CAN WE ESTIMATE THE LIKELIHOOD OF CLIMATIC CHANGES AT 2100?

An Editorial Comment

Why This Editorial?

In 1988, in the wake of massive heat waves in North America, James Hansen, Director of the NASA Goddard Institute for Space Studies in New York City, garnered international publicity for asserting that it was time to stop 'waffling' and accept the fact that the increase in temperature trends in the late twentieth century were likely to be due to human activities, in particular global warming induced by greenhouse gas increases. This controversial assertion earned for Jim Hansen the enmity of many in the fossil fuel industry and the praise of environmental groups. Therefore, twelve years later when Hansen and colleagues published a paper in the *Proceedings of the National Academy of Science* in August 2000 (Hansen et al., 2000) that some of Hansen's original detractors labeled a 'recantation', this, too, generated a great deal of controversy. Hansen et al. laid out a number of points, including a proposed low CO_2 emission scenario based upon trends of CO_2 increase in the past decade, which he details in a Response (Hansen, 2002) in this issue of *Climatic Change* to a critical Editorial Essay on Hansen et al., 2000 by Donald Wuebbles (Wuebbles, 2002). Even though the Hansen et al. work is two years old, the issues in the Hansen/Wuebbles exchange are as fresh as ever.

At bottom, Wuebbles objects to the fact that Hansen's low CO_2 emissions scenario is not consistent with the vast bulk of the published literature, in particular the Special Report on Emissions Scenarios (SRES) of IPCC (Nakicenovic and Swart, 2000), in which Hansen's emissions are below even the lowest emissions levels of that scenario. A lively exchange between Wuebbles and Hansen appears in this issue in connection with the plausibility of this scenario in the Hansen et al. 2000 paper. Wuebbles expresses concern about its potential for misinterpretation and the Hansen response defends the original paper. My Editorial is motivated by this lively and constructive exchange, which I believe raises a fundamental question that helped to create this debate – perhaps unnecessarily: The SRES scenarios for CO_2 emissions over the twenty-first century are not assigned probabilities by the IPCC authors. Therefore, any claimant of likely emissions, and thus future concentrations and attendant climate change, can simply assert his or her own opinion as to what constitutes the likelihood of a given degree of climate change a century hence without having to debate the considered judgments of dozens of scientists, three rounds of reviews and the IPCC's Review Editors' supervision – the rigorous

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process that SRES underwent – despite the fact that SRES self-consciously chose not to assign probabilities to any of their illustrative scenarios.

Therefore, let me point out some of the consequences of that deliberate choice not to discuss probabilities and how that can – and has – led to confusion on the part of the media and policy makers over the likelihood of potential 'dangerous' anthropogenic climate change in the next century, as well as spin-off debate of the kind in the Wuebbles/Hansen exchange in this issue. I believe this debate, and others like it – see Schneider 2002 in the context of a review of a book by Bjørn Lomborg in which it is asserted that only the lowest SRES scenario is plausible – could have been put into much clearer context had SRES or IPCC's other assessment bodies tried some estimation of likelihood for each future climate scenario. This Editorial Essay builds on my Commentary published in *Nature* (Schneider, 2001), which triggered several responses (e.g., Grübler and Nakicenovic, 2001; Pittock et al., 2001) and was followed by a series of related articles published in *Science* (e.g., Wigley and Raper, 2001; Reilly et al., 2001), in which several authors attempt to actually estimate the probability of climate change a century hence. Let me briefly recount the nature of the debate in this Editorial Essay, and then proceed to suggest that the next IPCC or other national or international assessments consider not only the probability of emissions scenarios, but also the likelihood of various steps in the entire chain of logic, including carbon cycle modeling, the sensitivity of climate to given concentrations of greenhouse gases and other radiative forcings (the latter three all assessed in IPCC, 2001a). In connection with the estimation of a probability distribution for climate sensitivity, see also Andronova and Schlesinger (2001), in which a simple climate/ocean model is combined with observed near-surface temperature records to estimate a very wide range of possible climate sensitivities, some 50% of which lie outside of the IPCC range.

Risk is Probability Times Consequences, not Consequences Alone

Figure 1 (Schneider and Kuntz-Duriseti, 2002, and Chapter 2 of IPCC, 2001b) shows the 'cascade of uncertainties' that leads to an increasing envelope of future possibilities owing to the addition of uncertainties at each step in the process. Thus, estimating the joint probability of a given degree of climate change at any future date, say 2100, is essential for any assessment of what constitutes 'dangerous anthropogenic interference in the climate system' – to quote the famous phrase of the UN Framework Convention on Climate Change language approved by over 150 nations in 1992.

That such an estimate of future climate change will necessarily be subjective must of course be borne in mind. Moreover, this estimate will change with time as knowledge is gathered and reassessed. Grübler and Nakicenovic (2001) – key authors of the SRES effort – responded to my call (Schneider, 2001) for

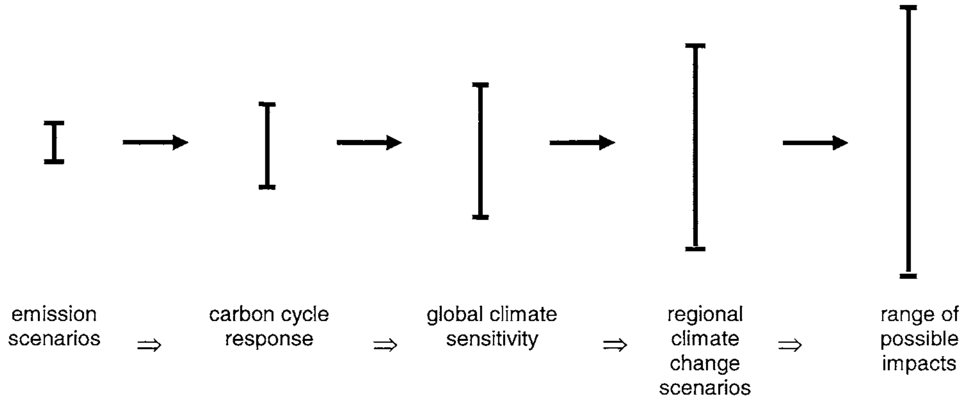

Figure 1. Range of major uncertainties typical in impact assessments showing the 'uncertainty explosion' as these ranges are multiplied to encompass a comprehensive range of future consequences, including physical, economic, social and political impacts and policy responses (modified after Jones, 2000, and the 'cascading pyramid of uncertainties' in Schneider, 1983).

probabilistic assessment of emissions and climate scenarios with a polite 'no thanks':

> So although we agree with Schneider in many respects, 'dangerous' levels of climate change will need to be identified by research into the adverse impacts on natural and human systems, independent of the question of how likely they are to occur, and covering the full range of scientific uncertainty. There is a danger that Schneider's position might lead to a dismissal of uncertainty in favor of spuriously constructed 'expert' opinion.

Indeed, I agree that any such estimates will be highly subjective and often carry a fairly low confidence (e.g., Moss and Schneider, 2000). But to duck the attempt to produce probabilistic estimates for such scenarios is to circumvent the classical definition of risk: probability times consequence. It is simply very difficult for policy makers to have a 'consequences alone' definition of risk such as that seemingly advocated by Grübler and Nakicenovic, in which only ranges of plausible scenarios are given 'independent of the question of how likely they are to occur' – that is, no probabilities are attempted to be attached to each of them. Without meaning to be facetious, let me suggest that if a range of possibilities were the sole basis of the scientific input for decision making, then we all should switch our professions towards studying and preventing the collision of the next massive asteroid with the earth, an event whose consequences would undoubtedly be orders of magnitude worse than any other environmental event we could imagine, anthropogenic or natural. The reason we do not all redirect our efforts instantly to the problem of a large asteroid collision is that its probability is typically given on the order of a millionth or less per year, whereas the probability of serious climate change is obviously many orders of magnitude higher than that – likely to be in the first decimal point.

Probability matters greatly in a resource constrained world in attempting to determine the priority for investments. Nevertheless, I agree with Grübler and Nakicenovic that the assignment of such probabilities is fraught with deep uncertainty – i.e., while risk is still probability x consequence, the probability of some level of future climate change is not determinable directly by any set of frequency experiments – so-called frequentist probabilities – and instead will rely to some degree on scientific judgments based upon as much empirical observation as is possible. But these empirical observations will not be of the future climate – impossible in principle before the fact – but rather the behavior of the subcomponents of a complex systems model, which is then used to make the future projection. Thus, subjective judgments – essentially Bayesian judgments about the plausibility of the assumptions and structure of the systems model – are made on the basis of limited empirical validation of the subcomponents of the systems model, not the performance of the overall system in the future, as that cannot be done empirically before the fact. Empirical data can and should be used in the construction of the elements of such systems models, and empirical testing of the whole systems model on past events (e.g., paleoclimatic events for a climate model or oil price shocks for an economic model) is desirable when possible, but rarely will these be exact analogies to the future events we are asked to assess. Such tests may affect our subjective confidence in the projections of our systems models, but cannot provide frequentist probabilities for future events. The very nature of such subjective probabilities makes them controversial, and some scientists believe it is better to offer no probabilities than subjective ones—as implied in the above quotation, for example. Indeed, these authors suggest that we are better off, as SRES did, assigning each representative scenario the label 'equally sound', meaning no distinguishing probabilities. Grübler and Nakicenovic have thus implied that a 'consequences alone' definition of risk is all we can deal with as assigning probabilities would be worse than nothing. I strongly disagree, using the asteroid collision argument made earlier as case in point.

Are Probabilities in Natural and Social Sciences Different in Kind?

Moreover, Grübler and Nakicenovic (2001) also argue that probabilities in natural science are different from those in social science, since we can perform frequency experiments in the former, whereas in the latter we must make judgments. Grübler and Nakicenovic say that

> in an interdisciplinary scientific assessment, the concept of probabilities as used in natural sciences should not be imposed on the social sciences. Probability in the natural sciences is a statistical approach relying on repeated

> experiments and frequencies of measured outcomes, in which the system to be analysed can be viewed as a 'black box'. Scenarios describing possible future developments in society, economy, technology, policy and so on, are radically different. First, there are no independent observations and no repeated experiments: the future is unknown, and each future is 'path-dependent': that is, it results from a large series of conditionalities ('what if... then' assumptions) that need to be followed through in constructing internally consistent scenarios. Socio-economic variables and their alternative future development paths cannot be combined at will and are not freely interchangeable because of their inter-dependencies.

However, natural scientific projections for the future still require judgments, as no frequency experiments can be made before the fact. We must still assume that our assumptions which govern the structural design of our systems models will hold in the future, often for values of dependent variables that are outside of the range of past experience. Moreover, there are conditionalities in natural science as well, and the solutions are, like Grübler and Nakicenovic rightly assert for social systems, 'path dependent' for natural systems as well as social systems. Therefore, I believe there is no in principle difference between natural and social sciences in this regard, since both require feedback mechanisms and contain path dependent systems. However, I agree there is one aspect in which social systems are harder to predict than natural systems. Although in both social and natural systems interactions among subsystems can cause alterations over time, in the case of social systems, changing beliefs and attitudes, themselves partially driven by information about how the system is evolving, can lead to modifications of policy choices. While the latter property of social systems is different in kind from natural system predictions, to me both natural and social systems models involve the necessity to model feedback processes, and thus are very similar. In essence, we need a systems model that explicitly deals with the many subcomponents that we believe will influence the evolving emergent properties of a complex socio-natural system, and that when social sciences are included, the system becomes more complex in detail, but not necessarily in principle. For us simply to redefine the classical definition of risk to consequences alone, because subjective probabilistic analysis is fraught with deep uncertainties, is in essence to offer no advice to the policy community as to how it should order its investments in alternative actions, for without probabilities it is very difficult to engage in risk management. And if we in the scientific assessment business do not offer some explicit notions of the likelihood of projected events, then the users of our products – policy analysts and policy makers – must guess what we think these likelihood estimates are. That is hardly preferable in my view to a carefully worded set of subjective probabilistic estimates in which our (often low) confidence in such estimates accompanies any likelihood statements.

Pittock and colleagues, however, while agreeing that risk management demands probabilities, suggest that there may be (Pittock et al., 2001) a sensible related strategy that can help: rather than an estimate of the likelihood of a particular event occurring, it might be best to study the likelihood of exceeding an identified critical threshold. Pittock et al. build on the suggestions of Lempert and Schlesinger et al. (2000), that we should look for 'robust solutions', in which a wide range of scenarios lead to similar policy responses. However, the Lempert and Schlesinger method of analysis still requires the construction of ranges of future outcomes – itself a subjective assessment. Subjectivity is simply inherent in all future projections, whether in natural or social sciences.

What is the Probability of 'Dangerous' Climate Change?

But the problem for decision makers of assessing the likelihood of 'dangerous anthropogenic interference with the climate system' is more complicated even than that. First of all, what is 'dangerous' is a value judgment about the relative salience of various impacts, such as loss of marketable value, human life, biodiversity, heritage sites or employment (the 'five numeraires', as it was labeled by Schneider, Kuntz-Duriseti and Azar, 2000). And, before such value judgments are even to be attempted, 'dangerous climate change' involves, as on Figure 1, a cascade of uncertainties in emissions, carbon cycle response, climate response, and impacts. That is, we must estimate probabilities for future populations, future levels of economic development, and potential technological props to that economic development, which influence the radiative forcing of the atmosphere via emissions of greenhouse gases and other radiatively active constituents. At the same time, we also must deal with the probabilities associated with carbon cycle modeling uncertainty and, no less, climate sensitivity estimated from climate models tested on paleoclimatic situations, among other 'validation' exercises. Schneider 2001 showed that one could arrive at very different estimates of the (subjective) probability of 'dangerous' climate changes in 2100 because of the lack of specification by IPCC of the independence of various scenarios or climate model sensitivities or their respective probabilities. Let me explain briefly below (but see Schneider, 2001 for details).

The IPCC Working Group 1 (IPCC, 2001a) lead authors cascaded the broad range of emissions scenarios – 6 representative 'storylines' offered by SRES as 'equally sound' scenarios –into radiative forcings that produce a wide temperature projection range via use of 7 general circulation models (GCMs), which themselves represented a range of equilibrium climate sensitivities from 1.7 to 4.2 °C warming for a doubling of CO_2 (and these 7 are a subset of 18 GCMs listed in Table 9.1 of the WG1 Third Assessment Report with an even larger range of climate sensitivities to radiative forcing represented).

The result of combining the sensitivity of the 7 GCMs (via tuning a simple model to each GCM response) with the 6 illustrative scenarios from SRES is the very highly visible WG1 TAR-revised 2100 temperature projection of 1.4–5.8 °C further warming – a big jump from the 1–3.5 °C of the Second Assessment Report, an increase not unnoticed by many policy makers.

The problem with lack of probabilities in each SRES scenario (and for each GCM climate sensitivity) is that it leaves to the imagination of any policy analyst (or maker) what they think the SRES team thought the probabilities were for each scenario or what the WG1 authors thought the likelihood was for the sensitivity of each of the 7 GCMs selected – let alone the probabilities for the sensitivities of each of the full 18 GCMs in Table 9.1. If all entries are given equal likelihood, then by combining these two sets the resultant probability distribution for 2100 global surface warming would not resemble either of these separate (i.e., scenarios and GCM sensitivities) uniform probability distributions. Rather the combined probability distribution would have a peak at the center – just like a typical bell curve (Jones, 2000). If no probability is assigned to scenarios or sensitivities in the middle of the range, and if no attempt to analyze whether any of these are independent from each other – and only the range outlier scenarios and climate sensitivities are used – then the probability distribution for 2100 warming would not be a peaked bell curve, but a much flatter distribution (Figure 2, modified after Schneider, 2001). Since the choice of some sub-set of the full set of scenarios (e.g., the family of 6 'illustrative scenarios' used by WG 1) and each GCM sensitivity is somewhat arbitrary, the fact that most of them have values between the range limiting scenarios or sensitivities means the sub-set will create the bell-shaped curve for their cascaded distribution.

In Figure 2, the grey bars show that for all 18 GCMs and all 6 illustrative scenarios a peaked curve is indeed obtained. Also, 23% of the values obtained for temperature increase in 2100 are greater than 3.5 °C – an illustrative threshold value chosen arbitrarily here, but one which would be suggested by many to have the potential to cause significant – perhaps 'dangerous' – climate damage (IPCC, 2001b). The dark bars show that almost the same results are obtained for the case in which the highest and lowest GCM climate sensitivities are trimmed from the data set – explaining why no occurrences of dark bars are seen in Figure 2 for very large warming (21% of these occurrences are for temperatures above the 3.5 °C threshold). Finally, the white bars in Figure 2 show the very much flattened distribution obtained by keeping all 18 GCM sensitivities in, but (following the logic of the SRES in which no probabilities are given to individual or families of scenarios: 'the writing team as a whole has no preference for any of the scenarios', said SRES authors) omitting all those scenario cases between the highest and lowest (that is keeping only A1FI and B1). Given the relative lack of 'middle value' scenarios, the shape of the probability distribution is much flatter. More importantly, nearly twice as large a percentage of values represented by the white bars are for 2100 temperature increases greater than 3.5 °C (39%). Clearly, a policy

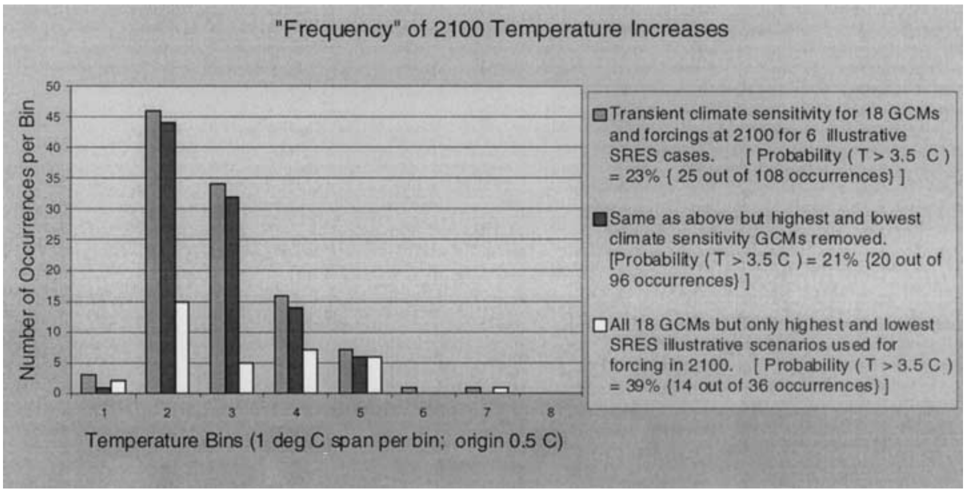

Figure 2. This figure contains histograms of the number of occurrences of temperature increases around 2100 in seven bins, each 1 °C wide and beginning at 0.5 °C (e.g., the bin labeled 3 represents the number of occurrences of temperature increases for each case shown between 2.5 and 3.5 °C of warming at 2100). The grey bars represent a case of the distribution that results from the product of (a) all 18 GCM transient climate sensitivities (in °C warming for a transient climatic forcing at the time of an equivalent doubling of CO_2) (from Table 9.1 of IPCC, 2001), and (b), all 6 forcings at the year 2100 (in W/m^2) from the 6 SRES illustrative scenarios (from Figure 18 of the Technical Summary of IPCC, 2001a). [To use the transient climate sensitivities for a wide range of forcings, they are first scaled by the equivalent to a doubling of CO_2; i.e., each of the 6 forcings are divided by 4 W/m^2 – a typical estimate for forcing when CO_2 doubles.] For the 108 numbers represented by the joint inclusion of 18 GCM sensitivities and 6 SRES illustrative scenario forcings, 25 out of the 108 occur in bins representing temperature increases in 2100 greater than a threshold value of 3.5 °C – a value many would consider to represent a potential for significant climate damage. The dark bars represent a trimmed case in which the highest and lowest climate sensitivities of the 18 GCMs on Table 9.1 of IPCC, 2001a are removed from the analysis. In this case only 21% (20 out of 96) of the occurrences are greater than the threshold warming of 3.5 °C. Finally, the white bars represent a case in which all 18 GCM sensitivities are used, but only two SRES forcings (the highest, A1FI, and the lowest, B1) are used. The number of occurrences (14 out of 36) greater than the threshold of 3.5 °C warming in 2100 represents a much larger likelihood (39%) of 'dangerous' climatic damage than for either of the other two cases.

maker concerned to 'avoid dangerous anthropogenic interference in the climate system', in the UNFCCC phrase, would be much more inclined to propose stronger policies and measures for a 39% 'dangerous' threshold crossing likelihood than for the 21% figure.

But what do these threshold-crossing likelihood figures mean? Unless probabilities are assigned to individual scenarios and GCM climate sensitivities, their joint distribution (the likelihood of temperature rise in 2100) will depend on the particular selection of scenarios and GCMs, as Figure 2 clearly demonstrates. Unless assessors apply decision analytic elicitation (e.g., Morgan and Henrion, 1990) or other techniques (e.g., Wigley and Raper, 2001 or Reilly et al., 2001) to estimate more consistently the subjective likelihood for each scenario and GCM sensitivity, policy makers will instead eventually have to guess what the scenario generators or climate sensitivity assessors think are the joint probability distributions for outcomes like that in Figure 2 – an outcome policy makers care about: the likelihood of various temperature rises in the future.

The likelihood of threshold crossing occurrences is thus quite sensitive to the particular selection of scenarios and climate sensitivities used. This suggests the urgency of assessing the relative likelihood of each such entry so that the joint distribution represented on this histogram has a meaning consistent with the underlying assessment of the components. Arbitrary selection of scenarios or sensitivities will produce distributions that could easily be misinterpreted by integrated assessors or policy makers as containing subjective probabilistic analysis when, in fact, they do not until a judgment is formally made about the likelihood of each scenario or sensitivity. For this reason the word 'frequency' appears with quotation marks in Figure 2 since it does not represent an intellectually or analytically justifiable probability distribution when the subcomponents are arbitrarily chosen without a 'traceable account' (e.g., Moss and Schneider, 2000) of how their selection was arrived at.

Scenarios of Social Conditions Affect More Than Emissions

One of the most important implications of the SRES exercise and its 'storylines' approach is not just the wide range of plausible emissions future policy makers need to consider. As noted in Chapter 2 of WG 3 (IPCC, 2001c), the conditions of economy and equity that drive emissions also help to precondition the capacity of a social group to adapt or mitigate climate change. In other words, not only emissions, but adaptive capacity that helps to determine climate damages (Chapter 18 of IPCC, 2001b) and mitigative capacity (Chapter 1 of IPCC, 2001c) that helps to determine the likelihood of various policy responses are all mutually dependent on how society is structured over the future. This insight is one of the most important positive developments of the SRES approach, and is certain to occupy integrated assessments of climatic change for the foreseeable future.

Summary

I appreciate the exchange between Wuebbles and Hansen, as it allows me to focus on one element that I believe will be a main debate point in all future climate assessments: how and when to assign probabilities to future projections. I have expressed a deep concern that the absence of probabilities for climate sensitivity in the IPCC or the choice to eschew using decision analytic or other techniques to elicit more consistent estimates of the subjective probabilities of the SRES emissions scenarios or storylines creates the potential for misunderstanding or misuse of the scenarios by interests claiming to determine the likelihood of ‘dangerous anthropogenic interference in the climate system’ by constructing arbitrary histograms like Figure 2. Nevertheless, the IPCC TAR and the SRES approach have been a major advance in the assessment of climate change effects, impacts and mitigation possibilities. It is essential that these activities continue and expand to deal with the few deficiencies that have been identified. I do not wish to have my concerns expressed in this Editorial misinterpreted as detracting from the enormous positive contribution that this vast international and interdisciplinary effort represented.

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