Some Thoughts on Uncertainties, Scenarios, and Thinking about the Future

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Like all of you…

…I deeply miss Steve, who I have counted among my friends and colleagues since we first got acquainted in climate sessions at AGU meetings back in the late '60s.

I am going to talk about some subjects that were close to his heart, and I wish he could be here to tell me where I have gone wrong.

First, a few comments on uncertainty:

If you have not seen CCSP 5.2, I urge you to download a copy from:

www.globalchange.gov/publications/reports/scientific-assessments/saps/sap5-2

I was lead author on this report, ably assisted by:

Hadi Dowlatabadi; Max Henrion; David Keith; Robert Lempert; Sandra McBride; Mitchell Small, and Thomas Wilbanks.

Best Practice Approaches for Characterizing, Communicating, and Incorporating Scientific Uncertainty in Climate Decision Making

U.S. Climate Change Science Program
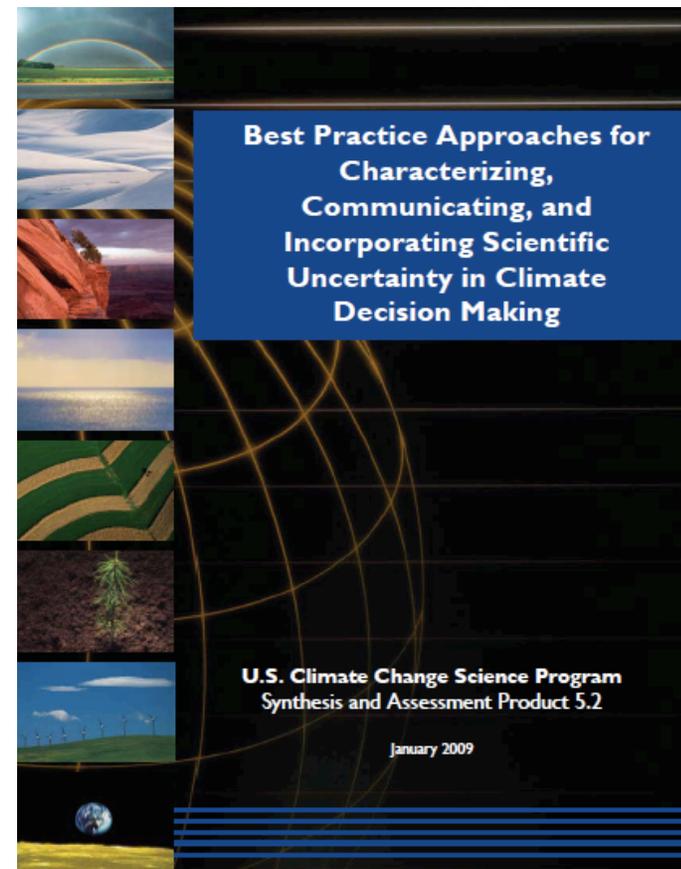Synthesis and Assessment Product 5.2

January 2009

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6. Propagation and analysis of uncertainty
7. Making decisions in the face of uncertainty
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Rather than talk about any of the details in this report, I'll instead comment on just three points.

Likelihood and Confidence

I have never been very happy with the formulation in the IPCC guidance.

If you have a precise and unambiguous statement about a possible future state of the world, then your subjective probability that this statement is true should suffice, i.e.:

$$P_{true} = 0.X \qquad\qquad P_{false} = 1-0.X$$

If you are uncertain about the value of some unknown but well defined quantity (e.g., climate sensitivity) then a subjective probability density function (informed by all available evidence and your state of knowledge) should suffice:

$$P(Q|\text{present evidence})$$

However…

…these assertions run into two problems. For any single individual, how confident the assessor is about the probability he or she assesses will depend on how solid the available evidence is.

For any group of individuals trying to collectively make such an assessment, things are further complicated by the fact that different folks will likely interpret the available evidence of difficulty.

Either of these situations can be characterized in terms of "second-order uncertainty."

On statements about the world:

In CCSP 5.2, we argue that it is generally good to try to avoid second-order uncertainty when possible. However, Figure 9.2 in that report suggests a strategy to deal with low confidence when assessing a statement about the future state of the world:

probability that a statement is true
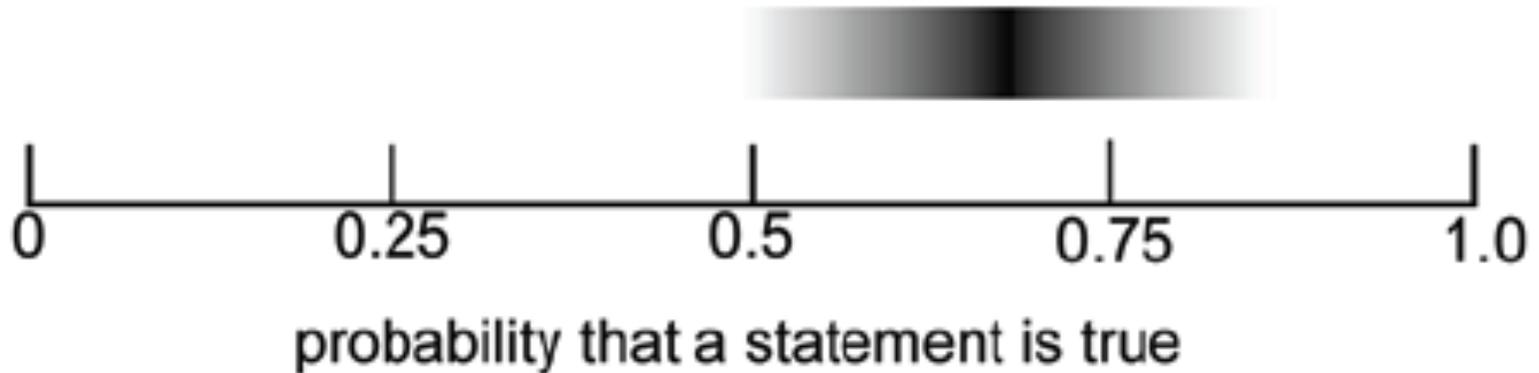
On assessments of an uncertain value:

When different experts have different views, report them side-by-side as box plots, so as to directly convey a sense of the distribution of views across the scientific community. For example:

Source: Kirsten Zickfeld, M. Granger Morgan, David Frame, David W. Keith, "Expert Judgments About Transient Climate Response to Alternative Future Trajectories of Radiative Forcing," *PNAS*, 2010.
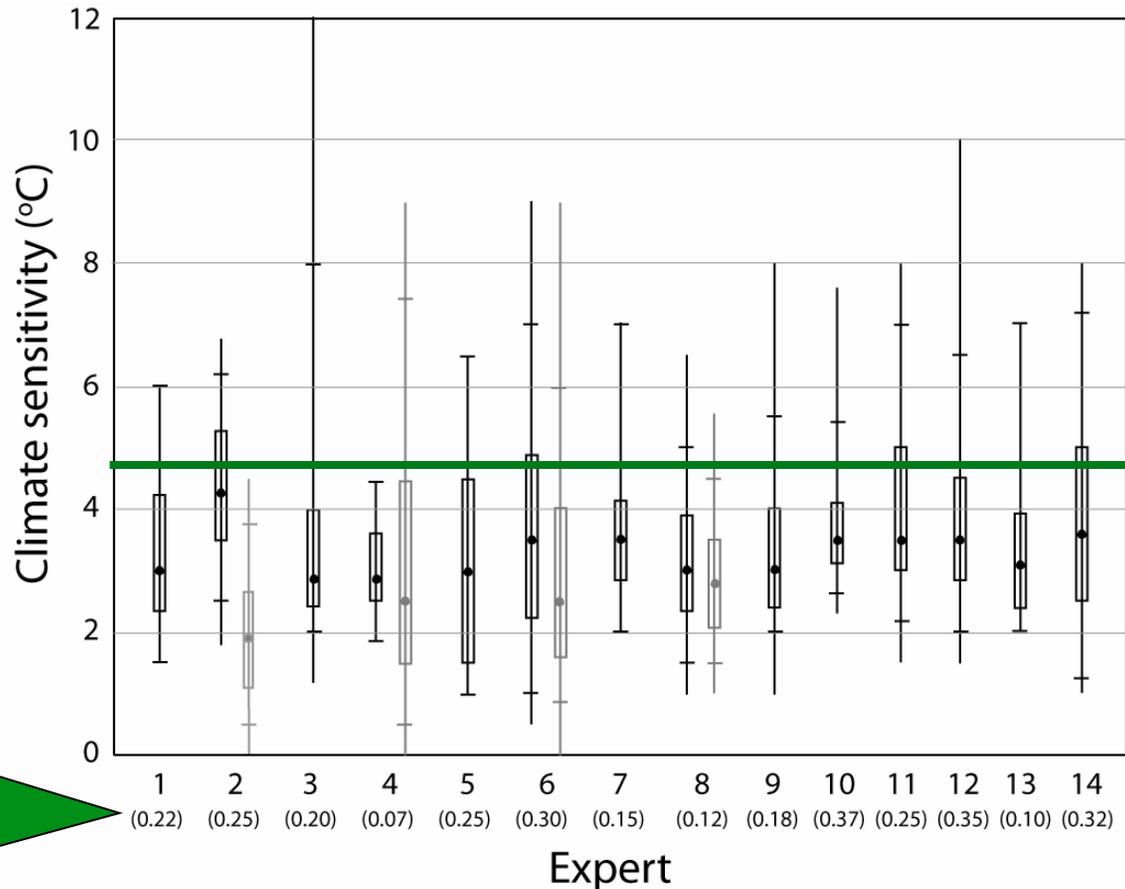
Probability allocated to values above 4.5°C

Expert elicitation and IPCC

At the planning meeting for the IPCC 5th assessment in Venice in July 2009 there was a decision that the next assessment could consider making use of expert elicitation.

Based on comments there, and on past experience, I would not expect WGI to do this, but I've offered help if a writing team in WGII or III would like to try.

At the request of Ron Prinn, we did an elicitation on radiative forcing from aerosols for the last assessment, but it was not cited or used by WGI because "expert elicitation is not science."

Source: M. Granger Morgan, Peter Adams, and David W. Keith, "Elicitation of Expert Judgments of Aerosol Forcing," *Climatic Change*, 75, 195-214, 2006.

Development of a traceable account

In most statements about uncertainty in past IPCC reports, it has been essentially impossible to know where the judgments have come from or how they were reached.

I can illustrate this with a short story:

> In several of the expert elicitation studies we have run, we think we have seen evidence that the sort of informal collective judgment process that IPCC writing teams have been using yield tighter distributions than the impression one gets from looking across a set of individually elicited distributions.

A story…(Cont.)

Together with two colleagues in behavioral social science we have a Ph.D. student trying to examine this possibility experimentally (using more general domains of knowledge such as weather, transit times, sporting events, etc.). Alex recently sent me a note that read: "I now pretty much understand what you have done in your elicitations, but despite having read a bunch of IPCC reports I can't really get any sense at all of how the IPCC generated their judgments of probability." He went on to ask for advice on how to learn this so that he could create a process as close as possible to that used by writing teams in his experimental studies.

Prof. Wändi Bruine de Bruin (SDS/EPP)

Prof. Paul Fischbeck (SDS/EPP)

Ph.D. student Alex Davis (SDS)

High tails

The IPCC process has placed heavy emphasis on getting consensus judgments about likely outcomes. That is fine, but what many decision makers most need is clear insight about the high tails of relevant distributions.

For example: A port operator about to install new piers with 2 m of freeboard probably doesn't care whether the best estimate value of sea level rise will be 0.3 or 0.5 meters over the useful life of the pier. On the other hand, he or she may be *very* interested to learn that several experts think there is a greater than 0.05 probability that seas will rise enough to make the piers useless before the end of their lifetime.

I understand that such estimates are much harder to produce: there is far less consensus: experts don't want to be accused of being alarmist, etc. But, we need to do a better job on this.

OK…enough about uncertainty

For the balance of this talk let me offer some thoughts on scenarios and thinking about the future.

I'll begin with three questions:

1. What quantities (or systems) do we really need to project or forecast and why do we need them?

2. Can we actually know the likely future values (or structures) of the quantities (or systems) of interest?

3. If so, how well can we know them?

What do we really need?

Note that while the SRES storylines laid out a great deal of detail, virtually no one made use of most of that detail.

For example, all that most climate modelers needed was a set of plausible future trajectories of emissions.

In my view, the move to the development of "representative concentration pathways" (RCPs) is a step in the right direction…

…but now, of course, folks are trying to hang a bunch of details on those RCPs.

Nobody is very good at predicting the future

Summary of forecasts of U.S. primary energy consumption for the year 2000 compiled by Smil (2003) as a function of the date on which they were made.
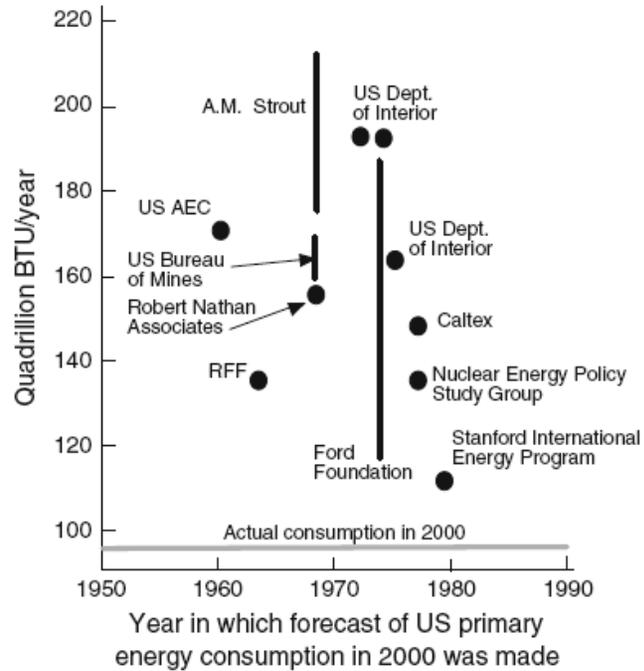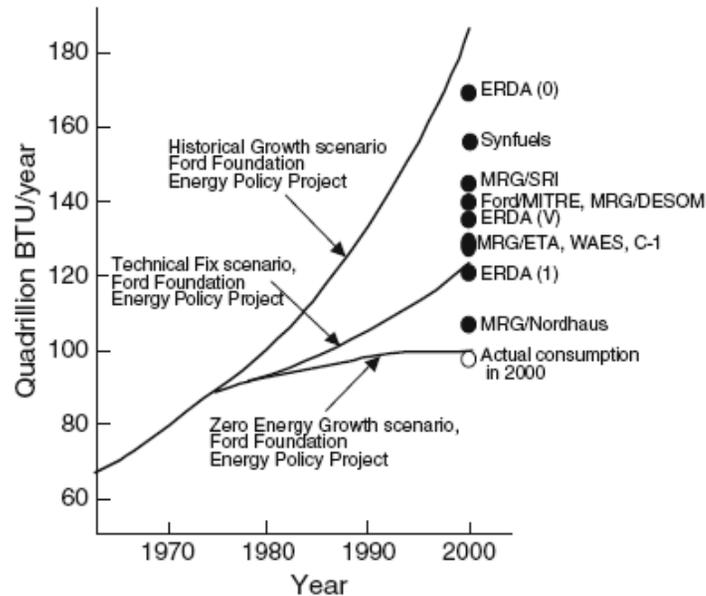
Comparison of forecasts of U.S. primary energy consumption for the year 2000 compiled by Greenberger in the early 1980s compared with three scenarios developed by the Ford Foundation Energy Project.

Figures from Morgan and Keith, 2008

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The fact that past projections…

…have done so poorly suggests strongly that in many cases we should adopt some different strategies.

I'll talk about three:

1. Model switching

2. Bounding analysis

3. Working the problem backwards

Models for…

…different parts of the climate problem are believable for different periods of time into the future.

For example, I would not trust a general equilibrium model of the world's economy to be very reliable for more than perhaps a decade or two. In my view, running it out a century or more is nuts.

## The Bayesian philosopher's solution:

Build separate models of all the possible futures, then combine them all by weighting each according to its likelihood of being the correct model.

Implication: the less you know, the more complicated your analysis should become.

An engineer's solution

Start with a complex model, merge to simpler order-of-magnitude models as you become less confident of the results, and finally switch to bounding analysis

For details see:
Elizabeth A. Casman, M. Granger Morgan and Hadi Dowlatabadi, "Mixed Levels of Uncertainty in Complex Policy Models," *Risk Analysis*, *19*(1), 33-42, 1999.

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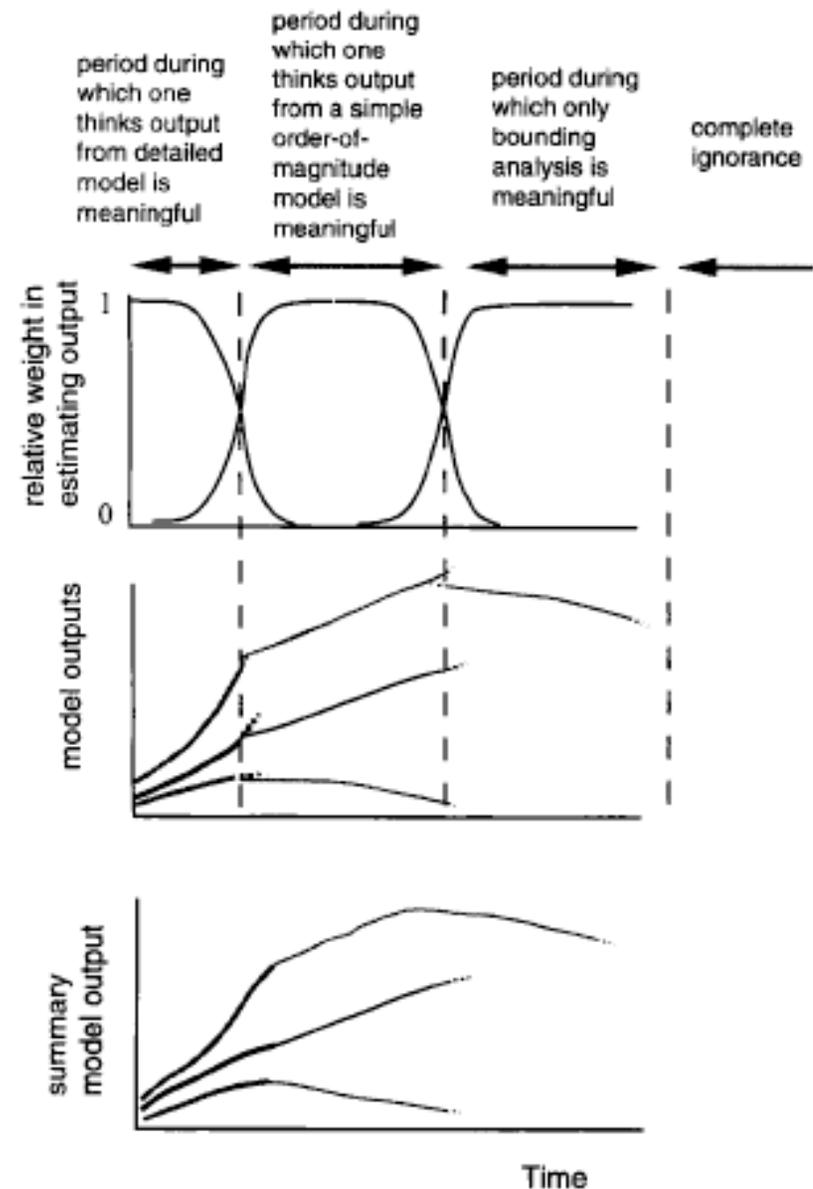
Problems with conventional scenarios

Analysts often use scenarios based on detailed story lines that spell out "plausible alternative futures." Typically no probabilities are assigned.

Rather than expanding people's judgment about the range of uncertainty about the future, such scenario-based analysis often leads to systematic overconfidence and an underestimate of the range of possible future outcomes.

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.

Improving the way we think about projecting future energy use and emissions of carbon dioxide

M. Granger Morgan · David W. Keith

Abstract A variety of decision makers need projections of future energy demand, CO_2 emissions and similar factors that extend many decades into the future. The past performance of such projections has been systematically overconfident. Analysts have often used scenarios based on detailed story lines that spell out "plausible alternative futures" as a central tool for evaluating uncertainty. No probabilities are typically assigned to such scenarios. We argue that this practice is often ineffective. Rather than expanding people's judgment about the range of uncertainty about the future, scenario-based analysis is more likely to lead to systematic overconfidence, to an underestimate of the range of possible future outcomes. We review relevant findings from the literature on human judgment under uncertainty and discuss their relevance to the task of making probabilistic projections. 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. We suggest that scenario based approaches make analysts particularly prone to such cognitive biases, and then outline a strategy by which improved projections, tailored to the needs of specific decision makers, might be developed.

For those of us who work on climate and energy policy it would be extremely useful to be able to predict a few simple things such as the future demand for energy and the future mix of energy technologies over the coming decades—if not as sharp point estimates, then at least as well-calibrated subjective probability distributions. However, the track-record of past efforts to make such predictions is anything but

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Indeed, in developing this paper…

…I found the following very troubling (and almost certainly true) assertion from one of the strong proponents of using scenarios:

> "Practitioners can find several advantages in using scenarios. First, they can use scenarios to enhance a person's or group's expectancies that an event will occur. This can be useful for gaining acceptance of a forecast…Second, scenarios can be used as a means of decreasing existing expectancies…Third…scenarios can produce greater commitment in the clients to taking actions described in them."

Source: W. Larry Gregory, "Scenarios and Acceptance of Forecasts," 519-540, in *Principles of Forecasting: A handbook for researchers and practitioners*, J. Scott Armstrong (ed.), 849pp., Kluwer Academic, 2001.

Probability and Scenarios

As most of you know, Steve Schneider 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.

"While acknowledging the logic of avoiding fruitless debate, I strongly argued…that policy analysts needed probability estimates to assess the seriousness of the implied impacts; otherwise they would be left to work out the implicit probability assignments for themselves… I urged the expert group to provide a subjective probability assessment for less expert users, but I was not persuasive enough, and the SRES authors expressed "no preference" for each scenario."

Source: Stephen H. Schneider, "What Is 'Dangerous' Climate Change?," *Nature*, *411*, 17-19, May 3, 2001.

Of course, Steve understood…

…that if we think of a scenario as describing a series of points over time through a multi-dimensional space of future possible socioeconomic conditions, scenarios cannot be assigned probabilities since, in any probability distribution over a continuous variable, the probability that attaches to any specific point value or line through that space is zero.

BUT…one *can* attach probabilities to intervals in that space.
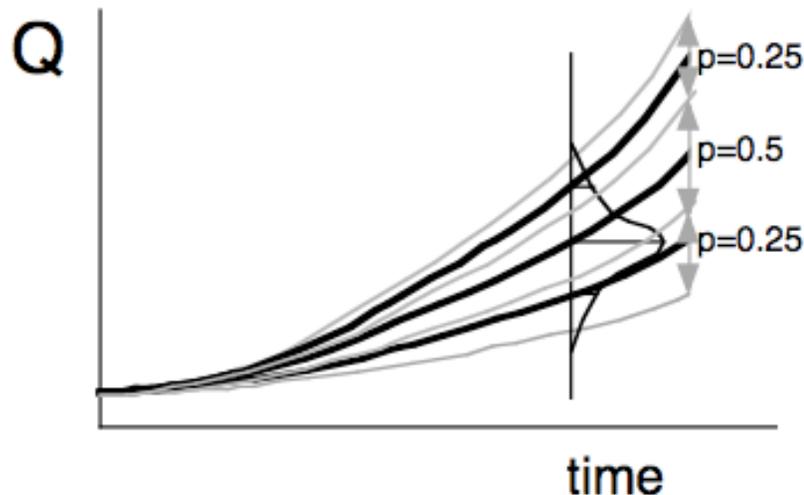
For more elaboration of these and related ideas see:

M. Granger Morgan and David Keith, "Improving the Way We Think About Projecting Future Energy Use and Emissions of Carbon Dioxide," *Climatic Change*, *90*(3),189-215, October 2008.
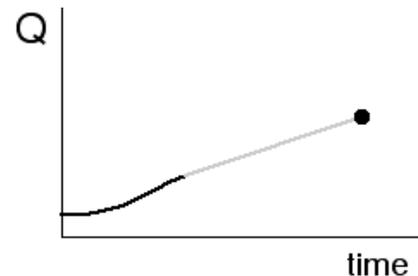
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2. Bounding Analysis

Given some quantity Q whose value you want to project in the future, rather than developing a few very detailed "story lines" instead work to build a list of:

All the developments that might lead to its having a high value
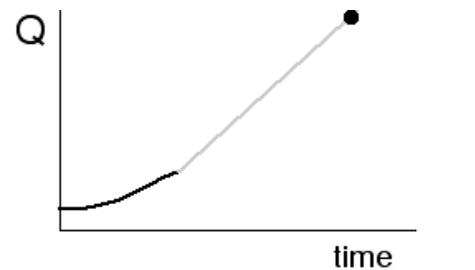
All the developments that might lead to its having a low value

Then, subject the resulting lists and analysis to repeated critical review and revision.

Again for details see: M. Granger Morgan and David Keith, "Improving the Way We Think About Projecting Future Energy Use and Emissions of Carbon Dioxide," *Climatic Change*, *90*(3), 189-215, October 2008.

An energy example

My recent Ph.D. graduate Vanessa Schweizer (who is about to move to a Post-doc at NCAR) has produced an illustration of this approach for the case of U.S. electricity demand in 2050.
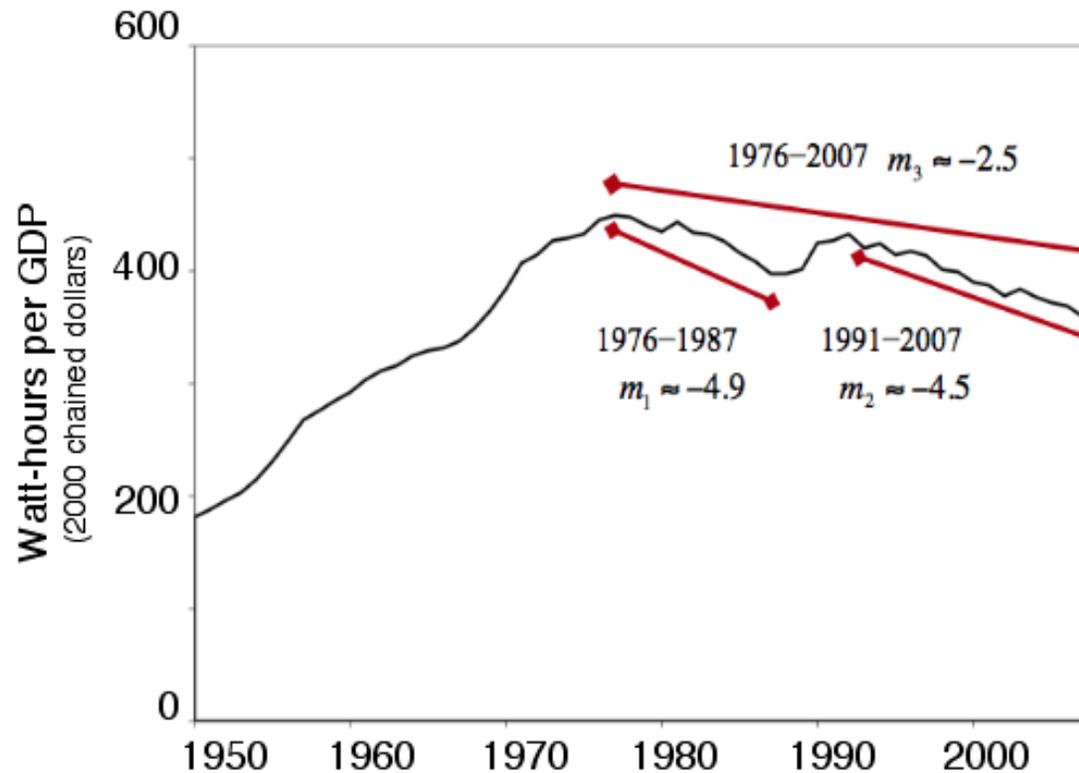

Dr. Vanessa Schweizer (EPP)

She starts with a high and low assumption about GDP growth.

- High assumes continued average rate since 1949 of 3.3% per year resumes, and continues for the next 40 years.

- Low assumes that the AEO/EIA low growth case of 1.8% per year continues for the next 40 years.

Bounding example…(Cont.)

Next she considers the evolution of electric energy/GDP

For the high case she assumes - 2.5W-hr/$GDP/year

For the low case she assumes - 4.5W-hr/$GDP/year

Bounding example…(Cont.)

Next, she identifies a set of technology and climate developments that could result in higher and lower electric demand.

I won't take the time here, but she does order-of-magnitude assessments for:

- Plug electric vehicles
- Air conditioning and heat pumps
- Water desalination
- Industrial process heating
- Energy efficiency
- And a few others that turned out not to be significant.

With the exception of climate, she assume no correlation structure.

She obtains these high and low bounds:
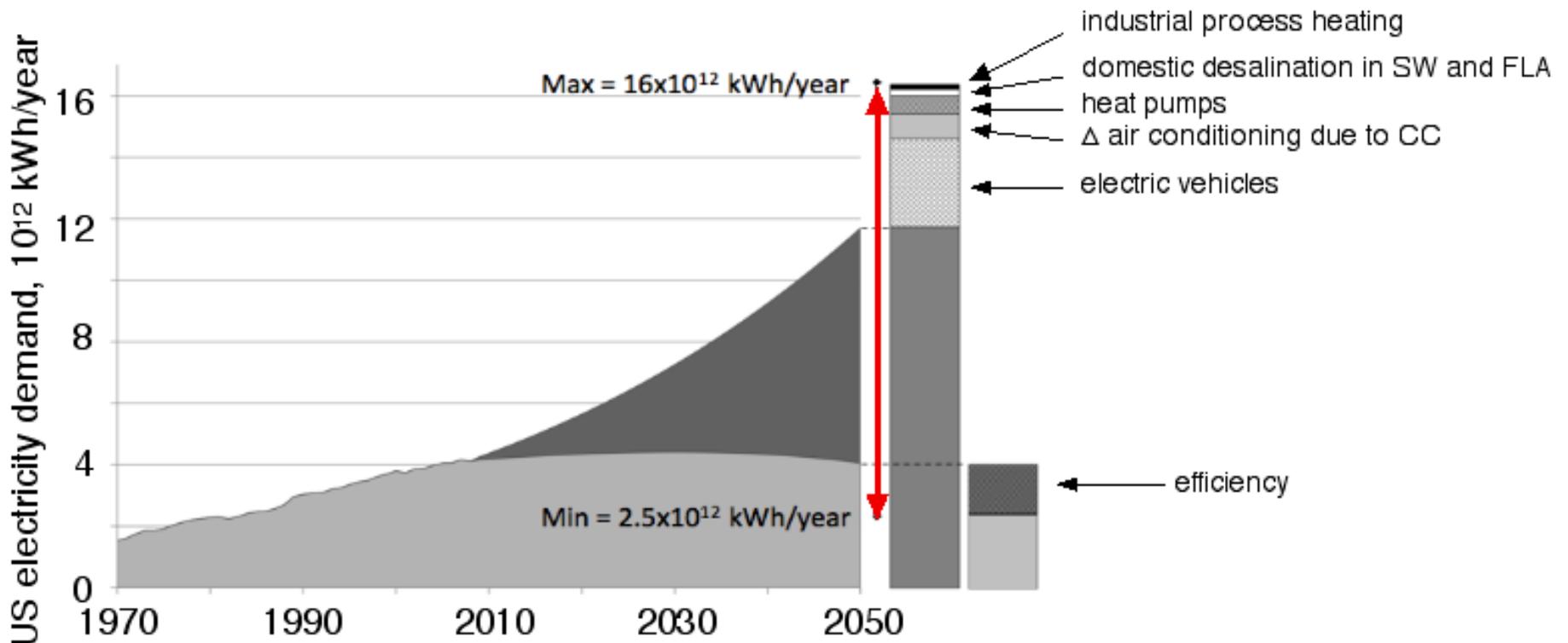
You might make different assumptions and derive different results, but the point is to try to set plausible bounds rather than make a forecast.

3. Working the problem backwards

We all have a strong desire to work in the direction of the causal influences:

| GHG emissions	→	Δ climate	→	Δ impacts	→	Valuation of impacts
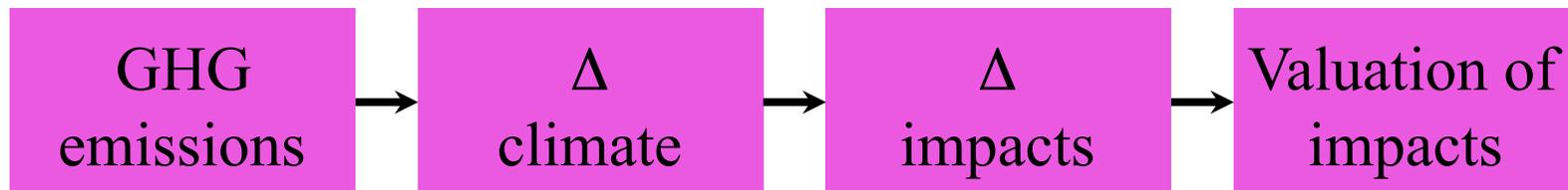
However, there is often so much uncertainty in the earlier steps that if we are honest, we get PDFs on the latter stages that are so broad as to be almost useless.

Sometimes a better strategy is to ask, what possible outcomes might we most care about, and then work backwards, to ask, what sorts of things would have to happen to lead to those outcomes.

Backwards…(Cont.)

Most people find it *very* hard to approach things in this way.

For example, several of us at this meeting tried to get the several regional and sectoral groups to adopt such a strategy in the 2000 U.S. National Assessment but found that folks without significant analytical experience were not comfortable doing this. Nevertheless, the approach deserves serious consideration.
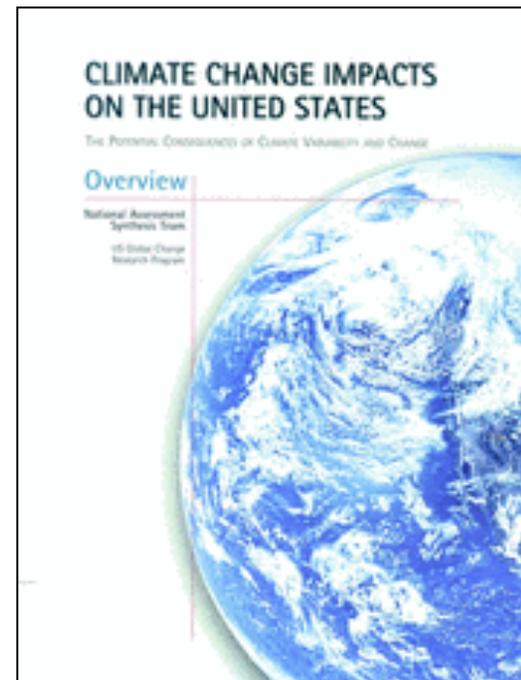
For details on the National Assessment experience see:
M. Granger Morgan, Robin Cantor, William C. Clark, Ann Fisher, Henry D. Jacoby, Anthony C. Janetos, Ann P. Kinzig, Jerry Melillo, Roger B. Street, and Thomas J. Wilbanks, "Learning from the U.S. National Assessment of Climate Change." *Environmental Science & Technology*, *39*, 9023-9032, 2005.

Sometimes the methodology…

…one uses introduces the largest source of (typically unstated) uncertainty.

The approach that is taken to time preference can be critically important.

While exponential discounting is widely used, there is considerable behavioral evidence that this is not always an appropriate way to address an issue that is as broad in scope and time as climate change. In the hope of sparking some new thinking by folks in the fourth assessment, we supported a comprehensive (50 page) review and study of this topic by a group in our Department of Social and Behavioral Science.

Time preference

Source:

Shane Frederick, George Loewenstein and Ted O'Donoghue, "Time Discounting and Time Preference: A critical review," *Journal of Economic Literature*, *XL*, 351-401, 2002.

Time Discounting and Time Preference: A Critical Review

SHANE FREDERICK, GEORGE LOEWENSTEIN,
and TED O'DONOGHUE[1]

1. Introduction

INTERTEMPORAL CHOICES—decisions involving tradeoffs among costs and benefits occurring at different times—are important and ubiquitous. Such decisions not only affect one's health, wealth, and happiness, but, may also, as Adam Smith first recognized, determine the economic prosperity of nations. In this paper, we review empirical research on intertemporal choice, and present an overview of recent theoretical formulations that incorporate insights gained from this research.

Economists' attention to intertemporal choice began early in the history of the discipline. Not long after Adam Smith called attention to the importance of intertemporal choice for the wealth of nations, the Scottish economist John Rae was examining the sociological and psychological determinants of these choices. In section 2, we briefly review the perspectives on intertemporal choice of Rae and nineteenth- and early twentieth-century economists, and describe how these early perspectives interpreted intertemporal choice as the joint product of many conflicting psychological motives.

All of this changed when Paul Samuelson proposed the discounted-utility (DU) model in 1937. Despite Samuelson's manifest reservations about the normative and descriptive validity of the formulation he had proposed, the DU model was accepted almost instantly, not only as a valid normative standard for public policies (e.g., in cost-benefit analyses), but as a descriptively accurate representation of actual behavior. A central assumption of the DU model is that all of the disparate motives underlying intertemporal choice can be condensed into a single parameter—the discount rate. In section 3 we examine this and many other assumptions underlying the DU model. We do not present an axiomatic derivation of the model, but instead focus on those features that highlight the implicit psychological assumptions underlying the model.

This paper provides an extensive review…

…of available empirical evidence on time preference, adds some new empirical results, and raises fundamental concerns about the appropriateness of always using conventional discounted utility (DU).

For example, here is a summary Frederick et al. provided of results from past behavioral studies of time preference:
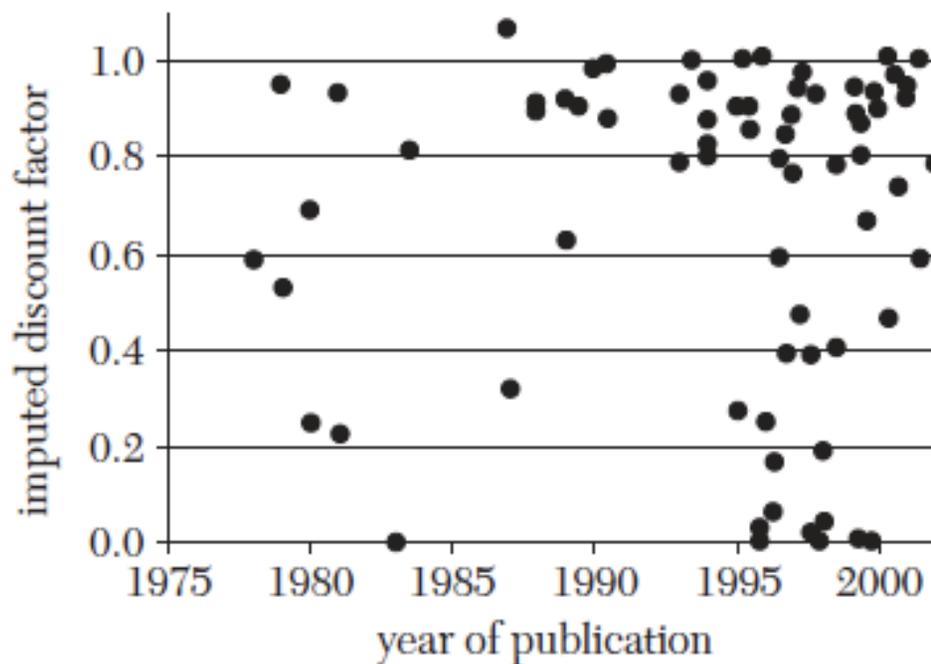
Time preference…(Cont.)

Frederick et al. conclude in part:

> "The DU model, which continues to be widely used by economists, has little empirical support, even its developers - Samuelson, who originally proposed the model, and Koopmans, who provided the first axiomatic derivations - had concerns about its descriptive realism, and it was never empirical validated as the appropriate model for intertemporal choices. Indeed, virtually every core and ancillary assumption of the DU model has been called into question by the empirical evidence collected in the past two decades…"

Unfortunately, as with our expert elicitation work, the IPCC did not cite or make any use of this work.

Finally, assumptions about…

…who the decision maker(s) is (or are) are often also a critical source of uncertainty. For example, it remains common to assume that there is a single utility maximizing decision maker for the entire planet or a large region.

Hadi Dowlatabadi showed many years ago that what is optimal climate policy for one nation or group is often not optimal for another. While economic impact analyses get fancier and fancier, too many continue to ignore this fact, or make overly strong assumptions about the preferences of different regions.

Source: M. Granger Morgan and Hadi Dowlatabadi, "Learning from Integrated Assessment of Climate Change," *Climatic Change*, *34*, 337-368, 1996.

Some bottom lines on scenarios

Before diving in to the details of building scenarios or other projections of the future, it is important to ask:

- Why do we need the result we hope to get and what are we going to do with it?

- Can any front-to-back projection we build pass the "laugh test"? Would you bet half of next year's salary on it?

- If we can't get a meaningful result of the kind we want, are there alternative approaches (order-of magnitude, bounding, etc.) that can get us something that is defensible?

- Are methodological assumptions being made in the analysis in which the projections will be used that introduce unstated uncertainties that are as great or greater than the uncertainties in the quantities or systems that we are projecting?

Thanks