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Abstract: Uncertainty bedevils some—but by no means all--components of the science and impact assessments of climate change. Uncertainty will not be eliminated from many aspects any time soon, so the best way to help policy-makers is to try to forge a consensus about the degree of confidence that can be assessed to each important conclusion. Thus a risk-management approach seems the most effective strategy for managing risks to our planetary life support system associated with human activities that are changing our climate. Classifying conclusions into those that are *well established*, fall in a category of *competing explanations* or remain *speculative* is one established approach to classifying risk, which, in turn, allows risk management activities to be built on a firmer factual foundation.

Biography. My name is Stephen Schneider. I am the Melvin and Joan Lane Professor for Interdisciplinary Environmental Studies, Professor of Biology, Professor (by courtesy) of Civil and Environmental Engineering, and a Senior Fellow in the Woods Institute for the Environment at Stanford University. I received my Ph.D. in Mechanical Engineering and Plasma Physics from Columbia University in 1971. Subsequently, I studied the role of greenhouse gases and suspended particulate material on climate as a postdoctoral fellow at NASA's Goddard Institute for Space Studies. From 1972 to 1996, I worked at the National Center for Atmospheric Research (NCAR), where I co-founded the Climate Project. I have consulted with federal agencies and/or White House staff in the Nixon, Carter, Reagan, G.H.W. Bush, Clinton, G.W. Bush and Obama administrations. In 2002 I was elected to membership in the US National Academy of Sciences.

I have been actively involved with the IPCC (Intergovernmental Panel on Climate Change), I was a co-author of "Uncertainties in the IPCC Third Assessment Report: Recommendations to Lead Authors for More Consistent Assessment and Reporting" in 2000 and of the cross-cutting theme paper #4: "Assessing the Science to Address UNFCCC Article 2" in 2004. I contributed to all four IPCC Assessment Reports and was most recently a Coordinating Lead Author of Working Group II Chapter 19, "Assessing Key Vulnerabilities and the Risk from Climate Change." For the 2001 IPCC Third Assessment Report (TAR) and the 2007 Fourth Assessment Report (AR4), I served as a member of the Core Writing Team for each of the Synthesis Reports, which integrate the contributions of Working Groups I, II, and III. Most recently, I participated in the IPCC Scoping Meeting for the Fifth Assessment Report in July 2009 and the IPCC Workshop on Detection and Attribution in September 2009.

Introduction. It is already well established that human activities are changing the climate. But how large and how fast will these changes be? What systems will be only partly disturbed and what other systems seriously disrupted? And how can our policy choices reduce the threat they pose to natural and social systems?

The policy problem is hard because the global scale of climate change and its subtly intensifying impacts contrast uneasily with the short-term, local-to-national scales of most management systems. Furthermore, significant uncertainties plague projections of climate change and its consequences.

Such projections stretch the traditional scientific method to directly test hypotheses because there can be no data for the future before the fact. Any prognostication into that unknown territory is, by definition, a model of the factors that are believed to determine how the future will evolve. But even though we can never fully solve the climate prediction problem we can go a long way toward bracketing probable outcomes, and even defining possible outliers.

Progress here depends on an international community of scholars, who repeat what others have done with different computer models, make comparisons across models of various designs, compare relevant aspects of simulations to existing observational data to test model performance from retrodiction of past changes, and pioneer new models as data and theory advance. Back in the early 1970s, when a reporter asked how long this model building and validation process would take to achieve high confidence, I said that our models were "like dirty crystal balls, but the tough choice is how long we clean the glass before we act on what we can make out inside." That is still the issue, even as models become more sophisticated and simulate the Earth's conditions increasingly well. What constitutes "enough" credibility to act is not science but a subjective value judgment on how to gauge risks and weigh costs and benefits—often in incommensurate units like dollars versus species lost or inequity generated.

Modeling Future Climate. How large are the scientific uncertainties, though? People often say that meteorologists' inability to predict weather credibly beyond about 10 days bodes ill for long-range climate projection over decades. This misses a key difference between the instantaneous state of the atmosphere—weather—versus its time and space averages—climate. Even though the evolution of atmospheric conditions is inherently chaotic and the slightest perturbation today can make a huge difference in the weather a thousand miles away and weeks hence, large-scale climate shows little tendency to exhibit chaotic behavior (at least on timescales longer than a decade). Good models can thus make reasonable climate projections decades or even centuries ahead if the processes forcing change are large enough to detect above the background "noise" of the climate system—the unpredictable part. The Intergovernmental Panel on Climate Change (IPCC)'s laboriously compiled projections combine such modeling with scenarios for greenhouse gas emissions based on different assumptions about economic growth, technological developments, and population increase (IPCC, 2000).

These scenarios, despite major differences in emissions, show paths for global temperature increase that do not diverge dramatically until after the mid-21st century. This has led some to declare that there is very little difference in climate change across scenarios, and therefore, emissions reductions can be delayed many decades. That is a big mistake. It takes many decades to replace current polluting energy systems. There is also delay between emissions and temperature change due to the thermal inertia in the climate system caused by the large heat capacity of the oceans. After the mid-21st century, there are large differences based on emissions over the next few decades in the projected temperature increases—and the risks of associated dangers—for the late 21st century and beyond. Some of these risks imply irreversible changes.

Much of the uncertainty contributing to the ranges of projected future temperature increase derives from the so-called climate sensitivity. How much warming can we expect a given amount of greenhouse gas to cause? It is often estimated as the equilibrium global mean surface temperature increase due to a doubling of atmospheric CO_2 from pre-industrial levels of about 280 Parts Per Million (PPM). The IPCC estimates that it is "likely" (there is a 66-90 percent chance) that the climate sensitivity is between 2 and 4.5 ºC and roughly a 5-17 percent chance that it is above 4.5 ºC (with the remainder being the chance it is less than 2 ºC). They also offered a "best guess" of 3 ^{o}C climate sensitivity. This is consistent with estimates made from looking at paleo climatic changes, where estimation of climate sensitivity is of the same magnitude as that estimated from climate models. If anything, paleo-climattic estimates are more prone to larger climate sensitivities and show little likelihood of smaller values less than 2 ^{o}C warming for a doubling of CO_2.

Our uncertainty goes beyond scientific understanding of the scale and distribution of climate changes from any single scenario of increasing greenhouse gases to include the trajectory of human development and our adaptive capacity. Moreover, future greenhouse gas emissions are heavily dependent on policy choices worldwide. But we do know that if we wait to act until an increase in undesirable impacts occur, the inertia in the climate system and in the socioeconomic systems that produce greenhouse gas emissions will have committed us to even more severe impacts stretched out over many decades to centuries.

Risk management framing is a judgment about acceptable and unacceptable risks. That makes it a value judgment. As with the Bayesian approach to probability, many traditional scientists are uncomfortable with that. I am one of them, but I am more uncomfortable ignoring the problems altogether because they don't fit neatly into our paradigm of "objective" falsifiable research based on already known empirical data.

Systems science also alerts us to the possibility of "surprises" in future global climate—perhaps extreme outcomes or tipping points which lead to unusually rapid changes of state. By definition, very little in climate science is more uncertain than the possibility of "surprises." But it is nevertheless a real one. Even so, it took several rounds of assessments just to get IPCC to mention surprises, let alone discuss formal subjective probabilistic treatment of such potentially irreversible, large changes.

Communicating Complex Science. We cannot eliminate all of the important scientific uncertainties, but we can be more precise about their extent. But that is only part of the scientists' job. We also have a responsibility to communicate all of this as well as we can. Communicating this complex systems science to policy makers and the public is difficult. Too often, confusion reigns when an advocate for strong policy cites a well-established severe outcome as the most important consideration, and another advocate from some enterprise institute disliking public control of private decisions cites speculative components of the systems analysis as if that is all there were. Not surprisingly, politicians, media, and just plain folks get frustrated by this "dueling scientists" mode of presentation, an unfortunate staple of the mainstream media.

Professional training also leads too many scientists to "bury our leads", as American journalists would put it, rather than finding effective ways to communicate complex ideas. Being straightforward and understandable is a challenge given the strong scientific tradition of full disclosure, which makes us lead with our caveats, not our conclusions. But what I call the "double ethical bind"—be effective in public versus full disclosure of the caveats—is not unbridgeable. It calls for the scientist to develop a hierarchy of products—ranging from sound bites on the evening news to get our findings headlined on the agenda, to short but meatier articles in semi-popular journals like *Scientific American*, to more in-depth websites, to full length books in which that smaller fraction of the public or policy worlds that actually want the details about the nature of the processes and how the state of the art has evolved—can find them (e.g., as I do in *Science as a Contact Sport*--Schneider 2009). Yes, it is very time-consuming, but it is also necessary for those in complex systems science fields like climate science to simultaneously be effective in public messaging and to honestly separate the components of this complex systems science that are well established from those best characterized as competing explanations from those which are still speculative.

Along with climate projections, scientists also have to explain how systems science gets done. We cannot usually do traditional "falsification" controlled experiments. What we can do is assess where the preponderance of evidence lies, and assign confidence levels to various conclusions. Over decades, the community as a whole can "falsify" earlier collective conclusions—like the sporadic suggestions in the early 1970s that the world would cool from human emissions (see chapter 1 of Schneider, 2009 for this history). But in systems science it sometimes takes a score of years to even discover that certain data was not collected or analyzed correctly as well as continuing to identify new data and such discoveries are rarely by individuals but teams and even assessment groups.

Two Kinds of Statistics. When I first got involved in discussing the range of outcomes in climate change, I didn't understand Bayesian versus frequentist statistics, but in fact that was the heart of the matter— how to deal with objectivity and subjectivity in modeling and in projections.

The English clergyman and mathematician Thomas Bayes (c. 1702–1761) formulated an approach to probability now called Bayesian inference. His key theorem was published posthumously in 1764. In essence, it expresses how our knowledge base—and prejudices—establish an *a priori* probability for something (that is, a prior belief in what will happen based on as much data and theory as is available). As we further study the system, obtaining more data and devising better theories, we amend our prior belief and establish a new, *a posteriori* probability—after the initial facts. This is called Bayesian updating. Over time, we keep revising our prior assumptions until eventually the facts converge on the real probability.

Since we cannot do experiments on the future before the fact, prediction is wholly a Bayesian exercise. This is precisely why the Intergovernmental Panel on Climate Change produces new assessments every six years or so, since new data and improved theories allow us to update our prior assumptions and increase our confidence in the projected conclusions, unless new data actually reduces our confidence, which happens sometimes as well.

That confidence still falls short of certainty for most aspects of the problem. For example, there is only maybe a 50/50 chance of sea level rising many meters in centuries to come, though that is not by any means a small chance of such a consequential outcome. Regardless, the conclusion cannot currently be labeled as objective, since the future is yet to come. However, we can use current measurements of ice sheet melting. We can compare them with 125,000 years ago, when the Earth was a degree or two warmer than now and sea levels were four to six metres (13 to 20 feet) higher. Because that ancient natural warming had a different cause (changed orbital dynamics of Earth around the sun) from recent and near future warming caused primarily from current anthropogenic greenhouse gas increases, we can't say with high confidence that a few degrees warming from greenhouse gases will also cause a four-to-six-meter rise in sea levels. But it undoubtedly indicates an uncomfortable—maybe 50/50 or more as I noted--Bayesian probability of something similar to that happening inexorably in the next few centuries. This indeed was the conclusion of the Synthesis Report of the IPCC's Fourth Assessment in 2007, for exactly those reasons.

Some statisticians and scientists are leery of Bayesian methods. They prefer to stick only with empirical data and well-validated models. But what do you do when you don't have such data? One analogous example is found in clinical trials in cancer treatments, a subject in which I have had a very personal interest (e.g., see *The Patient From Hell*—Schneider, 2005). The so-called "gold standard" is a double-blind trial where half the patients receive a placebo and the other half receive the drug being tested, and neither the patients nor the researchers know who got what until the data are analyzed. After five or ten years, if there is a statistically significant difference between the recovery rate of drug versus placebo the trial is declared successful. The trial isn't designed to pinpoint individual differences. Even if we knew the odds of recovery for the average person from different treatments, there is a wide spread in individual responses. So medicine should try to tailor treatments to the individual's idiosyncrasies if most effective treatments are to be realized. That makes some doctors—and many insurance companies—nervous, but in my view is by far best medical practice. Likewise, some scientists and many policymakers are nervous about Bayesian inferences based on the best assessment of experts, preferring hard statistics. But as there are no hard statistics on the future, Bayesian methods are all we have. They are certainly far better than no assessment at all and hoping that all will work out fine with no treatment—that is no climate policy in this analogy. If we care about the future, we have to learn to engage with subjective analyses and updating— there is no alternative other than to wait for "Laboratory Earth" to perform the experiment for us, with all living things on the planet along for the ride. Whether to take that chance what risk management is about.

Risk Management. The basics are that scientists can help policymakers by laying out the elements of risk, classically defined as ***consequence x probability***. In other words, what can happen and what are the odds of it happening? The plethora of uncertainties inherent in climate change projections clearly makes risk assessment difficult. The inertia in the climate and socioeconomic systems and the fact that greenhouse gases emissions will continue to rise given the absence of strong mitigation policies (or unexpected events like a prolonged global recession) indicate that globally, most policy makers have been reluctant to make long term investments beyond their expected terms in office. But that is changing both in some regions like the EU, and even in the US. These kinds of decision makers are increasingly wary of making what is known as a Type II error—fiddling while the earth burns. A Type I error is a false positive, which in this case would mean taking action against climate change which subsequently proved relatively needless. Scientists are often leery of making a Type I error when data are scarce for fear of misleading society into un-needed actions and being blamed for undue alarm. The other kind, a Type II error, is a false negative, and in this case would mean assuming it is preferable to do little or nothing until there is less uncertainty, and subsequently finding that serious climate change ensues unabated with much more damages than if precautionary policies had been undertaken to adapt to and mitigate the effects. So it appears that our scientists are often Type I and our future-oriented decision makers Type II error avoiders. A less charitable interpretation of those reluctant to invest in precautionary adaptation and mitigation measures is that they know that the really adverse outcomes will likely occur in the future when current decision makers are not in office and not likely to be held accountable. The short term incentives are to delay action and pass the risks and the recriminations on to the next generation. None of this is scientific risk assessment, but value judgments on where and how to take risks and make investments in policy hedges—in short, risk management. But risk management is put on a much firmer scientific basis when the managers are schooled in the best risk assessments that state of the art science can produce.

IPCC Guidance on Uncertainties. To help decision makers, the IPCC produced a Guidance Paper on Uncertainties (Moss and Schneider, 2000) which was a foundation for the 2007 Fourth Assessment Report. I prepared the original draft with Richard Moss, now at a DOE lab at the University of Maryland, after convening a meeting in 1996 in which about 2 dozen IPCC lead authors met with decision analysts to fashion a better way to treat uncertainties in scientific assessments. The final guidance eventually agreed to within IPCC was a quantitative scale. We would define “low confidence” as a less than 1-in-3 chance; “medium confidence,” 1-in-3 to 2-in-3; “high confidence,” above two-thirds; “very high confidence,” above 95 percent; and “very low confidence,” below 5 percent.

It took a long time to negotiate those numbers and those words—and they change somewhat from assessment to assessment cycle. There were some people who still felt that they could not apply a quantitative scale to issues that were too speculative or “too subjective” for real scientists to indulge in “speculating on probabilities not directly measured”. One critic said, “Assigning confidence by group discussions, even if informed by the available evidence, was like doing seat-of-the-pants statistics over a good beer.” He never answered my response—“Would you and your colleagues think you’d do that subjective estimation less credibly than your Minister of the Treasury or the president of the US Chamber of Commerce?”

So we had two things we wanted everyone to use—a set of numbers defining the probability ranges for words such as “likely”, and a set of qualitative phrases for our confidence in the results, going from “well established” if there was a lot of data and a lot of agreement between theory and data to “speculative” when there wasn’t much data and there wasn’t much agreement. We had "established but incomplete" and "competing explanations" for the intermediate cases.

And then for the next two years Richard and I became what a journalist later called "the uncertainty cops." I read two thousand pages of draft material for the IPCC's Third Assessment Report. People did not always use uncertainty terms according to our simple rules. For instance, they would say that because of uncertainties, we can't be "definitive." I wrote back, "What is the probability of a 'definitive'?" Early drafts would put the range of outcomes anywhere from a one to five degrees Celsius change in temperature. And then they would say in parentheses "medium confidence". That was completely incorrect. It was "very high confidence," because they were talking about the fact that *between* one and five degrees was a very, very likely place to arrive. But people didn't want to say "very high confidence" because nobody felt very confident about the state of the science at the level of pinning it down to, say, one degree. So Richard or I would help them to rewrite, and say that we have "low confidence" in specific forecasts to a precision of a half degree, but we have "high confidence" that the range is one to five degrees. Simple things like that were needed to achieve consistency of message. Many fewer such problems occur now that the IPCC authors have several assessments of experience using uncertainties guidance terminology.

Media False Balance. Meanwhile the political chicanery of ideologists and special interests was shamelessly exploiting systems uncertainty by misframing the climate debate as bipolar—"the end of the world" versus "it's good for you." The media compliantly carried it in that frame much of the time, too. But those were and are the two lowest probability outcomes. The confusion that bipolar framing has engendered creates in the public at large a sense that "if the experts don't know the answers, how can I, a mere lay citizen, fathom this complex situation?" To this, industry-funded pressure groups added the old trick of recruiting non climate-scientists who are skeptical of anthropogenic climate change to serve as counterweights to mainstream climate scientists. This spreads doubt and confusion among those who don't look up the credentials of the apparently contending scientists—and that, unfortunately, includes most of the public and too much of the media. The framing of the climate problem as "unproved," "lacking a consensus," and "too uncertain for preventive policy" has been advanced strategically by the defenders of the status quo. This is very similar to the tactics of the tobacco lobby and its three-decade record of distortion that helped stall policy actions against the tobacco industry, despite the horrendous health consequences and eventually billions of dollars in successful lawsuits against big tobacco.

In the face of such tactics, the IPCC assessment reports are intended to be the best achievable statement of current scientific consensus. But "consensus" is not necessarily built over conclusions, but the *confidence* we have in a host of possible conclusions. With that kind of information policy makers can make risk management decisions by weighing both the possible outcomes and the assessed levels of confidence—we know it well, sort of know it, or hardly know it at all. Scientists should just say what we do know and don't, or what is even knowable in a reasonable amount of time, and not leave something out of our assessment because it isn't a well-established consensus yet. It is the job of society, through its officials, to make the risk management decisions informed by our conclusions and accompanying confidence estimates.

Where next: A Personal Assessment? As I've said, normally science strives to reduce uncertainty through data collection, research, modeling, simulation, and so forth. The objective is to overcome the uncertainty completely — to make known the unknown. Short of that, new information may narrow the range of uncertainty. No doubt further scientific research into the interacting processes that make up the climate system can and will eventually—several decades I speculate--reduce uncertainty about the response to increasing concentrations of greenhouse gases. This is very unlikely to happen quickly, however, given the complexity of the global climate and the many years of high quality data which will be needed. Meanwhile, even the most optimistic "business-as-usual" emissions pathway is projected to result in dramatic, potentially dangerous climate impacts like increased area of wildfires in the US West, rising sea levels, intensified storms, more acute air pollution episodes, etc. That means making policy decisions before this uncertainty is resolved, rather than using it as an excuse for delaying action.

Risk management also means understanding what is truly uncertain, and what is not. Sometimes critics claim that there should be no strong climate policy until the science is fully "settled" and all major uncertainties resolved, whereas supporters of strong policies suggest the science is already "settled enough" and it is time to proceed with action to reduce risks because taking that chance with our planetary life support system is foolhardy. The science which demonstrates a significant warming trend over the past century *is* settled; moreover, it is virtually settled that the past several decades of warming have been largely caused by human activity and that much more is being built into the emissions pathways of the 21st century (IPCC, 2007). Sounds like the "settled already" side has won the debate: warming is occurring and human activities are the primary driver of recent changes.

That leaves the uncertainty about how severe warming and its impacts will be in the future, especially when projections for "likely" warming by 2100 vary by a factor of 6. The task then is to manage the uncertainty rather than master it, to integrate uncertainty into climate research and policymaking. This kind of risk-management framework is often practiced in defense, health, business and environmental decision-making. But the thresholds for action often seem lower. The US has a military that evaluates security precautions against many very low probability—but potentially dangerous—threats. Well, the climate change threat is not 1%. It's better than 50% for really significant trouble, and maybe 10-20% for absolutely catastrophic trouble. Who would take a 10% risk of crossing the street in the middle if there were that chance of being hit by a vehicle? We'd do it at the corner with the light in our favor at dramatically lower risk. We buy fire insurance for only a few percent risk of our house burning down. Thus the thresholds for risk aversion in health and business and defense categories are already quite low, and in fact much lower than many potential thresholds for irreversible climatic changes.

In my personal value frame, it is already a few decades too late for having implemented some climate policy measures, especially national standards for energy efficiency and public-private partnerships in new technology development. Had we begun mitigation and adaptation investments decades ago, when a number of us advocated them, the job of remaining safely below dangerous thresholds would be easier and cheaper. Similarly, beyond a few degrees Celsius of warming—at least an even bet if we remain anywhere near our current course—it is likely that many "dangerous" thresholds will be exceeded. Strong action is long overdue, even if there is a small chance that by luck climate sensitivity will be at the lower end of the uncertainty range and, at the same time, some fortunate, soon-to-be discovered low-cost, low carbon-emitting energy systems will materialize. For me, that is a high-stakes gamble not remotely worth taking with our planetary life support system. Despite the large uncertainties in many parts of the climate science and policy assessments to date, uncertainty is no longer a responsible justification for delay in either adaptation or mitigation policies.

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