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Certainty, uncertainty, and climate change

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Uncertainty abounds in issues related to climate science and climate changes, the impacts of those changes, and the efficacy of strategies that might be used to mitigate or adapt to change. There are, however, a few things about which we can be quite certain. There are also a number of things about which many people are certain, but should not be.

1 Certain and uncertain climate science

Despite the continued efforts of skeptics motivated by a desire for attention or short-term economic interests (Oreskes and Conway 2010), we can be certain about a number of basic facts: human activities have resulted in dramatic increases in the atmospheric concentration of carbon dioxide and a number of other greenhouse gasses; those increased concentrations are changing the climate and will continue to do so; one of those changes will be average warming on a planetary scale. Another unambiguous consequence of rising atmospheric concentrations of carbon dioxide will be the continued acidification of the world's oceans, which are already 30% more acidic today than they were in pre-industrial times.

While we still do not understand all the details of the physics, we can also be certain that factors such as clouds and water vapor, aerosol loadings, the extent of ice cover, and the strength of ocean circulation all play a key role in shaping the climate of today and of the future.

In its periodic reviews, the IPCC routinely discusses and provides consensus judgments about the nature and extent of scientific uncertainty about these and other factors. Thanks largely to the work of Moss and Schneider (2000) these discussions are no longer couched only in terms of general uncertainty words such as "likely" or "unlikely." Indeed, the climate science community has made more progress in understanding the importance of linking such words to quantitative statements about probability ranges than has any other community I know that is engaged in performing scientific assessments.

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The reason that it is important to provide such quantification is that there is clear empirical evidence that the same uncertainty word can mean different things to different people and can also mean different things to the same person in different contexts (Wallsten et al. 1986). Hence, if one only uses words, with no linked probabilities, assessments can become almost meaningless.

The health effects community has lagged further behind most others in understanding that when using uncertainty words it is important to be quantitative, this despite clear indications of the problem (Fig. 1). For example, there has been great reluctance to describe health damage functions in terms of subjective probability distributions (Morgan 1998). Linking probability words to numbers is important, but Budescu et al. (2009) have shown that even when people are constantly reminded of this linkage, different people may still draw very different inferences from the same word.

Fig. 1 Results obtained by Morgan (1998) when members of the Executive Committee of the EPA Science Advisory Board were asked to assign numerical probabilities to words that have been proposed for use with the new EPA cancer guidelines (USEPA 1996). Note that, even in this relatively small and expert group, the minimum probability associated with the word "likely" spans four orders of magnitude, the maximum probability associated with the word "not likely" spans more than five orders of magnitude, and there is an overlap of the probabilities the different experts associated with the two words. Caption from Morgan et al. (2009)

While the IPCC has yet to make use of them, there are methods that allow an even more precise characterization of uncertainties. “Expert elicitation” involves a set of techniques first developed in the decision analytic community (Spetzler and Staël von Holstein 1975; Morgan and Henrion 1990). Subsequently, the methods used to perform elicitation have been informed by the work of a number of experimental psychologists (Kahneman et al. 1982) who have demonstrated that, without being aware of it, people use a variety of cognitive heuristics when making judgments about uncertainty. While these heuristics work well in many settings, they can also give rise to a variety of biases when making judgments under uncertainty.

There is clear experimental evidence that both experts and laypeople are systematically over confident when making judgments about, or in the presence of, uncertainty (Fig. 2). Lest one conclude that the phenomenon of overconfidence is limited just to laypeople, the gray boxes in Fig. 3 indicate the recommended value of the speed of light from 1930 through the 1980s. Note that for a period of roughly two decades the range given for the recommended value (which should have included a consideration of all possible systematic as well as measurement error) did not even include the present best value. For a more recent example of the same problem, see the ongoing debate over the value of G, the gravitational constant (Davis 2010).

Expert elicitation cannot eliminate the problem of biases caused by the operation of cognitive heuristics, nor can it eliminate overconfidence. But unlike more informal methods such as group discussion (in which the same cognitive heuristics and tendency to

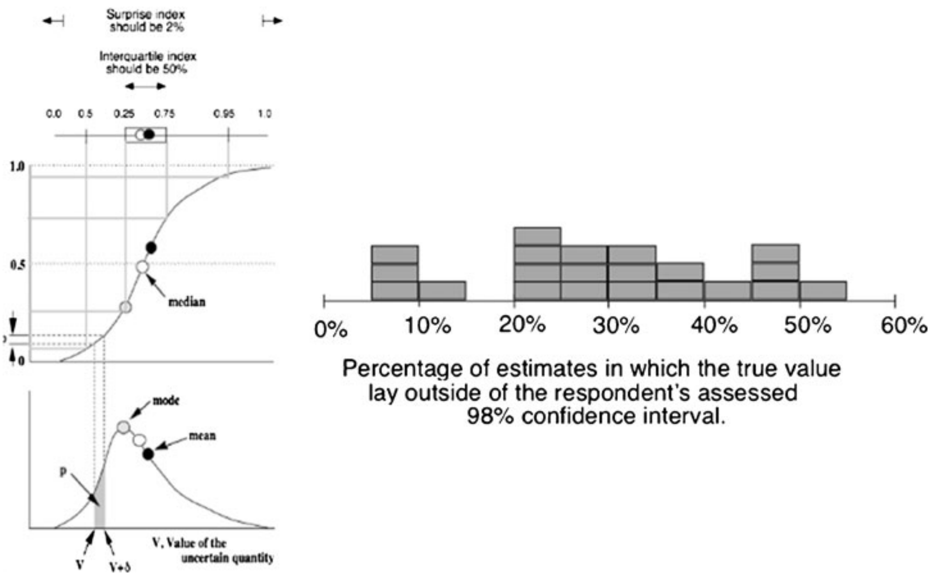

Fig. 2 We can define overconfidence in terms of a “surprise index” that reports how frequently a true value lies outside of the 98% confidence interval of an assessed probability (*left*). The histogram at the right summarizes data from 21 different studies in which, using a variety of methods, people were asked to produce probability distributions on the value of well known quantities (such as the distance between two locations), so that their distributions can be subsequently checked against true values. The number of respondents in these studies average was just over 700. The results clearly demonstrate that people are systematically overconfident (i.e., produce subjective probability distributions that are too narrow) when they make such judgments. Data are from Morgan and Henrion (1990) who, in compiling it, drew in part on Lichtenstein et al. (1982). Portion of figure and caption from Morgan et al. (2009)

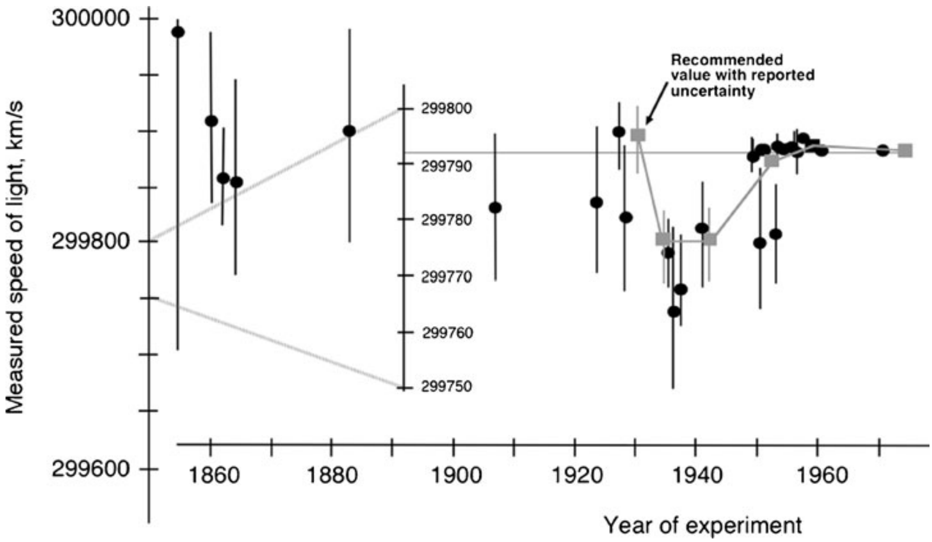

Fig. 3 Time series of reported experimental values for the speed of light over the period from the mid-1800's to the present (*black points*). Recommended values are shown in gray. These values should include a subjective consideration of all relevant factors. Note, however, that for a period of approximately 25 years during the early part of the last century, the uncertainty being reported for the recommended values did not include the current best estimate. Similar results were obtained for recommended values of other basic physical quantities such as Planck's constant, the charge and mass of the electron, and Avogadro's number. For details, see Henrion and Fischhoff (1986) from which this figure has been redrawn. Caption from Morgan et al. (2009)

overconfidence operate) what it can do is work systematically to try to identify and minimize such problems.

Since the early 1990s my colleagues and I have conducted four detailed expert elicitations in which we have obtained judgments from leading climate and ecosystem scientists about uncertainty in the value of a variety of key climate variables and impacts (Morgan and Keith 1995 and Morgan et al. 2006; Zickfeld et al. 2007 and 2010).

One hopes that research will lead to a reduction of uncertainty and at least some decision analysts appear to assume that this will always be the case. However, our respondents were all experienced scientists who understand that often research identifies unforeseen complexities, and thus, at least for a while, can increase rather than decrease uncertainty. Thus, for example, in Morgan and Keith (1995) we asked respondents to assess the probability that their uncertainty about the value of climate sensitivity would grow by 25% or more after a 15-year program of research at 1-billion \$/year. The responses we obtained ranged from 0.08 to 0.30 (average value of 0.19). We have found similar results in our more recent elicitations. While such results are not surprising to experienced scientists, they do come as a surprise to some analysts and decision makers who view research as always reducing uncertainty.

Quantitative expert elicitation can be a very useful tool to identify and display the divergence of opinion within a field. It can do so with much greater clarity than qualitative statements of the sort produced by IPCC writing teams. For example, Fig. 4 from Zickfeld et al. (2007) shows that at the time of the elicitation a clear split in opinion existed among ocean scientists about whether there was any significant probability that plausible levels of warming might start the AMOC on a course leading to collapse. At about that same time, the IPCC fourth assessment concluded, "Europe, particularly its north-western parts, owes its relatively mild climate

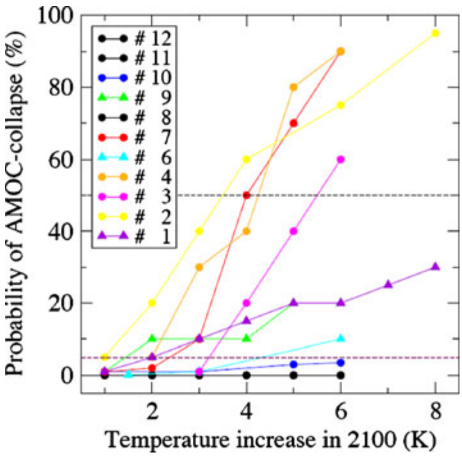

Fig. 4 Assessment by 12 experts that the Atlantic Meridional Overturning Circulation will have irreversibly begun a collapse (defined as >90% decease in strength) as a function of average global temperature by the year 2100. The results clearly display a divergence of opinion within the expert community, with four experts assessing the probability of collapse to be quite high and the balance judging the probability of collapse to be small or even zero. Figure from Zickfeld et al. (2007)

partly to the northward heat transport by the Atlantic MOC… Most models suggest increased greenhouse gas concentrations will lead to a weakening of the MOC…which will act to reduce the warming in Europe. However, in the light of present understanding, it is very unlikely to reverse the warming to cooling." Since the IPCC's predictive statement is about the temperature of Europe, not the strength of the AMOC, it is not a direct contradiction of the results in Fig. 4, but the two clearly convey quite different impressions. Similarly, an assessment conducted on aerosol forcing (Morgan et al. 2006) suggests a much greater diversity of opinion about possible uncertainty among experts in that field than is suggested by the summary plot in the IPCC fourth assessment, Fig. 5.

2 Climate impacts and abatement

The same methods of expert elicitation that we have applied to climate science can also be applied to assess uncertainty about the impacts of climate change and the likely efficacy and cost-effectiveness of technologies and strategies to reduce future emissions.

Figure 6 shows results obtained in an assessment conducted in 1999 of the impact of a $2xCO_2$ climate change on above ground and below ground biomass in boreal and in tropical forests (Morgan et al. 2001). In the years since that assessment was conducted, considerable progress has been made in modeling ecosystem response to climate change. However, the fact that at the time we conducted this assessment a set of leading ecosystem experts were not in agreement about the sign, let alone the magnitude, of the impacts, suggests that one should be cautious about reaching consensus too rapidly based upon the results from one or another leading ecosystem model since such models may not include a variety of the processes and uncertainties (pests, fire, etc.) that our respondents included qualitatively in their thinking when they participated in the elicitation.

In the context of technology for emissions abatement, there are a growing number of efforts to use expert elicitation. Figure 7 shows a result that we obtained from 18 experts

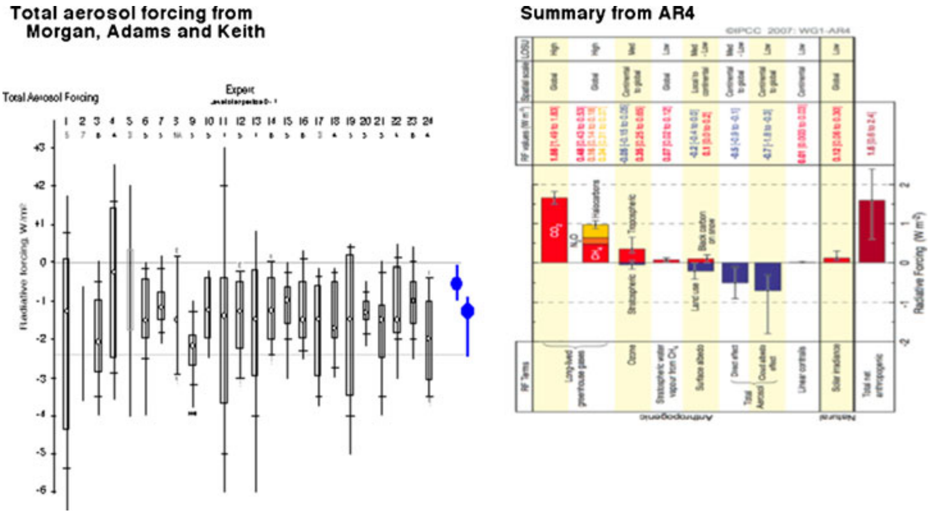

Fig. 5 Comparison of probability distributions (displayed as box plots) elicited from 24 experts assessing the total radiative forcing from aerosols (*left*) as compared with estimate of the value of such forcing as assessed in the IPCC fourth assessment (*right*). Note the significant diversity of opinion displayed across the set of 24 experts, and the fact that the range of value that some of these experts considered plausible extends outside the range suggested by the IPCC summary plot. Figure from Morgan et al. (2006) and from IPCC (2007)

who assessed the likely future cost and performance of 26 current and emerging solar PV technologies (Curtright et al. 2008). These results allowed us to conclude:

> Given…the low probabilities that many experts assess of meeting a price of \$0.30/Wp by 2050, and the wide dispersion in their assessments of efficiencies

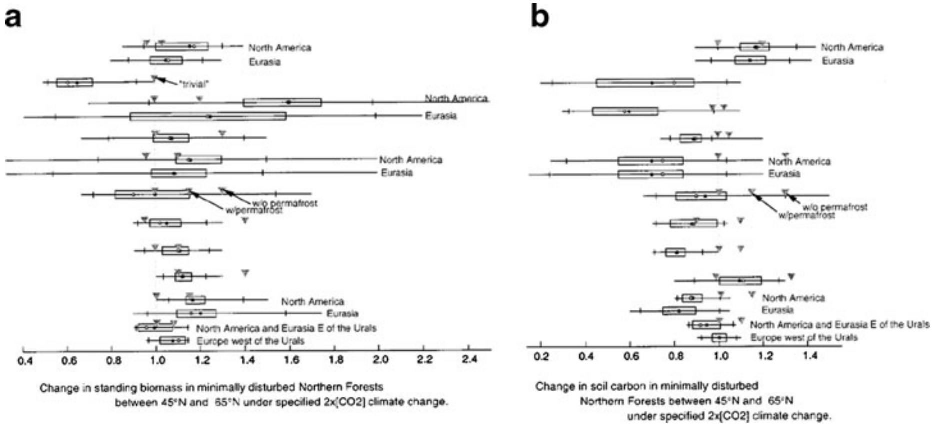

Fig. 6 Box plots summarizing elicited expert subjective probability distributions of (**a**) standing biomass and (**b**) soil carbon in minimally disturbed northern forests at least 500 years after a specified 2xCO_2 climate change. When not otherwise noted, results are for North America. Horizontal lines display the full range of the distributions. Vertical tick marks indicate the 90% confidence intervals. Boxes denote the 50% confidence intervals. Solid points indicate means, open circles indicate medians. The shaded triangles indicate the estimated range of response for a doubling of CO_2 alone, without any accompanying climate change. Note that the set of experts do not even agree about the sign of the impact. The figure and much of the caption are from Morgan et al. (2001) which also reported a similar plot for tropical forests that reports smaller impacts but an even more striking disagreement about their sign

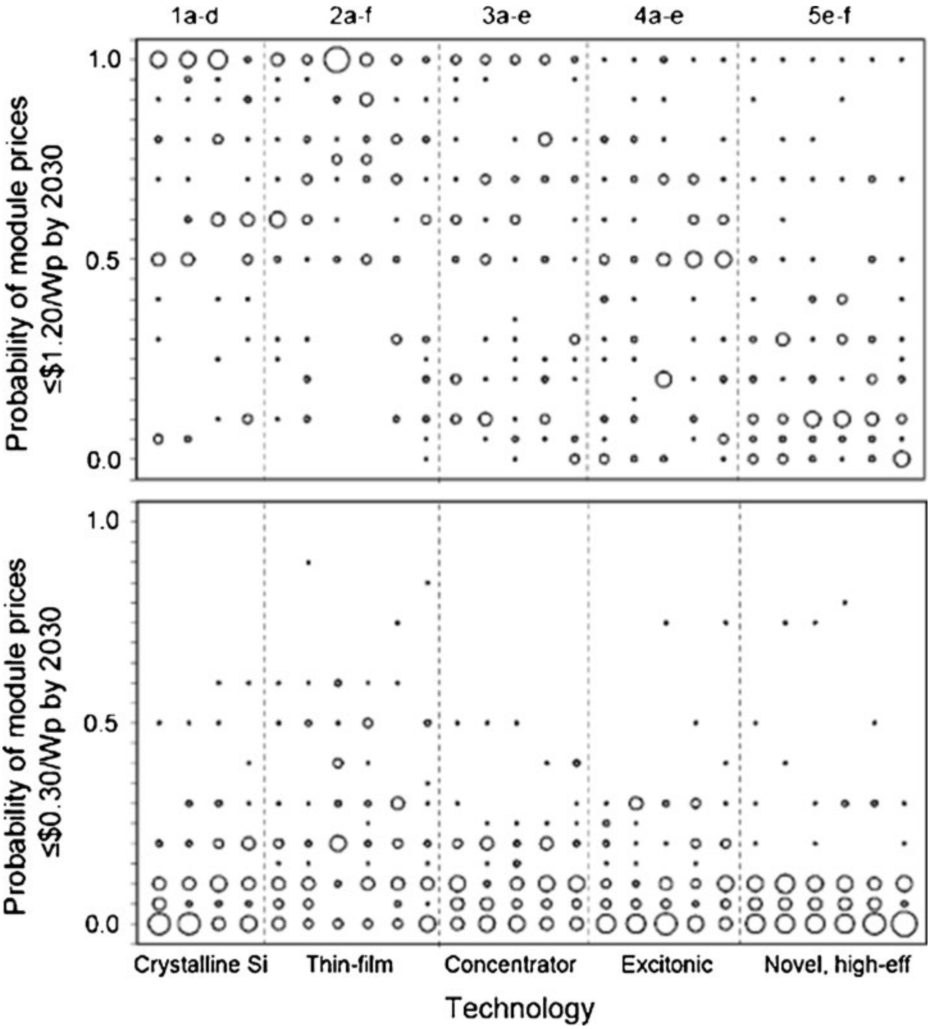

Fig. 7 Probability of achieving module prices of (*top*) \$1.20/Wp or less and (*bottom*) \$0.30/Wp or less by 2030. The results shown here are for all 26 current and emerging PV technologies and for all 18 experts, regardless of expertise level in a given technology. The circle diameter represents the number of experts who responded with the given probability for the given PV technology; the smallest circle corresponds to one expert, the largest corresponds to eight in both graphs. Figure and caption are from Curtright et al. (2008)

and prices, we conclude that PV may have difficulty becoming economically competitive with other large-scale, low-carbon bulk electricity options in the next 40 years. At the same time, it seems likely that PV will continue to expand into a variety of smaller scale markets. Of course, past efforts to make technical and energy-related predictions have often missed the mark (Smil 2003; Stern 1982). Unanticipated technical developments could similarly overturn the judgments herein, but before R&D reduces uncertainties, massively subsidized deployment of existing technology is arguably not the best way to increase the odds of such an outcome. (Curtright et al. 2008)

Today groups at the Kennedy School at Harvard, at the Fondazione Eni Enrico Mattei in Italy, at the University of Massachusetts in Amherst, Massachusetts and at Lumina Systems in California are engaged in studies using expert elicitation to assess the potential of, and research needs for, a wide range of future low energy technology.

3 Integrated and other assessment

My colleagues and I were among the first to call for the development of integrated assessment models to study climate change and its impacts, and to explore the implications of a variety of abatement strategies (Rubin et al. 1991–92; Lave et al. 1992; Dowlatabadi and Morgan 1993; Morgan and Dowlatabadi 1996). Between 1991 and 2001, Hadi Dowlatabadi led a team at Carnegie Mellon that built the Integrated Climate Assessment Model called ICAM. This model was constructed in the Analytica® software environment (Lumina 2011). From the outset, because we believed that describing and analyzing uncertainty should be the central focus of climate assessment, ICAM operated as a stochastic simulation that produced all of its outputs in the form of full probability distributions as a function of time and for different regions of the world.

We believed that some of the largest sources of uncertainty did not arise from uncertain coefficient values (although all the key coefficients were treated probabilistically). For this reason we populated the sub-parts of ICAM that dealt with climate, impacts and policy with a range of plausible alternative model functional forms (similar to what others term structural, systematic or epistemic uncertainty). We not only included alternative functional relations among key variables, we also included alternative normative assumptions about things that we knew would be important, such as time preference (Schelling 1995; Frederick et al. 2002; Heal 2009). We did this by adding a variety of logical "switches," in both the natural science and social science and behavioral parts of the model that allow the user to run the model with different combinations of structural and normative assumptions.

Several insights became quickly apparent (Morgan and Dowlatabadi 1996). For example, we found that across a wide range of assumptions no single climate policy was optimal for all parts of the world. Our most important finding was that, depending upon how we set the switches in ICAM, we could obtain a *very* wide range of results. It became obvious to us that the idea of framing integrated assessment in terms of a search for an "optimal" global climate policy, as opposed to looking for widely robust strategies, simply makes no sense. Unfortunately, for the past 15 years much of the integrated assessment community has gone merrily on seeking globally "optimal" policies.

4 Scenarios and their limitations

Besides integrated assessment modeling, scenario analysis has been widely adopted as a strategy for use in assessing the impacts of climate change. It is generally argued that scenarios are not intended to represent predictions about the future but rather serve as a tool to help people imagine a range of alternative futures. Sometimes scenarios are effective in achieving this objective. However, David Keith and I have argued (Morgan and Keith 2008) that too often rather than expanding peoples' judgment about the range of uncertainty about the future, scenario-based analysis leads to systematic overconfidence and to an underestimation of the range of possible future outcomes. This is because, through the operation of the cognitive heuristic of "availability," the more detail that one adds to the story line of a scenario, the more probable it will appear to most people, and the greater the difficulty they likely will have in imagining other, equally or more likely, ways in which the same outcome could be reached. (For more on the role of cognitive heuristics, the ubiquity of overconfidence, and their implications in the context of climate change, see Morgan et al. 2009.)

Recent years have seen raging arguments about whether one should attach probabilities to scenarios. In this connection, Keith and I have noted:

> Schneider (2001) and others have argued that without probabilities scenarios are of little value to climate scientists and impact assessors who are trying to understand how the climate is likely to evolve over the coming centuries…If we think of a scenario as describing a series of points over time through a multi-dimensional space of future possible socioeconomic conditions, then the developers of most of these scenarios are correct (but for the wrong reasons) that no probability should be assigned to scenarios. Viewed this way, scenarios cannot be assigned probabilities since, in any probability distribution over a continuous variable, the probability that attaches to any specific point value is zero. However, statements about the probability of scenarios *can* be made if the scenarios are specified using *ranges* of values for the socio-economic variables of interest. The probability that global energy use in 2100 is *precisely* 1,000 EJ/year must be zero; but it is perfectly sensible to assert under some set of assumptions, that in 2,100 there is a 20% chance that the value of global energy use will fall between 800 and 1,200 EJ/year. (Morgan and Keith 2008)

The climate community made progress when it moved from the previous highly detailed SRES scenarios (Nakicenovic and Swart 2000) to instead develop the much simpler representative concentration pathways (Moss et al. 2010). However, there is now a move by many in the assessment community to construct detailed socio-economic story lines for each of these. In this process, not enough attention is being paid to first thinking carefully about *why* this is being done, or about what assessors or decision makers will do with all the detail that is being added.

Two alternative strategies, which in my view have received way too little attention, are simple parametric or "what if" analysis, and backwards analysis. In backwards analysis, rather than run forward in the causal direction from emissions, to climate change to impacts, one starts with outcomes of particular concern and then work backwards to identify a small number of things that, if they happen, would be most problematic. Several of us tried very hard to promote such a strategy in the first US National Assessment (2000). However, as we noted in a subsequent evaluation of that experience:

> There was a strong inclination in most groups to approach the problem of assessment in a front-to-back manner (emissions → climate → impacts), rather than, for example, identifying key thresholds or nonlinearities in natural and social systems of concern and working backward to let those drive the work of the climate scientists and the choices of climates to be examined. At the Washington workshop on learning from the Assessment, the breakout group that considered this issue noted that there is "a big difference between driving an assessment from possible climate futures all the way to impacts as opposed to starting with people trying to manage messes in the world…" While such a threshold or parametric analysis may make sense to experienced policy analysts, it is clear from the National Assessment experience that such an approach is not readily comprehended by most nonexperts and does not come naturally to many natural and social scientists who are used to working forward through a causal chain.

> Thus, we conclude that an important element of preassessment guidance papers, and a training program, should be a tutorial in such methods, illustrated with concrete examples drawn from previous applications. (Morgan et al. 2005)

5 Uncertainty about the correct functional relationships among key variables

Uncertainty about the functional form that correctly characterizes the relationship among key variables in a model of physical, biological or social processes arises again and again in the context of climate change and its impacts. As explained above, in ICAM, we incorporated a range of alternative functional relationships in our models and explored the implication of switching between them. An alternative strategy is to model each plausible functional relationship, and then weigh each in proportion to the probability that one attaches to each being a correct description of reality. I am not aware of this approach having been adopted in the context of climate change. However, Evans and colleagues have applied such a strategy in the context of assessing carcinogenic potency (Evans et al. 1994a and 1994b) and Budnitz and colleagues have adopted a similar strategy in the context of assessing seismic risk (Budnitz et al. 1995 and 1998).

These strategies basically convert uncertainty about model functional form into uncertainty about a model parameter – namely the Bayesian or subjective probability that various alternative functional relationships correctly represent physical reality. Clearly, if the result is to serve as input to a non-liner model, it is better to keep the alternatives separate before running them through the model, reserving the possibility of combining until after making the non-linear transformation. However, in many cases, it may be best not to combine even then, but rather to use the range of results to gain improved insight for decision making.

Such strategies become less and less feasible as uncertainty grows. For example, while the physics of the ocean–atmosphere system will likely remain quite stable for centuries, it is far less clear that the functional relationships that describe socio-economic relationships will remain stable for similar periods. Some years ago, when I posed the problem of how best to model in such circumstances to a leading Bayesian philosopher, his response was that I should construct an ensemble of all the possible alternative models that might apply and then construct a probability distribution across that full ensemble of models. While this advice may be philosophically reasonable, it also carries the impractical implication that the less one knows, the more complex one's model should become. As an alternative, Casman, Dowlatabadi and I have suggested that as uncertainty grows one should progressively move from detailed models, to order-of-magnitude models, and ultimately on to simple bounding analysis (Casman et al. 1999).

6 Selecting the right tools and the need to develop new ones

While doing a good job of characterizing and analyzing uncertainty (Morgan and Henrion 1990), and of communicating uncertainty (Morgan et al. 2002), is very important, perhaps even more important is selecting the right tools to do climate-related assessment. Most of the conventional tools of policy analysis implicitly assume that:

1. There is a single (public-sector) decision maker who faces a single problem (in the context of a single polity);
2. Values are known (or knowable), static, and exogenously determined;

3. The decision maker should select a policy by maximizing expected utility;
4. The impacts involved are of manageable size and can be valued at the margin;
5. Time preference is accurately described by conventional exponential discounting of future costs and benefits;
6. The system under study can reasonably be treated as linear;
7. Uncertainty is modest and manageable.

As a number of us first articulated some years ago (Morgan et al. 1999), a useful way to think about the issue is illustrated in Fig. 8. Virtually all the classic conventional tools of policy analysis assume that one is addressing problems that lie near the origin in this space. However, many climate assessment issues lie far from the origin in this space. The implication of this is that before adopting and using classic tools for policy analysis to address such problems, one should think carefully about whether the assumptions upon which those tools are based remain valid.

Despite years of modeling that seeks an optimal global climate policy, it should be obvious to all that what is optimal for the Inuit of Northern Canada, the Quechua and Aymara-speaking peoples of the Andes, or the Anglo population of Australia will not be the same. How those, or dozens of other communities, will value goods, services and ecosystems 50 to 100 years from now is also deeply uncertain, and likely to depend in critical ways on cultural and path-dependent processes. For someone like me, who grew up in central New Hampshire, and would like my great-grandchildren to be able to enjoy a forest made up of white pine, birch, beach and sugar maple, the sort of change implied by Fig. 9, is anything but "marginal."

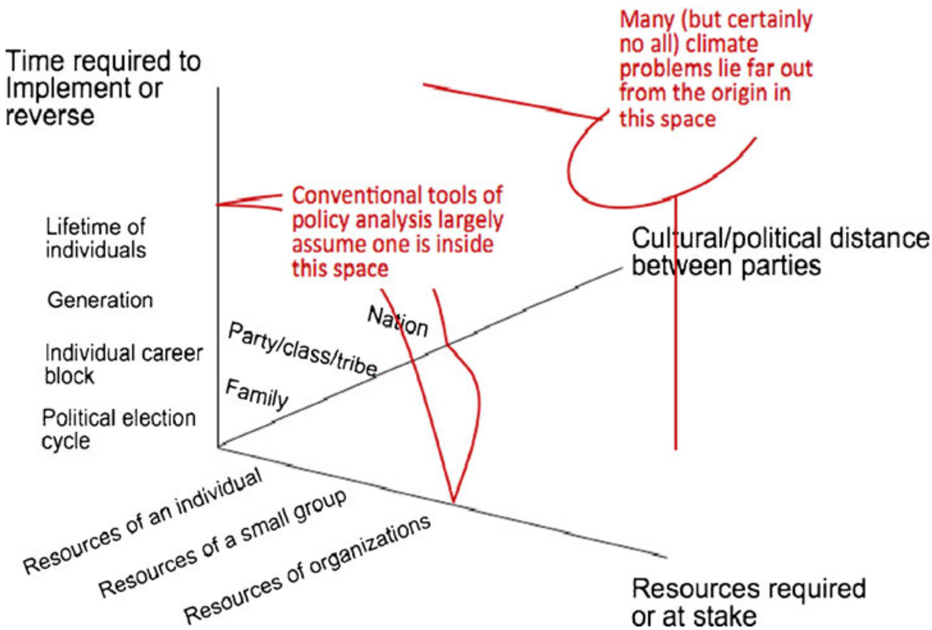

Fig. 8 Most classic tools for policy analysis have been developed to address problems that lie near the origin in this space. Many problems involving climate change lie far out from the origin, with the result that before adopting and using classic tools for policy analysis to address those problems, one should think carefully about whether the assumptions upon which they are based remain valid. Figure redrawn from Morgan et al. (1999)

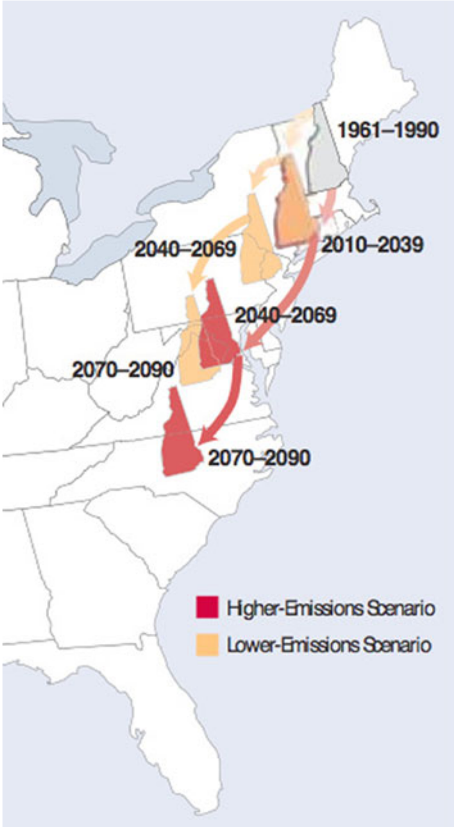

Fig. 9 Estimate from the North East Climate Impact Assessment of how the climate of New Hampshire may change over the course of the coming century. Clearly, the changes implied by such a shift in climate are anything but "marginal." Figure from Frumhoff et al. (2007)

In recent years, time preference has become a critical focus of debate in the assessment community. Many, but certainly not all, in the economic community have argued that this issue should be addressed using conventional exponential discounting and market rates. For example, in a slightly different context Viscusi (1995) writes:

> Chief among the results is that the estimated implicit rate of discount that workers use in valuing death risks does not differ in a statistically significant manner from the prevailing rates of return in financial markets during the time periods under study. Thus, there is no evidence to indicate that we should use a different rate of discount when weighting the long-term health benefits of policies that affect life extension as compared with other benefit and cost components that these policies may have. An appropriate *real* rate of return based on market interest rates is a reasonable starting point for this procedure. (Viscusi 1995)

There is a considerable literature in behavioral social science that addresses issues of time preference. In an unsuccessful attempt to inject some alterative thinking into the climate assessment community, our NSF-supported Climate Decision Making Center supported a review of that literature by Frederick et al. (2002). Figure 10 displays the range of results they found in reviewing published experimental studies. I sent copies of the resulting refereed paper to many involved in the fourth IPCC assessment, although as best I can tell it had no impact.

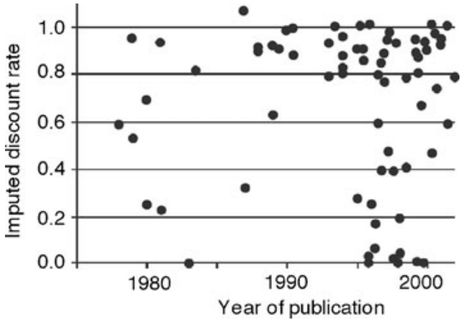

Fig. 10 Published values of experimentally determined discount factor by year of publication. Figure redrawn from Frederick et al. (2002)

In my view, the debate should not be over what coefficient to use in exponential discounting. Rather, we should be focused on the much more fundamental question of what strategies and functional forms we should be adopting with respect to valuing a variety of future changes that will profoundly impact future societies and ecosystems. Heal (2009) has taken an important next step in advancing discourse on this topic.

7 A few conclusions

The climate community has been a leader in dealing with uncertainty about coefficient values, and as Morgan et al. (2009) makes clear, has also made contributions to dealing with model uncertainty. More work is needed on both these fronts.

However, today, the even bigger challenge is for the assessment and decision making communities to assume a similar leadership role in developing and demonstrating new, more appropriate analytical methods and tools. Until that happens, conclusions reached about how to value the impacts of climate change, and how best to address climate mitigation, adaptation, and other responses, will be burdened by deep, if unstated, uncertainty.

In that connection:

1. Good methods are available to deal with uncertainty about the value of key coefficients. However, these methods have not yet seen adequate use in climate assessments, such as those conducted by the IPCC. It would not be difficult to do this. For example, once they have read the relevant literature, but before they begin discussions to reach a group consensus, each member of an authoring team could be asked to engage in an expert elicitation about the value of a few key coefficients. The range of results could then serve as an input to inform the process of developing a group consensus judgment.
2. In contrast to methods to explore uncertainty about the value of key coefficients, methods to address uncertainty about the functional form that most accurately describes the relationship among key variables are still in their infancy and are clearly not ready for use in contexts such as IPCC assessments. There is an urgent need for research to further develop and demonstrate such methods.
3. Those of us who perform impact and policy analysis need to think much more carefully before we simply pick up conventional tools such as benefit-costs analysis and exponential discounting and apply them to climate problems. More generally, we need to devote much greater attention to research that develops and demonstrates new, more appropriate methods to address problems that lie outside the "conventional zone" in Fig. 10.

4. Just as the USGCRP did a good thing in promoting a more systematic consideration of uncertainty by commissioning CCSP 5.2 on "Best practice approaches for characterizing, communicating, and incorporating scientific uncertainty in decision making" (Morgan et al. 2009), in a few years, once the research community has made more progress, similar best practice reviews of issues such as time preference and other issues that arise when performing analysis of problems that lie outside the "conventional zone" in Fig. 10, should be commissioned.

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