Certainly, the plethora of articles appearing in the literature attempting to do so since AR4 was published reveals a certain dissatisfaction in this community.^{104–110} Limits on the assessment's ability to judge uncertainty also were generated through the IPCC institution and process itself. We might call this *socially-derived* uncertainty, a category not generally recognized by other commentators on sources of uncertainty. Some of this socially-derived uncertainty can be managed by the institution, for example, by assuring greater continuity in how uncertainty bounds are calculated (or at least explained) across assessment reports. Other kinds of socially-derived uncertainty, like the subjectivity of the writing teams and the bureaucratic reorganization of report chapters, are inherent qualities of the IPCC institution itself, a bureaucratic dynamism that has both positive and negative aspects.

Differences of opinion among experts fall intellectually into the category of conflict uncertainty, but the word 'conflict' belies the eventual agreement of at least the key, influential authors in this situation. We propose a new, related category of *judgment uncertainty*, to suggest the fact that differences in expert judgment, without overt (or even covert) conflict, can play a significant role in assessment outcomes. In this case, the apparent increase in uncertainty was in part the result of an apparent convergence among authors—a judgment call of the writing group in place. Furthermore, judgment uncertainty may have been augmented by socially-derived uncertainty due to organizational structures and choices previously established.

CONCLUSION

Two cases is admittedly a small sample from the universe of scientific assessment. Further studies along these lines could address additional questions, such as differences in assessment approaches that arise from differences in the substantive content assessed. But our cases do offer important insights into the assessment process and the management of uncertainty in it.

We offer the following comments as a starting point for discussion. Our cases appear to be consistent with the taxonomy presented above, drawn from existing literature: both ozone and climate change assessments have faced parameter, conflict, and structural uncertainties. In addition, we have suggested that two additional terms—*judgment uncertainty* and *socially-derived uncertainty*—may be useful to underscore that scientists interpreting evidence may make divergent subjective judgments even absent direct conflict. This judgment uncertainty represents a new category, distinct from though often bearing a relationship to parameter, structural, and conflict uncertainty. Given identical data, honest and competent experts may come to different conclusions based on their own subjective weighing and evaluation of those data, which is influenced by their epistemic and social affinities.¹¹¹ Such judgment uncertainty is not eliminable, but it can perhaps be better managed, particularly through practicing increased transparency (e.g., ‘traceability’ in IPCC terminology) so deliberative practices become more accessible and amenable to analysis. Moreover, the institutional frameworks within which assessments take place may increase or decrease socially-derived judgment uncertainty, suggesting that social choices, such as how to organize the chapters in IPCC reports, are not ‘merely’ bureaucratic—they are not just housekeeping—but can have epistemic consequences.

Socially-derived uncertainty, a broader analytic category, refers to the ways that the human relationships and institutional arrangements contribute to how people decide to describe uncertainty in their particular assessment projects. Assessments and uncertainty characterizations are not created in a vacuum, but in specific social arrangements. These arrangements are significant and social scientists should continue to analyze the role of social relationships, institutional formation, and epistemic communities in scientific assessments.

Clear analytic categories are important because they facilitate understanding and discussion of the issues at stake. However, it is also important to move beyond taxonomy to examine the ways assessors deal with uncertainty in practice, in order to shed light on the overall process of writing assessments, the social dynamics that shape uncertainty, and the uneven handling of different types of uncertainty. Our cases suggest that, at least as a first-order approximation, scientists have done a good job dealing with parameter uncertainty, and have had more difficulty dealing with structural, conflict, and judgment uncertainty.

We have seen how, in the ozone case, scientists handled parameter uncertainty with Monte-Carlo

simulations; in the IPCC, scientists also employ multi-model ensembles. While ozone assessments dealt with some forms of uncertainty relatively successfully, the assessors also largely discounted the potential significance of heterogeneous reactions until they were motivated to reconsider them by the discovery of the ozone hole (structural uncertainty). In the WAIS case, AR4 declined to make an estimate of the dynamic ice loss contribution to sea level rise because the models were found to lack key processes (structural uncertainty) and authors could not reach consensus on how to represent the resulting effect of this uncertainty on sea level rise (conflict or judgment uncertainty).

It is perhaps not surprising that scientists working in assessments have done a good job addressing parameter uncertainty. Scientists have scientific techniques for doing so: sensitivity analysis, Monte-Carlo analysis, multi-model comparisons, and model ensembles. These techniques draw on scientific expertise to address what are understood and recognized as scientific questions (e.g., the impact of initial and boundary conditions on model outcomes). Structural, conflict, and judgment uncertainty are a different matter. Natural scientists do not have scientific techniques to address conflict among experts; the fact that different experts can come to different conclusions from the same data is awkward for men and women engaged in an activity that is supposed, at its core, to be logical and objective. It is difficult to objectively discuss subjectivity. Yet it seems clear that scientists must discuss it, or else run the risk that their results will, at minimum, be less useful than they should be, and at worst, suggest to the public that the so-called experts do not know what they are talking about. Moreover, if the impression arises that scientific experts cannot answer the questions the public needs answered, then support for scientific assessments may founder. The InterAcademy Council report (2010) evaluating the IPCC makes similar suggestions: to use qualitative uncertainty language as the first-line attempt at describing uncertainty; to look to expert elicitation to provide subjective expert opinion on crucial issues; and to quantify uncertainty only when there is both ample research and strong agreement within the research.⁴⁵ To characterize uncertainty where it cannot be quantified, natural scientists may need to collaborate more closely with experts from disciplines with insights on the value and limitations of subjective judgment.

Structural uncertainty is particularly difficult to address, because by definition (and in the extreme case) one cannot analyze what one does not know. In the ozone assessments, the structural uncertainty represented by the possible role of heterogeneous

chemical reactions was essentially ignored in the assessments, until its inclusion was motivated by the discovery of the Antarctic ozone hole. In the IPCC assessments, the potential for rapid disintegration of WAIS is a similar structural uncertainty, one that assessors will doubtless face once again in the Fifth Assessment.

One could easily imagine a missed process in ice sheet dynamics, analogous to heterogeneous chemistry, but remaining unrecognized for much longer. The IPCC and other assessment bodies deserve credit for their flexibility and adaptability toward finding new ways to assess the available science, and we encourage them to likewise enlarge these strategies to more comprehensive assessment of gaps in critical knowledge. What mechanisms, if any, does the IPCC or any other assessment process have for encouraging scientists to explore factors that may, like heterogeneous ozone chemistry, be left unexplored, but may turn out to be important, or even crucial? The history of science and technology could provide help here, in considering how other expert communities have tried to explore the domains of 'known unknowns' and 'unknown unknowns' and maintain a stance of openness to the possible significance of previously neglected matters.^{112–118} Furthermore, scientists conducting the research that is assessed in these documents could devote additional attention to taking such uncertainties into account in the peer-reviewed literature, giving the assessors the raw material they need to improve the characterization of uncertainty in assessments. In that way, while uncertainty can never be eliminated, its management in assessments could be improved significantly.

NOTES

^aIn the late 1970s and early 1980s several countries produced national assessments of ozone depletion. The Montreal Protocol on Substances that Deplete the Ozone Layer was adopted in 1987. It mandated periodic assessments of the science, environmental effects, and technological and economic impacts of ozone depletion (or recovery). These assessments have been published every 4 years since 1989.

^bThe treatment of heterogeneous chemistry in the 1985 'blue books' assessment is more nuanced, although still, on balance, dismissive. This assessment, for which the final versions of chapters were sent to the publisher 6 months after the ozone hole discovery was announced, contained two brief discussions of heterogeneous reactions, in Chapter 2 (on stratospheric chemistry) and Chapter 13 (on model predictions of ozone changes). The authors

of Chapter 13 wrote: 'A number of such reactions ["reactions which form or liberate NO_x and Cl_x from their reservoir species HNO₃, HNO₄, ClNO₃, HCl, and HOCl"]... have been demonstrated to be very slow in the gas phase but quite rapid on the surfaces of laboratory apparatuses [sic]. As discussed in Chapter 2, it is quite conceivable that such reactions may proceed at a significant rate as heterogeneous processes on stratospheric sulfuric acid aerosols. Definitive heterogeneous kinetic experiments are clearly needed to allow further assessment of their heterogeneous reservoir reactions' (Ref 67, pp. 781–782). However, despite the reference to Chapter 2, the authors of the second chapter seemed much less concerned about a possible role for heterogeneous chemistry, writing: 'In assessing the current evidence relating to the question whether or not aerosols perturb the homogeneous chemistry related to stratospheric ozone, it can be concluded that the effects are minor and are unlikely to change our overall picture of the chemistry of the stratosphere. Some modification of our detailed formulation of the behavior of temporary reservoir species may result from further characterization of their heterogeneous reactions in the laboratory. Local effects resulting from volcanic injections may be considerably more significant. Detection [sic] and understanding these effects is difficult, firstly, because the available baseline information does not allow unambiguous assignment of a given observation to the presence of enhanced aerosol loading and secondly the effects are expected to be subtle and of small magnitude' (Ref 67, p. 48). 'Depending on the chemical nature of the particular species, ODS may directly contribute to global warming, or contribute an indirect cooling effect (Ref 86, p. 1283).

^dWhile Monte-Carlo methods are valuable, they may not address all possible uncertainties, particularly those that are not easily captured in a single probability distribution. A recent NRC report⁹⁴ recommends combining Monte-Carlo simulations with other methods, such as sensitivity analysis and the use of alternative scenarios, to achieve a more rounded treatment of parameter uncertainties.

^eUncertainty due to possible ice dynamical changes in WAIS was also omitted from sea level rise projections for the 21st century in the Third Assessment. But observations of the ice sheets published between the two assessments strengthened the case that such processes might be important in both the long term and during the 21st century. It is also important to note that near the end of AR4, there was a passing attempt to provide an uncertainty estimate on the sea level rise that could occur during the 21st

century due to changes in ice dynamics, as follows: 'For example, if (the ice dynamic) contribution were to grow linearly with global average temperature change, the upper ranges of sea level rise for SRES scenarios shown in Table SPM.3 would increase by 0.1 to 0.2 m. Larger values cannot be excluded, but understanding of these effects is too limited to assess

their likelihood or provide a best estimate or an upper bound for sea level rise' (Ref 112, p.14). So, WGI at once put its toe in the waters of uncertainty judgment but also refrained from presenting a comprehensive picture, possibly reflecting both pressure from reviewers on the one hand, and a lack of internal consensus on the other.

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