MIT Joint Program on the Science and Policy of Global Change

Adjusting to Policy Expectations in Climate Change Modeling:

An Interdisciplinary Study of Flux Adjustments in Coupled Atmosphere-Ocean General Circulation Models

Simon Shackley, James Risbey, Peter Stone and Brian Wynne

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Adjusting to Policy Expectations in Climate Change Modeling:
An Interdisciplinary Study of Flux Adjustments in Coupled Atmosphere-Ocean General Circulation Models†

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Abstract

This paper surveys and interprets the attitudes of scientists to the use of flux adjustments in climate projections with coupled Atmosphere Ocean General Circulation Models. The survey is based largely on the responses of 19 climate modelers to several questions and a discussion document circulated in 1995. We interpret the responses in terms of the following factors: the implicit assumptions which scientists hold about how the environmental policy process deals with scientific uncertainty over human-related global warming; the different scientific styles that exist in climate research; and the influence of organizations, institutions, and policy upon research agendas. We find evidence that scientists' perceptions of the policy process do play a role in shaping their scientific practices. In particular, many of our respondents expressed a preference for keeping discussion of the issue of flux adjustments within the climate modeling community, apparently fearing that climate contrarians would exploit the issue in the public domain. While this may be true, we point to the risk that such an approach may backfire. We also identify assumptions and cultural commitments lying at a deeper level which play at least as important a role as perceptions of the policy process in shaping scientific practices. This leads us to identify two groups of scientists, "pragmatists" and "purists," who have different implicit standards for model adequacy, and correspondingly are or are not willing to use flux adjustments.

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† Revised Version, January 1999

1. INTRODUCTION

Controversy within science has often fascinated observers and practitioners of that science alike. How and why do differences of opinion and practice emerge between scientists working in the same specialty? Fascination (and engagement) with such controversy often grows in proportion to the public policy importance of such scientific knowledge. This is especially so in political cultures such as that of the USA where power is distributed, the relations between political factions are frequently antagonistic, and formal accountability for policy often rests upon technical knowledge. This has been shown by a large body of research on the role of science in regulatory policy making and risk assessment.[1,] According to many scientists, such controversy often distorts the real issues, as for example in the interpretations of climate change science promoted by "contrarian" or "skeptical" scientists and industry advocacy groups such as the Global Climate Coalition. Politicians and academics have argued that these views have received undue attention given the questionable credibility of many of their scientific arguments.[2] Climate change science is certainly not an equal contest between "two sides" as media accounts often imply (Edwards and Schneider, 1997).[2]

Such conflict may hinder scientific progress and intelligent policy because it is structured in binary terms—anthropogenic climate change, yes or no? Yet agreement among most climate change scientists that the contrarian interpretation is dubious does not imply that climate scientists entertain no differences of opinion among themselves. Such differences are often subtle, but in our view they concern key issues and may yield insights for both science and policy which the public conflict and its binary form obscure.

While the contrarians appear to distrust any knowledge supporting anthropogenic climate change, the mainstream climate science community trusts the basic tools, techniques and understanding of the profession: climate models, established data-sets, characterisation of key processes and feedbacks, *etc.* Disagreements within the mainstream community involve the relative strengths of such tools, the applicability of certain insights to particular arguments, and the degree of trust which can be invested in the different elements which together constitute the basis for the community's understanding.[3] Often, disagreements within this community are about the conditions under which different commitments might be appropriate.

Such differences within the scientific community are an important topic for climate scientists because they are more relevant to the everyday practices and preoccupations of the climate scientists than are the contrarians' arguments. They provide insight into the ways that climate scientists go about their work, think about scientific issues, build-up confidence in one approach rather than another, negotiate their standards of validation, consistency, accuracy, *etc.*, and define their identity

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[1] Jasanoff, S.: 1990, *The Fifth Branch: Science Advisers as Policymakers*, Harvard University Press, Harvard; Ezrahi, Y.: 1990, *The Descent of Icarus*, Harvard University Press, Harvard; National Research Council: 1994, *Science and Judgement in Risk Assessment*, National Academy Press, Washington D.C.; National Research Council: 1996, *Understanding Risk: Informing Decisions in a Democratic Society*, Stern, P. and Fineberg, H. (eds.), National Academy Press, Washington D.C.

[2] Brown, G.E.: 1997, "Environmental Science Under Siege in the U.S. Congress," *Environment*, **39** (2), 13-20 and 29-31. Edwards, P.N. and Schneider, S.: 1997, "The IPCC 1995 Report: Broad Consensus or 'Scientific Cleansing'?" *Ecofables/Ecoscience*, **1**, 3-9. Edwards, P.N., and Schneider, S.: "Self-Governance and Peer Review in Science-for-Policy: The Case of the IPCC Second Assessment Report," submitted manuscript.

[3] Hassol, S.J., and Katzenberger, J. (eds.): 1997, *Elements of Change 1996: Session Two: Characterizing and Communicating Scientific Uncertainty*, Aspen Global Change Institute, Aspen, Colorado.

in relation to other communities in and outside science. We will argue that a better understanding of such intra-community differences is of value to climate scientists themselves, because it can assist self-reflection and improve their presentation of the science to outside communities. In addition, a more nuanced understanding of the climate science community on the part of research funders, policy makers, NGOs, the media, and commentators on climate science, may help to build up trust between knowledge-producers and knowledge -users. Such trust may allow scientific uncertainty in the field of global climate change to be dealt with in a more constructive way than the typical view that uncertainty is a sign of weakness in scientific thinking and practice.

The Flux Adjustment Case-Study

In this paper we discuss a controversy that arose in climate change science in the late 1980s, over the use of "flux adjustments" or "flux corrections" in coupled Atmosphere-Ocean General Circulation Models (A/O GCMs). During interviews with climate modelers conducted in the early to mid-1990s, author Shackley came across strong opinions both in favour of, and opposed to, using flux adjustments. The controversy was not at that time discussed in the media, but was only evident when one talked informally to modelers at a number of centers. Authors Risbey and Stone, as climatologists themselves (but not then using coupled A/O GCMs) were concerned about the scientific assumptions behind the use of flux adjustments, and their implications for the reliability of model output. We also observed that discussions over flux adjustments came to have a more public and political content in the run-up to the Second Assessment Report (SAR) of the Intergovernmental Panel on Climate Change (IPCC), which was finalized at the end of 1995 and the beginning of 1996. For instance, a well known science correspondent wrote a piece about flux adjustment in *Science* entitled "Climate Modeling's Fudge Factor Comes Under Fire."[4] In the wake of the First Conference of the Parties to the Framework Convention on Climate Change (FCCC), held in Berlin in April 1995, several newspaper articles appeared in Germany making skeptical comments on the science of climate change, one of them citing the use of flux adjustments as supporting evidence.[5]

Author Shackley attended the Plenary Session of IPCC's science working group which met in Madrid in late 1995 to finalize the text of the SAR's Executive Summary. The IPCC process involves leading scientists from different countries contributing as lead authors to specific chapters of the IPCC's reports. A number of these advisory scientists were at the Madrid meeting. Industry representatives had been invited to submit comments on the draft text and they wanted the text changed so that the (in their view) still provisional character of GCMs was duly emphasised. A major justification for this was that coupled models required large flux adjustments to prevent climate "drift" in simulations of the current climate. For example, the Global Climate Coalition, an industry NGO, suggested inserting the following sentence in the Policy Makers' Summary:

> *Confidence in a coupled model's simulation of transient climate change is reduced if the climate drift is large and/or the feedbacks are seriously distorted by flux adjustment.*[6]

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[4] Kerr, R.: 1994, "Climate Modeling's Fudge Factor Comes Under Fire," *Science*, **265**, 1528.
[5] Flohl, R.: 1995, "Unsichtbare Hand lenkt Klimaforschung," *FAZ*, 12.4.95.
[6] Submission of the Global Climate Coalition to 5th IPCC WGI Plenary, November 1995.

At Madrid, the Global Climate Coalition's representative suggested inserting a box on flux adjustments in the Technical Summary. The advisory scientists present argued against this. In their judgment there was danger of over-emphasizing the importance of flux adjustments, even if there were indeed questions about their effects which could not currently be answered. Their view prevailed over that of the industry representatives. Similar arguments—that flux adjustments effectively invalidate coupled A/O GCMs—have been heard from industry scientists in other contexts.[7]

Author Shackley surmised from this (and other IPCC meetings he attended) that the advisory scientists' position was influenced by an awareness of the politically motivated attack on GCMs by some industry scientists (for the purpose of discrediting the scientific basis for, and hence delaying, the climate negotiations). The scientists were reluctant to give opportunities to critics by over-emphasizing (as they saw it) the issue of flux adjustments. It was indeed a sensitive issue. Drafts of the IPCC's supplementary report of 1992 also reveal that advisory scientists tailored the presentation of flux adjustments so that the issue would not assume what they regarded as undue prominence.[8]

This experience illustrates that advisory scientists and industry experts are not simply representing the scientific knowledge-base as accurately as possible in preparing expert summaries. They are also thinking about how different audiences will respond to the information presented, and they have an implicit idea of how the policy-making process works. It seems to us that industry experts are assuming that they can limit the policy implications of the IPCC's executive summary by including many caveats in the key sentences. The advisory scientists seem to be reacting against such attempts by limiting the use of such caveats. Therefore the mainstream advisory scientists and the industry experts / contrarians agree to some extent on the political consequences of such caveats. The latter however seemed intent on undermining the scientific case for climate change, presumably to hinder policy actions which are perceived to have negative consequences for the industrial sectors they represented. The former, on the other hand, were intent on defending the legitimacy of the scientific judgement which lay behind the IPCC's report and its Technical Summary from what they perceived as politically inspired attacks.

But how do such debates relate to the community of climate modelers, whose work and viewpoints the IPCC is supposed to represent? Do climate researchers indeed perceive flux adjustments as an issue which does not deserve prominence in summaries aimed at policy makers and non-specialists? Do flux adjustments significantly influence the status of climate models? Were these debates another instance of the industry and contrarian fringe trying to politicise a technical issue, best discussed within the climate modeling community? Below we set out to explore these questions.

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[7] For example, at a meeting Shackley attended in 1993 between modelers at the Hadley Centre and industry scientists and officials from the International Petroleum Industry Environmental Conservation Association (IPIECA). An IPIECA workshop in 1994 also concluded that: "Because of the physical non-reality of these adjustments, they raise questions about the physical basis of the models themselves. The adjustments could be an indication that some important physical processes in the climate system are missing or incorrectly represented in the models" (page 5, IPIECA, Experts Workshop on Critical Issues in the Science of Global Climate Change, London). This portrayal interprets flux adjustments, its effects and consequences, rather sceptically.

[8] See footnote 53.

Methodology

To assess the range of specialist opinion on flux adjustments we conducted a survey of climate modelers and other climate scientists. We posted a briefing note to climate scientists together with a covering letter outlining some of our interests in the issue, and requested responses to eight questions (see **Box 1**). The full list of respondents is shown in **Table 1**, from which it can be seen that responses were received from most of the major modeling Centers, and a number of the main scientific commentators on the models. The responses, comments and opinions of each respondent have, however, been kept strictly anonymous. (We have used quotes from our respondents in the text, and for consistency have encoded that of each respondent with a capital letter).

The evidence we obtained suggests that there are significant differences in opinion amongst modelers, indeed what could be termed different cultures of doing climate modeling, or "epistemic cultures."[9] The flux adjustment issue acted as a prism through which we could detect subtle, more

Box 1. Questions to Climate Modelers and Climate Scientists on Flux Adjustment (1995 Survey)

1. Our first question concerns the objectivity of the application of flux adjustment in control and perturbation runs. By "objectivity" we mean the extent to which the adjustments are the same in the control and perturbation runs or whether, some further *ad hoc* change is made to the flux adjustment values when applied to either or both of the control and perturbation runs.

2. Have you stated the theoretical rationale for flux adjustment in any publications or documentation? (If so, we would be grateful for references or copies.)

3. As far as you know, how does the application of flux adjustment differ between the key modeling centers?

4. Have you conducted any sensitivity-type tests on the effects of errors in the component models, and their compensation by flux adjustment, on the model's behavior (such as the stability of equilibria in the model, and the transient response)?

5. Have you conducted any tests of the robustness of the assumption of linearity implicit in using flux adjustment? If it is not currently possible to devise tests to explore the robustness, why is this so?

6. Is there any way of separating the sources of the errors which produce model drift? (*e.g.*, those which emerge randomly from the equations from those which arise because of errors in the numerical solution). If so, have you conducted any tests on the implications for flux adjustment of the sources of errors?

7. What, in your opinion, is the source of errors in your models, and/or other models, which leads to the need for flux adjustments?

8. It is sometimes claimed that increasing the resolution of the model will reduce the amount of flux adjustment presently applied. Do you share this opinion? What other approaches are there to reducing flux adjustment and how would you prioritize these?

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[9] There is a large literature on styles in science. The sources we have used most are: Rudwick, M.: 1982, "Cognitive Styles in Geology," in Douglas, M. (ed.), *Essays in the Sociology of Perception*, RKP, London.; Maienschein, J.: "Epistemic Styles in German and American Embryology," *Science in Context*, **4**(2), 407-427; Hacking, I.: 1992, "Style for Historians and Philosophers," *Stud. Hist. Phil. Sci.*, **23**(1), 1-20; Downey, G.L.: 1992, "Agency and Structure in Negotiating Knowledge," in Douglas, M. and Hull, D. (eds.)., *How Classification Works*, Edinburgh University Press, Edinburgh; and Knorr-Cetina, K.: 1991, "Epistemic Cultures: Forms of Reason in Science," *History Of Political Economy*, **23**, 105-122.

Table 1. Individuals Who Have Taken Part In, Or Contributed To, The Survey

Name	Institution	Country
George Boer	Canadian Centre for Climate Modeling and Analysis, U. of Victoria	Canada
Lawrence Gates	PCMDI, Lawrence Livermore National Laboratory	USA
Hal Gordon	CSIRO, Division of Atmospheric Research	Australia
Klaus Hasselmann	Max-Planck Institut fur Meteorologie, Hamburg	Germany
Martin Hoffert	Earth Systems Group, New York University	USA
Syukuro Manabe	Geophysical Fluid Dynamics Laboratory, Princeton University	USA
Bryant McAveney	Bureau of Meteorology Research Centre, Melbourne	Australia
Gerald Meehl	National Center for Atmospheric Research, Boulder	USA
John Mitchell	Hadley Centre, UK Meteorological Office	UK
Akira Noda	Climate Research Department, Meterologic Research Institute	Japan
Barrie Pittock	Climate Impact Group, CSIRO Division of Atmospheric Research	Australia
Stefan Rahmstorf	Institut fur Meereskunde, Kiel	Germany
Edwin Schneider	Center for Ocean-Land-Atmosphere Studies, Calverton	USA
Ronald Stouffer	Geophysical Fluid Dynamics Laboratory, Princeton University	USA
Hevre le Treut	Laboratoire Meteorologie Dynamique, Paris	France
Warren Washington	National Center for Atmospheric Research, Boulder	USA
Thomas Wigley	University Corporation of Atmospheric Research, Boulder	USA
John Woods	Graduate School of Environment, Imperial College, London	UK
Carl Wunsch	Massachusetts Institute of Technology, Cambridge	USA
Other Individuals who contributed specific comments		
Chris Gordon	Hadley Center, UK Meteorological Office	UK
William Ingram	Hadley Center, UK Meteorological Office	UK
Michael MacCracken	Office of U.S. Global Change Research Program	USA
Tim Palmer	European Centre for Medium-Range Weather Forecasting, Reading	UK
Roger Pielke	University of Colorado, Boulder	USA
David Roberts	Hadley Centre, UK Meteorological Office	UK
Anthony Slingo	Hadley Centre, UK Meteorological Office	UK

generic, differences in GCM modeling approaches. These different cultures result in different sets of standards by which climate change science is evaluated. What is a good piece of research according to those following one style, is not viewed so favourably by those working in a different style. The existence of different styles raises issues concerning the assessment of GCM modeling for policy purposes, a point we will return to at the end of the paper.

So far, this is familiar territory to those who have a professional or personal interest in understanding how science works. Something else took us by surprise, however, and this is the extent to which our enquiry provoked some strong reactions: while there was support for our project, the criticism was vociferous. What had we done to provoke such a reaction? The experiences at the IPCC, described above, provide some clues. Our intervention was, it appeared, being assessed by some respondents in a similar way to how they assessed the intervention of industry experts at the IPCC Plenary Sessions. That is, the potential policy ramifications of an enquiry such as ours, played an important role in shaping how some of our contacts responded. Somewhat unexpectedly, therefore, our research has shed light not only on a technical issue, but also on differences in how scientists react to an enquiry which is concerned with the interface of science and policy.

Before proceeding with our analysis of the modelers' responses, we provide a short overview of some technical dimensions of the flux adjustment issue.

2. USE OF FLUX ADJUSTMENTS IN CLIMATE MODELS

2.1 The Importance of Coupled Climate Models in the Climate Problem

Coupled A/O GCMs are the most sophisticated climate models, representing the state-of-the-art. These models provide potentially the best means for determining how rapidly the increased heating of ocean surface layers due to increases in greenhouse gases will be mixed into the deeper ocean layers. If it is mixed slowly, global surface warming will be fast, but if it is mixed rapidly, global surface warming will be slow. The mixing into the deeper layers is accomplished primarily by oceanic convection and the ocean's thermohaline circulation (THC), and these processes are best simulated by coupled A/O GCMs. To date, coupled A/O GCMs have not been able to produce a realistic equilibrium THC without using flux adjustments.[10] Indeed, when flux adjustments are removed in the GFDL GCM, the THC collapses. Thus the rapidity of global warming predicted by these models is ultimately influenced by their flux adjustments.[11] The same is true of simpler models, where the rapidity is controlled by an arbitrarily specified heat capacity and/or rate of mixing of heat into the deep oceans.

Coupled A/O GCMs *in principle* provide the best means for simulating climate change at regional scales. This is because they explicitly include latitude, longitude, and height dimensions (which are not all explicit in simpler models), and because they contain ocean dynamics (as well as atmospheric dynamics), which determine ocean circulations and influence regional climates. The surface fluxes of heat and moisture are an important component of the energy balance for a region. The veracity of regional simulations with coupled A/O GCMs is vitiated by the fact that the models require flux adjustments of heat and moisture which are often comparable to the actual fluxes produced by the model at many grid points.

2.2 Drift in Coupled Climate Models

When atmospheric GCMs and oceanic GCMs are coupled together, the climate of the coupled model tends to drift into a quite different climate state from the one that the models produce when spun up separately. The drift would be manifest for instance as a reasonably steady increase or decrease of sea surface temperature or salinity in the coupled system away from temperatures or salinities characteristic of the present climate. The drift is problematic because some of the feedbacks in the climate system depend on temperature or salinity, and it is important to represent the feedbacks correctly when simulating changes in the climate system. The size of the climate drift occurring in the models varies from model to model, but can be larger than the model's simulated transient greenhouse climate change response.[12]

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[10] IPCC, *Climate Change 1995: The Science of Climate Change*, Cambridge University Press, Cambridge, 1996.

[11] Even when flux corrections are used to provide a realistic THC, *i.e.*, to get a realistic mass circulation in the North Atlantic Ocean, as in the GFDL GCM, this does not guarantee an accurate mixing of heat. For example, in the same GFDL GCM, with a realistic mass circulation, the North Atlantic poleward heat transport is about 0.5 PW (Manabe and Stouffer, 1988: "Two stable equilibria of a coupled ocean-atmosphere model," *J. Climate*, **1**, 841-866), which is much weaker than observational estimates. Hall and Bryden (Hall, M. and Bryden, H.: 1982, "Direct Estimates and Mechanisms of Ocean Heat Transport," *Deep-Sea Research*, **29**(3A), 339-359) determine observational transport to be 1.2 ± 0.3 PW at 25^{o} N in the North Atlantic Ocean. There is as yet no data that can be used to estimate whether the THC's vertical heat transport is being simulated accurately.

[12] Schneider, E.: 1996, "Flux Correction and the Simulation of Changing Climate," *Annales Geophysicae*, **14**, 336-341.

2.3 Methods to Compensate for Climate Drift

The drift in coupled A/O GCMs is an indication that there are underlying errors in the models. Some modeling groups have attempted to compensate for the drift by adjusting the surface fluxes of heat and moisture (and sometimes also momentum) in the models, while other groups have chosen not to adjust the fluxes. Of the 17 coupled A/O GCMs listed in Meehl (1995) and IPCC (1996), seven employ flux adjustments.[13] Some of the models that do not use flux adjustments use other artificial constraining techniques to compensate for drift, such as truncating the northern boundary of the oceans, or restoring the deep midlatitude oceans' state to observations. Two recent A/O GCMs are sufficiently improved that their climate drifts, at least with respect to temperature, are greatly reduced. These are NCAR's Climate System Model (CSM) and the Hadley Centre's HadCM3 model.[14] Both of these models' surface temperatures are stable for multi-century runs, without the use of flux adjustments. (The CSM does however use a global correction to the freshwater budget to account for the absence of river runoff in the model.) Nevertheless both of these models do still have strong trends in ocean salinity.[14] The CSM simulation was stopped after 300 years because "sooner or later, the ocean circulation would have changed substantially away from the present climate."[14]

While flux adjustment has engendered much attention, it is important to remember that the real scientific problem is the model errors that underlie the drift, not whether a model group chooses to adjust the model fluxes to compensate for the drift or not. Flux adjustment is a palliative to address symptoms caused by model errors; it is neither the problem, nor the solution. For transient climate experiments with coupled models there is no clearly superior strategy at present, since there are scientific objections to using flux adjustments but also to not using it.

Model groups that do not adjust the fluxes in their models (or otherwise compensate for drift) carry out climate change experiments by subtracting the unperturbed climate produced by the coupled model (with its attendant drift) from the greenhouse perturbed transient climate produced by the coupled model (also with drift). Since the drift is present in both model runs, this procedure has the effect of canceling it out. The problem is that the feedbacks operating in the transient model experiment will be distorted by the presence of the drift as mentioned above. The hope is that the drift and climate change are small enough that the distortions will also be small. As noted by Sausen *et al*. (1988), the "validity of the method is limited to the finite time interval during which the model control climate has not drifted too far away from the initial (observed) climate state," but that this "normally excludes precisely those time scales that one would like to study with a coupled model."[15]

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[13] Meehl, G.: 1995, "Workshop on Global Coupled General Circulation Models," *Bulletin of the American Meteorological Society*; and Table 5.1., page 236, Gates, W., *et al*., "Climate Models: Evaluation," in IPCC (1996), *op. cit*. footnote 9.

[14] Boville, B., and Gent, P.: 1998, "The NCAR Climate System Model, Version One," *J. Climate*, **11**, 1115-1130; Gordon, C., C. Cooper, C.A. Senior, H. Banks, J.M. Greory, T.C. Johns, J.F.B. Mitchell, and R.A. Wood, 1999: "The simulation of SST, sea ice extents, and ocean heat transports in a version of the Hadley Centre coupled model without flux adjustments," *Climate Dynamics*, submitted.

[15] page 146, Sausen, R., Barthel, K. and Hasselmann, K.: 1988, "Coupled Ocean-Atmospheric Models of Flux Correction," *Climate Dynamics*, **2**, 145-163.

2.4 The Flux Adjustment Procedure

The actual method of applying flux adjustments to the models varies somewhat from group to group. Flux adjustment procedures attempt to reconcile the fluxes produced across the atmosphere/ ocean interface by the individual component models when forced by observed boundary conditions at the interface. If the component model fluxes are not the same when computed in this way, the coupled model will almost certainly exhibit a climate drift. This is due to the fact that each component model requires a different flux to achieve an equilibrium from the flux it actually receives in the coupled system. To provide a more concrete description of flux adjustment we summarize the procedure as it is implemented at GFDL.[16]

The atmospheric model is run alone to an equilibrium state using boundary conditions at the ocean-atmosphere interface supplied by observations (observed SST and sea ice in this case). The average seasonal and geographic distributions of the fluxes of heat and moisture are calculated over the last 10 annual cycles. The corresponding distribution of the surface momentum flux is also computed for use as a boundary condition for the ocean-only integration. In the ocean-only integration the ocean model is "spun up" towards equilibrium with the surface temperature and salinity relaxed toward seasonally and geographically varying observed values. The average seasonal and geographic distributions of the fluxes of heat and moisture needed to maintain the imposed distributions of SST, surface salinity, and sea ice are computed from the last 500 annual cycles. To reconcile the fact that the fluxes of heat and moisture computed in the atmosphere-only integration with realistic SST and sea ice differ from the fluxes of heat and moisture needed to maintain realistic surface conditions in the ocean-only integration, a flux adjustment is performed when the component models are coupled together. In this case, the fluxes of heat and moisture in the atmospheric component of the coupled model are modified by amounts equal to the difference between the two sets of fluxes derived in the single component integrations. The adjustment of the flux depends on season and geographical location, but does not change from year to year.

Regardless of how the flux adjustment is implemented, it is partly dependent on the boundary conditions used to spin up the ocean models. The common Haney (or Newtonian) method of restoring the ocean surface fluxes of heat (or moisture in equivalent formulations) to climatology *via* an equation of the form: $H(T_o) = k(T_{cl} - T_o)$ is problematic in that it cannot give both the right temperatures and the right flux. Consider the two extreme cases: When the model ocean surface temperature, T_o, matches climatology, T_{cl}, Haney restoration to climatology will give zero flux, which has to be wrong. Conversely, if the model has the right flux, it cannot have the right temperatures. The models tend to suffer from the first of these two afflictions, *i.e.*, right temperature and wrong flux. This occurs because the relaxation times used in the models are much shorter than the advection time scale in the real ocean, and so the models relax quickly to the climatological temperature fields.[17] In principle, the ocean models ought to use specified surface *fluxes* as boundary conditions, since the heat, moisture, and momentum capacities of the oceans

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[16] Manabe, S., Stouffer, R., Spelman, M. and Bryan, K.: 1991, "Transient Responses of a Coupled Ocean-Atmosphere Model to Gradual Changes of Atmospheric CO_2. Part I: Annual Mean Response," *J. Climate*, **4**, 785-818.

[17] Haney, R.: 1971, "Surface Thermal Boundary Condition for Ocean Circulation Models," *J. Phys. Oceanogr*., **1**, 241-248.

greatly exceeds those of the atmosphere. This has the difficulty that it produces an extremely unrealistic thermocline in practice, and that the observed fluxes are poorly known, though these can be partly constrained from the observed transports.[18]

2.5 Errors Introduced by Flux Adjustment

The flux adjustment procedure can introduce additional errors into the model simulation that would not be present in a non-flux-adjusted model.[19] The problem occurs because the equilibrium flux adjustment technique is designed to remove the model's drift, yet some of that drift should legitimately occur because the present climate is not in equilibrium with the post-industrial increase of greenhouse gases. The problem that the present climate is not in equilibrium with present greenhouse gas concentrations is known as the "cold start" problem in climate simulations, and requires that the model transient simulations start earlier in the century in order to simulate the correct warming rate at the present time.[20] If a climate model transient simulation starts from an equilibrium climate at the present time, then its initial trend must be zero. Since the real climate system is already responding to past increases in greenhouse gases with a non-zero present trend, a climate model run initiated in this way must therefore underestimate the initial trend and is said to suffer from a "cold start."

Schneider (1996) uses a simple model to show that while the error introduced into a linear system by the cold start initial condition error "decreases to zero with a time scale related to the relaxation time of the climate system, the error introduced by equilibrium flux correction to the current climate remains finite forever and is independent of the initial conditions."[21]. That is, the cold start initial condition error will eventually be forgotten as the climate simulation progresses and the response of the model approaches the response that would have occurred if the initial trend was not underestimated. The flux adjustment procedure introduces an error because the adjustments are calculated on the basis of a fictitious equilibrium climate. The fluxes are adjusted to ensure that the coupled model reproduces the present climate state, but do not take account of the fact that the present climate is not in equilibrium, but is undergoing forcing. The equilibrium flux adjustment is therefore guaranteed to be inappropriate. Furthermore, since the flux adjustment is maintained throughout the simulation, the model error introduced by flux adjustment is also maintained. This error will serve to distort some of the feedbacks in the model, and so it is not strictly true, as sometimes claimed (*e.g.*, Sausen *et al*., 1988), that the flux adjustment does not change the dynamical behaviour of the model.

2.6 Flux Adjustment and Tuning

Some of the general circulation modelers (GCMers) we surveyed view flux adjustment as just another way their model is "tuned" to the current climate. Viewed this way, flux adjustment is not unusual and is perhaps even inevitable, since all climate models must be tuned to some degree. In

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[18] Marotzke, J.: 1994, "Ocean Models in Climate Problems," in Malanotte-Rizzoli, P. and Robinson, A. (eds.), *Ocean Processes in Climate Dynamics: Global and Mediterranean Examples*, 79-109.

[19] Schneider, E.: 1996, *op. cit.* footnote 12.

[20] Hasselmann, K., Sausen, R., Maier-Reimer, E. and Voss, R.: 1993, "On the Cold Start Problem in Transient Simulations with Coupled Atmosphere-Ocean Models," *Climate Dynamics*, **9**, 53-61.

[21] Schneider, E.: 1996, *op. cit.* footnote 12.

the tuning process, model parameter values are adjusted over plausible ranges of their uncertainty (consistent with observations and/or theory). So long as parameter values remain inside plausible ranges, tuning does not violate known physical laws. However, flux adjustments as currently implemented in coupled climate models violate the conservation laws of heat, moisture, and momentum, and therefore have no physical basis. This would rule out flux adjustment as a defensible form of tuning.

2.7 Rationale for Flux Adjustment

Most modelers who have used flux adjustment have also provided a rationale to support its use (see column one, **Table 2**, which summarises the responses we received to the questions in Box 1). We have encountered at least two different rationales for using flux adjustments. The first rationale, as introduced by Sausen *et al*. (1988), is that the method is valid so long as the adjustment terms are small. In practice the adjustments are as large as the actual fluxes themselves,[22] which would seem to invalidate use of the method according to this criterion. The caveat that the flux errors need to be small has not received much attention in subsequent papers discussing use of the procedure. Indeed, none of the scientists in our survey indicated that they had made any tests of this assumption (see Table 2).

A second rationale for the validity of the method introduced by some of the modelers is that the flux adjustment is legitimate if the individual component models (atmosphere and ocean) are reasonably good at simulating the present climate. The very fact that the coupled models need flux adjustments of order 100% suggests *a priori* that there is a problem with the atmosphere and ocean model component simulations. Furthermore, the ocean component models are using boundary conditions to spin up the ocean models that cannot be right (as discussed in sections 2.4 and 2.5), and this will prejudice the performance of the coupled model system. In addition, testing the component models does not test the feedbacks and interactions that only come into play in the fully coupled system. According to studies with simple coupled models, these feedbacks do affect the system's climate sensitivity.[23]

It is also debatable whether the individual component models do a good job of simulating the present climate. Some of the modelers seem to accept at face value the simulation of the model means and seasonal cycle as fair tests of the models.[24] Yet, the atmospheric models are run with climatological sea surface temperatures, and are therefore forced to get about the right mean and seasonal cycle of temperature. A more realistic test of the atmospheric models is to check whether a model gets the temperatures right for the right reasons. That is, does the model simulate the different components of the heat balance correctly, such as the top of atmosphere (TOA) fluxes and

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[22] Gates, W.L., Cubasch, U., Meehl, G., Mitchell, J., and Stouffer, R.: 1993, "An Intercomparison of Selected Features of the Control Climates Simulated by Coupled Ocean-Atmosphere General Circulation Models," Geneva, World Meteorological Organization Publication WMO/TD-No. 574.

[23] Nakamura, M., Stone, P. and Marotzke, J.: 1994, "Destabilization of the Thermohaline Circulation by Atmospheric Eddy Transports," *J. Climate*, **7**(12), 1870-1882; Marotzke, J. and Stone, P.: 1995, "Atmospheric Transports, the Thermohaline Circulation, and Flux Adjustments in a Simple Coupled Model," *J. Phys. Ocean.*, **25**, 1350-1364.

[24] For example, in Chapter 5 of the IPCC 1995 report, Gates, *et al*., 1996, *op*. *cit*. footnote 10.

transports. Stone and Risbey[25] and Gleckler *et al*.[26] show that the atmospheric models have gross discrepancies in their TOA fluxes, atmospheric transports, and implied ocean transports. The ocean models are run alone using artificial techniques that restore their temperature and salinity fields towards observations as discussed above, which compromises the test of means and seasonal cycle for these models when run this way as well.

Table 2. Summary of Responses to the Questions Posed to the Sample

Modeler	Is the rationale for flux adjustment given? (Question 2)[a]	Have tests of the sensitivity to errors been conducted? (Question 4)	Have tests of the linearity assumption been made? (Question 5)	Is resolution considered to help flux adjustments? (Question 8)	What are the main sources of errors? (Question 7)	Purist or Pragmatist
I	N/A [b]	Yes	No	Yes + clouds	clouds	purist
K	Yes	(not possible)	(not possible)	Yes	clouds	pragmatist
H	Yes	No	No [c]	No	clouds	pragmatist
F	Yes	— [b]	—	—	—	pragmatist
B	Yes	Yes	No	Yes	boundary layer, clouds	pragmatist
L	N/A	Yes	—	—	—	purist (?)
N	Yes	—	—	—	clouds	purist (?)
R	Yes	No	No	Yes		pragmatist
S	No	No	No	Yes	clouds, subgrid transport in OCGM	N/A
T	No	No	No	Yes	Boundary layer, clouds	N/A
D	Yes	—	—	—	—	?
G	N/A	—	—	Yes	—	purist
Summary of Responses	*Most modelers who have used flux adjustments have also provided a rationale.*	*Mixed set of responses. Some have done preliminary tests, others have not; others say that such tests are impossible.*	*No modelers have attempted to test this assumption. There is disagreement about whether such a test is in fact possible.*	*Almost all believe that increasing resolution will reduce flux adjustment.*	*All respondents point to the central importance of the representation of clouds as a source of errors.*	

[a] The question numbers refer to the list of questions in **Box 1** (page 5).
Answers to Question 1 all confirmed the objective use of flux adjustments (as defined in the question).
Answers to Questions 3 and 6 were generally not provided. Not all respondents provided sufficient information to be included in the table.
[b] N/A: not applicable; — indicates that the respondent did not provide a clear answer.
[c] (would require much better paleoclimate data)

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[25] Stone, P. and Risbey, J.: 1990, "On the Limitations of General Circulation Climate Models," *Geophysical Research Letters*, **17**(12), 2173-2176.

[26] Gleckler, P., *et al.*: 1995, "Cloud-radiative Effects on Implied Oceanic Energy Transports as Simulated by Atmospheric General Circulation Models," *Geophysical Research Letters*, **22**, 791-794.

2.8 Flux Adjustment and Climate Change

Sausen *et al*. (1988) and other modelers claim that the flux adjustments "have no influence on the dynamics of the system in climate response or sensitivity experiments" because the "constant additive fluxes cancel when considering the deviations of the climate state relative to some reference state."[27] The extent to which this is true will depend on the magnitude of the errors introduced into the model simulation by use of the equilibrium flux adjustment procedure as discussed above. Even if it were the case that the flux adjustment did not introduce any additional distortions in the model feedbacks (as the non flux adjusted technique does), this misses the point that the feedbacks would still be distorted in the model by the underlying errors causing the model drift.

In order to obtain a correct simulation of climate change by using a model with flux adjustments, one must assume that the erroneous model processes causing the original coupled model to drift do not contribute to any of the feedback processes in the model. This assumption is *prima-facie* weak, and has been shown to be wrong for most kinds of model errors by Marotzke and Stone.[28] They use a simple coupled model to show that even though the correct mean state may have been obtained by flux adjustments at the sea surface, the transient behaviour of the model is erroneous. They also show that the correct stability behaviour of the model can be recaptured if the conventional additive scheme is replaced by appropriate alternative schemes. Since the conventional additive flux adjustment scheme leaves the feedbacks associated with fluxes that have erroneous representations uncorrected, it is not much of a saving grace to argue that the model has the same wrong feedbacks as in the unadjusted version of the model. As indicated in Table 2, whilst some modelers have conducted preliminary tests of the model's sensitivity to errors, others have not and others do not believe such tests are possible.

Some modelers have pointed to an apparent agreement between the results of flux adjusted and non flux adjusted models as indicating that the adjustment does not significantly affect the climate simulation.[29] This also misses the point that there are underlying errors in the models which are present whether the models use flux adjustments or not. In fact, warming trends predicted by coupled A/O GCMs differ substantially.[30] There are a number of reasons for this, including use of models with different sensitivities and different rates of mixing of heat into the deep ocean, and different trace gas increase scenarios. Until recently there were no "clean" comparisons that used the same coupled A/O GCM and same scenario, with and without flux adjustments. Analyses with simpler coupled A/O models certainly show a difference if the integrations are started from the

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[27] Sausen, R., Barthel, K. and Hasselmann, K.: 1988, "Coupled Ocean-Atmosphere Models With Flux Corrections," *Clim. Dyn*., **2**, 154-163. This belief persists, as indicated by one of this paper's reviewers, who informed us that flux adjustments have "absolutely nothing to do with the key question of the overall climate sensitivity to greenhouse gas increases."

[28] Marotzke and Stone: 1995, *op. cit.* footnote 23.

[29] This was stressed by a number of our respondents. It was also observed by the first author at the IPCC WGI Plenary in Madrid, November 1995.

[30] Murphy, J.M. and Mitchell, J.F.B.: 1995, "Transient Response of the Hadley Center Coupled Ocean-Atmosphere Model to Increasing Carbon Dioxide, Part II: Spatial and Temporal Structure of Response," *J. Climate*, **8**, 57-80. Sokolov, A., and P.H. Stone, 1998: "A flexible climate model for use in integrated assessments," *Climate Dynamics*, **14**, 291-303.

current climate.[31] However, more recently a clean comparison using an A/O GCM has been conducted. This study, using the Hadley Centre Model HADCM2, found that the:

> *... global-average temperature response of our model to CO_2 increasing at 1% per year is about 30% less without flux adjustment than with flux adjustment. The geographical patterns of the response are similar, indicating that flux adjustment is not causing any gross distortion. ... Although the response in both cases lies within the generally accepted range for the climate sensitivity, systematic uncertainties of this size are clearly undesirable, and the best strategy for future development is to improve the climate model in order to reduce the need for flux adjustment.* [32]

This study indicates that the earlier intuition that models with and without flux adjustment would agree may not be a robust one. Another recent study by Fanning and Weaver[31] using an idealized coupled model also finds that the global response in a model with and without flux adjustment differs. Unlike the HADCM2 study, however, Fanning and Weaver find that the regional response also differs. If this proves to be a more general finding it is important since a major rationale for using GCMs is that they—unlike simpler models—have the potential to provide detailed regional simulations. Where even the global response is concerned, however, it is unclear whether and how model errors and artificial adjustments on regional scales might influence the global response. One modeler, for example, stated: “if the atmosphere gets its interactions with the ocean wrong in one of the critical areas (like the places where the ocean convects) one can potentially change the gross, *i.e.* global behaviour” [G].

2.9 Progress Towards Solutions

The only real solution to model drift is to find the underlying causes for the errors and reduce them to the point where the drift and flux errors are small. Kerr cites one modeler to the effect that “large flux adjustments may soon be a thing of the past, thanks to increases in computer power.”[33] Many of our respondents were confident that increasing resolution would *inter alia* reduce flux adjustments (see Table 2). This optimism is sometimes based on plots of annual zonal mean flux adjustments for updated versions of the models, which do show large reductions. However, zonal averaging may result in substantial cancellation of errors around a latitude circle, and annual averaging may mask errors at seasonal time scales. For instance, in the case of the UKMO model (version UKTR) though higher resolution reduces flux errors in average representations, there are still many regions in the model where the flux adjustment is of order 100%.[34]

Furthermore, Gleckler and Taylor find that for the ECMWF model, convergence of ocean surface heat fluxes is not achieved by T106 resolution, which is substantially greater resolution than is routinely used by present climate models.[35] Thus, there are reasons to be cautious about the

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[31] Hasselmann, *et al*.: l993; Schneider: 1996; *op. cit*. footnotes 20 and 12. Fanning, A.F. and Weaver, A.J.: 1997, “On the Role of Flux Adjustments in an Idealized Coupled Climate Model,” *Climate Dynamics*, **13**, 691-701.

[32] page 1943, Gregory, J.M. and Mitchell, J.F.B: 1997, “The Climate Response to CO_2 of the Hadley Centre Coupled AOGCM With and Without Flux Adjustment,” *Geophysical Research Letters*, **24**(15), 1943-1946.

[33] page 1528, Kerr, R.: 1994, *op. cit*. footnote 4.

[34] Presentation of James Murphy at Experts Workshop on Critical Issues in the Science of Global Climate Change, IPIECA, Woods Hole, 3-5 October 1994.

[35] Gleckler, P. and Taylor, K.: 1992, “The Effect of Horizontal Resolution on Ocean Surface Heat Fluxes in the ECMWF Model,” *PCMDI Report No. 3*, Lawrence Livermore National Laboratory, Livermore, California, 28 p.

rate at which model drift will likely be reduced by increasing the model resolution. All our respondents pointed to the representation of clouds as presenting the greatest source of errors, though parameterising the boundary layer and sub-grid transport in OGCMs, were also mentioned (see Table 2). This list is consistent with the factors identified in the IPCC's Second Assessment Report 1995.

3. WHY IS FLUX ADJUSTMENT USED OR NOT?

The above section suggests that there is no scientifically "correct" answer to the flux adjustment issue. The scientific arguments for and against its use are clearly articulated and the dilemma that there is no simple correct answer to the question of whether flux adjustment should be used in future projections of climate change is widely acknowledged. Based on our survey, moreover, it appears (somewhat surprisingly we think) that climate modelers tend to agree on the reasons *why* flux adjustment is used, as well as accepting *most* of the arguments for and against its use. The question therefore emerges, why do some modelers use adjustments, while others do not?

3.1 Different Interpretations of the Same Model Errors

Disagreement among GCMers seems to arise from different interpretations of the *implications* of flux adjustment for the reliability of climate models. The advocates of using flux adjustment argue along the lines of respondent F:

> *… if modelers have good reason to believe that the individual AGCMs or OGCMs are reasonably correct physically, they are justified in coupling the two together to look at the response of the coupled system to external forcing, even though the coupled system is not able to reproduce the mean climate system correctly. The response characteristics are governed by the (tested) response characteristics of each individual system and are independent of the problem that the overall system is only weakly stabilized and is therefore difficult to tune to reproduce the observed climate.* [F]

Yet as noted in section 2 one of the prime areas of disagreement between scientists is precisely whether the simulation of mean climate by current AGCMs and OGCMs *is* indeed adequate. As one scientist critical of the use of flux adjustments argued:

> *… the oceanographic models that are coupled to the atmospheric ones are so primitive that I have no confidence in any integration carried out for longer than a year or two.* [G]

It appears that we can identify two groups of scientists who use different standards which we dub "the pragmatists" and "the purists." We introduce these categories to describe the situation in the debate over flux adjustment, but we also argue that with some minor modification they do have wider applicability in describing different approaches to climate modeling. The pragmatists are those modelers who use flux adjustments in coupled A/O GCMs in climate change work. They tend to accept that the current AGCMs and OGCMs produce sufficiently good simulations to permit meaningful coupling to take place. This decision depends in part on what criteria are used to assess the adequacy of the simulation, but such criteria are rarely fully explicated.

By contrast the purists, who tend to avoid and criticise the use of flux adjustments, do not fall into one tidy group. Some seem primarily interested in how well the model simulates the dynamical features of the circulation and apply seemingly more rigorous, yet still private and informal, standards of model adequacy. Other purists are more interested in the model's simulation of energy fluxes, *i.e.* its thermodynamics. While the pragmatists tend to be reasonably satisfied with the AGCM's largely successful simulation of surface temperatures, the purists note that the pragmatists do not typically pay attention to transport statistics that show errors as large as 50% in meridional heat transport, which would imply corresponding errors in the surface heat fluxes.[25,26]

It would be quite misleading, however, to suggest that the pragmatists are unaware of the problems with their models. It is rather that they do not consider the problems to be so significant as to invalidate the use of GCMs for climate change experiments.[36] The lack of agreed-upon standards provides some flexibility in the assessment of a model's control simulation, a flexibility which can justify the use of models (as producing a sufficiently good control simulation) in climate change experiments. Alternatively it can be used to justify the need for further model developments and improvements prior to more applied work.

It is not possible for us to state precisely what these different criteria and standards are because they are part of the informal tacit knowledge of particular scientific *cultures*. These enculturated rules and standards determine what work is considered worthwhile, and what is considered to be a successful piece of work. They are determined in part by scientific practices and arguments which are *routinised* within institutions; they come to be taken for granted, and are used without the explicit critical scrutiny which is the hallmark of science. Such routinised knowledge and practice are, however, necessary to *any* scientific practice since too much questioning and skepticism could paralyze practice and innovation.

A good example of routinised practice is the use of boundary conditions to force A and O GCMs; the resulting simulations follow, to some extent, from the specification of the boundary conditions. Modelers are at one level aware of this limit to the independent evaluation of GCMs; yet at another, more practically relevant level, they do not allow such questions to change their everyday work—the simulations they plan, how they interpret them, and so on. A second example is the assumption that the model's response to the doubling of CO_2 will occur in approximately the same linear regime as the current climate state. Not only is this assumption implicit when flux adjustments are used, but it has also been a routine assumption in climate change experiments using AGCMs (coupled to more or less simple ocean models) for many years. As respondent F put it:

> *This has been standard procedure in AGCM modeling for many years, prior to the introduction of flux correction in coupled models. For example, if one uses an AGCM alone, even with prescribed SST, the atmospheric temperatures can show errors of a few degrees in various parts of the globe. It is none the less permissible to use the atmospheric model to determine the change in the atmospheric circulation induced, for example, by a change of the SST distribution—even when the change is smaller than the error of the model—provided the model is in the correct linear regime.* [F]

While these may be valid assumptions, there is little formal justification to be found in the published literature, though we know of two supporting studies.[37] It appears to be a pragmatically

[36] There will always be an open-ended character to the model validation process. Knorr-Cetina, K.: 1991, "Epistemic Cultures: Forms of Reason in Science," *History of Political Economy*, **23**(1), 105-122; Oreskes, N., Shrader-Frechette, K. and Belitz, K.: 1994, "Verification, Validation and Confirmation of numerical models in the Earth Sciences," *Science*, **263**, 641-646.

[37] Hansen, J., Lacis, A., Rind, D., Russell, G., Stone, P., Fung, I., Ruedy, M. and Lerner, J.: 1984, "Climate Sensitivity: Analysis of feedback mechanisms," (*Climate Processes and Climate Sensitivity*, Hansen, J. and

necessary article of faith and not obviously denied by experience. As one, purist climate scientist put it:

> *The people doing the CO_2 doubling and related studies like to claim ... that even though the mean state is artificially maintained, that the perturbations will still be realistic. I know of no actual demonstration that this is true. It is easy to construct counter examples, at least as thought experiments.* [G]

Indeed, none of the climate modeling Centers which responded to our survey have attempted to *test* the key assumption of linearity which is a necessary condition for flux adjustments to be legitimate (see Table 2). In fact there was skepticism over whether any such tests are currently available. Some purist modelers worry that the linear assumption is valid, however, only if the drift in the model is slow and continuous. In some coupled runs without flux adjustments, the drift changes rapidly for no known reason; *e.g.*, there have been periods of sudden warming in one coupled control run [D, 15 April 1994]. These purist modelers also ask whether the climate sensitivity of the model changes if you apply a small perturbation rather than a large one (*ibid*.).

Certain tacit assumptions and practices have become routine in GCM studies, and these have been extended to coupled models without any serious questioning of their validity in this new context. Such exploratory and often ambitious extension of concepts and practices to new contexts is typical and probably necessary in most scientific practice.[38] At the same time, however, past case studies have shown how the policy and political significance of extension increases when public policy commitments could be based on the resulting knowledge.[39]

We are not suggesting here that pragmatic modelers are simply "duped" into using flux adjustment through adoption of existing criteria and standards. They, and the purists, are also informed by their own ideas and experience of how models can best be evaluated or tested and improved (though these perceptions may also be shaped by routinised expectations). The pragmatists tend to argue that despite the errors the best way to improve knowledge is to go ahead and couple models together, using flux adjustments to get a realistic response. This is for them more desirable than waiting until models and observations have improved sufficiently that flux adjustments are not needed, since that:

> *… implies that we cannot learn anything without our models "being perfect." This is clearly not the case and stultifies the search for new understanding of the climate system while leaving estimates of potential climate change to less sophisticated approaches.* [M]

It is even argued by some modelers that it is precisely in the coupling of models together that their individual errors become most apparent, and thus capable of being addressed in further

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Takahashi, T. (eds.), Geophysical Monograph 29, AGU, Washington, D.C., pp. 130-163) shows that the response of global mean surface temperature in an AGCM to a doubling of CO_2 is linear. North, G. Yip, K.J., Leung L.-Y., and Chervin, R.: 1992 "Forced and Free Variations of the Surface Temperature Field in a GCM," (*J. Climate*, **5**, 227-239) shows that the regional response of the surface temperature to various idealized forcings in an idealized AGCM are linear to a good approximation.

[38] Schon, D.: 1963, *Invention and the Evolution of Ideas*, London: Tavistock.

[39] Cf. Wynne, B.: 1991, "After Chernobyl: Science Made too Simple," *New Scientist*, 26th January 1991; Wynne, B.: 1996, "May the Sheep Safely Graze? A Reflexive View of the Expert-lay Knowledge Divide," in Lash, S., Szerszynski, B. and Wynne, B. (eds.), *Risk, Environment and Modernity: Towards a New Ecology*, Sage Publications, London, pp. 44-84; Krohn, W. and Weyer, J.: 1994, "Society as a Laboratory: The Social Risks of Experimental Research," *Science and Public Policy*, **21**(3), 173-183.

research [Q]. Yet, there is a serious dilemma here since (as noted in Section 2) the pragmatists also state, at least sometimes, that flux adjustment is only legitimate if the simulation by the component models is sufficiently accurate. Hence, the argument about coupling as a way of learning more about component model errors (and therefore improving them) potentially threatens the legitimacy of using flux adjustments for generating projections of climate change (if it implies that the uncoupled models are substantially inaccurate).

Another modeler also used a comparative perspective in assessing A/O GCMs with flux adjustment:

> *I do think that the models using flux corrections have some skill. They clearly represent an improvement to the slab ocean models used before. The worst effect of using flux correction is to give the feeling that the models are realistic—and to delay research concerning a number of physical features. Let us consider clouds. You can correct easily the absence of low cloudiness through flux correction. In your experiments you then implicitly assume that low clouds are unchanged. This is certainly wrong. At the same time, as the parameterisation of low clouds appears difficult, using flux correction is a good way to delay the moment we will have to face this issue.* [N]

The two modelers quoted above (M and N) both accept that using flux adjustments has some pragmatic validity, but appear to disagree on the extent to which it should continue to be used in the future. The argument against using it is that further understanding will not emerge until the models are improved such that they do not require flux adjustments. Use of flux adjustment, according to N, may act to conceal the model's errors and to lull the modeler into a false belief that the model's performance is better than it really is. Note, however, that this is a *social* judgment about how scientists interpret models, and about how they act on that interpretation. It suggests that scientists' use of flux adjustment may well (for whatever reason) all too easily cover-up model errors, thus obstructing scientific learning.

We also want to draw attention to the significance in modeling and model evaluation of *expectations* about how the science will unfold, and about how performance of supporting technologies (*e.g.,* computer power) will develop. There was an informal belief at a number of modeling Centers in the early- to mid-1990s that increased model resolution would reduce the need for flux adjustments (also see the responses to our survey, Table 2).[40] Even when individual experiments had shown this not to be the case, the belief that increased resolution would be a major means by which flux adjustment would be reduced was still stated to us.[41] This has to be seen in

[40] An internal report at one Center noted the scale of flux adjustment in an early 1990s coupled model run and commented that it was: "not surprising, since the coarse resolution of the ocean model forces the use of an undesirably large horizontal viscosity in order to achieve computational stability" (page 1, Murphy, J.: 1991, "Transient response of a coupled ocean/atmosphere model to a gradual increase in CO_2," Hadley Centre, Bracknell, UK, June).

[41] There are, of course, different ways of interpreting this. It could be that our informants simplified their accounts in the belief that the first author, as a non-modeler, would not understand the more complex account. It has to be noted that the same scientists have now represented their understanding at that time differently—indicating that they always had seen the issue of flux adjustment, and its solution, as being much more complicated than just increasing model resolution. However, an alternative interpretation would be that the first author uncovered an uneven distribution of knowledge and understanding—*i.e.* some of our respondents did believe the simpler version at the time, largely because they were not directly involved in coupling atmosphere and ocean GCMs, and had received their understanding second-hand from those colleagues who were more involved. The role of "institutional forgetting" may be important here.

the context of the routinised faith in the last 20 or 30 years that increased computer power would be available in the immediate future, often at exponential rates of increase, and that this would in turn lead to better GCMs, especially for Numerical Weather Prediction (NWP), but also for climate change. Faith in the development of computer power is largely justified by experience, but this does not mean that flux adjustments will be reduced by increased resolution.[42] Other sociological studies of modeling have illustrated that model improvements in the here-and-now, and hopes of future improvements, are frequently confused.[43] Thus the expectation that higher resolution would lead to less need for flux adjustment was arguably important in legitimatizing the present use and policy authority of *existing* models.

But what explains why some climate modelers adopt a purist as opposed to a pragmatist stance? Below we identify four factors which we believe help account for the difference. The first is institutional mission and funding. The second, which is closely connected with the first, is the relationship of the modeling to policy making processes. The third is the relationship between the modeling and how the model output is used. The fourth is the "style" of climate modeling, which relates to different disciplinary, institutional and personal career trajectory backgrounds.

3.2 Institutional Mission and Funding

Identifying the intended *purpose* of the climate research goes some way towards clarifying the different perspectives of the purists and pragmatists. The purists are principally interested in analysing, developing and improving state-of-the-art climate models for the purpose of developing a better representation of the key processes involved in climate and climate change. They are less interested in climate change projections for policy, which they regard as requiring improved models, and they do not restrict the potential application of their models to studies of anthropogenic climate change. Many are concerned with academic questions concerning physical processes in the atmosphere and ocean, which they use models to study as and when appropriate. Flux adjustment appears, to them, as an unscientific fix, because it actually obscures the errors which can (if clearly defined) give clues to the interesting and productive research questions. Hence, to them, "covering up" those errors denies one of the key means by which research advances.

On the other hand, the pragmatists are much more involved in using and applying models for various purposes usually concerned with the study of past and/or future climate change, and especially in assessments of anthropogenically-induced climate change. The pragmatists' mission is tightly linked to current policy, political and public concerns surrounding the enhanced greenhouse effect (not necessarily directly but mediated by funding agencies and government departments, as will be discussed). They use models which are as near the state-of-the-art as possible, given the need for a model which can provide answers about anthropogenic climate change.

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[42] On the more general issue of whether increased resolution is a better approach to model development and use, than say model process or feedback development or use of ensemble runs, debates have continued over many years, both in the NWP and climate modeling communities, *e.g.*, Toth, Z. and Kalnay, E.: 1993, "Ensemble Forecasting at NMC: The Generation of Perturbations," *Bulletin of the American Meteorological Society*, **74**(12), 2317-2330; Brooks, H. and Doswell, C. III: 1993, "New Technology and Numerical Weather Prediction—A Wasted Opportunity?" *Weather*, **48**(6).

[43] Keepin, B. and Wynne, B.: 1984, "Technical Analysis of IIASA Energy Scenarios," *Nature*, **312**, 691-695.

It might seem obvious that the best climate models should be used for policy-relevant climate change experiments, meaning the most complex and highest-resolution models. However, the computer time required to run a GCM increases steeply with added complexity. The computer requirements of coupled A/O GCMs for a climate change simulation of say 100 years are already massive, taking several months on large supercomputers. The pragmatist modeler has to ask what is being gained through including such complexity and higher resolution, and this is frequently a difficult question to answer in a straightforward way. What is clear to pragmatists, however, is that empirical or theoretical validation of the more complex parameterisations is rarely available. While many feel intuitively that as high as possible a resolution is required, straightforward evidence that increasing the resolution improves the ability of the model to simulate processes most important to climate change is not readily available.[44]

A further complication for pragmatists wishing to use more complex models is that GCMs tend to come as a package in which the parameterisations, resolution, input variables and tuning are co-generated and interconnected through a process of internal mutual adjustment. Changing just one of them is unlikely to improve the model, in fact it often makes the model worse, because it puts the system components "out of kilter" with one another. But changing each part of the model is a large task, and one that requires a dedicated model development effort, of the sort that some of the purists are engaged in. Thus engaging in model development and testing militates against actually doing climate change simulations since the available resources are generally not capable of supporting fully both kinds of efforts. Also, improving the A/O GCMs used in transient climate change runs is not an incremental process. Rather, improvement requires a considerable leap in complexity, and such advances do not occur in a predictable fashion. The pragmatists recognise most of the limitations of their current models, but there may be little alternative available to them if they intend to conduct assessments of anthropogenic climate change over the next 50–100 years.

Use of flux adjustment appears to reduce the need for computer resources and this may be an important, though not usually acknowledged, reason why flux adjustment proved popular with the major climate change modeling Centers in the late 1980s and early 1990s. For example, at Center 3, where flux adjustment was not used, the modelers found that they had to perform many short runs with their coupled model in order to test out different tunings, as described in an interview:

> *It* [the coupled A/O GCM] *took over a year of tuning. Each time you change something you have to run the model for at least 5 years … And you say that isn't quite the right thing to do, and you have to go back and change something else and then you say perhaps it wasn't quite long enough and you run it for 10 years. … So the tuning exercise can be very long and painful and takes up a lot of resources and it's unclear at lots of times what you have to do.* [D, 15 April 1994]

This level of tuning was not necessary with the flux adjusted models, because flux adjustment removes much of the drift created in model coupling. Hence, by using flux adjustment the pragmatists could proceed more rapidly with the long transient climate change simulations.

[44] The only factor which clearly improves at higher resolution is the simulation of regional precipitation, but other factors such as the simulation of diabatic heating and storm tracks do not. Risbey, J. and Stone, P.: 1996, "A Case Study of the Adequacy of GCM Simulations For Assessing Regional Climate Changes," *J. Climate*, **9**, 1441-1467.

Furthermore, some pragmatist modelers have stated that a major rationale for using flux adjustment has been to bring the atmosphere and ocean models into equilibrium more rapidly. As one put it during an interview:

> *… If you use these flux correction techniques you can then get the ocean into equilibrium much quicker than without them. And that's probably a main motivation [for initially using corrections]. You have a very long thermal inertia in the ocean. Now there are ways of speeding the deep ocean up. Strictly speaking they're only valid if you don't have a seasonal cycle. … Getting the deep ocean into equilibrium is an issue which is not totally divorced from flux correction because by specifying the ocean … constraining the surface temperatures and salinities when you run the model does tend to bring the model into equilibrium much quicker.* [B, 11 March 1993]

To observe the relevance of this simple distinction of key objectives and mission, consider the comments of the following pragmatist modeler concerning the consequences of climate drift in coupled models:

> *We do know that substantial distortion is likely to result if the adverse effects of climate drift are not ameliorated. Can we trust numerical experiments which do not use flux adjustments or its equivalent?* [K]

The critical question here, though, is "trust" for what purpose? While K is referring to assessments of the anthropogenic greenhouse effect, a purist might want to question whether we can put all that much trust in the future projections of any numerical experiments with current models, irrespective of whether flux adjustment is used or not (which is not to say that he or she denies the importance of the anthropogenic greenhouse effect).

In effect, there are two somewhat independent stages of evaluation involved here. There is the first-stage evaluation of the component A and OGCMs with respect to their adequacy for climate change simulations. Purists generally would find the models inadequate, and pragmatists would find them adequate. However, once an A/O GCM is actually in use for climate change simulations it has effectively been decided that the component models *were* adequate. Hence the second-stage evaluation concerns whether flux adjustments should be used or not, once closure has been reached on the question of the adequacy of A/O GCMs. At this second-stage some purists might accept the need for flux adjustments given the prior decision (which they would not accept) of component A and OGCM adequacy. Those purists who do actually couple together A and OGCMs appear to be less willing than most pragmatists to close-down the first-stage evaluation of component models and the perceived extent of the errors makes them reluctant to use flux adjustment since this simply obscures these errors rather than helping eradicate them.

Many climate change modeling centers do not have the luxury of devoting all or most of their time to model development and production of control runs to the standard they might like. They are compelled for reasons of funding obligations, pressure from funding agencies and government departments, and/or to some extent compel themselves for reasons of desired high public status and public relevance, or more mundanely, in order to pursue a particular trajectory in climate research upon which they have embarked (and have invested resources in), to use coupled A/O GCMs in climate change prediction experiments.[45] As respondent O put it:

> *In my group we provide climate change scenarios for decision makers now, so we cannot afford to say to governments: "We cannot give you advice from GCMs because they have a flux correction, and that is possibly hiding errors in the GCMs." Rather, we have to take the results from flux-corrected and non-flux-corrected GCMs and make our own judgments as to which is better for the purpose of giving advice now. If a flux-correction enables us to get a reasonable answer, caveats and all, then we use it, but state the caveats. If and where we know the flux corrections give seriously misleading answers, then we do not advocate using them.* [O]

Such different institutional *raisons d'être* affect the criteria for evaluating models. Modeler O made a useful distinction between Type I and Type II Error Science. In O's own words:

> *… please distinguish between doing ideal "good science" which has no time constraints and aims at the 95% or 99% confidence level (Type I Error science) and practically-oriented pro-tem ["for the moment"] science aimed at providing timely advice in areas of social responsibility where decision-making is needed despite uncertainty. This requires consideration of Type II errors also,* i.e., *what is the chance of rejecting a correct theory because it cannot be established at the 95% or 99% probability level? …* [Such science] *must focus on the best interim advice, uncertainties, caveats, and all. The longer-term development of "pure" science requires more stringent standards, but may be less useful for urgent decision-making*. [O]

Thus the policy-defined rationale and expected use of the model shapes the criteria used to assess scientific practices and knowledge-claims.

3.3 The Role of Policy Processes

Without flux adjustment the credibility for policy makers of the output from running coupled A/O GCMs might be seriously threatened because of the drift (and this may limit the period of time during which it is sensible to run the model). How plausible is it, though, to suggest that the concerns of policy makers for long-term scenarios may be one factor which has swayed modelers in deciding to use flux adjustment? We do not have any direct empirical evidence to support such a claim, and in any case we feel that if it occurs, it does so through indirect influences more than *via* explicit policy-led demand. Such indirect avenues could operate *via* research funding agencies—especially when such agencies are also government departments—and through indirect pressure from scientists' participation in policy-orientated scientific assessment organizations, especially the IPCC. To the extent that institutions like the IPCC and its deliberations constitute an important arena for negotiation of status, credibility, and influence, perceptions of policy needs are built seamlessly into scientific interactions. Internal and external audiences for scientific research become blurred.

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[45] Relevant here is the changing funding context for research. In many countries, policy-usefulness of knowledge has become more emphasized in the past decade or so. Elzinga, A. and Jamieson, A.: 1995, "Changing Policy Agendas in Science and Technology," in Jasanoff, S., Markle, G., Petersen, J. and Pinch, T. (eds.), *Handbook of Science and Technology Studies*, Sage, London; Gibbons, M., Limoges, C., Nowotny, H., Schwartzman, S., Scott, P. and Trow, M. (eds.): 1994, *The New Production of Knowledge,* Sage, London.

GCMers may, in short, come to feel that realistic scenarios of future climate change are a necessary research output, to feed into the IPCC and similar scientific assessment processes. The advisory scientists are influenced by their understandings of what the policy world and research agencies desire from them, and by the competitive dynamic instituted by the IPCC, in which the policy-oriented modeling groups aim to be the first (or at least close runners-up) to present some new modeling work which is seen to have policy significance.[46] In this context, the ability of flux adjustments to reduce the computer resources needed to get the coupled model into equilibrium was an important advantage in the early- to mid-1990s, when some national funding agencies were clamouring for state-of-the-art model runs to feed into their national negotiating positions and into the IPCC's scientific assessment (specifically the supplementary report of 1992 and the second assessment review of 1995). Hence, scientific managers and advisory scientists, especially those involved in the IPCC, have an important role in deciding for modelers what counts as policy significant as well as what needs to be known urgently.[47]

Some modelers seem to recognise the benefits of flux adjustment to outside audiences. As one more purist modeler put it:

> *It's more politically pleasing to show flux corrected results because in the control run the model looks like the observations.* [D, 15 April 1994]

We speculate that long-term projections are seen by (some) modelers to be needed by the policy community, even urgently as modeler [O] expressed it above. Policy responses to climate change, even in the short-term, depend for their justification upon medium- to long-term projections. So, for instance, the proposal of the European Union at the December 1997 Climate Convention in Kyoto to reduce emissions of greenhouse gases by 15% by the year 2010 would not be credible without the existence of longer-term projections. This is because it is only in the medium- to long-term (say 30–100 years plus) that global climate change comes to look obviously significant to nonspecialists, *i.e.*, the anthropogenic signal has by this stage clearly emerged from the noise of natural variability and the impacts loom larger. Long-term projections are also required to assess the mitigative effects of proposed short-term emission reduction policies. In the early- to mid-1990s, long-term projections of GCMs hardly seemed serious in policy terms unless flux adjustment was employed. Such projections in turn support policy approaches which assume that reasonably certain knowledge of the medium- to long-term future is available for planning purposes.

Hence, the scientifically do-able problem and the needs of policy may have been constructed together, and the validation of flux adjustments as a technique cannot, we suggest, be divorced entirely from policy requirements. Use of flux adjustments therefore may imply an implicit model

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[46] A case in point is the competition between the two major European climate modeling Centers to provide new findings of policy significance and use prior to the April 1995 First Meeting of the Parties to the Framework Convention on Climate Change in Berlin (in that instance relating to the role of sulphate aerosols).

[47] A thought experiment may be useful to illustrate the role of policy pressures. Imagine the reaction to non-flux adjusted climate change model runs from senior research managers, environmental policy makers and international negotiators in government. The simulation of current climate in the control would look patently wrong to the policy maker. This might well reduce the latter's confidence in the simulations of future climate change using the same model, as well as possibly in the perceived trustworthiness of climate modelers. Flux adjustments obscure the control's errors so making it possible to present results which are credible, visually compelling to the policy maker and extend to the time of CO_2 doubling.

of policy, and of its knowledge-needs; likewise, policy may contain an implicit version of what is credible and good science in the climate change modeling domain (see **Figure 1**).

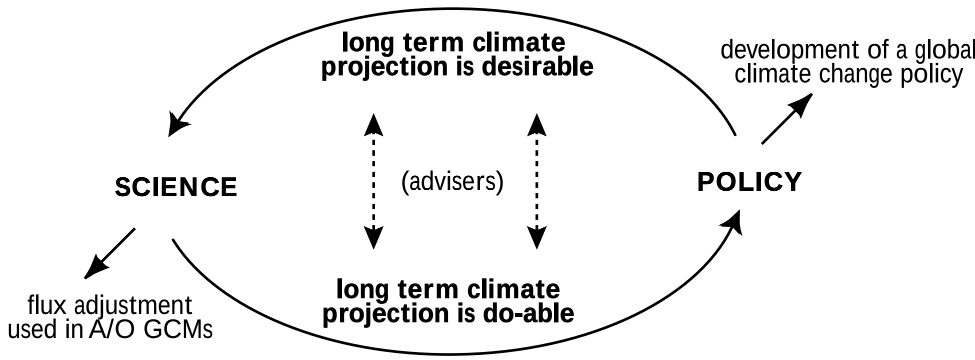

Figure 1. The mutual influence of science and policy in the field of climate change modeling. Direction of arrow indicated the key message received by one group from another: *e.g.*, modelers receive message from policy (*via* advisers) that long term climate projection is a desirable policy-deliverable.

3.4 The Role of Requests from Scientific Users of GCM Output

According to several of our respondents, climate impacts scientists have exerted a significant pressure on GCMers to provide scenarios of future climate change. This may have influenced their decision to use flux adjustment in two ways. Firstly, time-dependent scenarios require the use of coupled models; and flux adjustment has been seen as necessary to make such runs appear realistic. Secondly, according to some GCMers, the impacts community has also demanded a high quality control simulation. As one put it:

> *Many of these forces [to use flux correction] come from the impacts community who tend to hold to the view that a good quality control simulation is a necessary, but not sufficient, condition for obtaining a good climate change response. We feel that while it is very satisfying to have a good control climate it is not necessary (but is certainly desirable). … We are well aware of the requirement that one's model must remain visible through use of modeling output by outside users; that these outside users often place a higher value on the quality of the control climate in small regions means that often a model may be rejected from further consideration using criteria quite different from that of the original modeler.* [I]

This was confirmed by yet another modeler, who noted during an interview that:

> *… climate impacts people are much more interested in the flux corrected models because the observed state looks very close to the model control—for all the wrong reasons but it looks more pleasing.* [D, 15 April 1994]

These sorts of pressures upon modelers could be exerted indirectly, especially through funding agencies. Yet, while the impacts community may be a significant influence in some national contexts, in other countries the GCMers are relatively immune from the requests of the impacts community.[48] To illustrate the divergence in opinion among modelers towards the role of model users, it is worth mentioning that one respondent thought that model users, through their demands, could actually help create the circumstances in which model development and improvement would occur.

3.5 Different Epistemic Styles

Our final explanatory variable refers to differing epistemological approaches to climate modeling, though both use GCMs. The distinction is between those climate modelers who have always emphasised the thermodynamical features of the climate system, who tend to be the pragmatists, and those who emphasize a more dynamical approach to understanding climate change, who tend to be the purists. All climate specialists are cognisant of the fact that climate is a product of the interaction of thermodynamics and dynamics, so we are not suggesting that any modelers propose either dynamics or thermodynamics as the appropriate lens to view the models. Our point is rather the importance of the relative emphasis on the contribution of each, especially when modelers are assessing the characteristics and response of the global system as *a first approximation*.[49]

For the dynamicist, the need for flux adjustment is a very telling indication of the limitations of the GCMs which have been used by thermodynamicists to study the transient climate change response. The underlying causes of the need for flux adjustment are the large model errors which arise, and which also result in the control simulations of the separate A and O GCMs being poor (in the purists' view). The dynamicists look to the low resolution and the relatively simple physical parameterisations in the A/O GCMs, as the likely explanation for the errors and hence the need for flux adjustment. For example, oceanographers and purist ocean modelers have tended to look askance at the low resolution of OGCMs used in anthropogenic climate change experiments. How can such models produce good simulations, they ask, when their resolution is much larger than the scale of ocean eddies (important to the transport of energy though just how important is contested) or of key topographic features of the ocean, which play an important role in ocean circulation patterns?

Thermodynamicists agree that the need for flux adjustment arises because of model-errors, but they do not have so much confidence that the root problem is dynamical in character, hence do not automatically believe that increasing resolution or the complexity of parameterisations will improve things. As they see it, it is largely inevitable that the ocean and atmosphere models do not produce consistent fluxes. This is partly because the A and OGCMs have been developed independently, with their separate interrelated tuning, parameterisations, resolution and so on. The flux into the ocean from the atmosphere or vice versa has not been one of the key constraints guiding the development (tuning, *etc.*) of the models. Thus errors are almost inevitable. Also, the thermodynamicists see the two systems as thermodynamically independent (to a first approximation). If the two systems are "out of synch" then it is a relatively simple matter of adjusting the "knob" which represents the flux

[48] If anything, in many countries it is the GCMers who have an indirect effect upon the work of the climate impacts community, not the other way around. For example, some modelers have looked rather disparagingly at impacts work which uses equilibrium, rather than transient, GCM results—they have seen it as rather out of date. Such perceptions appear to filter through to, and influence, the impacts community, and to those funding the impacts work.

[49] See Shackley, S., "Epistemic Lifestyles in Climate Change Modeling," in Edwards, P. and Miller, C., *Changing the Atmosphere* (forthcoming).

between the ocean and atmosphere in order to bring them back into synch. One modeler explicitly used the comparison of two electrical circuits to explain this:

> *By using a constant flux one is simply shifting the "working point" of the system. It is like applying a constant off-set to the mean voltage which drives an electronic amplifier which is used only to amplify time-variable signals.* [F]

This distinction between thermodynamicist and dynamicist approaches is closely related to work-experience backgrounds, and hence to institutional mission. This, in fact, makes it difficult to clearly distinguish epistemic styles as an entirely independent variable. The thermodynamicists are often those who have pioneered the use of GCMs in climate change modeling. The dynamicists come more from weather forecasting and NWP. They have not populated anthropogenic climate change studies so much to date because their approach required tools and computer resources which are only now becoming widely available. At an earlier point, when only low-resolution coupling of (what dynamicists saw as) poor models with simple physics was possible, the dynamicists would have ruled out doing anthropogenic climate change experiments as not worth while. The thermodynamic-pragmatic modelers had, additionally, already occupied the territory of anthropogenic climate change, but the growing interest in the subject in the late 1980s and early 1990s, and the large influx of resources into the field, made the area increasingly attractive to dynamicists. Larger computer resources, better observational data-sets and more physically-based parameterisations were available, allowing more complex, high resolution models to be developed and run.[50]

3.6 Summary and Outline Sketches of Selected Modeling Centers

In reality, the above-mentioned factors are interconnected and involved—to a greater or lesser extent—in each case where decisions to use flux adjustment or not have been made. Scientists have only so much room for manoeuvre when developing research programs given their skills, know-how, resources, opportunities for funding and ability to justify the research in terms of wider societal goals; all these elements must be coordinated and some supporting overlap convincingly argued for.[51]

Table 3 attempts to summarize the particular combination of factors operating at a number of key climate modeling Centers in the early- to mid-1990s. The different organizational cultures of the four modeling Centers are important though of course not comprehensive factors in understanding how and why flux adjustment is accepted, or not accepted. There was considerable discussion among researchers at Center 1 concerning whether to use flux adjustment, and what its possible effects are, reflecting a strong contingent of dynamically-oriented scientists and a close relationship to NWP research. However, the collective and hierarchical organization of Center 1 ensured that the decision of the senior staff to use flux adjustment was acceded to. The decision was heavily influenced by the very close relationship to policy makers in the government agency

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[50] An example is the parameterisation of ocean eddies by Gent, P., Willebrand, J., McDougall, T., and McWilliams, J.: 1995, "Parameterizing Eddy-Induced Tracer Transports in Ocean Circulation Models," *J. Phys. Oceanogr*., **25**, 463-474.

[51] Fujimura, J.H.: 1987, "Constructing 'do-able' Problems in Cancer Research: Articulating Alignment," *Social Studies of Science*, **17**, 257-293.

Table 3. Outline Sketches of the Organizational Cultures of Four Climate Modeling Centers†

	Center 1	Center 2	Center 3	Center 4
1. Scientific styles	Mixture of purist and pragmatist, but hierarchical organization and competition with other centers favors pragmatism.	Mixture of purist and pragmatist. Pragmatism favored by charisma of leader and competition.	More strongly purist. Pragmatism occurs but not dominant; use of flux adjustments is more openly contested internally.	Mixture of purist and pragmatists. Charisma of leader and competition with others tends to favor pragmatism.
2. Institutional Mission and Funding	Applied mission to provide projections to policy makers, *i.e.* to generate "policy useful" knowledge of climate change.	Self-imposed mission to produce state-of-art climate projections for policy makers	Aims to support the wider academic community in the field of climate change.	To produce state-of- art climate projections which raise conceptual issues, in addition to being of policy relevance.
3. Policy Roles / Relations	75% funded by government. Much cooperative involvement in research management from the main funder. High visibility in government policy and media circles. Close links to IPCC WGI.	Close links to IPCC WGI. High visibility in government policy and media circles.	Controlled by funders and universities. The clientele is purist-oriented and this feeds back onto research conducted. Pragmatism less acceptable especially given relative lack of direct links to policy; where they occur they are indirect and multiple.	Funded by government agency, and high profile in government, policy and media circles.
4. Relations to impacts community	Close relations. Equilibrium and now transient runs from A/O GCMs provided.	Close relations. Equilibrium and now transient runs from A/O GCMs provided.	Links with other climate modelers as important as those with impacts community. Hence has more knowledgeable and critical "customers" of model simulations than those of Centers 1, 2, and 4.	Not especially close links to impacts community at present.
5. Use Flux Adjustment?	Yes	Yes	No	Yes

† circa the early- to mid-1990s

which funded the Center, and for which senior scientists played an important advisory role, and by the Center's commitment to providing policy-useful knowledge to the IPCC WGI. Policy makers wanted a research product that was effective in policy and political terms, meaning a prediction.

At Center 3 decision-making was relatively more distributed. There separate fiefdoms largely pursue their own agendas, though the predominantly dynamical-purist modeling culture (which was reflected in, and reinforced by, the character of the academic climate modeling research community which was the main client and funder of the Center) was probably the driving influence behind the decision not to use flux adjustment. In Centers 2 and 4, the dominant role of a single, charismatic group leader—through which much funding and credibility had been secured historically—was important; the modelers are likely to have acceded to the preference of this individual concerning whether or not to use flux adjustment. In both Centers 2 and 4, this individual was a well-established and highly-regarded thermodynamically-oriented pragmatic

GCMer and these Centers are also more closely related to policy than Center 3. A further factor in the case of all Centers was the competition to provide the wider world, and especially the IPCC WGI, with the latest state-of-the-art model runs, this acting as an indicator of policy usefulness.[52]

In terms of organizational cultures, the hierarchical and cohesive character of Center 1, which reflects a long historical legacy, facilitates top-down decision making by senior scientists and research funders. The fluid boundary between the Center and NWP work permits more purist / dynamicist scientists to develop alternative connections and networks contributing to the task of climate change prediction (though still as part and parcel of the same hierarchical culture). Center 3, by contrast, has much less clearly defined external boundaries, and the active involvement of the outside academic research community reflects the permeability of the boundary and has a significant influence upon the internal adoption of a more purist style. Within Center 3, however, the boundaries are very well defined and reinforced by different sets of outside relationships and the existence of distinctive modeling styles. Relatively pragmatist and purist approaches become competitors at Center 3, but the criticism of pragmatism in an organization where power is relatively distributed appears to influence the modelers working in that style, such that they are less pragmatist than modelers in Centers 1, 2 and 4. These features help to account for the survival of a relatively purist-oriented style of modeling in a modeling team which also provides projections to the IPCC.

The manner in which flux adjustment has been handled internally in modeling Centers is also revealing. For instance, representation of flux adjustments to outsiders by one modeling Center over a period of a year and a half tended to down-play its significance and implications. Documents intended for outside consumption, and contract reports to funding agencies, tended to present flux adjustments as a minor technical issue, the size and non-physical basis of which was not made clear. For the purposes of internal documentation, however, the more problematic features of flux adjustment, and the assumptions behind its use, was much more openly acknowledged and discussed. Even more openness about the indeterminacies occurred during the course of verbal discussions which we had with modelers. It then became apparent that there was in fact disagreement within the modeling Center as to the desirability, possible effects and causes of the need for flux adjustment, as well as how best to attempt to reduce it. The opening up of the intellectual issue and debate corresponded to journeying from outside the center into its innermost offices and settings such as the informal morning coffee-break. The more open account of flux adjustment internally was not considered by the key scientific advisors at the apex of the Center to be pertinent to the external scientific debate or policy use of that science.

One sees similar internal openness and debate coupled with external communications that tend to minimize debate in the treatment of flux adjustments in the IPCC process—for example comparing draft reports with published chapters and policymakers' summaries.[53] There is here not

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[52] We feel that we could have extended this analysis to other modeling Centers. For example, the Center in Australia that uses flux adjustment (CSIRO) has a more policy-oriented mission, and adopts a more pragmatic approach to its research than the BMRC, which does not use flux adjustments. In France, both climate GCM centers do not use flux adjustments. This is consistent with the tradition of theoretically-oriented mathematical research in France and the relative autonomy of the scientific community *vis-à-vis* policy makers. It may in addition reflect the relative lack of policy engagement by the French administration with the climate change issue.

[53] A discussion took place over an early draft of the Executive Summary of Section B of the IPCC 1992 report which contained two possible versions. Both versions explicitly stated that the models: "continue to display the

only a simplification, but a translation of flux adjustment into a routine technical problem which more research within the same trajectory will diminish. We speculate that many advisory scientists' judgements were influenced both by science (representing the state-of-the-art and frequently tacit knowledge and understanding within a particular, circumscribed community) and a view of policy (the need for "closure," consistency, certainty, rejection of indeterminacy as somehow irresponsible, a united scientific front, *etc.*). The definition of do-able problems has both scientific and policy dimensions to it. Flux adjustment as a scientific issue which is rather indeterminate and premised on *a priori* assumptions, may have been regarded as an issue which could all too easily "blow up" through being misunderstood and not put into the proper context, or through criticisms from within the wider peer community itself. Meanwhile, in order to maintain legitimacy among scientific colleagues, a richer and more multidimensional account of the issue is provided in other places (actually in the same document in the case of IPCC reports).

4. THE POLITICAL CONTEXT OF FLUX ADJUSTMENT

Nearly all our respondent climate modelers stated that flux adjustment is a necessary technique they would prefer not to have to use. It is no secret within the community that it is an arbitrary and nonphysical adjustment, which goes against the spirit of physically based modeling. But how significant is flux adjustment in the wider scheme of things? Some of our respondents regarded flux adjustment as a relatively minor issue and expected that it would disappear as a problem in the near future:

> *As a scientific issue, I think flux correction will wither. I am much more concerned about the problems of representing clouds, and I suspect that the inaccuracies due to using flux correction are minor compared to the uncertainties due to cloud.* [B]

> *This [flux adjustment] is just one of many prominent scientific concerns.* [C]

Clearly, such respondents were not anxious for flux adjustment to assume importance in communications with outside audiences. More generally, there appeared to be some anxiety over how to discuss flux adjustment. Should modelers discuss it in detail and describe it as "arbitrary" or a "fudge factor," in which case they risked losing credibility for GCMs as "fudged" or "fixed" to produce a "correct" answer? In the contentious atmosphere of climate politics, with wealthy opponents out to deny the IPPC's claims, this could be risky. Is it not likely to engender

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same overall strengths and weaknesses identified in the first [1990] assessment" and that "continuing validation tests indicate a slow but steady upward trend in the confidence which we can attach to their results." Inserted between these two statements in version 2, however, was the following addition: "among the major weaknesses is the need for substantial corrections to the air-sea fluxes in order to reproduce the present climate. *The impacts of these corrections on the ability to model GHG-induced climate change cannot be assessed* a priori" (stress added). Although neither version was eventually used intact, the caveats of version 2 were not included in the 1992 report. Revealingly, a reviewer commented about version 2 that it was "too defensive—not much [has] changed in validation [since IPCC 1990]." The advisory scientists were perhaps thinking strategically about the significance of representing quite legitimate scientific reservations about flux adjustment upon the credibility to a range of political, policy and media audiences of the IPCC reports (and especially the Executive Summary) and process (*e.g.*, a consistent relation with the IPCC 1990 report was preferred). They may also have been thinking about how the scientific peer community itself would respond to such flagging of flux adjustments given that little new validation work had been conducted since the earlier report.

skepticism in a public not familiar with the full scientific context of climate modeling? As one scientist put it:

> *Flux adjustment is only one of the particular features that the climate modeler will assess in this overall critical process. If a non-modeler is to sensibly take part in the assessment he needs to understand the whole model and the whole critical process. It is relatively easy (and not helpful) for non modelers who wish to rubbish the process to pick on different aspects of model formulation in isolation.* [A]

A high level of knowledge and understanding of A/O GCMs is not likely to be imparted in a short communication. Even purist and dynamicist modelers seem reluctant to present flux adjustment in overly-critical tones in public, but rather reserve criticism for internal dealings within their own peer community. Hence, one purist-oriented researcher responded as follows to our enquiry by questioning our implicit assumption that flux adjustment was of interest to non-GCMers:

> *I think a wider public is only interested in knowing how reliable the model scenarios are, which the IPCC has tried to communicate (and which is quite hard to judge), but they would hardly want to debate the nitty gritty of running models, like convection parameterisations, flux corrections, resolution problems,* etc. *You'd have to know a lot about models to take part in a discussion of how serious flux corrections are, and I think the public will probably have to wait until the modelers have discussed this amongst themselves and have come to some sort of conclusion on this matter.* [J]

Such scientists are effectively drawing a strong boundary between science and policy, with flux adjustment and its implications falling squarely within science. Some scientists appear to view outside scrutiny of our sort as an implicit threat to proper scientific assessment, and in particular as an attempt to discredit the A/O GCMs and climate modeling more generally. One of our respondents took this further by questioning our assumption that there was any public policy issue involved in the debate over the use of flux adjustment:

> *… coupled A/O GCMs per se, regardless of whether they are flux corrected or not, have a minuscule role in the policy debate.*[54] *Their role in the science that feeds into the policy arena is, likewise, small. The credibility of the science does not depend on these models. … It [flux adjustment] is only a policy concern in that it may influence opinions of a minority who feel (or search for evidence that) scientists are somehow downplaying or hiding model deficiencies. In this sense, the history of how this issue has been presented is interesting. However, the problem is far more general and widespread than just flux correction.* [C]

The danger of inadvertently assisting the cause of the greenhouse contrarians is clearly expressed in the above. Since, according to this view, GCMs are relatively uncoupled from the provision of scientific knowledge for policy, and since flux adjustment is but one of many scientific concerns surrounding GCMs, then the conclusion is clear:

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[54] Respondent C is surely incorrect to claim that A/O GCMs have little role in the policy debate. A/O GCMs have been used to calibrate the upwelling-diffusion/energy balance model used to make projections in the Second Assessment Report of the IPCC. GCMs also have a wider "symbolic" authority. (Shackley, S., Young, P., Parkinson, S. and Wynne, B.: 1998, "Uncertainty, Complexity and Concepts of Good Science in Climate Change Modeling: Are GCMs the Best Tools?" *Climatic Change*, **38**, 155-201.)

> *By concentrating on such a minor issue you are in danger of, not only not seeing the wood for the trees, but conveying that blindness to others.* [C]

Respondent C is here drawing on a sense of the policy and political implications of a study of flux adjustments. Though C clearly believes that flux adjustments are a nonissue, and therefore not relevant to public debate, we encountered other respondents who seemed to go even further in implying that even if flux adjustments were an issue, the outside world should not be privy to this information. To many outside the climate field, that scientists have such a highly developed sense of the policy and political implications arising from scientific issues may seem quite remarkable. It should come as little surprise, however, given the intense politicization of climate science, particularly in the USA, but also in the IPCC WGI, where coalitions of contrarian scientists, and representatives of industry and the oil states, have tried to cast doubt on the scientific case for climate change by emphasizing imperfections such as flux adjustment. Respondents like C are drawing upon their experience of working as advisory scientists, especially at the IPCC. Unlike A and J they perceive the science-policy boundary to be fuzzy in practice if not by preference. Some respondents implicitly supported the view that advisory scientists should attempt to control the science-policy interface to limit opportunities for political manipulation while preserving the authority of science. The dilemma that such policing could also undermine science's public credibility through being seen as arbitrary and authoritarian was not acknowledged.

Not all of the responses to our survey were negative in tone, however. Some modelers welcomed discussing flux adjustment and this seemed related in some cases to personal contacts and the building-up of trust between ourselves and the respondents. Several respondents thought that an enquiry such as ours would assist in reducing misunderstanding of flux adjustment among commentators, policy makers, and model users. One purist-oriented respondent thought that an enquiry would make the scientific community itself more open to questioning accepted wisdom and practice and to promoting the removal of model errors through extensive model development. This respondent explicitly regarded better models as improving science's contribution to policy. He regarded the science-policy interface as being robust to exposition of uncertainties in the science base. Such respondents place more trust in the ability of the political and policy process, in its interactions with scientific assessment, to deal intelligently with uncertainties, and accept the necessary role of scientific judgement.

Further research would be required to understand fully the basis for these different responses to our survey. However, the institutional affiliation of the respondent seems to us to matter. Generally, where the respondent is part of a large climate modeling effort in a confident, well-resourced, and respected organization, our enquiry was seen more positively or at least not as a threat. Also dynamicist-oriented and purist modelers seemed to us to be more favorable to a broader debate on flux adjustment for obvious reasons. One such modeler was, however, critical of us and we suggest that this scientist—mistakenly—read into our survey a critique of GCMs, and of the climate GCM community, which lent itself to the contrarian science cause. Finally, those most critical of our study tended to be involved in formal scientific assessment, especially the IPCC. It seemed like "second nature" for them to think about the political implications of our enquiry. However, there were a few exceptions to this from advisory scientists who seemed to be less concerned to control the flow of information between science and policy, though we are not in a position to fully understand why there are such differences.

5. CONCLUSIONS

One consequence of the highly politicised conflicts between contrarian scientists and the mainstream as represented by the IPCC has been the relative neglect of controversies and differences *within* the mainstream. Indeed, the presence of the contrarians, ready to make the most of uncertainty and disagreement, may have inhibited the expression of such intra-peer community differences. This has perhaps been exacerbated by the widespread belief among mainstream climate scientists that many contrarians are motivated primarily by their political interests and beliefs rather than by wishing to contribute to a peer community debate. We interpret the negative and defensive responses we received to our enquiry from some respondents as reflecting (*inter alia*) distrust of interest in model limitations from outside the peer community.

Our study indicates, however, that the debate over flux adjustment is more than a case of the politically-motivated mis-representation of science. We have argued that there are different "cultures" of GCM modeling with differences in opinion concerning flux adjustments. To identify these cultures required detailed empirical exploration of GCM modeling Centers and interviews and correspondence with researchers. We identified a "pragmatist" approach, in which GCMs were being used for climate change simulations, usually for policy-oriented scientific assessments of the anthropogenic greenhouse effect. Flux adjustments (and other short-cuts such as the spin-up of ocean models with artificial boundary conditions) are part and parcel of this approach. By contrast, "purists" regard the GCMs used by the pragmatists as inadequate, riddled with model errors, requiring a substantive model development program. Those with the purist approach were suspicious of flux adjustments as potentially covering-up model errors, influencing the model's variability, and leading to complacency in model improvements, and they entertained concerns about the linearity assumption behind the use of flux adjustments (which indicates more fundamental, if suspended, theoretical divergences).

These pragmatist and purist cultures emerge from the interplay of a heterogeneous range of factors including: organizational mission, individual and collective research trajectories (including past work experience and identification of future priorities and ambitions), funding patterns, involvement in providing climate-impacts scientists with scenarios, the role of hierarchical management and /or charisma of leaders of research groups, and different epistemic styles (those who regard anthropogenic climate change as more of a thermodynamic problem than a dynamic problem at the first approximation, and vice versa). The modelers' judgement about using flux adjustments, and how to represent its use to non-modelers, emerges in an institutional-political context which stresses the importance of presenting a clear storyline about the likelihood and broad-brush pattern of future climate change to the outside world.

While in some centers purist and pragmatist approaches were in conflict with one another (*e.g.,* Center 3) in others the purist and pragmatist identities seemed to co-exist quite happily and, arguably, the organization as a whole benefitted from their co-habitation (*e.g.,* Centers 1 and 2). In these latter cases, the potential conflict between the two approaches was mediated and minimised by one or more of the following:

- priority being given by an authoritative and charismatic individual leader, or by a hierarchical decision-making structure, to what one scientist termed "predictive understanding" (*i.e.,* simulations which generated both policy-relevant predictions and research-relevant insights);

- the acceptance of a large influence from the research funder requesting that state-of-the-art climate predictions be conducted; and,
- alternative networks of collaboration and research evaluation available for the purist modelers in Centers 1 and 2, *e.g.*, in model development work for numerical weather prediction and, ultimately, for climate change predictions.

The purist-dynamicists' arguments at Center 3 (reinforced by a collaborating academic audience) may have dissuaded the pragmatist modelers from adopting the more overtly pragmatist measures, such as flux adjustments. As one of the more pragmatically-oriented modelers at Center 3 put it: "I guess we've been one of the conservative groups" [respondent E, interview 14 April 1994]. We should emphasise that identifying differences in scientific styles of this sort does not imply that rational exchanges between the two styles cannot occur, since many common commitments still exist which bridge these differences.

In the last section we also explored the reactions to our study from the scientists we contacted. Unexpectedly, the responses we received revealed much of interest concerning how different scientists interpreted our interest in flux adjustments. Some wished to draw a strong boundary between science and policy, and to limit flux adjustment as an issue to science, best dealt with by the peer community alone. Others appeared more willing to bend the boundary so that it included commentators who could (probably) be trusted. Yet others operated with a fuzzier boundary between science and policy, usually obtained from direct experience working in scientific assessments. Some in this last group wanted scientists to keep tighter control on what information and perspectives passed through that fuzzy zone, while others seemed more relaxed about commentaries such as ours. A more definitive identification and characterisation of these forms of "boundary-work"[55] and an explanation of their differences, would require further research. We note, however, that our findings correspond with other sociological studies of science. These studies have shown the negotiated character of accepted boundaries between science and policy, the ultimately contingent and contextual character of such settlements, and the necessity of stabilising such boundaries and definitions in order for science to play its necessary role in public policy (*ibid.*).

We have argued that debates over flux adjustment are a prism through which to explore the social, policy and scientific assumptions and commitments of climate modelers. But what are the possible uses of such insights? Charles Darwin once commented that: "How odd it is that anyone should not see that all observation must be for or against some view if it is to be of any service."[56] In the spirit of that remark, we use our observations to advance a view of the role of science in climate change policy which has been developed in the last decade or more, a view which contrasts with the traditional idea of science as a pure realm which independently informs policy.

- We suggest that our analysis of the internal debate on flux adjustment helps climate scientists, research funders and managers, climate model users (including policy makers)

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[55] See Jasanoff, *op. cit.* footnote 1; Gieryn, T.: 1995, "Boundaries of Science," in Jasanoff, S., Markle, G., Petersen, J., and Pinch, T. (eds.), *Handbook of Science and Technology Studies*, Sage, London, 393-443.

[56] Quoted in Ruddock, R.: 1981, *Ideologies*, Manchester Monographs 15, Dept. of Adult and Higher Education, Manchester University.

and journalists to better understand the social, policy and institutional dimensions to controversies and differences of perspective within climate science. This may permit a more constructive and discriminating dialogue to take place between different modeling perspectives, as well as raising, and we hope clarifying, the role of policy considerations in scientific decision-making.

- In more social science theoretical terms, we believe that our study provides further evidence for a "mutual construction" of climate science and policy.[57] That is, the modelers seem to have an implicit view of the policy process which informs not only the way they present their work, but what it is they actually do, their practices, and standards of evidence. Hence the world outside of the direct peer community is incorporated into negotiations over what is known, knowable, and worth doing within science. More speculatively, these scientific practices, and the storylines they produce, may be incorporated into what is known, knowable and considered worth doing within policy, such that the commitments at this deeper, often tacit, level within science and policy come to reinforce one another, without being deliberated on explicitly (as summarised in Figure 1).

- Though there is diversity in the modelers' views of the needs of the policy process the dominant strain is perhaps one which sees a need to carefully manage scientific uncertainty lest policy development be hindered. This view of the vulnerabilities of the policy process to scientific uncertainty is also shared by the climate change contrarians, who operate to uncover and exploit uncertainties. Such a perspective certainly reflects a good dose of *realpolitik* and experience of environmental policy making in industrialised countries. It is especially pertinent in the USA, where research on risk assessment and regulation, and decision-making on environmental controversies, reveals the strong pressures to reveal uncertainty and use it to undermine a position against the interests of some policy actor. In that sense, we might say that the advisory scientists who are wary of opening-up scientific debate to a wider audience on an issue such as flux adjustment are being sensible politicians.

- Yet there is another way to view this approach to uncertainty, that it is an ultimately brittle view of the science-policy process (at least in more fragmented and antagonistic political cultures such as the USA) because it requires forms of policy and science that seldom withstand strong critical scrutiny, especially as the political stakes rise.[58] The robustness of "science for policy" knowledge depends greatly on the kind of policy culture it may imply or require, and on whether this kind is robust. An alternate view to that of *realpolitik* sees politicization of climate science as more or less inevitable. Scientists may as well accept that

[57] Shackley, S. and Wynne, B.: 1995, "Global Climate Change: The Mutual Construction of an Emergent Science-Policy Domain," *Science and Public Policy*, **22**(4), 2128-230.

[58] Wynne, B.: 1996, "SSK's Identity Parade: Signing Up, Off-and-On," *Social Studies of Science*, **26**(2), 357-391. We are interested in going beyond the binary assumption that one party is right or wrong, or that someone somewhere can be correct and policy can flow unproblematically from such simple truth (as expressed, for example, in Wildavsky, A.: 1995, *But Is It True?*, Harvard, Mass., one chapter of which concerned the debate over global warming). The National Research Council report, *Understanding Risk*, (*op. cit.* footnote 1) provides a useful account of elements of what such an alternative might entail.

as a given and find ways to cope constructively with such a political reality. Additionally, climate change science is not like conventional basic research, where greater precision and probabilistic assessments of uncertainty are (to some extent) feasible. The indeterminacy of such "post-normal science"[59] is more akin to the "regulatory science"[60] which has striven to provide assessments of the safety to humans of drugs, food additives, chemicals and the like, with many problems emerging in the capacity of that science to identify and provide probabilistic assessments of potential risks. All this suggests that climate change policy makers cannot expect a quantified level of certainty, at least not with respect to all of the issues which are likely to be critical for policy. Are advisory scientists who "manage" the climate science-policy interface in danger of suggesting too much confidence in the ability of science to produce certainty? Post-normal science also requires that we need to find ways to communicate informed agreement and disagreement, and informed judgements concerning levels of confidence in knowledge-claims,[61] as well as divulging the processes by which assumptions are formed and disagreements resolved. A wider appreciation of the indeterminate character of climate change science among policy actors and the wider public would possibly reduce some of the political effectiveness of the contrarians, because uncertainty and limitations in climate knowledge would cease to be a prime cause for disbelief or policy inaction.

- If openness, acknowledgments of compromise, and expression of uncertainty were the initiative of mainstream scientists, rather than of hostile contrarians, then legitimate dissent might have its place and the extra-scientific community would not have reasons to expect the presence of "dirty secrets." This might contribute to rebuilding relationships of trust (where that is needed) between scientists, politicians, policy makers, and the public. In turn, this might allow areas of consensus to emerge on beliefs about climate change, in contrast to the present situation which has led to endless debate and paralysis

Finally, and more speculatively, we suggest that there are correspondences between the scientific styles we have identified and scientists' implicit ideas of policy. The purist tends to suggest that policy can only be rational and legitimate if its founding knowledge is highly elaborate and close to the best physical understanding. The pragmatist view on the other hand accepts that adequate and defensible policy can be made on the basis of approximate knowledge embodying epistemological norms. This latter view of policy could be seen as partly symbolic and implying what sociologists have suggested, namely that "realist" languages serve the tacit roles of binding together and supplying a discursive arena for an epistemic community which shares an interrelated set of beliefs about nature and, to some extent, human responsibility and policy prescription.

It is not necessarily paradoxical that some purists maintain that public policy commitments can be achieved even with knowledge which is explicitly provisional and uncertain and openly debated. Rather this position may imply a belief in levels of public maturity in the face of uncertainty which

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[59] Funtowicz, S. and Ravetz, J.: 1993, "Science for the Post-Normal Age," *Futures*, **25**(7), 739-755.
[60] See references *op. cit*. footnote 1; Salter, L.: 1988, *Mandated Science*, Kluwer, Dordrecht.
[61] Hassol and Katzenberger, *op. cit*. footnote 3.

the pragmatist frequently denies as possible or feasible. The pragmatist stance operates in scientific assessments for policy by permitting openness, approximation and contingency *within* a circumscribed community of scientists and policy makers. But at the same time it holds to a less negotiable view of climate science as certain expert knowledge which is transferred in a linear fashion to policy making. Purists seem less likely to adopt such a dualistic approach to the evaluation of knowledge within and outside the extended peer community.

Thus the debate over flux adjustments involves different normative ideas: of the proper boundary between science and policy; the proper background or foreground of scientific assumptions and disagreements which cannot be resolved by straightforward appeals to nature alone; and the proper distribution of responsibility for dealing with contingency between the domain of science and the domain of public policy. It is perhaps in this area of where responsibility should lie that our findings most impinge on debates about the role and definition of precaution in the face of uncertainties about climate change.

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