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ABSTRACT This paper adds a new dimension to the role of scientific knowledge in policy by emphasizing the multivalent character of scientific consensus. We show how the maintained consensus about the quantitative estimate of a central scientific concept in the anthropogenic climate-change field – namely, climate sensitivity – operates as an 'anchoring device' in 'science for policy'. In international assessments of the climate issue, the consensus-estimate of 1.5°C to 4.5°C for climate sensitivity has remained unchanged for two decades. Nevertheless, during these years climate scientific knowledge and analysis have changed dramatically. We identify several ways in which the scientists achieved flexibility in maintaining the same numbers for climate sensitivity while accommodating changing scientific ideas.

We propose that the remarkable quantitative stability of the climate sensitivity range has helped to hold together a variety of different social worlds relating to climate change, by continually translating and adapting the meaning of the 'stable' range. But this emergent stability also reflects an implicit social contract among the various scientists and policy specialists involved, which allows 'the same' concept to accommodate tacitly different local meanings. Thus the very multidimensionality of such scientific concepts is part of their technical imprecision (which is more than just analytical lack of resolution); it is also the source of their resilience and value in bridging (and perhaps reorganizing) the differentiated social worlds typical of modern policy issues. The varying importance of particular dimensions of knowledge for different social groups may allow cohesion to be sustained amidst pluralism, and universality to coexist with cultural distinctiveness.

Anchoring Devices in Science for Policy:

The Case of Consensus around Climate Sensitivity

Jeroen van der Sluijs, Josée van Eijndhoven, Simon Shackley and Brian Wynne

Experts started drafting assessment reports for policy-makers when research on anthropogenic climate change (and especially climate modelling) was still in an early stage of development. 'Assessment' is the analysis and review of information derived from research in order to help someone in a position of responsibility to evaluate possible actions, or to think about a problem. It does not usually entail doing new research. Assessment means assembling, summarizing, organizing, interpreting, and possibly reconciling pieces of existing knowledge, and communicating them so that they

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appear relevant and helpful for the deliberations of an intelligent but inexpert policy-maker.[1] Assessments of anthropogenic climate change have been conducted since the 1970s.

A key element in these assessments has been 'climate sensitivity'. From about the early 1960s, it was commonplace to illustrate the 'sensitivity' of climate models in terms of their response to a doubling in the concentration of atmospheric CO_2. The first assessment of the climate problem that made an inventory of individual estimates of climate sensitivity from the literature, and then used that inventory to present a range for climate sensitivity, was the study by the Ad Hoc Study Group on Carbon Dioxide and Climate of the US National Academy of Sciences (NAS) in 1979.[2] This was followed by a more comprehensive study, leading to the influential NAS'83 assessment report, *Changing Climate*.[3] Both NAS assessments had an international impact. They have subsequently been quoted by advisory bodies and policy documents in many countries, including the Intergovernmental Panel on Climate Change.

A further milestone in the emergence of a climate risk assessment community was the international 'Conference on the Assessment of the Role of Carbon Dioxide and of Other Greenhouse Gases in Climate Variations and Associated Impacts' in Villach (Austria), in 1985. This meeting was sponsored by the United Nations Environmental Programme (UNEP), the World Meteorological Organization (WMO) and the International Council of Scientific Unions (ICSU).[4] It succeeded in bringing together scientists from all over the world to form an international panel, and was a major step in interfacing science with policy. Several follow-up studies and conferences were held in response to the recommendations of the Villach conference, all aimed at furthering climate policies.

The Toronto Conference in 1988 marked the beginning of high-level political debate on the risks of anthropogenic climate change.[5] It recommended a world-wide CO_2 emission reduction of 20% (relative to 1988) by the year 2005. In 1988, independent of the Toronto Conference, UNEP and WMO established the Intergovernmental Panel on Climate Change (IPCC), chaired by Bert Bolin, a Swedish climate scientist. This panel consisted of three Working Groups: I (WGI) to assess the science; II to assess the impacts; and III to formulate response strategies. In 1990, IPCC WGI issued its first report (hereafter to be called the 'IPCC'90' report) – a comprehensive state-of-the-art report, with an executive summary for policy-makers.[6] Hundreds of scientists from all over the world contributed to this report. At the Second World Climate Conference in Geneva (1990), the IPCC assessment was accepted by over 137 attendant countries as a vital scientific basis for international negotiations on a climate convention. From 1990 onwards, the Framework Convention on Climate Change (FCCC) was prepared under the auspices of the United Nations, and took effect on 21 March 1994. Updates of the assessment were issued in 1992 and 1994.[7] A new state-of-the-art IPCC report was completed in 1995, and accepted in July 1996 by the Conference of Parties to the Climate

Convention as the primary source of scientific and technical advice for the implementation of the FCCC.[8]

The IPCC plays a clear mediating role between science and policy in assessing the risks and consequences of anthropogenic influences on the climate system. It has become an elaborate international means for securing consensus in the case for climate policy, although the notion of 'consensus' commonly employed is not straightforward. For instance, precisely what 'knowledge' is the object of that widely proclaimed consensus is open to debate. During two decades of assessment practice, climate research has expanded enormously, with scientific knowledge of the climate system and the complexity of 'state-of-the-art' climate models changing accordingly. Consequently, successive assessments have had to deal with new insights, theories and data. It is all the more surprising that some results of the assessments look very stable over time, and that consensus seems to exist concerning some of the key results. One of the most important model outputs for assessment has been *climate sensitivity* to CO_2-doubling. We found that the estimate for this quantity has remained constant from 1979 up to the present. The history and prevalence of large scientific uncertainties make anthropogenic climate change an excellent case for investigating the processes by which consensus is constructed and maintained in assessment practice.

We have analyzed the concept of climate sensitivity, as it is used in each of the major assessments we have mentioned. We investigated the backgrounds to the establishment of their ranges for climate sensitivity, as well as variations in the meaning of the concept used in these assessments. Our major sources were the assessment reports themselves. We also interviewed key persons involved in the assessments (mainly lead authors of the relevant sections of the reports), to bring to light considerations and decisions underlying the figures, when these were not clear to us from the texts themselves. Using theoretical notions about the flexible science–policy interface, we interpret the reasons behind the apparent stability. We asked some of the scientists contacted for their comments on our findings, and have incorporated their reactions in our discussion.[9] In this paper, we explore how and why consensus on the climate sensitivity range has persisted, despite the massive uncertainties which are widely acknowledged to pervade the field of climate change.

Science for Policy

Although there have been several attempts to provide alternative models of how science and policy interrelate, science studies have often continued to use concepts which tend to reinforce entrenched ideas of 'science' and 'policy' as distinctly defined, separable worlds. Leading scientists in many fields of policy-relevant science have explicitly emphasized the importance of scientific consensus for policy legitimacy, and perceive it as an independent prior variable. David Collingridge and Colin Reeve have rightly challenged this conventional view that scientific disagreement, or uncertainty, compromises policy authority and effectiveness.[10] In their 'under-critical' and 'over-critical' models, Collingridge and Reeve argue that science is used either to legitimate policies developed for non-scientific reasons, or is ignored if the consensus contradicts policy or there is scientific 'dissensus'.

In some respects, the history of the role of knowledge about anthropogenic climate change supports Collingridge and Reeve's thesis. Several authors have suggested that the uptake of climate-change science by policy-makers occurred only when the institutional and political circumstances facilitated it, namely in the mid- to late-1980s,[11] when a 'window of opportunity' opened up in the socio-political landscape.[12] In such cases, scientific knowledge appeared to be the trigger for policy uptake, even though the socio-political setting was perhaps the primary reason. This argument is supported by the fact that a scientific consensus existed before the global-warming issue was taken up by policy-makers, thus illustrating the secondary importance of science in the emergence of policy windows.

A major limitation to such theoretical approaches is that they do not readily account for how new scientific insights are absorbed into the scientific assessment process. Josée van Eijndhoven and Peter Groenewegen have shown that in a case where new scientific data and new practical situations arose, experts serving on advisory bodies on environmental standards showed considerable flexibility when drawing policy conclusions from scientific data; they emphasized that the connections between given scientific data, expert interpretation of these data and policy meaning are more like chains of linked arguments and beliefs.[13] New findings therefore do not necessarily imply support for any change in policy that might be indicated by the new insights.[14] In some cases, particular linkages may be formed through a set of assumptions or convictions shared in an intermediary community which come to 'fix' particular interpretations as 'given'. Van Eijndhoven and Groenewegen's account thus emphasizes the actively constructed character of scientific knowledge in policy, as opposed to the idea of 'information' simply being 'transferred' from science to policy as a passive form of legitimation. They argue that more scientific knowledge often increases policy flexibility by introducing more possibilities for interpretation. This could be seen as greater uncertainty, hence as less policy cohesion. Scientific uncertainty, however, is itself socially modulated, so greater argumentative flexibility does not automatically translate into greater social disagreement and policy weakness.

Van Eijndhoven and Groenewegen's empirically based findings are largely consistent with more theoretical accounts within the sociology of scientific knowledge which question the treatment of science and policy as distinctly defined worlds. From this perspective, accounts such as Collingridge and Reeve's, and those that emphasize 'windows of opportunity', are in danger of reducing all change to social factors – which in itself, of course, *presupposes* a distinction between the 'social' and the 'natural'. The actor-network approach of Bruno Latour and his co-workers, with its fruitful and radical dissolution of analytical precommitment to categories such as the natural and the social, has opened the sociological door (as it were) to impure categories and hybrid forms.[15] Although most of their work focuses on the building of sociotechnical networks rather than on explicit forms of policy-making concerning (for instance) environmental protection, the issue raised by their work about how categorical distinctions that fundamentally shape the world (such as 'science' and 'policy') come themselves to be constructed and reproduced, can also cast new light on policy-oriented science.

An implication of Latour's work is that if science and policy are co-constructed through processes which occur in tandem, it becomes difficult to explain the one by using the other. *Contra* more conventional accounts, particular forms of science do not always come to coincide with policy agendas by coincidence, happy circumstance or opportunism: instead, science has a mediating and structuring role *vis-à-vis* policy, and *vice versa*. This can be observed especially in the way in which scientific research agendas come to shape the policy debate and policy formulation.

Several other bodies of work have been sensitive to the way in which hybrid communities and networks are built, linking the worlds of 'science' and 'policy' in more complex ways than has been recognized by conventional models. For example, Leigh Star and James Griesemer's notion of 'boundary objects' (which live in research and other worlds, allowing a broad community of meaning across diverse social worlds with, at the same time, diverse meanings invested in those uniting concepts by different communities) corresponds to the multivalent character of 'consensus', and the importance of hybrid roles.[16] And in her work on regulatory science, Sheila Jasanoff has also shown how US policy agencies have developed hybrid communities of advisers who combine the roles of scientific actor and policy actor, to negotiate credible regulatory policies. These hybrids are rendered more credible precisely through their discursive 'repurification' into the distinct public categories of 'science' and 'policy'.[17] In denying the existence of role-ambiguity, these discursive repurifications implicitly rely on some objective grounding in nature and scientific roles as their source of authority.

An explicit part of the actor-network approach is that constructs beyond (and even within) the laboratory involve 'heterogeneous engineering', and stabilization of networks of aligned identities of natural, physical and social actors. Although the actor-network approach has been criticized for its unduly monovalent conceptualization of the ensuing sociotechnical networks, the components of these networks-in-the-making are explicitly regarded as being more radically open to new identities and relationships than conventional perspectives allow. Thus although both the actor-network approach (in its original Paris mode) and Van Eijndhoven and Groenewegen's concept of 'flexible argumentation chains' may lead in different ways to an overly one-dimensional characterization of science-policy constructs, both are potentially consistent with a more multivalent concept of scientific knowledge and consensus, as suggested by the concept of 'boundary-objects'.

The CO_2-doubling Temperature: A History of Sticking to the Same Numbers

In this section we discuss the concept of *climate sensitivity.* We explain why it is a problematic quantity, how it is being estimated, and why it is a key element in the assessments. We then explore how the range of values for climate sensitivity has been constructed and maintained in successive reports, and discuss how a single 'best guess' figure has been extracted from this range. We argue that 'climate sensitivity' has different meanings and functions for a wide range of actors involved in the climate debate. In this respect the concept works as a *boundary object* managing the interface between different social worlds – climate modelling, climate impacts research, climate policy making – and acts as an 'anchor' that fixes the scientific basis for the climate policy debate.

The Concept of 'Climate Sensitivity'

Carbon dioxide (CO_2) is the major anthropogenic greenhouse gas that is widely believed to produce global warming through increasing absorption of thermal (long-wave) radiation in the atmosphere. 'Climate sensitivity' is the model-calculated potential global surface air temperature change in equilibrium following an instantaneous doubling of atmospheric CO_2-concentration. It cannot be measured by conventional laboratory methods: an ensemble of multiple copies of Planet Earth with a doubled CO_2-concentration, and in sufficient number to provide a statistically satisfying set of measurements to determine this quantity, are not available to science. It is also very difficult (though not impossible) to derive the climate sensitivity from the geological record because: (a) the atmospheric CO_2-concentration is not the only climate-influencing factor which has changed over time; (b) there are uncertainties and indeterminacies in the measurement of CO_2-concentration, and in the other climate-forcing variables; and (c) climate change processes and feedbacks may also have been different in the past. Hence climate models are the principal tools for investigating climate sensitivity.

Computer models of the climate system developed over the past few decades have incorporated CO_2 through its interaction with thermal radiation (which CO_2 absorbs and re-emits, warming or cooling in the process, depending on the energy balance): this affects climate variables. The climate sensitivity is calculated in a climate model by doubling the CO_2 atmospheric concentration instantaneously, and then allowing the model to reach a new equilibrium which lets the interactive model processes adjust to the perturbation. This equilibrium temperature is compared with the result of running the same model run with the CO_2-concentration kept constant. The difference between the two runs yields the equilibrium temperature change (climate sensitivity). Thus the climate sensitivity is the *potential* temperature change that will be realized fully only if a new equilibrium is established after a doubling of the CO_2-concentration. It is generally agreed that, in reality, the climate will take a long time to reach equilibrium because the thermal inertia of the oceans (which have a much larger heat capacity than the atmosphere) introduces a 'lag effect'.[18] Consequently, the temperature actually realized will always lag behind the equilibrium temperature that corresponds to a given CO_2-concentration.

The *realized* temperature change is the estimate of the non-equilibrium temperature change at the moment in time when the (gradually increasing) CO_2-concentration will have doubled. According to IPCC'90, given the current rate of increase in CO_2-concentration, 'the realized temperature rise at any time is about 50% of the committed temperature rise if the climate sensitivity ... is 4.5°C and 80% if the climate sensitivity is 1.5°C'.[19]

A further difference between 'climate sensitivity' and 'realized temperature change at a given time' is that the former generally refers to the temperature change induced by a forced change in only one isolated variable – the 'radiative forcing' caused by a doubling of the atmospheric CO_2-concentration.[20] In the model calculations of climate sensitivity, all the other climate-influencing variables – such as planetary albedo (reflectivity), aerosol concentration (particles suspended in the atmosphere) and evaporation – change only through their involvement in the internal feedback loops. The 'realized temperature change' will depend on changes in other external forcing variables that influence climate. An example is the local cooling effect of anthropogenic sulphate aerosol particles in the atmosphere.[21]

The estimates are generally based on the simulation results of General Circulation Models (GCMs) which are widely regarded as the most advanced climate models available at present.[22] GCMs are idealized mathematical representations of the climate system, including the atmosphere, ocean, ice and land surface, together with the processes, interactions and feedbacks that serve to couple these components.[23] In GCM models the atmosphere and ocean systems are represented by a three-dimensional set of grid-points. Physical laws, such as the equation of state for a gas, the hydrostatic balance, the conservation of mass, the conservation of energy, and so on, are used to calculate the fluxes of heat, mass, momentum, and the like, between the grid points. The resolution of the GCM grid is typically 3 to 4 degrees of latitude and longitude, with 10 to 20 layers in the vertical dimension.[24]

Six GCM-modelling groups have dominated the field of anthropogenic climate change, though many more climate-modelling groups are now moving into this area of research:[25]

- NCAR: US National Center for Atmospheric Research (Boulder, CO);
- UKMO: UK Meteorological Office model (the Hadley Centre group of John Mitchell);

- GISS: US NASA Goddard Institute of Space Studies (the group of Jim Hansen);
- GFDL: Geophysical Fluid Dynamics Laboratory (Princeton, NJ, USA, the group of Suki Manabe, Richard T. Wetherald and Ronald J. Stouffer);
- CCC: Canadian Climate Center (the group of George J. Boer);
- MPI: Max Planck Institut für Meteorologie, Hamburg (the group of Klaus Hasselmann).

The main differences between the models lie in the resolution, the feedbacks taken into account, and the way in which clouds, convection and ocean heat transport are modelled. Major uncertainties in the calculation of the climate sensitivity result from the representation of cloud formation, as well as from the omission of potentially important feedbacks within current models. Given that the importance of these uncertainties is currently not known, the estimates of climate sensitivity using current GCMs might be inaccurate.

The GCMs available at present are formulated as deterministic rather than stochastic models. That is, for each individual GCM run, they provide a numerical result for climate sensitivity which is a point value, without calculating an uncertainty range. A range of values is produced by combining individual estimates of climate sensitivity from different models, expert judgements and insights from palaeo-climatic studies.

The Construction of the Estimate of Climate Sensitivity

In this section we analyze how the experts in successive assessments have translated the model results and other evidence into a range for the CO_2-doubling temperature. In 1979, the US National Academy of Sciences' Ad Hoc Study Group on Carbon Dioxide and Climate, chaired by Jules Charney (from MIT), produced the first notable assessment of the CO_2-doubling temperature. This group started by making an inventory of existing GCM results, which gave a range of 2°C to 3.5°C. Following an additional examination of feedback mechanisms not yet included in the models at that time, they argued that:

> As we have not been able to find evidence for an appreciable negative feedback due to changes in low- and middle-cloud albedos or other causes, we allow only 0.5°C as an additional margin for error on the low side, whereas because of uncertainties in high-cloud effects, 1°C appears to be more reasonable on the high side.

This argumentation brought them to their conclusion:

> We estimate the most probable global warming for a doubling of CO_2 to be near 3°C with a probable error of ±1.5°C.[26]

Table 1 lists the estimates given for climate sensitivity in the assessments that have figured in the international arena since then. It shows that the

TABLE 1

Range of individual GCM CO_2-doubling results, estimated ranges of climate sensitivity and 'best guesses', as reported in successive assessments

Assessment	Reported range of GCM results for CO_2-doubling (°C)	Concluded range for climate sensitivity(°C)	Concluded 'best guess' (°C)
NAS'79	2–3.5	1.5–4.5	3
NAS'83	2–3.5	1.5–4.5	3
Villach'85	1.5–5.5	1.5–4.5	3
IPCC'90	1.9–5.2	1.5–4.5	2.5
IPCC'92	1.7–5.4	1.5–4.5	2.5
IPCC'94	not given	1.5–4.5	2.5
Bolin'95	not given	1.5–4.5	2.5
IPCC'95	2.1–5.2	1.5–4.5	2.5

stated consensus range for climate sensitivity has remained unchanged for two decades, even though the range of the individual GCM-results has changed over time.

In analyzing how the range of GCM outcomes has been translated into the estimated climate sensitivity range, we find several trends. First, the range of GCM outcomes of CO_2-doubling calculations widened between 1979 and 1985, 2°C being added to the upper end and 0.5°C to the lower end of the NAS'79 range (see Table 1). Second, there is a clear shift in the mode of reasoning used to 'translate' the results of GCM CO_2-doubling calculations into the estimated climate sensitivity as it appears in the assessment's conclusions. The Charney committee (NAS'79) *widened* the range of GCM outcomes for climate sensitivity from 2°C–3.5°C to 1.5°C–4.5°C by including a margin of error based on an expert assessment of the shortcomings of the models. The full scientific text of the Villach'85 meeting shows that the GCM outcomes for climate sensitivity range from 1.5°C to 5.5°C.[27] Yet, without any further argumentation within the report itself, this range is *narrowed down* in the official Conference Statement – the section directed to policy-makers – back to the 1.5°C–4.5°C estimate.[28] This raises the question of why the policy-makers' summary did not deviate from the previously accepted estimate, in spite of the wider range given in the full scientific text. According to Robert Dickinson, the author of the chapter of the Villach report that presents the wider 1.5°C–5.5°C figure:

> My 5.5 for Villach was inferred by showing you would get at least that if you took the current GCM with the strongest ice albedo feedback and combined it with the model with the strongest cloud feedback, so that both strong feedbacks were in the same model. At the meeting Suki Manabe was personally sceptical that such a large number could be achieved, and I recall that led the meeting to adopt the previous range.[29]

One climatologist who attended Villach has subsequently accounted for the meeting's rejection of the 5.5°C figure as too high for three reasons: first, from the 'intuitive judgement' that the climate system was unlikely to exhibit such a high sensitivity; second, if the climate system was indeed so sensitive our models would be unable to represent it since, even at the point of CO_2-doubling, the system would be in a state well beyond current variability over such time-scales, yet our models are calibrated to simulate the current and past climate with its lower degree of variability; and third, that since Dickinson used a statistical approach for his analysis, physical scientists are at liberty to interpret the results quite flexibly.[30]

Regarding the second reason given by this climatologist, there is a paradox common to much simulation modelling. The greater the degree of extrapolation from past conditions, the greater must be the reliance on a model as the instrument of prediction; hence, the greater is the degree of difficulty in doing just this.[31] Modelled change which deviates too far from the current state is likely to be unreliable. For example, with an increase of 5.5°C, Antarctic sea-ice could be completely wiped out, with massive changes in physical processes and feedbacks which would affect climate, but which are not yet reliably taken into account by current models. None of the three reasons, however, or any others, is included in the text of the Villach'85 report. It is notable that embedded in these three reasons is a self-confirming circular element in the science.

While the 1.5°C–4.5°C NAS'79 figure included an error margin based upon an expert assessment of the model-uncertainties, the Villach'85 1.5°C–4.5°C estimate does not include such an uncertainty assessment. In other words: although the numbers are exactly the same, they differ significantly in connotation, and thus in their meaning.

IPCC'90 employed the following line of argument in arriving at its 1.5°C–4.5°C estimate. The GCM results evaluated in the IPCC'90 assessment produced a range of 1.9°C–5.2°C.[32] However, IPCC made a selection of these GCM results, which narrowed the range back down to 1.9°C–4.4°C, a range which fits more closely with the previously accepted 1.5°C–4.5°C estimate. The IPCC's argument reads as follows:

> On the basis of evidence from the more recent modelling studies ... it appears that the equilibrium change in globally averaged surface temperature due to doubling CO_2 is between 1.9 and 4.4°C. The model results do not provide any compelling reason to alter the previously accepted range 1.5 to 4.5°C (US National Academy of Sciences, 1979; Bolin *et al.*, 1986).[33]

The IPCC chose 1988 as the dividing-line beyond which results were defined as 'recent'. By doing so, the GCM results that fell outside the 1.5°C–4.5°C estimate were excluded. These were a 4.8°C result from the GISS model from 1984, and two 5.2°C results from the UKMO model from 1987. The IPCC gave no reasons for excluding the calculations from before 1988, nor did they provide scientific arguments as to why the recent results are automatically better than the less recent ones. It is particularly strange that the GISS results were omitted, because the GISS model has been regarded, at least by some climatologists, as one of the better models for the study of anthropogenic climate change,[34] (although each of the GCMs has strengths and weaknesses).[35]

At first sight it might be considered legitimate to skip the two 5.2°C UKMO 1987 results, because these were succeeded by three recent results from the same modelling group which were included by IPCC'90 (from 1989, indicating values of 2.7°C, 3.2°C and 1.9°C). However, the main scientific difference between the 1987 and the 1989 UKMO results is the way in which clouds are represented in the model. In the 1987 simulations, clouds were represented as a function of relative humidity (RH) (that is, cloud formation occurs when the water vapour exceeds a given threshold). In the 1989 UKMO simulation, clouds were represented by an equation for cloud liquid water (CLW) (that is, an attempt was made to represent cloud formation in terms of more fundamental physical processes). It is not claimed, however, that the CLW representation is *better* than RH representation (or *vice versa*). In their 1989 paper, John Mitchell, Cath Senior and William Ingram state explicitly that 'although the revised cloud scheme is more detailed it is not necessarily more accurate than the less sophisticated scheme'.[36] The fact that both schemes are scientifically tenable therefore contributes to the uncertainty range.[37] For the purposes of the IPCC's scientific assessment, the UKMO 1987 simulation results cannot be discarded on the grounds that they have been 'replaced by new results'. There is also an inconsistency in the way in which model results were selected in IPCC'90, because their selection did include a 1986 4.0°C result of Manabe and Wetherald (from the GFDL model).

In conclusion, our analysis shows that IPCC'90 excluded those model results which did not accord with the previously accepted 1.5°C–4.5°C estimate, without providing a clear or consistent justification. In the 1992 supplement to the IPCC'90 report, it was concluded that:

> There is no compelling new evidence to warrant changing the equilibrium sensitivity to doubled CO_2 from the range of 1.5–4.5°C as given by IPCC 1990.[38]

However, if we read the report in more detail, we see that the new GCM results evaluated include figures of 4.8°C (CSIRO) and 5.3°C (LMD), and later on we read:

> New equilibrium GCM simulations have widened the range slightly to 1.7°C (Wang *et al.*, 1991a) and 5.4°C (Senior and Mitchell, 1992a), but no dramatically new sensitivity has been found.[39]

Again, no clear argumentation is given as to why IPCC'92 found no compelling evidence for changing the previously accepted estimate – why, in the policy-makers' summary, the model results mentioned in the scientific part of the report (which show a high value of 5.4°C) were not judged to provide such evidence. The only considerations mentioned in the text which might have contributed to this decision are the following:

> Recently, additional estimates of the climate sensitivity have been made by fitting the observed temperature record to the evolution of temperature

> produced by simple energy-balance climate/upwelling models, assuming that all the observed warming over the last century or so was due solely to increases in greenhouse gases ... Schlesinger *et al.* (1991) obtain a value of 2.2±0.8°C. ...[40]
>
> Energy-balance model considerations bring previous estimates of sensitivity (IPCC, 1990) more in line with the IPCC 'Best guess'.[41]

The IPCC experts are here drawing upon studies which use observations of the climate system over the last century, together with simple climate models, to derive an estimate of the climate sensitivity independently of GCM results. In doing so, they need to employ many untestable assumptions in order to relate 'realized' to 'equilibrium' temperature change, and many of these assumptions can be defined quite flexibly. This adds further ambiguity by introducing another method to deduce individual estimates of the climate sensitivity.

According to the first author of the section on the climate sensitivity in the IPCC'92 report (Lawrence Gates, of the US Lawrence Livermore National Laboratory), the climate sensitivity range was not extended in IPCC'92 because:

> In the absence of a comprehensive exploration of parameter space in the doubled CO_2-context, there appeared to be no compelling scientific evidence to change the earlier estimated 1.5–4.5°C range (which was itself an educated guess) since such a step would have given greater credibility to any new values than was justified.[42]

And according to Bert Bolin, chairman of IPCC, who wrote to one of us on this point:

> In the preparations of the 92 assessment there was an extensive discussion about whether the uncertainty range could possibly be reduced to 2–4°C or not. Since there were no good scientific arguments to do so, the estimate remained unchanged. . . .

. . . and he added that:

> The importance [of being] able to justify a change scientifically was more important than the need for continuity in the results from the assessments.[43]

Both these quotations reveal that the IPCC experts felt a great need for unambiguous scientific evidence before changing the range. Apparently, however, there is no equally great need for such evidence to *maintain* the range. What is clear from the comments of Dickinson, Bolin and Gates is that the initial 1.5°C–4.5°C range is not derived from a procedure they regard as scientifically sound. Apparently, the need for scientific rigour applies more strongly to *changing* the climate sensitivity range than it does to its maintenance. This suggests that the scientific status of the temperature range is much lower than is generally perceived by the public, and this is not solely due to (necessary) simplification for public and policy comprehension. To argue that changing the values would require weighty scientific justification is also implicitly to acknowledge that the public view of the 1.5°C–4.5°C figures is that they have been rigorously and precisely justified scientifically, whereas they have not. In the IPCC'95 report, the pattern of argumentation is the same as in the previous IPCC reports: the range is not changed because 'No strong reasons have emerged to change these estimates of the climate sensitivity'.[44]

In summary, the estimated range for climate sensitivity has remained unchanged over two decades even though the range of GCM outcomes has changed, as a result of shifts in modes of reasoning. This was achieved firstly by narrowing down the 'domain of types of uncertainty' included within the climate sensitivity range. In other words, in the first assessment a margin of error was included to account for the shortcomings of the model, whereas this was not done in later assessments. This is all the more surprising given that it was this initial assessment which established a range that future assessments have not proved able to change. Second, those GCM results lying outside the previously accepted estimate were screened out by disqualifying them as constituting 'no compelling evidence', or as 'not recent', but without providing any sound explanation. We suggest, however, that the experts were not engaged in these processes consciously, but rather were responding to a wider set of contingencies than 'purely scientific' considerations.

The 'Best Guess' and the Uncertainty about the Climate Sensitivity Range

Since it is a product of deterministic models, the 1.5°C–4.5°C range is not a probability distribution. There have, nevertheless, been attempts to provide a 'best guess' from the range. This has been regarded as a further useful simplification for policy-makers. However, non-specialists – such as policy-makers, journalists and other scientists – may have interpreted the range of climate sensitivity values as a virtual simulacrum of a probability distribution, the 'best guess' becoming the 'most likely' value. This ambiguity about the meaning of the range may have assisted its uptake by such communities.

In producing a 'best guess' for IPCC'90, the experts made the assumption that each of the three primary climate feedbacks (water vapour and lapse rate, cloud, and ice albedo) from the different models had a normal distribution of errors. The range of climate sensitivity estimates produced from combining these distributions is from 1.7°C to 4.1°C, with the mid-point at 2.9°C. However, the mid-point value chosen was 2.4°C because it was argued that, after combining the feedbacks, the higher end of the estimates for sensitivity was more sensitized to a change in forcing than the lower end, and hence the distribution was skewed towards the higher estimates. This approach has its limitations: first, it assumes that, as a first-order approximation, the three feedbacks identified capture the majority of the sensitivity; second, it assumes that the estimates of the sensitivities are in fact normally distributed, whereas there is no *a priori* reason for such an assumption; and third, as IPCC'90 states, the method assumes that the errors are independent of one another. IPCC'90's 'best guess' was 2.5°C, this being a more 'convenient' figure than 2.4°C.

Apparently, there was some discussion at IPCC'90 about whether the best-guess value should be changed to 3°C, this being closer to the middle of the range of model results (1.9°C–5.2°C) – approximately 3.5°C – and the figure of 3°C had also been accepted in the previous 1979 and 1983 NAS assessments. Some experts argued that the IPCC should not change the best guess unless it was very confident that it was scientifically justified, which was exactly the same argument as successfully maintained the range at 1.5°C–4.5°C during IPCC'90. In the case of the 'best guess', however, the argument was rejected. According to one participant, this was because of evidence from statistical analysis that the best guess was approximately 2.5°C, and also because, if the sensitivity was 3°C, more observational evidence of warming should have emerged. Both arguments are problematic, however, for reasons already noted above: (a) the statistical method is regarded by modellers as less rigorous than GCM output, and hence as allowing more flexibility in the interpretation of the output, which led the IPCC to reject Dickinson's statistically derived range of 1.5°C–5.5°C; and (b) climate sensitivity, as defined by the IPCC, cannot easily be related to the observed temperature change, since the latter does not include all the relevant forcing factors and is not at equilibrium (illustrating yet further the ambiguity in the definition of climate sensitivity).

According to an industrial scientist we interviewed, some of the scientific organizers during preparations for IPCC'90 wished to go even further than providing a 'best guess', and asked the modellers to provide a probability value for the 1.5°C–4.5°C range. This source claims that the Chairman of IPCC WGI argued that scientists should be able to use their own intuitive judgement in providing a probability value for the range. A figure of 80% likelihood was quoted, according to this source – that is to say, giving a 20% chance that the sensitivity would be out of this range.[45] That pressures to provide subjective probability judgements were being exerted upon the experts is revealed by the following statement made by a participant modeller (with our added emphasis):

> What they were very keen for us to do at IPCC [1990], and modellers refused and we didn't do it, was to say we've got this range 1.5–4.5°C, what are the probability limits of that? You can't do it. It's not the same as experimental error. The range is nothing to do with probability – it is not a normal distribution or a skewed distribution. *Who knows what it is?*

Informally at least, some climate scientists are prepared to provide a probability estimate for the climate sensitivity range, and to indicate how confidence levels might have changed over time. The reluctance of the assessment community to do so formally in a publication is probably a reflection of the lack of any explicable scientific methodology for underpinning this exercise. Recently, however, a few attempts have been made by decision-analysts and climate-impact assessors to obtain from climate modellers their subjective probability distributions for climate sensitivity.[46] Hence, once again because the tacit knowledge of modellers is not formally included in the climate sensitivity range, the changing interpretations of the latter are not effectively communicated to the non-specialist. Such scientific judgements are seemingly discredited in policy-relevant contexts such as the IPCC, but in the absence of elaboration they come to look somewhat arbitrary and inconsistent. In a comment on an earlier version of our paper, the climate modeller Stephen Schneider stressed the *ad hoc* character of the range:

> The range was never established by a firm decision-analytic protocol in the first place, but rather was a heuristic from a responsible, but somewhat sloppy, community in the 1970s.

In the assessments, no method for screening outlier estimates was ever established which was other than *ad hoc*. According to Schneider, guessing was anathema to these experts: and any new procedure would also be *ad hoc*, and would not yield more rigorous results. Consequently they just let the range stay as it was.

The 'best guess' and the uncertainty accounted for in the range have not been as consistent as the 1.5°C–4.5°C temperature range. Inclusion of these elements in the argumentative chains has been a source of additional flexibility in linking consistency with new knowledge. By allowing for less consistency in the best guess than in the range, modellers have been able to conduct a debate which included new scientific understanding, while also allowing for consistency in the high and low limits of the range, thus introducing more flexibility. Support for this interpretation comes from the decision taken at Villach'85 to change the range from 1.5°C–5.5°C in Dickinson's chapter to 1.5°C–4.5°C in the policy-makers' summary. IPCC'90 conducted an analysis similar to Dickinson's, but decided instead to change the 'best guess' value from 3°C to 2.5°C, rather than change the range.

Different Meanings and Functions of Climate Sensitivity

The concept of 'climate sensitivity', and the '1.5°C–4.5°C temperature range', are ambiguous entities. First, there is ambiguity regarding what the number range implies (for example: the total range of possibility; 90% confidence interval; or an educated guess?); and second, there is ambiguity about the scientific status of the number range (well-established knowledge, or an educated guess?). In Table 2 we list some statements relating to the meaning of the 1.5°C–4.5°C range from the policy-makers' summary, or equivalent, of successive assessments reports. We then discuss some of the key ambiguities and changes in the (apparent) definitions of climate sensitivity, and the meaning of the associated 1.5°C–4.5°C temperature range.

Equilibrium Change or Realized Change?

Table 2 indicates that until 1990, the climate sensitivity was designated as an equilibrium temperature change (entries NAS'79, NAS'83, Villach'85 and IPCC'90). Yet, strangely enough, the IPCC'92 and Bolin'95 quotations make no reference to the notion of equilibrium. However, the IPCC'95 quote re-includes the notion of equilibrium, and makes a clear distinction between transient and equilibrium responses to CO_2-doubling. The linguistic imprecision of leaving out the notion of equilibrium may allow other experts and policy-makers to interpret the range as referring to

TABLE 2
Formulations used to present the 1.5°C–4.5°C estimate of climate sensitivity in the parts of the assessments directed at the policy-makers

NAS'79:	Summary: 'When it is assumed that the CO_2-content of the atmosphere is doubled and statistical thermal equilibrium is achieved, the more realistic of the modelling efforts predict a global surface warming of between 2°C and 3.5°C'. (p.1) After discussing model shortcomings and assessing their consequences for the figure the NAS concluded: 'We estimate the most probable global warming for a doubling of CO_2 to be near 3°C with a probable error of 1.5°C'. (p.2)
NAS'83	Executive summary: 'Results of most numerical model experiments suggest that a doubling of CO_2, if maintained indefinitely, would cause a global surface air warming of between 1.5°C and 4.5°C'.
Villach'85:	Conference statement: 'The most advanced experiments with general circulation models of the climatic system show increases of the global mean equilibrium surface temperature for a doubling of CO_2-concentration, or equivalent, of between 1.5 and 4.5°C . . . values outside this range cannot be excluded'. (p.xxi)
IPCC'90:	Policy-makers' summary: 'The long term change in surface air temperature following a doubling of carbon dioxide (referred to as the climate sensitivity) is generally used as a benchmark to compare models. The range of results from model studies is 1.9–5.2°C. Most results are close to 4.0°C but recent studies using a more detailed but not necessarily more accurate representation of cloud processes give results in the lower half of this range. Hence the model results do not justify altering the previously accepted range of 1.5 to 4.5°C'. (p.xxv)
IPCC'92:	'. . . the evidence from the modelling studies, from observations and the sensitivity analyses indicate that the sensitivity of global mean surface temperature to doubling CO_2 is unlikely to lie outside the range 1.5 to 4.5°C'. (p.5)
Bolin'95:	'There is uncertainty about the most likely change of climate that would be associated with a given increase of greenhouse gases in the atmosphere, but earlier estimates of warming by 1.5–4.5°C for a doubling of the equivalent carbon dioxide concentration, remain unchanged. It is important to stress that this range does not include zero. In other words, the scientific community is confident that, if greenhouse gases continue to increase, there will be a climate change'.[47]
IPC'95:	Technical summary: 'The likely equilibrium response of global surface temperature to a doubling of equivalent carbon dioxide concentration (the 'climate sensitivity') was estimated in 1990 to be in the range 1.5 to 4.5°C, with a "best estimate" of 2.5°C . . . . No strong reasons have emerged to change these estimates of climate sensitivity'.

actual change, with further committed change still to be realized. Such leeway in the interpretation of the 1.5°C–4.5°C range allows data from a range of disciplines associated with GCM modelling and climate change to be more readily introduced into the argumentative chains.

CO_2 or Equivalent CO_2: Is a Single Value for the Climate Sensitivity Possible?

In scientific textbooks on climate modelling, the *climate sensitivity parameter* is usually defined as the response of the globally-averaged surface air temperature to a unit-change in forcing.[48] This quantity was designed as a simple measure of intercomparing feedback mechanisms in 0-dimensional climate models. Later, the same parameter was also used to investigate and intercompare feedback mechanisms in GCMs.[49] The GCMs at that time usually calculated an equilibrium climate for an increase in radiative forcing corresponding to CO_2-doubling – namely 4 W/m^2. The climate sensitivity parameter was then calculated by dividing the global mean CO_2-doubling temperature by this 4 W/m^2. From the very beginning, it was recognized that this method implicitly assumed that the climate sensitivity parameter 'is essentially independent of the type of forcing (e.g. a change in solar constant, an increase in atmospheric CO_2, or incorporation of natural tropospheric aerosols)'.[50]

The assessment community that emerged in the late 1970s used the CO_2-doubling temperature as a tool for investigating the origins of the different results of models (initially simple ones). During the 1980s, climate sensitivity was increasingly used by the assessment community, however, as a way of simplifying ever more complex models into a simple indicator for exploring and representing the risk of climate change. A significant shift thus took place in the identity of 'climate sensitivity' during the 1980s. From its original identity as a heuristic tool for model inter-comparison and understanding of the significance of different processes and feedbacks in climate change, climate sensitivity became an objective indicator or feature of the climate system which could be measured, empirically and with a model. This reification in scientific assessment of what was originally a hypothetical research entity continues to be a significant source of ambiguity in the present identity of climate sensitivity.

In IPCC assessments, the term *climate sensitivity* indicates the globally-averaged surface temperature increase associated with a doubling of atmospheric CO_2-concentration. Although the *climate sensitivity parameter* and the *climate sensitivity* (to CO_2-doubling) are in principle different concepts (and are expressed in different physical units), they are used in an inconsistent way, both in IPCC reports and in scientific publications.[51] At some places in the IPCC'95 report, the term *climate sensitivity parameter* is used to mean the climate sensitivity to CO_2-doubling.[52] The confusion of the concepts can be illustrated from the glossary in IPCC'95:

> In IPCC reports, climate sensitivity usually refers to the long-term (equilibrium) change in global mean surface temperature following a doubling of atmospheric CO_2 (or equivalent CO_2) concentration. More generally, it refers to the equilibrium change in surface air temperature following a unit change in radiative forcing (°C/Wm^{-2}).

As can be seen from Table 2, in the policy-makers' summaries, the NAS'79, NAS'83, IPCC'90 and IPCC'92 assessments use a more restricted definition of climate sensitivity which refers specifically to *CO_2-doubling* rather than to an equivalent change in radiative forcing. If the climate sensitivity is defined relative to *CO_2* only, we will refer to the *narrow definition*, but if it is defined relative to *forcing* associated with CO_2-doubling, or to equivalent CO_2-concentration, we will refer to the *wide definition.*

Villach'85 referred to '*doubling of CO_2-concentration, or equivalent*', while the Bolin'95 quote and the IPCC'95 quote (in Table 2) also talk of doubling of the *equivalent* carbon dioxide concentration. These ambiguous formulations introduce flexibility into the interpretation of the concept for which the range is given, since it is no longer clear whether the wide or the narrow definition is intended. The wider definition does seem to be partially embedded in the definition provided in the full scientific text of IPCC'92, when they say that climate sensitivity '*is a measure of the response of a climate model to a change in radiative forcing*' associated with CO_2-doubling.[53] However, in the policy-makers' summary of IPCC'92, the narrower definition is used.

Wei-Chyung Wang and his colleagues have shown that the use of an equivalent CO_2-concentration rather than the individual spatio-temporal forcing characteristics of each greenhouse gas may lead to an incorrect assessment of greenhouse warming. In their study, they included the greenhouse gases CH_4, N_2O, CFC-11 and CFC-12. They calculated a global surface equilibrium warming of 4.2°C using the equivalent CO_2-concentration, but 5.2°C using increased individual greenhouse gases.[54] Recent work on the cooling effects of sulphate and other aerosols in GCMs also illustrates the various current definitions of climate sensitivity.[55] Such work has questioned the assumption that the mechanisms of response from different sources of radiative forcing would be nearly the same (because different response mechanisms and feedbacks are implicated). It really might not be possible to simplify a multi-causal climate perturbation by means of a single climate sensitivity parameter.[56]

The experts can move between two definitions of climate sensitivity: the narrow one, which is especially suitable for providing a common currency in which policy-makers and other scientists can talk about how a given set of models responds to a specific, policy-relevant, anthropogenic forcing; and the wider one, which includes all sources of radiative forcing and which is a more suitable basis for dialogue with, and for maintaining credibility with, other climate scientists. The flexibility thereby acquired also permits the experts more leeway in accounting for why the 1.5°C–4.5°C range should not be changed – that is, they can shift from the narrow, CO_2-only definition to the wider definition and introduce aerosols, the historic record, and so on, as reasons why the sensitivity range should remain the same. If the wider definition is used, the use of historical data (as in the first quotation we presented from IPCC'92, pp. 301–302 above) becomes a more legitimate consideration.[57]

This episode illustrates that within the climate-modelling research community, the concept of climate sensitivity is much more complex and indeterminate than is acknowledged in the IPCC reports or other assessments cited, and causes attendant problems in stabilizing the associated temperature range.[58]

Ambiguity Regarding what the Range of Numbers Means

As we have already noted, the assessment reports themselves do not indicate how the 1.5°C–4.5°C range for climate sensitivity should be understood. When we asked Robert Dickinson about this he commented:

> Villach [1985] like most committee considerations of this topic could not agree on what the range meant; i.e. whether it was a one-sigma or two-sigma probability range or something else; I expect all would agree it was never intended to be the total range of possibility. That means, I suppose, that the numbers could live indefinitely, provided we changed their definition with further understanding. What this all means is that there is no good agreement upon methodology to determine what is the uncertainty range, and that the perceived uncertainty (as opposed to real if such a thing exists) has not changed much in the last 18 years.[59]

In delivering the IPCC Statement to the first session of the international negotiations on a climate treaty,[60] Bert Bolin clearly used the range to claim that the scientific community is confident that no climate change is an impossibility (see Table 2, entry Bolin'95). This claim is much stronger than Dickinson's qualification of the number range, and than the qualification provided by the Villach'85 conference statement (Table 2) that '*values outside this range cannot be excluded*'. In similar vein are our earlier quotes from the modellers themselves, referring to the range as an '*educated guess*', or stating simply: '*who knows what it is?*'.

The results of different runs from different models have been combined, to perform a kind of intercomparison. Collective 'authorship' is used to confer authority on the 'scientific' result. However, the combination process does not systematically identify and examine the implications of the different GCM model-structures, or of the different design of each individual model-run. Although attempts have been made (by attaching probability-distributions, for example) to increase the authority and precision of the overall climate sensitivity estimates, the flexibility of the process has made it possible to move the definition of the concept, and the meaning of the associated temperature range, between different groups and over time without overtly changing the figures.

Multiple Functions and Uses of Climate Sensitivity

Given these multiple meanings and definitions, it is perhaps not surprising that climate sensitivity has a range of uses. We will now argue that the 1.5°C–4.5°C temperature range acts as an index linking different policy worlds and scientific worlds.

For the GCM modellers themselves, one major use of the concept 'climate sensitivity' is that it serves as a benchmark for comparing GCM models.[61] In addition, climate sensitivity (in its wider definition) is implicated in discussions between modellers over whether different forcings produce different sorts of responses. This research issue is concerned with the spatio-temporal characteristics of different forcing factors, and the micro-physical properties of the atmosphere. The wider definition of climate sensitivity is in effect a hypothesis that the global equilibrium temperature response at the surface is independent of the source and type of climate forcing. This contrasts strongly with the narrow definition of climate sensitivity, which is not based on these assumptions. Those climate modellers who use simpler models than GCMs also use climate sensitivity as a parameter. They can use the temperature range to compare elements of their models, or to provide 'independent' evidence for comparison with GCMs. Climate sensitivity is in addition a useful device with which to aggregate diverse bodies of knowledge – such as GCMs, simple climate models, observational data and palaeoclimate data. In preparing assessments of climate change for the policy world, climate sensitivity summarizes a highly complex field of science in such a way that it can more easily be appreciated by policy-makers. It provides policy-makers and advisors with a 'window' into the world of GCM modelling.

In the impact-assessment communities, the temperature range is called a 'three-fold range': 'high estimate' (4.5°C), 'best estimate' (2.5°C), and 'low estimate' (1.5°C).[62] By avoiding a precise definition of what the 1.5°C–4.5°C range is, and by using the ambiguous terms 'high', 'best' and 'low estimate', a broad community of meaning can exist across the diverse social worlds involved in the climate issue while, at the same time, differing meanings are invested in these uniting concepts by different communities.[63] Climate sensitivity also allows those who use GCM-output (such as the 'climate impact' community, who assess the consequences of climate change on agriculture, hydrology, ecology, and so on) or those who work with GCM modellers by adding new processes to the models (such as ecological modellers and atmospheric chemists) to have a way of indexing the range of different GCMs. It is important to have a simple way of indexing GCMs given that these scientists cannot possibly understand GCMs in all their complexity, or be privy to the tacit knowledge which surrounds the practice of developing and applying the models. Climate sensitivity is also a useful indicator and an interpretive resource in research which couples different GCMs with ecological/chemical models, or which uses different GCMs to drive impact models. Integrated Assessment

Models (IAMs), which are used for scenario studies of the climate problem,[64] combine a simplified climate model with a range of other models – impacts, carbon-cycle, atmospheric chemistry, economics, and so on. IAMs directly use the consensus estimate for climate sensitivity as an input parameter.

In summary, for policy-makers, the climate sensitivity range functions as a highly aggregated 'consensus-summary' of scientific understanding of the climate problem, and is a way of evaluating future model runs. For scientific users of GCMs, the climate sensitivity range is a useful way of creating a small range of climate change values which covers the range of likely certainties, and it is also a simple interpretive resource for those coupling GCMs with other models. Finally, for GCM modellers, basic research questions (for instance, over whether climate sensitivity varies with the spatio-temporal characteristics of the forcing) are raised by the wider definition of climate sensitivity, and this maintains the interest of the research community.

The 1.5°C-4.5°C Temperature Range as an *Anchoring Device* in the Climate Debate

We have illustrated how ambiguity over the precise meaning and application of climate sensitivity does not hinder the use of the concept, but facilitates the emergence of a common community of climate researchers – modellers, impact specialists, policy analysts and so forth. We have also argued that the ambiguity about whether climate sensitivity is a heuristic methodology for investigating and comparing model feedbacks, or an objective feature of the climate system to be calculated by the use of a model, reflects the establishment of the present model-based analytical framework for calculating anthropogenic climate change. Yet how does this 'social binding' role of ambiguity coexist with the apparent stability of the temperature range for two decades?

The experts have to negotiate support and credibility for their assessment reports both with their scientific peer groups and with policy 'customers'. Their problem consists of translating scientific knowledge into a form appropriate for policy actors, while keeping favour with the surrounding research communities. Star and Griesemer's concept of 'boundary-objects' is useful here, since it addresses the question of how heterogeneity in the perspectives and practices of the various actors involved in scientific work can coexist with the co-operation between these actors required for carrying out assessments.[65] Given that, without translation, a particular sort of scientific practice – in this case, scientific assessment – will not occur, how is translation achieved without alienating those other actors by over-coercion (which is unlikely to succeed)? One means, Star and Griesemer suggest, is through boundary-objects – these being, for example, relatively stable and reproducible things, people, projects, texts, maps and ideas which facilitate (or make possible) communication between different actors or 'social worlds'. The climate sensitivity range appears to function much like a boundary-object, helping to hold a variety of scientific and policy endeavours together in a common envelope of interpretation, while more specific meanings emerge for the different constituencies.

Note, however, that this boundary-object analysis does not require that the temperature range be stable. Flexible interpretation around a common core meaning could coexist with a shifting consensus range, although a degree of inertia would attach itself to the initial concept because of the need for some renegotiation between the social worlds involved, were the temperature range to change. However, the level of inertia surrounding the climate sensitivity temperature range appears to be much higher and more influential than we would expect from this consideration. We therefore introduce the concept of an *anchoring device* to describe a highly stable boundary-object in a context of scientific and social flux.

Anchoring devices are highly aggregated and multivalent consensus knowledge constructs, interfacing between science and policy. Compared to boundary-objects (which seem to emerge out of more horizontal social interaction), anchoring devices seem to function as a means of managing uncertainty in that they limit 'drifting' in the primary scientific case, and thus serve to constrain the discourse (implying a more vertical set of social interactions). Not all the social worlds implicated have the same ability to change the range: for example, climate-impact specialists have less influence on the range, and on the definition of climate sensitivity, than GCM-modellers. Nevertheless, climate-impact scientists still contribute to ambiguity in the meaning of climate sensitivity in the wider scientific and policy context, through their social and cognitive uptake of the concept, even if climate modellers might consider that usage to be 'wrong'.

We next want to understand how the temperature range became 'anchored'. We suggest that this emerged from the interplay of a range of circumstances (listed below) bearing upon the climate modellers – some more connected to their own social world of research, some more related to other scientific fields and policy worlds. At this stage, our comments are speculative, but they are based on our interviews and discussions with scientists and policy actors. It is currently difficult to obtain more definitive sources of evidence to test our hypothesis. We identified the following circumstances which may have contributed to the anchoring we observed:

1. There is the significant influence of peer-review within climate modelling. How will colleagues view a change in the temperature range, given that no better methods for its calculation exist than the ones already used to derive a consensus of 1.5°C–4.5°C? Additionally, a change in the temperature range would focus attention on the methods originally used and their inadequacies, which could be potentially embarrassing for the climate-modelling community.

2. We note that modellers are wary about suggesting that the most recent model calculations are automatically 'better' than earlier ones. However, experience indicates that policy actors often do make this presumption, sometimes with important political repercussions.[66] This consideration relates to the indeterminacy surrounding climate-change modelling, which limits the confidence of modellers in any single model run. Hence the assessment community has invested much of their confidence in the consensus range as agreed by previous assessment exercises, and has been wary of relying too heavily on more recent calculations – which are relatively few in number, and less analyzed by the research community.
3. The emergence of a distinctive analytical framework or paradigm for climate-change modelling in the 1970s is a further contribution to the maintenance of the temperature range. The most important three feedbacks within climate models (sea-ice albedo, water vapour/lapse rate, and cloud properties) have persisted since that time, despite the addition of more and more processes and complexity. Although climate models have changed enormously, much of the temperature response to a doubling of CO_2 is still produced by the same three feedbacks as were dominant in the early climate models. Paul Edwards also has stressed the role of models in the creation of a public space, including shared knowledge, shared values, and access to common tools and data, for consensus-building on global change issues.[67]
4. The need for some consistency in the scenarios of climate change used by the climate-impacts community may also contribute to the maintenance of the temperature range (perhaps through the influence of funding agencies).
5. Advisory scientists may also have felt a need to create and maintain a robust scientific basis for policy action, which in our case means a consistent range of climate sensitivities. In many studies of science for policy, this has been seen as the prerequisite for maintaining support and credibility from all the actors and social worlds involved. If the apparent scientific rationale for policy were to be too closely tied to new scientific findings, the basis for policy actions could be undermined, especially within the context of a highly politicized and polarized societal debate on the issue at hand (a parallel case being the ozone-depletion issue).[68]

A political demand for scientific consensus and unambiguous quantitative information in the assessment process would be likely to grow as science moves nearer to the context of policy making, and to the political process surrounding the climate negotiations. Hence, as the scientific assessment process became formalized at the IPCC, and as the climate negotiations have progressed, so the demand from policy-makers for the presentation of certainty, consistency and robustness of scientific knowledge may have become more pressing upon scientists. Supporting evidence for this comes from two close observers of the IPCC process: John

Lanchbery and David Victor have argued that the combined effects of the size of the IPCC, the consensus mechanism, and its role of providing balanced scientific judgments, all tend to lead to the IPCC rarely making recommendations of a radical nature, or reaching conclusions which are at all controversial.[69] We have seen that modellers were clearly reluctant to make strong claims in an IPCC consensus document about the best-estimate, or the likelihood of the sensitivity falling outside the 1.5°C–4.5°C range.[70] The experts, taking into account the need for some consistency and certainty, presented the model-derived range of climate sensitivity values as being well within the bounds of what can be stated with certainty, while treating precise probability statements as lying beyond these bounds. Changes in the 'best guess' figures, and in the implicit (and informal) distribution function, absorbed new insights and knowledge without challenging the impression of certainty emerging from the stability of the 1.5°C–4.5°C range.

Even before the emergence of the IPCC assessment process, we have found evidence of social pressure against any deviation from the climate sensitivity range in public and policy contexts. In an internal KNMI memorandum, Professor C.J.E. Schuurmans provided an account of the Toronto 1988 Conference on the Changing Atmosphere. He ends with some personal impressions, one of which reads:

> Schneider (NCAR) objected to the inclusion in the Conference Statement of the 1.5°C to 4.5°C temperature increase within 50 years. He thought that such an exact estimate was unwarranted in such a report. Yet, the conference was not willing to drop these numbers, as they were adopted from the Villach report, which forms the scientific basis of conferences such as this one. In general, questioning scientific judgements at this conference was not popular.[71]

In our view, it is the interplay of the five elements we have listed which accounts for the anchoring of climate sensitivity, despite changing knowledge and multiple interpretations, which we understand to be accommodated by the multivalency of boundary-objects.[72]

Conclusions and Discussion

According to the predominant 'classical' view of science for policy,[73] more knowledge means less uncertainty, which leads to more policy cohesion. On the other hand, as controversy studies in science studies indicated long ago,[74] more scientific knowledge often leads to more elaborate scientific conflict, and thus to greater (or, anyway, continuing) uncertainty. More recent studies support the view that policy conclusions and expert interpretations are 'underdetermined' by any given scientific knowledge, because of the repertoire of interpretive possibilities at each link in the argumentative chain. New data introduce more flexibility, although negotiated interpretive links, once made, are often consolidated as if they were determined naturally. Hence, in the science–policy nexus, scientific knowledge can be argued to be (at least in some respects) mutually, inseparably and synchronically constituted with policy 'responses' and processes, and even with the identities of policy actors. Thus the dominant discourse, in postulating transfer of separately predetermined knowledge from science to policy, may conceal a more complex and indeterminate process of mutual validation between these worlds.

In this paper we have added a new dimension to understanding the role of scientific knowledge in policy by emphasizing and examining the multivalent and anchoring character of scientific consensus. The estimated range of the climate sensitivity to CO_2-doubling of 1.5°C–4.5°C has remained remarkably stable over two decades, despite the huge growth of climate science. We have argued that, while the meaning of such terms appears 'natural', and negotiation of their magnitude appears to be the sole issue at stake, in practice tacit negotiation of those basic physical, social and policy meanings, including actors' subject-identities (domains of responsibility and agency, and peer-group responses, for example) is occurring through this ostensibly purely 'objective' process.

We have used Star and Griesemer's notion of the boundary-object (which holds widely separate communities of different practice together in a larger 'minimalist' shared identity, while at the same time allowing local communities to assign their own specific local meanings to these 'common' objects in the boundary) to describe and help understand the public stability of the 1.5°C–4.5°C temperature range, while it is, in its precise meaning, simultaneously unstable and ambiguous. Rather than interpret this combination of instability and ambiguity as evidence of unscientific imprecision and even duplicity, it seems more appropriate to treat it as an illuminating part of the intrinsic 'underdetermination' of knowledge-claims from given premises, modelling assumptions and observations. It also seems to be a likely property, especially, of knowledge that is shared and developed across open and diverse networks.

Consensus in such networks is much more complex and multi-dimensional than a simple agreement based on shared beliefs and uniform interpretations. Consensus here seems to be better seen in the spirit of Knorr-Cetina's notion of 'genealogy',[75] which involves a shift from 'consensus formation' to selection through processes of reconfiguration. 'Genealogy' is used here in the sense of a temporal ordering of agreement formation, involving distinctive 'generations' of efforts organized into overlapping sequences. However, drawing on research on experimental high-energy physics (HEP), Knorr-Cetina has suggested that the nature of 'consensus' may vary from one arena of practice to another, depending upon the 'social ontology' of the arena.

One key property of the anthropogenic climate-change arena – in contrast with Knorr-Cetina's HEP arena, and Star and Griesemer's zoology museum – is its policy relevance. This seems to add new dimensions to the role of climate sensitivity and the associated temperature range as a boundary-object, aspects which did not figure in the zoology museum pieces analyzed by Star and Griesemer. The whole arena in which climate risk assessment takes place is much more politically-charged and controversial, with much greater societal stakes and resources implicated, than in the (by comparison) arcane world surrounding a museum of vertebrate zoology. Star and Griesemer's boundary-object passively allowed essentially separate bilateral relations to coexist between the object and each social-cognitive world with which it interacted, and which it helped sustain. The existence and form of one such set of relations did not strongly constrain the possibility and form of any other. In the climate sensitivity case, the 'boundary-object' plays a more proactive role, and spans different social worlds which are more complex and ill-defined. As a result, tacit differences of meaning attributed to the boundary-object may be articulated more explicitly, as more active, detailed and robust co-ordination is sought between those diverse areas of practice and interpretation. However, one would expect this process to be limited by counterpressures. For example, the IPCC tries to maintain its legitimacy and credibility with research communities, yet at the same time aims to offer usable products to policy communities. The more interfaces across which a boundary-object comes to operate, and the more proactive a vehicle (for enrolment) it is, the greater the tension may become between the tangential need for differences to be articulated and the need for flexibility.

We have also introduced the concept of an *anchoring device* to describe a boundary-object which exhibits inertia and helps stabilize flux in a socio-scientific domain. Anchoring devices seem to manage uncertainty in that they prevent the primary scientific case (founded on informed judgement) from drifting, and this serves to constrain the related policy discourse. We argue, however, that anchoring can only be understood as an unintentional consequence of the interplay of a set of judgements concerned with peer-review, policy uptake of new findings, paradigmatic analytical commitments and the policy treatment of uncertainty and change in knowledge. The positive effects of anchoring are that it creates a common plenum within which negotiation of positions beyond the immediate scientific questions can be conducted – for example, over the putative construction of an as yet non-existent new collective social identity of global agency and responsibility that focuses on greenhouse gas controls, adaptation to projected climate-change effects, technology transfer, and a range of further socio-technical innovations and corresponding identity changes. Without such anchors there might be no coming together at all of disparate parties, and thus disintegration of any incipient policy community. Consequently, the germane issue that is opened up by our theoretical approach is: which actors and which forms of argument are enfranchised, and which disenfranchised, by the use of such an apparently precise quantitative discourse in constraining and shaping the negotiations process? Process and substance are not so cleanly separable as is sometimes implied.

The tacit *projective* roles played by scientific constructs such as climate sensitivity, and the mutually constitutive relationship between the science and the putative social or political order, are important. The 'user and policy communities', to whom climate science assessment has been directed, are ambiguous and diffuse, to say the least. Their imagined character may in fact provide the necessary conditions for the emergence of such wide-ranging boundary-objects.[76] In a more clearly defined set of social worlds, in which meanings and identities are better attuned, interpretive flexibility might well be less. The paradoxical prospect is that the more far-reaching sociotechnical endeavours, those which may massively refashion the social and material world, rely most on ambiguity embedded within superficially precise scientific knowledge, and on the creative imagining of putative identities.

Other insights from our analysis are more challenging to the contemporary science and policy surrounding global climate change. Even within the climate-science community, multi-dimensionality of meaning is often regarded as a lack of precision, which is usually seen as antithetical to 'good science'. Conventional normative notions of 'good science' in research cultures may not, however, necessarily be appropriate for science in policy arenas, even though this is typically how these notions have been institutionalized in policy advisory processes.

Anchoring introduces an additional potential for confusion if a particular interpretation articulated by or for one group (say a particular set of users) is encountered by another social group with different (realized or emergent) needs, understandings, assumptions and interests. Furthermore, this kind of confusion can involve evasions of responsibility. For example, the expression of climate sensitivity as a quantitative range with upper and lower limits invites further questions, such as: 'what is the best estimate?' and, 'is the upper limit the same as the worst case?'. According to Table 1, the climate sensitivity could be 5.4°C rather than 4.5°C. This difference can have a significant impact on the answer to policy questions such as: 'how high should our dikes be to maintain our safety standard to prevent the risk of flooding in the next decades?'. These questions are not born of ignorance and immaturity, but are a reasonable step from the initial representation. Who – the policy user, or the scientist-advisor – is responsible for any ensuing misunderstanding of the science, and ensuing possible mistaken policy-commitments, is an open question.

What is more, the anchoring properties and capaciousness of the climate sensitivity concept may lull the policy consciousness from recognizing issues beyond what are in essence variations around the existing expressed range of sensitivity. For example, the very success of the 1.5°C–4.5°C range, including the arguments about where the best estimate value is, what the margins mean, and so on, may defocus policy attention from other relevant questions, such as:

- the possibility of regional differentiations and variations outside of the range;
- the possibility of more abrupt changes in climate as it passes through possible critical thresholds;

- whether we have really addressed, or only presumed through accumulating practice to answer the original climate research question: namely, *whether* long-term climate processes and human interference are scientifically feasible over the time-scales claimed;
- whether the cohering climate concept of greenhouse gas emissions is really the most important, effective and acceptable way of defining international priorities with regard to global environmental change and its close associates – human poverty, inequity, and global consumption and land-use patterns.

The last two questions exist in the margins of the current climate scientific debate, but are screened out of mainstream issue-framing and its related assessments and representations of the 'science for policy'. They would upset the existing commitment by most (though by no means all) policy actors and institutions to the idea of smooth and manageable forms of anthropogenic climate change, corresponding with the idea of its intellectual amenability to the modern epistemic culture of prediction and control. The more fundamental scientific questions do not automatically imply dissipation of policy action. On the contrary, they could well sustain, and be sustained by, a different, more urgent and radical public policy agenda. In such an agenda, climate sensitivity would not necessarily be an anchoring concept. Thus anchoring and the flexible quality of conceptual boundary-objects have a double-edged character which it may be important, not only to acknowledge (after all it is akin to Kuhn's notion of the double-edged quality of scientific paradigms in normal science), but systematically to examine – for example, for their deeper cultural dimensions and forms of reinforcement.

Notes

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15. See, for example, Michel Callon, John Law and Arie Rip (eds), *Mapping the Dynamics of Science and Technology* (London: Macmillan, 1986); Bruno Latour, *Science in Action: How to Follow Scientists and Engineers through Society* (Cambridge, MA: Harvard University Press, 1987); Latour, Philippe Mauguin and Geneviève Teil, 'A Note on Socio-Technical Graphs', *Social Studies of Science*, Vol. 22, No. 1 (February 1992), 33–57.

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18. See, for example, Tom M.L. Wigley, 'Climate Scenarios', in the collected papers of the European Workshop on Interrelated Bioclimatic and Land Use Changes (Noordwijkerhout, The Netherlands, 17–21 October 1987), 52; or Houghton, Jenkins & Ephraums (eds), op. cit. note 6.
19. Houghton, Jenkins & Ephraums (eds), op. cit. note 6, xxvi.
20. 'Radiative forcing' is defined as the net effect of a greenhouse gas on the average net radiation at the top of the troposphere, caused by a change in either solar or infrared radiation. It perturbs the balance between incoming and outgoing radiation. See Houghton *et al.*, op. cit. note 8, 49.
21. Some recent models do include the anthropogenic sulphate aerosols.
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26. US NAS, op. cit. note 2, 2.
27. Robert E. Dickinson, 'How Will Climate Change? The Climate System and Modelling of Future Climate', in Bolin, Döös, Jäger & Warrick (eds), op. cit. note 4, 206–70, at 262.
28. Bolin, Döös, Jäger & Warrick (eds), op. cit. note 4, xxi.
29. Email message from Robert Dickinson (25 April 1995).
30. Interview with climate modeller (11 April 1993). In particular, according to some modellers, the skewed distribution of the estimates of climate sensitivity, due to the amplified positive feedbacks at the higher sensitivities, was not properly taken into account by Dickinson.
31. M. Bruce Beck, 'Understanding Uncertain Environmental Systems', in Johan Grasman and Gerrit van Straten (eds), *Predictability and Nonlinear Modelling in Natural Sciences and Economics* (Dordrecht, The Netherlands: Kluwer, 1994), 294–311.
32. Houghton, Jenkins & Ephraums (eds), op. cit. note 6, 81.
33. Ibid., 138. The two works cited are, respectively, NAS'79 (op. cit. note 2) and Villach'85 (op. cit. note 4).
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38. Houghton, Callander & Varney (eds), op. cit. note 7, 111, Table B2.
39. Gates *et al.*, op. cit. note 37, 118.
40. Ibid.
41. Ibid.
42. Email message from W. Lawrence Gates (18 May 1994).
43. Fax message from Bert Bolin (17 August 1994).
44. Houghton *et al.* (eds), op. cit. note 8, 34.
45. Interview with an industrial scientist (24 June 1993). This same source claims that the modeller who came up with the 80% probability value did so by canvassing the opinions of eight modellers, five of whom replied. Four said that the sensitivity would fall into the 1.5°C–4.5°C range; one said not. Hence the 20%.
46. See, for instance, M. Granger Morgan and David W. Keith, 'Subjective Judgements by Climate Experts', *Environmental Science and Technology*, Vol. 29 (1995) 468A–76A, and James G. Titus and Vijay Narayanan, 'The Risk of Sea Level Rise; A Delphic Monte Carlo Analysis in which Twenty Researchers Specify Subjective Probability Distributions for Model Coefficients within their Respective Areas of Expertise', *Climatic Change*, Vol. 33 (1996), 151–212.
47. Bert Bolin, 'IPCC Statement to the First Session of the Conference of Parties to the UN Framework Convention on Climate Change' (Berlin, 28 March 1995).
48. For example, as defined by Jeffrey T. Kiehl, in Kevin E. Trenberth (ed.), *Climate System Modeling* (Cambridge: Cambridge University Press, 1992), 321.
49. Robert D. Cess and Gerald L. Potter, 'A Methodology for Understanding and Intercomparing Atmospheric Climate Feedback Processes in General Circulation Models', *Journal of Geophysical Research*, Vol. 93, Series D, No. 7 (1988), 8305–14.
50. Ibid., 8306.
51. We find increasingly that the scientific literature uses the IPCC's definition of climate sensitivity. The emergence and spread of the concept in the early 1980s seems to have been of importance to the current framing of the anthropogenic climate change discourse, and *vice versa*.
52. For instance, see Houghton *et al.* (eds), op. cit note 8, 423.
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55. For example, K.E. Taylor and Joyce E. Penner, 'Response of the Climate System to Atmospheric Aerosols and Greenhouse Gases', *Nature*, Vol. 369 (30 June 1994), 734–37.
56. See also Tom M.L. Wigley, 'Outlook Becoming Hazier', *Nature*, Vol. 369 (30 June 1994), 709–10.
57. For the quote, see note 40. The recent IPCC'94 report also uses a wider definition in the full scientific text: see Houghton *et al.* (eds) (1995), op. cit. note 7, 169–70.
58. In a personal communication (4 July 1994), Mike E. Schlesinger stressed the underdetermined character of the notion of climate sensitivity: 'The number of unknowns is much greater than the number of independent pieces of information we have'.
59. Dickinson, loc. cit. note 29.
60. More precisely, the Conference of Parties to the UN Framework Convention on Climate Change, held in Berlin on 28 March 1995: see note 47.
61. For example, from IPCC 1990: 'climate sensitivity . . . is generally used as a benchmark to compare models': see Houghton, Jenkins & Ephraums (eds), op. cit. note 6, xxv.
62. Note that a translation of the 'best guess' into the 'best estimate' has taken place in such uses of climate sensitivity, implying greater certainty in the figure.
63. A remaining issue concerns the extent to which these sources of flexibility arise from linguistic imprecision – that is, the lack of any clear use of an unambiguous definition of climate sensitivity – rather than the lack of its existence. If scientists kept to more precise definitions of climate sensitivity, then it should in principle be possible to

reduce the interpretive flexibility from changes in the connotations of the range, and in the definition of climate sensitivity (and, to a lesser extent, from changes in the types of uncertainty accounted for, and in the implications of the range). The fact that scientists have not sought more precision by tightening up their definitions, however, does not reflect any conscious desire on their part to maintain interpretive flexibility in order to facilitate boundary interactions. This would imply that the advisory scientists are exercising too much conscious and instrumental strategy. Rather, it reflects the hypothetical (hence pre-definitional) character of climate sensitivity from a climate research perspective. Linguistic precision may be rejected because it would commit scientists to the notion that climate sensitivity, in its wider formulation, is actually a 'proven' scientific concept. The IPCC can give the appearance of exercising linguistic precision because the narrow definition it uses is purely operational – as befits 'science for policy' perhaps, though as we have seen the IPCC trades heavily upon definitional flexibility.

64. For example, the Integrated Model to Assess the Greenhouse Effect (IMAGE), developed at the (Dutch) National Institute of Public Health and Environmental Protection (RIVM), and the Atmospheric Stabilization Framework (ASF), developed at the (United States) Environmental Protection Agency (EPA).
65. Star & Griesemer, op. cit. note 16.
66. For example, President Bush's science adviser, Allan Bromley, used the UKMO's 1989 model calculations (which produced a climate sensitivity of 1.9°K by inclusion of water phase change in the representation of clouds) as a reason for questioning the robustness of the scientific basis for human-induced climate change. According to an article in *Science*: 'Asked by Gore whether the scientific evidence is inadequate to justify curbs on greenhouse emissions, Bromley responded that recent adjustments to climatic models by British climate modeller John Mitchell have made him uneasy about the reliability of predictions ... "That such simple and obvious changes in [Mitchell's] model can make major changes in predictions underscores my own feeling ... that we have a substantial distance to go yet" ': see Marjorie Sun, 'Global Warming Becomes Hot Issue for Bromley', *Science*, Vol. 246 (3 November 1989), 569. This article prompted a British official to write to a colleague in government: 'It is rather frustrating to see John [Mitchell]'s results being used in this way ... John places no more confidence in the 1.9°K result than the 5.2°K result' (letter dated 15 November 1989). This sort of experience of how the latest results of model simulations are widely received by influential policy-makers such as Bromley may be quite important in how modellers and other advisors have come to represent the consensus view of the climate sensitivity.
67. Paul N. Edwards, 'Global Comprehensive Models in Politics and Policy Making', *Climatic Change*, Vol. 34 (1996), 1–16.
68. See also: Yaron Ezrahi, 'Utopian and Pragmatic Rationalism: The Political Context of Scientific Advice', *Minerva*, Vol. 18, No. 1 (1980), 111–31; and Arie Rip, 'Expert Advice and Pragmatic Rationality', in Nico Stehr and Richard V. Ericson (eds), *The Culture and Power of Knowledge: Inquiries into Contemporary Societies* (Berlin: Walter de Gruyter, 1992), 363–79. Similar ideas were expressed in a lecture by Willem J. Kakebeeke (Assistant Director General for the Environment at the Netherlands Ministry of Housing, Physical Planning and the Environment) at the 280th Scientific Meeting of RIVM (the Netherlands National Institute for Public Health and Environmental Protection, Bilthoven, 24 March 1994).
69. John Lanchbery and David Victor, 'The Role of Science in the Global Climate Negotiations', in Helge Ole Bergesen and Georg Parmann (eds), *Green Globe Yearbook of International Co-operation and Development* (Oxford: Oxford University Press, 1995), 29–40.
70. There is even circumstantial evidence to suggest that modellers were dissuaded from staking out claims based on intuitive and tacit judgements, because of fears that some industrial scientists might criticize the absence of robust methodology.
71. KNMI (Royal Netherlands Meteorological Institute) memorandum DM-88-12 (De Bilt, July 1988).

72. We emphasize that it would be misleading to stress the role of policy too much, for three reasons: first, there is a lack of supporting empirical direct evidence; second, there exist alternative interpretations of the IPCC from that presented above (for example, regarding the IPCC as making rather bold claims, as in the somewhat controversial 1995 report); and third, it is questionable whether relatively minor changes in the temperature range might have had significant destabilizing effects upon climate policy-making and the international negotiations. We thank Stephen Schneider for discussions on these issues.
73. Brian Wynne, 'Carving out Science (and Politics) in the Regulatory Jungle', *Social Studies of Science*, Vol. 22, No. 4 (November 1992), 745–58.
74. See, for example, Dorothy Nelkin, 'The Political Impact of Technical Expertise', *Social Studies of Science*, Vol. 5, No. 1 (February 1975), 35–54.
75. Karin Knorr-Cetina, 'How Superorganisms Change: Consensus Formation and the Social Ontology of High-Energy Physics Experiments', *Social Studies of Science*, Vol. 25, No. 1 (February 1995), 119–47.
76. Benedict Anderson's account of colonial authorities' construction and use of maps and censuses shows that the abstract assumptions about societies built into those constructs (for example, ethnic distinctions, political borders, and the like) eventually began to order them materially, as routine administrative functions began to reflect and consolidate them in the identities and relationships of their subjects. In imagining into being a global policy community, practitioners of global climate science may likewise reshape the world in the image of their scientific tools and analyses, though the significant sources of resistance are diverse and quite unknown: see Benedict Anderson, *Imagined Communities* (London: New Left Books, 1981, revised and extended 1991).

Jeroen van der Sluijs is a research associate at the Department of Science, Technology and Society, Utrecht University. His research focuses on the management of uncertainties in (integrated) climate risk assessment. His doctoral dissertation, *Anchoring Amid Uncertainty: On the Management of Uncertainties in Risk Assessment of Anthropogenic Climate Change* (Utrecht, 1997), contains further detail on the topics contained in this paper; copies are available from the author.

Josée van Eijndhoven is Professor of Technology Assessment at the same Utrecht department, and Director of the Rathenau Institute in The Hague.

Simon Shackley has worked since 1992 on the sociology of climate change science and policymaking at the Centre for the Study of Environmental Change (CSEC) at Lancaster University. He is soon to take up a new post as Lecturer in Environmental Management and Policy at the Manchester School of Management, University of Manchester Institute of Science and Technology (UMIST).

Brian Wynne is Professor of Science Studies at Lancaster University, and Research Director of CSEC.

Addresses: (JvdS & JvE) Department of Science, Technology and Society, Utrecht University, Padualaan 14, NL-3584 CH Utrecht, The Netherlands; fax: +31 30 2537601; email: j.vandersluijs@nwsmail.chem.ruu.nl

(SS & BW) Centre for the Study of Environmental Change (CSEC), Lancaster University, Bowland Annexe, Lancaster LA1 4YT, UK; fax: +44 1524 846339; email: b.wynne@lancaster.ac.uk; shackley@msmail.lancs.ac.uk