CLIMATE MODELS AND THE VALIDATION AND PRESENTATION OF GREENHOUSE CHANGE THEORY

by

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ABSTRACT

We identified problems in validating greenhouse change theory in a science-policy context. We focused on GCMs, noting the heavy reliance upon 3-D GCMs for developing greenhouse change scenarios. As a case study GCM validation, we analysed simulation of energy transports in the three major GCMs used in greenhouse climate studies of NCAR, GFDL, and GISS.

Comparison of energy transports in the GISS GCM calculated directly from model motions and indirectly from the net flux of radiation at the top of the atmosphere indicated that the indirect technique is satisfactory. The atmospheric energy transports calculated using the indirect technique were found to be similar in all GCMs, but too efficient, being much larger than observations. For the total energy transports significant differences were obtained between the three GCMs in the NH, with NCAR and GFDL less than observations and GISS greater than observations. We used a 1-D EBM to demonstrate that the climate is potentially quite sensitive to the energy transport. To test implications of the differences in energy transport between GCMs, we parameterized the EBM with output from the GCMs and observational data, separately in each case. Using an increase of solar constant by 2% as a proxy for CO_2 doubling in the EBMs, similar sensitivities were obtained between equivalent EBM representations for each GCM and with the GCM doubled CO_2 sensitivities. The differences in energy transport between GCMs were apparently compensated for in the equivalent EBM representation, though the sources of compensation are not yet clear.

We described a framework for validating GCMs and greenhouse change, comparing and contrasting the approach and requirements of science and policy communities. We noted potential mismatches in operation and requirements between the communities, and the current inability to incorporate uncertainties in critical evaluation of greenhouse change. We considered communication of validation information from science to policy by analysing science journal literature and its media interpretation. Instances of misinterpretation in the media were identified and attributed partly to a failure to provide context appropriate to a broader audience in the journal literature. We argued that the addition of context is required when science journal literature has clear policy implications and is undergoing scrutiny by the media.

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GLOSSARY OF PRINCIPAL SYMBOLS

The principal symbols of physical and mathematical quantities used in this report are listed below. Some symbols formed from principal symbols by adding subscripts, and others that are used in only one place are not listed. Some quantities are denoted by two different symbols for clarity of notation in different sections. Usage should be unambiguous in any given section.

Symbol	Name or Definition
a	Radius of the earth
A	Longwave - temperature parameter
B	Longwave - temperature parameter
D	Dynamical transport efficiency
F	Meridional energy transport flux
F_{TA}	Net downward flux of radiation at the top of the atmosphere
H_n	Insolation component
I	Outgoing longwave radiation at the top of the atmosphere
P_n	Legendre polynomial
q	Solar constant
R	Radius of the earth
S	Meridional distribution of solar radiation
T	Sea level temperature
T_T	Total meridional energy transport
x	Sine of latitude
z	Geopotential height
α	Albedo
β	Climate sensitivity
γ	Lapse rate
δ	Albedo parameter
θ	Latitude
σ	Stefan - Boltzmann constant
φ	Longitude

1 INTRODUCTION

1.1 Intent, background and thesis definition

The subject material for this thesis is greenhouse[1] theory, its validation in climate models, and subsequent presentation. Greenhouse theory is used here to refer to the body of knowledge associated with changes in the atmospheric greenhouse effect. We consider the problems associated with the question of whether the information obtained from greenhouse science and climate models is credible enough to support policy decisions. Long the domain of the scientist, greenhouse theory is now also part of the purview of the policy analyst, the politician, the property owner, and the proletarian. The thesis examines climate models used in developing greenhouse change scenarios, and explores the growing links between greenhouse science and greenhouse policy. These links will be highlighted through a consideration of the problems of validating greenhouse theory in scientific and policy domains. We will present a framework for the purpose of thinking about issues involved in validating greenhouse change theory in science and policy. A basic link between the validation process in science and policy arenas involves the communication of validation information (what supports theory, what undermines theory, what is known, what is unknown, what is knowable, what is uncertain, etc.) from scientists to policymakers. Attention will be given to assessing how well scientific information on validation is presented to the policy community.

The impacts associated with current greenhouse predictions are potentially serious, and warrant much attention. In the thesis however, only a small part of the greenhouse issue is considered. We examine the theory of greenhouse change from the point of view of what matters for policy. If policy measures involving public expenditures and lifestyle changes are to be implemented in response to greenhouse predictions, then policymakers require some justification to act. They will need to know if the theory is

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[1] The term 'greenhouse' is objectionable to some, primarily on the grounds that the physics associated with the workings of a glass greenhouse are not very analogous to those associated with the atmospheric greenhouse effect. The term is widespread however, and the analogy does have some merit (see discussion in Bohren, 1987), so its usage will be continued here.

'valid'; that is whether the predictions from the theory are credible and useful.[2] A climate model is valid to the extent that it is plausible and produces results at some levels of aggregation that are useful. At some other levels of aggregation the results will not be very useful because the model is not designed to resolve certain scales or processes, or contains assumptions that do not hold well under certain conditions. In a validation study, bounds are implicitly set as we seek to determine whether there is any range of aggregation of output results that can be considered credible and useful.[3]

In this thesis, analysis will be confined largely to the issue of credibility, and we will not consider actual policies that might be implemented in response to greenhouse change. The credibility of greenhouse theory with regard to policy has not been directly addressed since beginnings were made in U.S. National Academy of Science reports in 1979 and 1982 (NRC, 1979/1982).

Given that different scientists accord greenhouse predictions with varying degrees of credibility, assessment of credibility by non-scientists in the policy domain is bound to be difficult. No attempt is made to answer the question of credibility definitively for policy makers. That assessment largely boils down to a value judgement, and must be made individually and collectively by interpreting available knowledge through value systems. The focus is on the problems of assessing credibility of greenhouse theory in science and policy arenas, and on identifying characteristics of the greenhouse issue that can be used by science and policy communities to establish a basis for examining policy credibility.

Greenhouse warming is a complex issue characterized by a high degree of uncertainty, making policymakers particularly sensitive to the way that scientists present information relevant to establishing credibility in the issue. Scientific inputs initiate the policy process, alerting society to the existence of a problem (potential greenhouse warming) and the need for policy responses.

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[2] We take a practical approach (considering what is useful) rather than a philosophical approach (i.e. can a model or theory ever be validated) to the problems of validating theory, since this is more immediately applicable to the policy making process.

[3] This definition is general enough to apply to validation in science or policy arenas, though in science it may occasionally be irrelevant, since we are sometimes only interested in understanding processes in models and do not care about the validity of the results otherwise.

Once a scientific issue takes on policy dimensions further scientific research is stimulated and the results continue to filter through to the policy arena. The presentation of these results shapes the policy process by either bolstering or reducing the perception of the problem and the case for a response. We will attempt to carry out a preliminary assessment of the manner in which scientists have been presenting validation information in greenhouse change theory to the policy community.

Thus far, essentially two tasks have been constructed. The first is to highlight problems encountered in assessing the credibility of greenhouse theory in scientific and policy arenas, and to identify links to facilitate closer coupling of information between the two arenas. To this end, the second task involves assessing how well scientists are doing at translating knowledge on validation of greenhouse theory from the scientific domain to the public domain. This is still far too broad, so we reduce both problems to a consideration of fundamentals in the hope that narrow but basic approaches to the problem serve some utility.

Assessing the credibility of greenhouse theory from the point of view of policy entails analysing the nature of predictions from the theory, since it is the predictions that are the ultimate input of the science to the policy process. For greenhouse theory, the 'prediction inputs' to policy take the form of 'how big a temperature rise or precipitation change, how fast, and where?'[4] By and large, these predictions come from three-dimensional general circulation climate models (hereafter simply 'climate models' or 'GCM's').[5] As

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[4] More correctly, it is probably the impacts on social systems associated with these predictions that is meaningful to policy makers, and not so much the predictions themselves. In other words, 'so what if the temperature rises five degrees on average?' In practice however, the impacts are incredibly difficult to determine accurately, and add another layer of uncertainty to the problem. In addition, the predictions from the models are by no means totally uniform, so that it is difficult to agree on a climatic scenario on which to study impacts. For these and other reasons, policymakers still find it useful to focus on the raw climatic predictions.

[5] The terminology is a little confusing, since 'GCM' is used to refer to both general circulation weather models used in weather forecasting, and general circulation climate models used in climate studies. Though the two types of models have many similarities (indeed, some climate models are modified weather models), there are also crucial differences. The differences stem from the need to consider different time scales and therefore different physical processes in one type that are not important in the other type. In this work, the usage of GCM as climate model is always intended. Sometimes climate models are further classified as atmospheric (AGCM) and oceanic (OGCM), or as a coupled ocean-atmosphere GCM.

noted in the thesis, in a philosophical sense the predictions (and levels of confidence associated with them) are not purely the result of the 3-D GCMs. Input to the 3-D models and interpretation of the results is aided by a host of simpler models, physical reasoning, and observations of past climatic behaviour. In order to make reasonable quantitative predictions about future climatic changes however, it is necessary to use a GCM to incorporate anything like the full degree of complexity of the climate system.[6] Manabe (1983) notes that use of three-dimensional GCM's is necessary for a comprehensive assessment of the influence of various feedback mechanisms upon the sensitivity of climate, as well as for simulating the latitudinal and geographical distribution of greenhouse climate change. Hence, much of the Congressional testimony in the U.S. on greenhouse change has been by GCM modelling groups on the results of their model predictions.[7] Representatives of various modelling groups considered in this work have all testified before U.S. Congress on greenhouse change predictons. Because of the central role of GCM's in determining greenhouse predictions, we will focus on GCM's in assessing the credibility of theory. In particular, we will analyse data from the three major climate models of the National Center for Atmospheric Research (NCAR), the Geophysical Fluid Dynamics Laboratory (GFDL), and the NASA Goddard Institute for Space Studies (GISS).[8] For these fully three dimensional models, data was made available in a form that facilitated intercomparison. Modelling groups at the United Kingdom Meteorological

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[6] Note that the raw output of an individual 3-D climate model is generally not considered to be a hard forecast or prediction. Rather, the output is the result of a sensitivity study, say to doubling of carbon dioxide in the atmosphere. Results of sensitivity studies effectively become predictions or projections when people start to have confidence in certain aspects of the results. Confidence may come through consistency with theoretical principles or observations, or confirmation through other sources (other 3-D models or simpler models). This is typical of the way that science works, and should not be considered unusual. The forecast does not consist of every tiny detail yielded by the model experiment, but usually just of the broader scale features, or those features that seem to be grounded on plausible physical mechanisms, or appear as an overall trend. In a weather forecast model, the patterns yielded on a particular day are intended to be predictions for that day. Climate however, is really only meaningful in an average sense, and model patterns for individual years are not intended to be forecasts for those particular years.

[7] Readers may well be familiar with the recent testimony of Hansen (1989) (Director of the NASA GISS climate group) which received media attention (New York Times, 1989d) after being altered on fairly blatant ideological grounds by the Office of Management and Budget.

[8] Running a 3-D climate model requires large resources in financial, technological, and intellectual capital, and there are currently few 3-D climate models worldwide. The best known climate modelling groups are listed here. To aid the reader in identifying the groups in subsequent discussion, members of the groups who are prominently featured as authors on most of the groups papers are listed as well: NCAR (Washington), GFDL (Manabe), GISS (Hansen), UKMO (Mitchell), and OSU (Schlesinger). UKMO is the United Kingdom Meteorological Office, and OSU is the Oregon State University.

Office (UKMO) and Oregon State University (OSU) have also performed greenhouse climate experiments, but their results are not directly included in analysis here. For UKMO, output from the CO_2 experiments was not available at the time thesis research was undertaken. The OSU model has severly limited vertical resolution (2 levels) and is therefore not well suited for direct comparison with the other 3-D models.

Further refinement of scope is necessary, since the GCM's are for all intents and purposes just as complex as the real climatic system.[9] It is impossible to verify all aspects of GCM performance in simulating the earth's climate and how it might change (however, attempts to verify even isolated aspects of model performance have not been extensive to date). To keep this study manageable, we will consider a single process to validate, albeit a fundamental one. We have chosen to validate the energy transport because it is fundamental to the simulation of climate, and until now has not been scrutinized for the three major models. The results from a validation of energy transports do not yield definitive answers to questions of credibility of the models, but constitute an important case study from which to explore this issue.

The role of energy transport can be understood in terms of the earth's annual radiative energy budget. The earth receives significantly more annually averaged solar (short wave) energy in the tropics than in the polar regions. The emitted longwave energy is more uniform over the globe, and is unable to completely offset the equator to pole gradient in the solar heating, resulting in a strong equator to pole net heating gradient. This radiative heating gradient is the fundamental driving force for the atmospheric and oceanic general circulation (Ramanathan, 1988), and results in a poleward energy transport. Since the energy transport reflects the action of both radiative and dynamical processes, analysis of it may indicate where model deficiencies are occuring. We will also use a one-dimensional energy balance climate model to study how sensitive the climate is to the energy transport.

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[9] Of course, the GCM's are gross simplifications of the real climatic system, but they capture many of the essential processes and feedbacks, and possibly even some of the intrinsic internal behaviour. As such, they are far too complex and sophisticated for us to fathom all their interconnections. While we can perform sensitivity experiments with GCM's, and examine important processes in isolation with simpler models, the GCM's remain black boxes to some degree.

The transport plays an important role in determining the iceline position on the earth, which in turn influences the climate through the ice-albedo feedback mechanism.[10] The energy transport also determines the meridional temperature gradient, which influences regional climate. Validation of the energy transport is thus an important component in the overall verification of GCMs used for developing greenhouse change projections.

If the transports play a fundamental role in determining the climate, yet are not modelled well as we suspect, what are the implications for credibility of the models and greenhouse predictions? We will consider this question, and outline the process by which credibility is generally established. There are no sufficient tests that can be performed to validate the models, so that confidence is established in the predictions by considering a gestalt of theory and observations. Understanding this point is crucial for the policy analysis of greenhouse theory if any sense is to be made of it at all by the non-climatologist. Otherwise, the greenhouse validation debate can degenerate into a series of apparent refutations and confirmations.

The second task is to address the problem of translating available knowledge on validation of greenhouse theory from the scientific domain to the public domain, in so far as this is relevant to the formulation of policy responses to greenhouse change. This translation of knowledge goes on at many levels. One of the most direct forms of translation occurs through Congressional testimony of scientists to politicians. One of the more far reaching channels of communication is between scientists and the media in press, radio, and television interviews, and through popular articles in newspapers and magazines. All communication fora have various shortcomings, and the lack of available time and space is common to all of those listed above. By far the most sophisticated source of information on greenhouse theory (and also the root source) is the scientific journal literature. Keeping with our focus on fundamentals, we have chosen to

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[10] Ice-albedo feedback begins when an initial warming causes sea ice and snow to melt. This shrinking of the snow and sea ice margins results in a lower albedo in these regions. Incrementally more incoming radiation can then be absorbed in the formerly ice- and snow-covered areas, thereby resulting in further warming, more snow and ice melt, and so on until equilibrium is established. For cooling, the reverse occurs. Ice grows and increases the albedo and energy loss, leading to further cooling and growth of ice.

analyse this medium for the translation of greenhouse theory (and the problems of validating theory) to the policy level.

The science journal literature on greenhouse theory has been interpreted in the press throughout its history. Press coverage of journals is instrumental in disseminating knowledge of the theory publicly. Press coverage also shapes the receipt of knowledge of theory through interpretation of the journal articles and resultant presentation. We believe that media interpretation of the greenhouse journal literature is important in linking science with policy and in determining how the policy community assesses the credibility of greenhouse theory. Thus, we will consider how greenhouse theory and controversies relating to it are presented in the journal literature, and how the journal articles are then interpreted by the media. The New York Times will be used as the principle media source, because it is well archived and is among the most sophisticated of the presses in its discussion of environmental issues. Misinterpretation in the media of research results relevant to validating greenhouse theory is frequent, and various patterns and types of misinterpretation can be identified. We will attempt to classify them and show how the failure to provide appropriate context contributes to the occurence of misinterpretation. Though instances of misinterpretation of the validation of theory probably result from errors by the media as well as scientists, we primarily will be concerned with the role of scientists.

Through the sequence of analyses described above, it is hoped that we can identify what the major problems associated with validation of greenhouse theory are, and how the problems are being dealt with by the science community in presentation to the policy community. A longer range goal is to have scientists think about the validation problem in relationship to the policy framework when communicating validation information. Any serious attempt at this last problem would try to account for the factors motivating scientists in framing their results, which is too big a task for this thesis.

In subsequent chapters we will briefly outline greenhouse theory and the role of climate models, before reviewing previous work on validation of climate models. We will then describe the three climate models considered here, the output used from the models, and the observational data used to compare with the models. The results for validation of the models will be presented, and implications of the results considered. Implications of errors in the energy transport on climate sensitivity will be examined by parameterizing 1-D energy balance models with output from the GCMs. Taking the validation issue in a wider context, chapter eight will present a science-policy validation framework. The penultimate chapter will consider how well greenhouse theory is presented, and conclusions will follow.

2 GREENHOUSE THEORY

2.1 Greenhouse effect

Perhaps a good way to introduce greenhouse theory is through a breakdown and definition of parts of the theory. We note that greenhouse theory has been discussed widely in the scientific literature and the press without close attention to language definition. This has resulted in a blurring of distinction between key parts of the theory.

Following Handel (personal communication) we will use the terms 'greenhouse effect' and 'greenhouse change' to refer to different parts of greenhouse theory. The greenhouse effect refers to the well known and basic radiative physics whereby absorption in the infrared by various trace gases (primarily water vapour, carbon dioxide and ozone) and clouds, maintains a temperature at the earth's surface and in the lower troposphere considerably greater than the equivalent blackbody temperature of the planet.[11] The radiatively active trace gases absorb the longwave radiation emitted by the warm surface of the earth. This energy is reemitted back to the surface, and also back into space at much colder atmospheric temperatures. This yields a net trapping of radiative energy or greenhouse effect. Note that there is nothing anthropogenic in this definition, nor even anything time dependent.

The above description of the greenhouse effect can be set in quantitive terms quite easily as follows: The effective radiating temperature of the earth, T_e, is determined by the requirement that infrared emission from the planet balance the absorbed solar radiation thus:

$$4\pi R^2 \sigma T_e = \pi R^2 (1-A) q_o \qquad (2.1)$$

where R is the radius of the earth, σ the Stefan-Boltzmann constant, A the albedo of the earth, and q_o the flux of solar radiation at the mean earth-sun distance. Choosing realistic values of $A=0.3$ and $q_o=1367 W/m^2$ (following

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[11] The references to earth and its primary greenhouse gases could easily be removed to allow the definition of greenhouse effect to apply to other planets as well.

Hansen et al. 1981) yields $T_e \approx 255K$. The mean surface temperature of the earth is about 288K. The 33K difference between this and the blackbody value of 255K is due to the greenhouse effect of trace gases and clouds. Greenhouse gases cause the infrared opacity of the atmosphere to increase, and the mean radiating level to be above the surface. To achieve radiative balance, the temperature of the surface and atmosphere must increase until the emission of radiation from the planet again equals the absorbed solar radiation.[12]

The mathematician Jean-Baptiste Fourier described the greenhouse effect in 1827 (Fourier, 1827). Fourier essentially understood that the earth's atmosphere is largely transparent to solar radiation, while it retains some of the longwave radiation emitted from the surface. Early investigations of this basic greenhouse effect were concerned with explaining why the temperature at the surface of the earth is as warm as it is. Ramanathan (1988) notes that the greenhouse effect is as important as solar energy in maintaining the temperature of the planet.

2.2 Greenhouse change

'Greenhouse change' refers to an alteration of the greenhouse effect related to the time dependant build up of greenhouse gases (carbon dioxide, chlorofluorocarbons, methane, etc.) in the atmosphere, that perturbs the climatic system from global radiative equilibrium.[13] The distinction between the terms greenhouse effect and greenhouse change is useful, since the greenhouse effect is well understood and widely acknowledged in the climatoligical field, whereas greenhouse change is less well understood, and still disputed in various details within the climate community.[14]

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[12] Hansen et al. (1981) describe a useful analogy to the surface temperature resulting from the greenhouse effect in terms of the depth of water in a leaky bucket with constant inflow rate. If the holes in the bucket are reduced slightly in size (increased concentration of greenhouse gases), the water depth (infrared opacity) and water pressure (tropospheric temperature) will increase until the flow rate out of the holes (the radiation leaving the planet) again equals the inflow rate (the radiation entering the planet).

[13] Greenhouse change could be defined more broadly to refer to changes in trace gas concentrations on paleoclimatological time scales, leading to warming or cooling. In this paper it will be used mostly in the sense of recent changes (plus and minus one hundred years or so from the present), and so refers to the largely anthropogenically induced greenhouse change.

[14] It does not enhance the policy debate to lump the greenhouse effect in with greenhouse change, thereby attributing unnecessary uncertainty to a part of theory that has been widely accepted.

Tyndall (unpublished)[15] and later Arrhenius (1896) and Chamberlin (1897) were the first to apply the notion of greenhouse change by considering changes in atmospheric carbon dioxide concentration. In particular, they were interested in how past changes in carbon dioxide concentration were related to past glacial epochs. The greenhouse theory of climate change invoking carbon dioxide variations is now viewed as one of the major mechanisms for understanding climate variations of the past (Ramanathan, 1988).

With the work of Arrhenius (1908) greenhouse change began to be viewed as a possible consequence of human activity as well as being related to longer term natural variations. George Callendar circa 1940 was the first to suggest that human activity had already led to greenhouse change. Callendar effectively synthesized information on fossil fuel consumption and CO_2 emissions, atmospheric CO_2 concentration, CO_2's radiative characteristics, and marine and land surface temperatures between 1880 and 1935. He combined these observations with simplified models of air-sea exchange of CO_2 and surface radiation energy balance climate models to develop a theory of greenhouse change, the essence of which is yet to be disproved (Ramanathan, 1988). The theory contends that increased atmospheric CO_2 concentration can be attributed to CO_2 released by fossil fuel combustion, since the exchange of CO_2 between the air and the intermediate ocean waters is not fast enough to absorb the excess CO_2 from industrial activities. Further, CO_2 strongly absorbs radiation in the 12-17μm region, and hence the increased CO_2 would radiatively heat the surface. The increased CO_2 heating is then invoked by Callendar to help explain the observed global warming.[16]

Subsequently, short term global surface air temperature changes (1940-1970) levelled off somewhat, and probably helped contribute to a decline in attention to Callendar's theory. Following the work of Plass, Revelle, Suess, and Keeling in the 1950's, greenhouse theory underwent a significant refinement with the paper by Manabe and Wetherald (1967). They utilized a 1-

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[15] The reference to Tyndall's involvement here is speculative (Handel, personal communication).

[16] It is interesting to note that Callendar is not distressed by the possibility of warming, since like Arrhenius (1908), he views the warming as protection against pending ice ages.

D radiative-convective model to demonstrate that the surface and troposphere are so strongly coupled by convective heat and moisture transport that the relevant greenhouse forcing that governs the surface warming is the radiative perturbation of the troposphere as well as that of the surface. The 1-D radiative-convective models opened the way for the development of 3-D climate models, and still form an important tool for interpreting the 3-D model results.

For a comprehensive description of greenhouse theory, the reader is referred to Ramanathan et al. (1987). We shall proceed with a basic description of a 3-D climate model, and model CO_2 equilibrium experiments. The particular NCAR, GFDL, and GISS climate models will be described in the chapter on models.

2.3 Climate models and climate

Before discussing the role of climate models in greenhouse theory, we will briefly outline the approach taken in constructing a 3-D model of the climate system. According to the traditional approach (outlined in Schneider and Dickinson, 1974), a climate model attempts to account for the basic factors governing climate (solar radiation, ocean circulation, ice cover, vegetation, winds, etc.) by relating them by the equations expressing conservation of mass, momentum, and energy, together with the thermodynamical and chemical laws governing the change in material composition of the land, sea, and air. The set of coupled non-linear three-dimensional partial differential equations yielded by the theory are solved subject to the external input of solar radiation, and for a given initial state of the earth-atmosphere system. In principle, the time dependency of the equations allows for modelling of the evolution of the dynamical and thermodynamical state of the atmosphere, land and ocean. The equations are solved on a three-dimensional grid representing latitude, longitude, and height. Some models use a combined grid and spectral representation for computational efficiency and accuracy. Typical GCM resolutions are at best a few degrees of latitude and longitude horizontally, and a few kilometres or so vertically. Processes occuring on scales larger then these are explicitly treated, whereas phenomena occuring on smaller scales such as cumulus convection, condensation, turbulent transport, and cloud formation must be parameterized.[17]

The task of modelling climate on a computer involves performing an enormous number of operations. Following Stone (personal communication), consider solving for say 10 primary variables over a 3-D grid of 100 latitude points x 100 longitude points x 10 vertical levels. This yields 10^6 unknowns at each time step. For 15 minute time steps over a typical simulation period of say 30 years, the number of unknowns to solve for is about 10^{12}. Large computers are required for such an undertaking.

Schneider and Dickinson (1974) note that "the technique of selecting appropriate spatial resolution and representation, time step size, and parameterization technique for use in the construction of approximate theories of climate is a large part of the art of 'modeling', and the particular choice of these elements, along with the choice of physical and chemical factors, determines the model." The use of 3-D models is a relatively new addition to greenhouse science, with Manabe and Wetherald (1975) among the first of the 3-D model papers in the literature. The article by Schneider and Dickinson provides a good overview of climate modelling for those interested in further details. For a good description of a single 3-D climate model, Hansen et al. (1983) is comprehensive and includes description of sensitivity experiments as well.

2.4 Greenhouse change modelling experiments

To model the response of the climate system to greenhouse forcing, the amount of CO_2 and other trace gases is simply increased in the radiation scheme of the model. This reduces the amount of energy leaving the atmosphere, which in turn alters the temperature and then other climate variables in the model such as winds, cloud cover, and precipitation patterns. All that modellers require to generate the response of the system to an initial perturbation is the ability to calculate the physical forcing terms in the fundamental equations, in this case absorption of radiation by gases in the

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[17] Parameterization is short for parametric representation.

equation for conservation of energy (Rind et al. 1988). Ideally, this is performed as a transient experiment, demonstrating the gradual response of the climate system to the forcing. Transient experiments are still in their infancy however. Hansen et al. (1988) represents the first published results of a transient experiment.

For consistency of comparison across modelling groups, we have selected results from the more traditional GCM CO_2 equilibrium response experiments to study. In the equilibrium CO_2 response experiments, the model is first run for a number of years (typically 10 or 20 years of model time) with boundary conditions and forcing factors as they are today. The last ten years or so of this control run provides an important test of how well the climate model simulates the real climate system. The next step in the equilibrium response experiment is to introduce a perturbation of one of the boundary conditions or forcing factors. In the experiments considered here, this is an instantaneous doubling of atmospheric carbon dioxide concentration.[18] Note that accurate simulation of current climate in the control run is important for the experiment, since the doubling of CO_2 induces a perturbation on the current climate. If the current climate is not correct, then the accuracy of the perturbed climate simulation may be called into question in so far as non-linearities are important and cannot be linearized with sufficient accuracy. Because of the importance of the control run in the CO_2 sensitivity experiment, later analysis of transports in the major 3-D models will be for results from the control run simulations.

After instantaneously doubling CO_2, the model is again run for a number of years for the perturbed run. The difference between the perturbed run and the control run is a measure of the sensitivity of the model to the prescribed change between two equilibrium conditions. The commonly quoted 2 to 5K temperature change for a doubling of CO_2 in the atmosphere is based on the results of these equilibrium experiments with GCM's.[19]

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[18] The change of carbon dioxide in the models is usually taken as a surrogate for a change in all of the greenhouse gases (Kellogg and Zhao, 1988).

[19] The global surface air temperature changes resulting from these experiments for the models considered in this work are: NCAR:3.5K; GFDL:4.0K; and GISS:4.2K, (and for UKMO:5.2K).

Arguments advocating the need for policy responses to greenhouse change frequently make use of the GCM temperature predictions for a doubling of CO_2 in the atmosphere. Current GCM predictions are rather sobering, and are quoted to stress the gravity of the problem. The greenhouse policy arguments usually note that if the temperature response is as predicted by the GCM's, then the rate of temperature change will be virtually without precedent, and may have major implications for ecosystems and social systems. We note then, that some arguments for greenhouse policy responses are predicated to some degree on the results of GCM model simulations, and so investigation of the credibility of the models is a policy-relevant pursuit.

2.5 Greenhouse change controversies

From a policy perspective, a lack of consensus in a scientific community on a particular theory weakens the likelihood that the theory will be taken seriously, and policies implemented. Since disputes are part of scientific development and will always exist to some degree, policymakers must ascertain whether existing disputes are significant or relevant to the range of possible policy options or not. With this in mind, we turn to a discussion of greenhouse theory disputes.

In greenhouse theory, the greenhouse effect is virtually undisputed. With respect to greenhouse change, the important parts of the theory are the determination of the greenhouse forcing, and the climatic response to the forcing. The magnitudes of the radiative forcings associated with changes in trace gas concentration are reasonably well understood, and not much argued about. However, greenhouse change is disputed as to whether the climatic change resulting from the greenhouse forcing will be as large as the models predict, and as to whether the change is yet detectable.

Criticizm of the magnitude of the model predictions is based on the crudity of the models in neglecting or oversimplifying various processes (ocean response, clouds and moist dynamics, vegetation, hydrology, etc.), and ocassionally on conflicting results from some simple models. In regard to conflicting results, the work of Idso (1980) and Newell and Dopplick (1979) using surface energy balance considerations stands out. These works predicted an order of magnitude less temperature response to CO_2 doubling than the GCM's. These results have been explained by Ramanathan (1981) on the basis that the "important tropospheric CO_2 radiative heating is ignored by both Newell and Dopplick (1979) and Idso (1980)."

Probably the most potent criticizms related to global scale shortcomings in the GCM's involve the representation of clouds and the role of cloud feedbacks. For the current climate, Ramanathan et al. (1989) find that clouds play a major role in determining the radiative balance of the atmosphere, and presently constitute a negative feedback. That is, clouds presently cool the planet (through their albedo effect in reflecting shortwave radiation) more than they heat it (through their greenhouse effect in trapping longwave radiation). The crucial question regarding greenhouse change is how the cloud mix would change in response to greenhouse forcing. For the current GCM's employing predictive cloud schemes, cloud changes provide a significant positive feedback. For the GISS model this occurs as a result of a small increase in mean cloud height (enhancing cloud greenhouse effects)[20], and a small decrease in cloud cover (decreasing their albedo effect) (Hansen et al. 1984). Since current cloud schemes in the models are quite crude and cloud feedbacks contribute significantly to the predictions, the model predictions contain significant uncertainty. Uncertainty alone implies nothing about the direction of potential error in the feedback, though there are some indications in this regard, which we will consider.

Changes in cloud feedback can occur as a result of changes in cloud composition, coverage, and height. The 3-D models consider (albeit crudely) changes in cloud coverage and height, but not composition. As mentioned, the changes in coverage and height in the models are such as to lead to positive feedback. As regards cloud composition, it is thought that the greater availability of water vapour for condensation in a greenhouse warmed atmosphere could lead to a systematic increase in cloud liquid water contents, though the relevant physics are not well understood (Somerville and Remer, 1984). In the 1-D radiative-convective model of Somerville and Remer, the

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[20] As cloud top height is increased, the cloud top temperature becomes progressively lower than the surface temperature, reducing the emission of longwave radiation from cloud top to space, and thus enhancing the radiative heating of the troposphere and surface.

result of increased cloud liquid water content (for clouds other than thin cirrus) is to increase the albedo effect of clouds more than the greenhouse effect of clouds. This yields a negative feedback for cloud optical thickness changes, and possible decrease of climate sensitivity by a factor of two. Mitchell (1988) notes that although the results of Somerville and Remer may be qualitatively correct, cloud liquid water content depends on factors other than temperature. In the limited observational data employed by Somerville and Remer, there is little change in cloud liquid water content with temperature above 273K.

Another negative feedback mechanism was proposed by Twomey et al. (1984). Twomey pointed out that an increase in tropospheric particulates will lead to an increase in the number density of cloud condensation nuclei, which can enhance cloud optical depth. A variant of this feedback involving biogenic processes was proposed by Charlson et al. (1987). Over the oceans, the number density of cloud condensation nuclei is influenced by the emission of dimethyl sulfide from marine organisms. An increase in surface temperature could lead to an increase in dimethyl sulfide emissions, and thus also to an increase in cloud optical depth. Ramanathan (1988) notes that while the above microphysical processes are certainly plausible, the global extent of these processes has not yet been satisfactorily demonstrated.

Some of the cloud physics not included in the model cloud schemes could provide significant additional positive feedback also (Ramanathan and Handel, personal communication). For instance, cirrus clouds may be of larger areal extent and longer lived than allowed for in the models, which are more concerned with vertical heat transport than the microphysics of cloud formation. Cirrus shields would probably provide an additional positive cloud feedback due to the dominance of their greenhouse effect.

In a 1979 assessment of cloud feedback effects on temperature, NRC (1979) allowed a +1°C error in CO_2 doubling temperature response due to uncertainties in high cloud effects. Writing prior to such work as Somerville and Remer (1984) and Twomey et al. (1984) they were unable to find evidence for an appreciable negative feedback due to changes in low and middle cloud albedos or other causes, and allowed only 0.5°C as an additional margin for error on the low side due to cloud feedback uncertainty.[21]

The point of this discussion of cloud feedbacks is to illuminate the controversy and complexity, and to recognize that criticizm of the magnitude of the warmings produced by the models is not unfounded, but doesn't necessarily invalidate the predictions either.

Other areas in which GCM's are criticized is over the representation of oceans, ond over the validity of equilibrium response as opposed to transient response experiments. Schneider and Thompson (1981) attemped to weigh the seriousness of deliberately neglecting heat capacity of the deep oceans in most models and of using a fixed-CO_2 increase instead of a time evolving scenario of CO_2 increase. The actual mixed layer of the oceans is generally deeper in high latitudes than in low latitudes, and would therefore tend to heat up more slowly in high latitudes. Thus, although albedo/temperature processes would cause high latitudes to warm more in equilibrium, mid-latitudes and some high latitude regions would not necessarily warm up as fast as lower latitudes during the transition period towards the new equilibrium. Just how important this process is depends on how rapidly the CO_2 concentration actually increases with time. If CO_2 increases rapidly, neglecting the transient would be a more serious error than if CO_2 increases more slowly. In addition, Schneider (1984) notes that atmospheric water vapour increases with surface temperature increase, accounting for perhaps 25 to 40% of the equilibrium surface temperature response to CO_2 increase. During the transient warming period, oceanic response may well lag that of the atmosphere, thereby temporarily reducing the water vapour increment that would eventually occur after equilibrium was reached.

The transient warming response would also be affected by the rates at which water from the ocean mixed layer mixes with water below it, and by changes in the rate in response to climatic change. In the Hansen et al. (1988) transient response experiment, the uptake of heat perturbations by the ocean beneath the mixed layer is approximated as vertical diffusion. Bryan and

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[21] Their overall estimate considering all sources of uncertainty was 3°C±1.5°C.

Spelman (1985) investigate CO_2 perturbations in a coupled ocean-atmosphere model. During the greenhouse warming episode, their model yields a partial collapse of the ocean thermohaline circulation[22], allowing the ocean to take up twice as much heat as would be predicted by a simple diffusive approximation under normal climatic conditions. Bryan and Spelman conclude that "an enhanced sequestering of heat would produce a negative feedback for greenhouse warming. However, the partial collapse of the thermohaline circulation found in the numerical experiment would also affect the global carbon cycle, possibly producing a climatic feedback as strong as that caused by an enhanced uptake of heat from the atmosphere." Broecker (1987) outlines ocean circulation processes not included in the climate models with simple mixed layer oceans, that could lead to abrupt climate changes. He warns against assuming that the climatic response to increased greenhouse gases will be a relatively smooth gradual warming over a period of about 100 years. In initial experiments with a coupled ocean-atmosphere model, Stouffer et al. (1989) report at least one surprise in the model simulation. This relates to the upwelling of cold water off the coast of Antarctica in the model in response to greenhouse forcing, leading to cooling in that region.

Though transient response experiments are in the offing now, much of the uncertainty related to the crude representation of the oceans in climate models will exist for decades to come, and we should not expect to see quick improvement over the model simulations discussed here.

The other major area of controversy relevant to greenhouse theory is over whether greenhouse change is yet detectable. The detection problem is incredibly difficult since the rough estimates of climate response times currently available would have the greenhouse signal perhaps only just beginning to emerge from the background of climatic noise. In addition, it is difficult to quantify climatic variations due to other forcing factors (solar variations, volcanoes, etc.).[23] Thus, it is difficult to provide unambiguous answers to the detection question at this stage, though it is expected that it will become easier with time as the greenhouse signal emerges more clearly from

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[22] The thermohaline circulation is the circulation that is driven by the buoyancy flux in the ocean.

[23] Hansen et al. (1981) have attempted this exercise, but have been criticized in Clark (1982) for using up all available degrees of freedom in fitting observations.

the background noise. For a more indepth discussion of greenhouse detection problems, refer to Clark (1982) and Ramanathan (1988).

Detection efforts and controversies have concentrated mainly on the temperature signal, particularly at the surface (air) for the global average over the last one hundred or so years.[24] The major recent compilations of global surface air temperature of Jones et al. (1986) and Hansen and Lebedeff (1987) show a warming of about 0.5K for the last century. This warming has been called into question mostly on the grounds of possible contamination by urban heat island effects from growing population centres over the period. Both the Jones et al. and Hansen and Lebedeff works have included attempts to screen out data contaminated by urban effects, and to remove any remaining urban heat island effect bias. Analysis of this processed data for urban warming contamination has been carried out mostly for the U.S. portion of the globe (which represents perhaps an upper limit to the degree of contamination). Karl and Jones (1989) find the urban bias to be a substantial portion of the overall trend for the U.S. in the Jones et al. and Hansen and Lebedeff data sets (but this trend is still small relative to the global trend). However, subsequent reworking of the U.S. data by Hansen et al. (1989) (where the 'et al.' includes Karl) found that the urban bias was not as significant as determined by Karl and Jones. The Karl and Jones work was in error due to an incorrect mapping of the U.S. temperature data in one of the data sets (Hansen, personal communication).

Though the detection problem is difficult, it has been the centre of attention, since unambiguous detection of a greenhouse climate signal would give added impetus to the credibility of greenhouse theory, and therefore to policy iniatives as well. It is probably reasonable to surmize that if people believe that greenhouse change is here, they are more likely to believe the model predictions for how further changes will take place, and more likely to press for policy responses. Conversely, if a clear refutation of any detection of expected greenhouse climate signals could be given, this would undermine credibility of the theory, and tend to dampen greenhouse policy initiatives.

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[24] The global scale is the best place to look for greenhouse signals, since it is the least noisy.

Among climatologists, Hansen (1988) is perhaps the most outspoken on greenhouse detection[25] in claiming that "the global warming is now sufficiently large that we can ascribe with a high degree of confidence a cause and effect relationship to the greenhouse effect." Recent greenhouse journal literature and press coverage of that literature has reacted and responded directly and indirectly to the above statement of Hansen.[26] Because of the importance that belief in detection or lack thereof of a greenhouse signal has for credibility of the theory and policy responses, we will pay particular attention to this issue in the chapter on presentation of theory.

The greenhouse detection issue is also impacted by a problem regarding the nature of the climate system, and policy responses to changes in the system. The problem stems from the fact that the time scales for response of the climate system to greenhouse forcing are relatively long - of order decades to centuries (Hansen et al., 1985), so that by the time greenhouse change can be detected in a manner convincing to all, the earth will already be committed to a further greenhouse warming by the trace gases currently present in the atmosphere. Thus, Ramanathan et al. (1989) note that "even if the trace gas concentrations were to stop increasing today, the planet will still warm by 0.8-2.4K. (The three-fold range is the currently perceived uncertainty in model predictions.) If, on the other hand, the currently observed growth rates continue unabated, the committed warming can double within the next 50 years." The policy leverage available in reducing the committed warming is actually quite significant. Hansen (1989) notes that if the growth rates of greenhouse gases, CFC 11 and CFC 12 had not been reduced in 1978 as a result of the aerosol CFC ban in some countries, the decadal greenhouse forcing produced by CFC's would currently be as large as that due to CO_2. As it stands, the CFC contribution to the decadal forcing is about half that due to CO_2.

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[25] Among climatologists, Hansen is also somewhat unique in having directed a concerted, sustained, and productive effort to theoretical, modelling, and observational aspects of greenhouse theory. Readers should note the possibility for bias here in the author's institution's cooperation with the NASA GISS group, but we feel that the above comment is reasonable nonetheless.

[26] See Kerr (1989) for one version of the controversy surrounding Hansen's statements.

2.6 Greenhouse problems

In this work, we will not consider those aspects of the greenhouse issue that might fall under the term 'greenhouse problem(s)'. 'Greenhouse problem(s)' refers loosely to difficulties resulting from alteration of ecosystems and social systems due to climate change brought on by greenhouse change. The plural form follows a distinction by Clark (personal communication), that the resulting problems from greenhouse change will be very different for different people in different places.

3 VALIDATION OF CLIMATE MODELS

3.1 Approach

Validation of 3-D climate models is crucial to validating greenhouse change predictions because of the central role of the 3-D models in generating the predictions. Here we present a brief review of strategies employed to validate the 3-D climate models. Before proceeding however, note the point made by Keepin (1986) that " 'model validation' is a misnomer because it is actually impossible to show that a model is valid. The most one can do is to show that it is not invalid, and even this can be a formidable task. Thus model validation is equivalent to establishing a necessary but not sufficient condition for 'believing' the results from a model". Schneider (1987) notes that "the accuracy of a climate model cannot be proved conclusively; it can only be verified by circumstantial evidence."

In general however, validation of climate models has been neither thorough nor systematic, so that opportunities to generate necessary circumstantial evidence have been missed. With the exception of the GISS model (Hansen et al., 1983) basic climate diagnostics have not been systematically calculated or saved for the models to compare model performance with observations and to ensure internal consistency of dynamical processes. Mean model fields are saved, but other basic statistics such as variances of model variables have not generally been saved or published from model runs in order to examine climate variability as simulated by the models.[27] The reasons for a less than comprehensive approach to model validation among most of the modelling groups are not clear. It may be a low priority because of resource or funding constraints, or perhaps even because it appears to be less interesting than more direct model development projects.

Validation of 3-D climate models is made difficult by the fact that we do not have detailed knowledge of anything but the present climate state and its

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[27] Steps in this direction have now been taken with the publication by Rind et al. (1989) for the GISS model.

seasonal changes to compare with the models. Knowledge of past climates is lacking in detailed information, and future climatic states have not yet evolved to test the model predictions. Contrast this with the luxury of validating weather forecast models. In weather forecasting, the initial state of the atmosphere is determined as well as possible, then the model is run out for a week or two. Subsequent to the forecast, detailed knowledge of the actual evolution of the atmosphere over the period of the forecast can be collected, and a direct validation of the transient forecast is possible. While a satisfactory direct verification of 3-D climate model performance such as this is not possible at the present time, indirect methods can be used involving analogs of various kinds. We shall discuss some of these presently.

The variety of validation tests that are left once a direct comparison with the transient climatic response is ruled out can be grouped into three categories (following Wigley and Santer, 1988). These are, validations of the internal physics and subgrid scale parameterizations of the models, validations against present climate with the model in the control run mode, and validation against other climate states in the perturbed-run mode. We will touch on each of these strategies, before considering validation of model energy transports. Validations in each of the categories are necessary in order to gain some confidence in the credibility of the 3-D models, though validations in a single category alone are not sufficient to demonstrate the veracity of the models. As we proceed, we will rediscover Keepin's point that it is possible to devise many testable necessary conditions to validate greenhouse theory, but no real testable sufficient conditions. This is an important point to grasp, and implications of this for policy will be discussed in later chapters.

3.1.1 Internal validations

For internal validations, the accuracy of each internal process or parameterization is tested separately by comparison with observations, or by comparison with the results of more detailed models of these processes. Prime candidates for internal validation ate the radiative transfer schemes, and subgrid scale details such as cloud and land surface processes parameterizatons (Wigley and Santer, 1988).

A recent organizational effort has been directed towards intercomparison of radiative codes in climate models (ICRCCM) (Luther et al., 1988). The relative accuracy of various radiation codes can be assessed via ICRCCM, but determination of the absolute accuracy of the schemes is prevented by a lack of high quality observational data to compare with. Outstanding uncertainties in the radiation schemes relate to the treatment of water vapour, carbon dioxide, and methane. The water vapour uncertainties are particularly important because of the major role played by water-vapour feedback in climate sensitivity.

The limitation of internal validations in isolation is that they cannot guarantee that the complex interactions of many individual model components are properly treated. To investigate these interactions it is necessary to consider the response in the control run and perturbed run modes.

3.1.2 Perturbed-run validations

The most obvious perturbed-run validation technique is to compare the perturbed-run equilibrium model results of one 3-D model with another. This is not a sufficient test since the model physics across models is similar[28], and agreement could simply indicate this. Obtaining agreement in the perturbed run is more difficult than in the control-run however, since agreement of some features in the control run can be partly explained by tuning[29] of the models about the same current climate. We will discuss some of the features of the perturbed-run comparisons for the NCAR, GFDL, and GISS models in a later section.

Another approach to validating the doubled CO_2 equilibrium results in the models is to compare the resulting climate response with warm year analogs in the past century. Schneider (1984) explains the rationale as follows: "Perhaps there is something characteristic about the regional

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[28] The conservation equations are the same for each climate model, though the parameterization schemes vary somewhat from model to model. The physics and numerics in the NCAR and GFDL model are relatively similar, but quite different from the GISS model.

[29] Tuning refers to the adjustment of empirical parameters that govern model-simulated processes which are not calculated by integrating conservation laws.

climatic patterns in 'warm years', and that the distribution of regional climatic anomalies in the warmest few years of the past century might be repeated if long-term warming were to be created in the future by CO_2 increases." Studies such as that by Pittock and Salinger (1982) have noted some broadscale agreement with such an approach. A problem with this approach however is that "a CO_2 induced warming will not necessarily result in similar seasonal or regional patterns of warmer, cooler, wetter, or drier climates, since CO_2 increase from human activities is an external forcing and individual warm years of the twentieth century could well be internally caused rather than externally forced. Analogously, Lorenz (1979) showed that inferences of cause and effect for longer time 'forced' climatic variations can be very different from those described by looking at shorter period 'free' climatic variations" (Schneider, 1984).

Instead of choosing warm years, analogies of warmer periods can be chosen from the paleoclimatological record. Hansen et al. (1981) note that reconstruction of regional climate patterns in the altithermal (5000-9000 years ago, when the earth was a degree or two celsius warmer than at present) show some similarity to 3-D climate model results. Manabe and Bryan (1985) used a coupled ocean-atmosphere climate model with simplified geography and various CO_2 perturbations from 1/2 to 8 times the present value to consider paleoclimatic implications. They found that "in general, the climatic signature obtained from the model appears to be consistent with a CO_2 hypothesis for the climatic changes in the Cenozoic with the following exception: the tropical sea surface temperature in the model has a small but significant increase with increasing atmospheric CO_2 concentration, while tropical sea surface temperature as deduced from the isotopic record appears to have no systematic trend during the Tertiary."

Paleoclimatic validation studies have numerous problems associated with them, not least of which is the problem of separating different forcings. In addition, the time scales of the relevant forcings may well be different. Anthropogenically induced greenhouse forcing occurs over a period of order 100 years, while longer period forcings such as orbital variations occur on time scales of 10000 years or longer. This is significant, since detailed time-series analysis of various climatic indicators has shown that the climatic response lags behind the forcing, and that the response is frequency dependent and non-linear (Imbrie and Imbrie, 1980). Though the causes for warming on paleoclimatological time scales may be different from the contempory greenhouse forcing, Kellogg and Zhao (1988) note that warmer climatic periods do have one important feature in common with a greenhouse warmed earth. This is that "the equator-to-pole temperature difference was, and will be in the future, less than the present norm, and the large scale circulation patterns may therefore respond similarly to this change in forcing."

3.1.3 Control-run validations

The basic rationale behind control run validation is that if a model fails to simulate important features of the observed climate in the control run mode, then it will also fail to simulate these features in a perturbed mode. This may not be fatal since it is the difference between the runs that counts. However, non-linearities such as the ice-albedo feedback mechanism could cause problems for the perturbed run if the control run is in error. That is, if the non-linearities cannot be linearized with sufficient accuracy, then it is important to have the simulation of the control climate at least approximately correct.

Validation of the current climate simulation in the control run has the advantage that it can be compared directly with observations of the real climate. Features in the model can be tuned to match observations of the current climate. For example, the surface albedos can be tuned to ensure that surface temperatures are in reasonable agreement with the observations by reflecting more or less radiation from the surface as desired. The models are just too complex however for modellers to be able to tune all variables to match observations, so validation of the control run is still a useful exercise. In later consideration of the control run simulation of energy transport in the models, we will compare the transports with observations, and between models.

Different climates can still be considered in the control run by looking temporally at the simulation of seasonal climate features, or spatially at the simulation of regional average climates in the model, and comparing with observations. The rationale for this approach is that a good seasonal model forced by time varying solar insolation may also be a good greenhouse forced model. Stone et al. (1977) were the first to test this approach on a 3-D GCM, using the GISS model of Somerville et al. (1974) (a model distinct from the current GISS model). July and January model simulations were compared with each other and with climatological data on seasonal changes. Stone et al. found that the model simulated accurately the "northward displacement of the mid-latitude jets, the low-latitude Hadley cells, the tropical rain belt, the trade winds, and the ITCZ in July compared to January, the reversal of the Indian monsoon, and the weakening of the zonal meridional circulations and the decline of eddy ectivity in the summer." Manabe and Stouffer (1980) also used a 3-D climate model to compare the regional pattern of seasonal temperature range from a seasonal solar forcing simulation with observed values. Their study employed a 68m deep passive thermodynamic mixed layer ocean, which allows tuning of the average seasonal temperature range (though observations suggest that this choice of depth is reasonable). Nonetheless, the geographic distribution of response in the model results from internal model processes and is not tuned. Manabe and Stouffer found "that with some exceptions, the model succeeds in reproducing the large scale characteristics of seasonal and geographical variation of the observed atmospheric temperature." Schneider (1984) points out that "it is necessary that a valid model reproduce seasonal climatic features, but that this may not be sufficient to engender much confidence that the model will also have a valid response to a different forcing, like CO_2 increase. Nevertheless, the GFDL results were encouraging on the 'natural experiment' of seasonal forcing and response, having successfully 'passed' this necessary conditions test."

Another useful test of a model's control climate is examining how the variability simulated by the model compares with the variability displayed by the observed climate. The ability to reproduce the observed variability in the control run lends confidence to a models' ability to provide a reasonable simulation of the variability associated with a changed climate in the perturbed run. Knowledge of climatic variability is important for determining climatic impacts on ecosystems, since it is often the climatic extremes that produce the most damage. Validation of climate variability has been carried out for the GISS model by Rind et al. (1989) for temperature and precipitation. They find that "the modeled variability is often larger than observed, especially in late summer, possibly due to the crude ground hydrology" in the model.

Validations of the control run with current climate and between models have been carried out considering most of the climatically relevant variables (temperature, pressure, precipitation, soil moisture, cloud cover, etc.). Some of the results of these comparisons will be presented in the next section as they relate to the NCAR, GFDL, and GISS models.

3.2 NCAR, GFDL, GISS model validations

Direct comparisons of the NCAR, GFDL, and GISS model control runs with observations have been made by the modelling groups in presentation of results. The papers relevant to the versions of the models considered in this work are: NCAR (Washington and Meehl, 1984), GFDL (Manabe and Wetherald, 1987), and GISS (Hansen et al., 1983). Without considering the results in detail here, Hansen et al. find that "it is verified that the major features of global climate can be realistically simulated with a resolution as coarse as 1000 km." Washington and Meehl find "good agreement between the present model simulation and many observed quantities." A particular deficiency of their model is that "sea ice forms equatorward of its observed position partly as a result of the lack of poleward heat transport by the ocean." On a broad scale, similarly reasonable agreement is obtained by the GFDL model. The UKMO model is not considered in this work, but Wilson and Mitchell (1987) conclude that "the model simulation of the observed climate with the present level of atmospheric CO_2 (323 parts per million by volume (ppmv)) is fairly good."

Moving from the broad scale down to the regional scale, as would be expected, model results do not compare as favourably with observations or one another. Grotch (1988) compares NCAR, GFDL, and GISS model results with observations of surface air temperature and precipitation and finds that "although the models often agree well when comparing seasonal or annual averages over large areas, substantial disagreements become apparent as the spatial extent is reduced, particularly when detailed regional distributions are examined." Wigley and Santer (1988) consider regional scale simulations from the GISS and other models. For pressures over Greenland and the seasonal cycles of the latitude and intensity of the Iceland low and Azores high, they find that model simulations are "generally poor."

Obtaining accuracy at the regional scale is important for at least two reasons. From the point of view of validation, errors in the smaller scales in the control run are important since the smaller scale processes could alter the energy budget in such a way as to influence the simulation of broad scale features in a perturbed run. Accurate simulation of the regional scales would also be very useful for climate impact studies and concomitant policy responses.

Schlesinger and Mitchell compare differences between the control run and doubled CO_2 run in the NCAR, GFDL, and GISS models relative to one another. They note global temperature increases of 3.5°C to 4.2°C for these models, and precipitation increases of 7.1 to 11%. For smaller scales, Schlesinger and Mitchell find that "the geographical distributions of the CO_2-induced warming obtained by the recent simulations agree qualitatively but not quantitatively. Furthermore, the precipitation and soil moisture changes do not agree quantitatively and even show qualitative differences." Kellogg and Zhao (1988) examine the soil moisture response to a doubling of CO_2 in the above three models plus those of OSU and UKMO. For North America, they note considerable differences in the results for soil moisture distribution changes, but are able to note areas of agreement also. The areas of agreement are "consistent with the results of studies of past warmer periods." These studies highlight the fact that surface moisture results in the GCM's are critically dependent on the specification of soil properties and the parameterization of hydrological processes in the models (Gates, 1989).

Gutowski et al. (1988) have examined surface energy fluxes for the control run and a doubled CO_2 run in the versions of the NCAR, GFDL, and GISS models considered in this work. They find that the surface energy balance is dominated by longwave radiation. Specifically, the upward and downward fluxes of longwave radiation are the largest components (though they act in opposite directions). For the global average control climate, individual surface fluxes agree to within $25W/m^2$, but this difference is a "large fraction of the net longwave radiation at the surface ($-60 W/m^2$), and it is as large as any of the global average seasonal changes in these fluxes". For CO_2 doubling, they find that intermodel discrepancies are only small fractions of the flux changes. They note that this apparent agreement should be viewed with caution "because flux changes associated with doubled CO_2 are about the same size as the differences between models in their control climate: $25 W/m^2$". For regional changes, intermodel surface flux discrepancies are up to twice the difference found for the global average, and often disagree over the sign of flux changes when CO_2 doubles. Gutowski et al. note that such differences appear to be closely asociated with differences in model hydrology (like the soil moisture results).

In summary, for GCM validations, differences in control climates are frequently related to differences in the schemes for parameterization of physical processes. For the three GCM's under consideration here, the major differences in the representation of different physical processes will be outlined in the chapter on models. In validation of GCM's in the perturbed run mode, model discrepancies are frequently related to differences in the control simulations of the respective models (and thus indirectly also to the different parameterization schemes employed).

GCM's simulate current climate much better on the larger scale than the regional scale, most obviously because of the limited resolution employed by the models. Simulation of the doubled CO_2 climate is also more reliable on larger scales for the same reason, and additionally (as noted by Mitchell et al., 1987) because "the regional response of climate models to small perturbations is shown to be highly dependent on the unperturbed simulation."

Most of the work on validating the 3-D climate models has been at the level of specific validations of one aspect or another of model performance. Overall assessments of models and their validity in regard to greenhouse problems are less common. U.S. National Research Council reports in 1975, 1979, and 1982 have addressed this question. Their conclusions based on earlier versions of the GFDL and GISS models and on simpler climate models show a general confidence in the ability of the models to simulate broader aspects of greenhouse change. NRC (1975) focused on the current climate simulation by the models, and note that "the several atmospheric and oceanic GCM's have now reached the point where reasonably accurate simulations of the global distribution of many important climatic elements are possible, and where their coupling into a single dynamical system is now feasible." NRC (1979) concluded that "the predictions of CO_2-induced climate changes made with the various models examined are basically consistent and mutually supporting. The differences in model results are relatively small and may be accounted for by differences in model characteristics and simplifying assumptions." NRC (1982) conclude that "comparisons of simulated time means of a number of climatic variables with observations show that modern climate models provide a reasonably satisfactory simulation of the present large-scale global climate and its seasonal changes", and that "model-derived estimates of globally averaged temperature changes, and perhaps changes averaged along latitude circles, appear to have some predictive reliability for a prescribed CO_2 perturbation." In the NRC reports, ultimate confidence in the general predictions from the 3-D models is based on the fact that 3-D models verify results from simpler models (radiative-convective and heat balance models) that can be understood in purely physical terms.

3.3 Energy transport validations

The poleward energy transport on earth receives contributions from the atmosphere, the ocean, and drifting sea ice (Bryan, 1982). In this work we are interested in examining the annual total meridional energy transport in the major climate models and observations. Validations of climate model energy transports have not been extensive to date.

Observational efforts aimed at calculating energy transports have used a variety of direct and indirect methods, and combinations of the two. Indirect methods can be used to calculate the total energy transport from radiative fluxes at the top of the atmosphere measured from satellite observations (e.g. Oort and Vonder Haar, 1976). This technique has also been employed by Carissimo et al. (1985), whose observational data is chosen for this work. Further discussion of this data will be given in the chapter on data. To calculate the contributions by atmosphere and ocean to the total transport, one component can be calculated directly from observations, and the other component inferred as a residual. Since the observational data for direct calculation of ocean transport is less reliable than for the atmosphere, Carissimo et al. calculate the ocean transport as a residual. Direct calculations of ocean transport have been made (e.g. Wunsch, 1980), but mostly for the Atlantic Ocean. Indirect methods to calculate the ocean transport have been based on surface heat flux considerations (e.g. Hastenrath, 1980).

Miller et al. (1983) have used essentially the indirect method to calculate ocean transport, but based on surface heat flux data generated by an atmospheric GCM. Miller et al. used a one year simulation of the GISS GCM to calculate the model ocean transports. For the annual global oceanic heat transport, their results compare reasonably well with the results of Oort and Vonder Haar (1976), Hastenrath (1980), and other studies, though there are considerable differences between observations, particularly in the Southern Hemisphere.

Covey (1988) compares atmospheric and oceanic energy transports from GCM simulations and observations. The GCM's discussed include the GISS model and others. However, the versions of the NCAR, and GFDL models discussed in the paper are not the same ones as considered here, since the versions we consider do not include ocean transports. Covey finds that the total energy transport (atmosphere plus ocean) appears to be approximately the same in the models and in satellite observations of irradiances at the top of the atmosphere. In addition, Covey finds that "in the models most of this transport takes place in the atmosphere whereas the combined satellite and radiosonde observations indicate that half or more of the transport takes place in the oceans." Previous satellite based studies have found that the oceans are more effective in transporting energy in low latitudes, while the atmosphere is perhaps more effective in middle and high latitudes (at least in the Northern Hemisphere). Covey may be a little too enthusiastic in donwplaying the role of ocean transports in the models, and overplaying the atmospheric transport. Some of the numbers cited in comparison between models and observations in Covey's paper are plainly incorrect, or the comparisons inappropriate.[30] We

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[30] In particular, p156, paragraph 2. The value given by Covey for the GISS model is the total energy transport, not the atmospheric transport as implied by the context and the comparison with the atmospheric transport in Carissimo et al. (1985). The same may also be true of the GFDL value quoted.

treat Covey's findings with some skepticism pending the results of further studies.

4 MODEL DESCRIPTIONS

4.1 Model hierarchies

The 3-D GCM's are most useful when used in conjunction with simpler models so that particular features or processes can be studied in isolation as well as in context. A hierarchy of models with dimensions from 0-D to 3-D has been developed to study climate. While the 3-D models include radiation, dynamics, surface processes, and resolution in time and space, the simpler models include only some of these processes explicitly, neglecting the others, or relying on their parameterization.

The simplest models are usually of the energy balance or radiative-convective type. The horizontally averaged (0-D) energy balance models (EBM's) calculate the surface temperature as a balance between incoming solar and outgoing infra-red radiation. Because of the simplicity with which vertical dependences are incorporated into EBM's it is relatively easy to generalize the models to allow for horizontal variations. This necessitates the parameterization of the horizontal redistribution of energy by atmospheric and oceanic transport. A 1-D latitudinally varying model is used in this work and will be described subsequently. North et al. (1981) have reviewed EBM's.

The 1-D radiative-convective (RC) models compute the vertical (usually globally averaged) temperature profile by explicit modelling of the radiative processes and a 'convective-adjustment' which re-establishes a predetermined lapse rate. The RC models have been reviewed by Ramanathan and Coakley (1978). Two-dimensional statistical dynamical models (SDM's) deal explicitly with surface processes and dynamics in a zonally averaged framework, and have a vertically resolved atmosphere. The SDM's have been reviewed by Saltzmann (1978). In 3-D GCM's, the 3-D nature of the climatic system (atmosphere, or oceans or both) is incorporated, and an attempt is made to represent most physical processes believed to be important. The NCAR, GFDL, and GISS 3-D models will be described subsequently.

4.2 One-dimensional energy balance climate model

One-dimensional energy balance models varying as a function of latitude have been useful for examining the coupling between latitudinal variation of temperature and temperature dependent albedo. In the latitude dependent models it is assumed that the rate at which heat enters each infinitesimal latitude belt during the year is exactly balanced by the loss rate. For the *i*th latitude strip

$$(\text{net horizontal transport out})_i + (\text{infrared out})_i = (\text{solar absorbed})_i \qquad (4.1)$$

We employ a 1-D EBM in this work to study the sensitivity of the climate to the energy transport. We will use the simple EBM formulation given by North (1975), which involves approximating the earth's temperature structure by the first two Legendre polynomials in a series expansion.

To set up the EBM it is necessary to assume that all energetic fluxes can be parameterized in terms of the temperature at the earth's surface. Although the characteristic temperature of the earth as a radiating body is determined by the temperature of the atmosphere far above the surface, this temperature can be empirically related to that at the surface. The flux of infrared radiation to space, I, is taken as the prime dependent variable, and is represented empirically by

$$I = A + BT \qquad (4.2)$$

where I is in W/m^2, T is the surface (sea level) temperature (°C) and $A=213.04W/m^2$ and $B=1.63Wm^{-2}{}^{\circ}C^{-1}$. The values of A and B are from Wang and Stone (1980). Latitudinal dependence is represented by x, where x is the sine of latitude. I is a function of x.

The absorbed solar heating is given by the form $(q/4)S(x)a(x,x_s)$, where q is the solar constant, S(x) is the mean annual meridional distribution of solar radiation (which is normalised so that its integral from 0 to 1 is unity), and the absorption coefficient $a(x,x_s)$ is one minus the albedo of the earth atmosphere system. The quantity x_s is the sine of the latitude of the ice or snow line. The function S(x), which is determined from astronomical calculations, is uniformly approximated within 2% by

$$S(x) = 1 + S_2P_2(x) \tag{4.3}$$

with S_2=-0.482, and $P_2(x)$ is the second Legendre polynomial, $(3x^2-1)/2$. We use a parameterization for the zonal earth-atmosphere albedo, α, from Wang and Stone (1980)

$$\alpha = \alpha_0+\alpha_2P_2(x)+\delta u(x-x_s) \quad \text{where } u(x-x_s) = \begin{cases} 1 & x>x_s \\ 0 & x<x_s \end{cases}, \tag{4.4}$$

α_0=0.316, α_2=0.146 and δ=0.186.

The horizontal transport of energy (meridional divergence of the flux) is given by the amount of heat per unit time leaving a latitudinal strip. The flux divergence is modelled by $-\nabla.(D\nabla T)$ where D is a thermal diffusion coefficient. D is calculated from the iceline condition for the current climate. Following Wang and Stone (1980) we set T_s=-13.0°C (temperature at the iceline) at x_s=0.95 for roughly the current value of the solar constant (1365W/m^2). This yields D=0.355. In terms of x, the energy balance equation for the unknown, I may then be written

$$-\frac{d}{dx}(1-x^2)D\frac{d}{dx}I(x) + I(x) = (q/4)S(x)a(x,x_s) \tag{4.5}$$

from which the transport can be derived. Equation 4.5 can be solved by expansion in Legendre polynomials, which have the advantage that they are the eigenfunctions of the spherical diffusion operator

$$\frac{d}{dx}(1-x^2)\frac{d}{dx}P_n(x) = -n(n+1)P_n(x) \tag{4.6}$$

For a mean annual model with symmetric hemispheres, the boundary condition that the gradient of I(x) must vanish at pole and equator yields I(x) in the form

$$I(x) = \sum_{n\ \mathrm{even}} I_n P_n(x) \tag{4.7}$$

Substituting 4.7 into 4.5 yields

$$[n(n+1)D+1]I_n = (q/4)H_n(x_s) \tag{4.8}$$

where

$$H_n(x_s) = (2n+1) \int_0^1 S(x)a(x,x_s)P_n(x)\ dx \tag{4.9}$$

For the two mode approximation, the solution of the temperature structure in equation 4.8 is reduced to the solution of three coupled algebraic equations

$$I_0 = (q/4)H_0(x_s) \tag{4.10}$$

$$6DI_2 = (q/4)H_2(x_s) - I_2 \tag{4.11}$$

$$I_s = I_0 + \frac{1}{2}I_2(3x_s^2-1) \tag{4.12}$$

The unknowns in these equations are the two coefficients of I in the truncated expansion, I_0 and I_2, and x_s. For the parameterizations we have chosen, H_0 is a third order polynomial, and H_2 is a fifth order polynomial as follows:

$$H_0(x_s) = [1/2\ \delta\ S_2]\ x_s^3 + [\ \delta - 1/2\ \delta\ S_2\ ]\ x_s - 1/5\ \alpha_2 S_2 + 1 - \alpha_0 - \delta \tag{4.13}$$

$$H_2(x_s) = [9/4\ \delta\ S_2]\ x_s^5 + [\ 5/2(\ \delta - \delta\ S_2)\ ]\ x_s^3 + [\ 5/4\ \delta\ (\ S_2 - 2)\ ]\ x_s - 2/7\ \alpha_2 S_2 + S_2\,(1 - \alpha_0 - \delta) - \alpha_2 \tag{4.14}$$

Stone (1978b) notes that 4.10 corresponds to the statement that the global absorption of solar radiation is balanced by the global emission of infrared radiation; 4.11 to the statement that the poleward diffusion of heat compensates the net radiative heating in low latitudes and the net radiative cooling in high latitudes; and 4.12 is the definition of the position of the edge of the polar ice cap. These equations can be combined into a single seventh-order polynomial equation for x_s

$$I_s = (q/4) \left[H_0(x_s) + \frac{H_2(x_s)(3x_s^2-1)}{2(6D+1)} \right] \tag{4.15}$$

This equation defines a function $x_s(q)$ which describes how the extent of the polar ice cap changes as the solar constant changes. The result for our parameterizations is illustrated in figure 7.1. The result is similar to that obtained by North (1975) with a different albedo parameterization. The result has been discussed extensively in the literature (e.g. North et al., 1981)

4.3 NCAR, GFDL, GISS models

In this section we will briefly outline similarities and differences between the versions of the NCAR, GFDL, and GISS models considered in this work. Original descriptions of the models are available for NCAR in Washington and Meehl (1984), GFDL in Manabe and Wetherald (1987) and GISS in Hansen et al. (1983, 1984). Major characteristics of the models have been outlined adequately by Schlesinger and Mitchell (1985, 1987) and Gutowski et al. (1988), and we shall follow their descriptions. Table 4.4 in Schlesinger and Mitchell (1985) provides a nice summary of much of the information that will be presented below.

All three atmospheric GCM's integrate the primitive equations in spherical coordinates for the entire globe, and all three represent the vertical derivative in these and other equations by finite differences with nine vertical sigma (pressure coordinate) layers between the surface and the top of the atmosphere. The sigma layers are unevenly spaced to give highest resolution near the surface and the tropopause. The GISS model represents the horizontal derivatives by finite differences with an 8° latitude by 10° longitude resolution. The NCAR and GFDL models employ the spectral method to evaluate the horizontal derivatives, but the advection terms are evaluated by the transform method on a 4.5° latitude by 7.5° longitude grid. In all three models the physics terms such as the diabatic heating are also computed on the grids of the models. The global distribution of land and ocean is realistic within the horizontal resolutions of the models, as is the earth's topography, albeit smoothed.

Each of the atmospheric GCM's is coupled to a mixed layer ocean. In the NCAR and GFDL models the depth of the mixed layer is constant, and there is no heat transport. In the GISS model, the depth of the mixed layer is prescribed to vary seasonally on the basis of observations, and the oceanic heat transport is prescribed geographically and seasonally. The oceanic heat transport used is deduced by requiring a match of the model's simulated control sea surface temperatures with observations (the so-called 'Q-flux' method).

The calculations of large-scale condensation and snow are essentially the same in all three models. The calculations of sea ice, soil moisture, and surface temperature are essentially the same in the NCAR and GFDL models and different in the GISS model. Fractional sea ice cover can occur within a grid box in the GISS model, but not in the NCAR and GFDL models. The GISS model has two layers within the ice, wheras the NCAR and GFDL models have a single layer. This is also the case for the number of soil layers used for the soil moisture and soil temperature calculations. The NCAR and GFDL models have a prescribed field capacity of 15cm everywhere, whereas the GISS model uses a geographical distribution with values that range from 2 to 65cm for both layers together.

The treatment of terrestrial and solar radiation is similar in all three models in that they include the principal absorbers and emitters, H_2O, CO_2 and O_3. The NCAR and GISS models also include absorption of solar radiation by O_2, and the GISS model also includes aerosols and solar absorption by NO_2, as well as the effects of N_2O and CH_4 in the longwave radiation calculation. The solar calculation essentially follows the method of Lacis and Hansen (1974) in all three GCM's, although different band models are used in the longwave in each GCM. The solar constant in the GISS and NCAR models is 1367 and 1370W/m^2 respectively, which is within the current observational uncertainty. The GFDL model uses a value of 1467W/m^2. The rationale for this choice of solar constant in the GFDL model is not known to us, however we will later investigate the implications of this choice for the simulation of energy transport in the current climate. The GISS model includes the diurnal and seasonal cycles of solar radiation, whereas only the seasonal cycle is included in the NCAR and GFDL models. In all models, the albedos of snow-free land and open ocean vary geographically, but do not change when the climate changes, except in the GISS model where the ocean albedo depends on the surface wind. Each model's surface albedo changes as snow and sea ice cover change. Snow and sea ice albedos are constants in the NCAR model, whereas these albedos depend on factors such as depth and underlying vegetation in the GFDL and GISS models.

Clouds form in the models as a result of either large-scale condensation or cumulus convection. The GISS model does not allow clouds at pressures less than 100mb. The NCAR model allows clouds to form in all layers except the lowest and two highest. The GFDL model allows clouds to form in all layers. Clouds in all three models are produced by diagnostic relations and are not advected, so that clouds can appear and disappear from one time step to the next. Clouds from large-scale condensation occur in each model when a relative humidity criterion is met. In both the GFDL and NCAR models, convective clouds apear when a relative humidity requirement is satisfied for grid box layers undergoing convective adjustment. The GISS convective cloud parameterization includes a convective mass flux dependence. Cloud optical depths, albedos and emissivities are functions of cloud location and the number of layers a cloud encompasses.

In summary, it appears that the physical parameterizations and numerics are similar in the NCAR and GFDL models, while the GISS model differs from the others in both these characteristics. The physics in the GISS model appears to be more realistic than that in the other models, having been developed *ab initio* for climate studies. One of the principal differences relevant to the transport validations considered here is the inclusion of ocean heat transport in the GISS model, but not in the other models. The horizontal resolution is coarser in the GISS model than in the other models however. Also important for energy transport considerations is the large value of the solar constant employed in the GFDL model (1467W/m^2 , which is 7% larger than the values chosen to be consistent with observations in the GISS and NCAR models). Only the GISS model includes the diurnal as well as the seasonal cycle. The other major difference between models is in the parameterization of moist convection.

5 DATA

5.1 Climate model output

The principal data sources used on this study are from 3-D GCMs and observations of the Earth's climate. The data used are primarily those relevant to calculating energy transports, and to parameterizing the 1-D EBM.

The climate model data[31] used in this study consists of output from the control run (1xCO_2) and doubled CO_2 runs in equilibrium experiments of the NCAR, GFDL, and GISS models described previously. Most of the model output has been supplied via Bill Gutowski and associates at Atmospheric and Environmental Research (AER) (AER Climate Group, 1987). The length of integration for each of the model experiments is as follows. GISS: 45 unaccelerated solar cycles for 1CO_2, 35 unaccelerated cycles for 2CO_2. GFDL: 49 unaccelerated solar cycles. NCAR: For the 1CO_2 simulation, 12 solar cycles with calendar accelerated by 9 (40.6 days per solar cycle), then 4 solar cycles with acceleration of 3 (121.7 days per solar cycle), finally 11 unaccelerated solar cycles. For the 2CO_2 simulation, only the 4 accelerated (3x) and 11 unaccelerated solar cycles.

All output from the models has been supplied as temporal averages obtained from the final years of an experiment's run. For NCAR, the averaging period for the output is the final 3 years, while for GFDL and GISS it is the final 10 years. Washington and Meehl (1984) have noted that the 2xCO_2 and 1xCO_2 simulations for the NCAR model had not reached quasi-equilibrium by the end of these experiments, because of the lack of computing resources available at the time. Washington and Meehl partly justify the early termination of their experiment by claiming that the 3-D model should approach equilibrium with a 50m ocean at the same rate as their experiments with a 1-D radiative-convective (RC) model coupled to a 50m ocean. Stone (personal communication) notes that this justification is invalid however, since the RC model has half the sensitivity of the GCM, and therefore only half

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[31] Since the use of the word 'data' to refer to output from computer models is considered inappropriate by some people (see discussion in Bryce, 1989), its use will be kept to a minimum here. Where possible, we will use the word 'output' instead.

the response time. Schlesinger and Mitchell (1987) note that the NCAR $1CO_2$ experiment was cooling at 0.4K per year, and the $2CO_2$ experiment was cooling at 0.21K per year. After the Washington and Meehl (1984) results were published, the $1CO_2$ and $2CO_2$ experiments were extended by 5 years and 2 years, respectively, and showed a smaller secular cooling trend in both experiments. The NCAR output supplied for this study is the same as that considered in the Washington and Meehl (1984) paper; that is, prior to the integration extension. The implications of the lack of equilibrium in the NCAR model will be considered in later analysis.

Model analysis here uses annual means, which were derived from the seasonal mean output supplied for NCAR and GISS and the monthly mean output supplied for GFDL.

All model output supplied by AER was formatted on the respective model grids. For the transport calculations we are only interested in the zonal means (average around a latitude circle), and so all the relevant flux data was converted to zonal mean form. The latitude spacing for the respective models is shown in table 5.1. The flux data included the net shortwave and longwave radiation at the top of the atmosphere and at the surface, as well as the surface fluxes of sensible and latent heat. For the 'indirect' method we use to calculate model energy transports, only the flux data at the top of the atmosphere is needed. The indirect method will be described in the chapter on energy transports. To test the validity of the indirect method, one would like to compare results with direct calculations of the energy transport from model derived motions and temperatures in the model atmosphere and ocean (if included). The relevant data to calculate the transport directly was not saved from the NCAR and GFDL model runs, nor was the transport calculated. However, the atmosphere and ocean energy transports were calculated and saved from the GISS model run. Reto Reudy of GISS supplied annual mean zonal mean energy transports from the GISS model control run to allow a comparison with the indirect method to be made.

Gridded model output of the surface air temperature, precipitation, and soil moisture was also supplied from each of the models by AER. The surface air temperature was used in conjunction with lapse rate data to calculate sea level temperatures for each of the models. The sea level temperatures were used in parameterizing 1-D EBMs to be 'equivalent' to the 3-D GCMs.

5.2 Observational energy transport data

Observational energy transport data for the current climate was taken from Carissimo et al. (1985). They estimated the poleward energy transports in the atmosphere-ocean system for the annual mean and four seasons based on satellite measurements of the net radiation at the top of the atmosphere, atmospheric transport data, and atmospheric and oceanic storage data. Estimation of the total (atmosphere plus ocean) energy transport was made using the indirect method from satellite observations of the net flux of radiation at the top of the atmosphere. Note that our model derived estimates for the total energy transport follow the same technique used to generate the observations. Carissimo et al. obtain the atmospheric transports from *in situ* observations and infer the oceanic transport as a residual. The Carissimo et al. data is zonally averaged with latitude spacing every 10° as shown in table 5.1.

Errors in the Carissimo et al. transport data occur principally due to uncertainties in the satellite measurements of radiative fluxes. Carissimo et al. apply various schemes to correct the radiative flux data to ensure global radiative balance. For the curves corrected by the different techniques, some spread exists at mid-latitudes, yielding an error estimate in the transports on the order of $0.5x10^{15}$W in mid-latitudes. To gain an estimate of the contribution by errors that cannot be corrected by requiring global energy balance, Carissimo et al. consider the difference between independent satellite estimates that are in global balance. For the case of the annual radiation budget, Carissimo et al. take as independent estimates the transport curves computed by Hastenrath (1982) for various satellite samples, but using the same systematic correction. The spread in the Hastenrath curves is comparable with the spread in the Carissimo et al. curves corrected for global imbalance. Therefore, for the Carissimo et al. data, the "total uncertainty in the transport of energy by the atmosphere and ocean amounts to about $1x10^{15}$W in mid-latitudes and somewhat less in the tropics and high latitudes."

5.3 Surface air temperature data

The surface air temperature data used to represent observations was taken from Sellers (1965). The data is averaged annually and zonally with latitude spacing every 10° as shown in table 5.1. The 1-D EBM employed later in this study uses sea level temperatures, not surface temperatures, and so the Sellers surface air temperature observations (like the model surface air temperatures) were converted to sea level temperatures using lapse rate data and topographic data as described below.

5.4 Sea level temperature derivation

We are interested in sea level air temperatures since these make more physical sense as a driver of meridional energy transports than do surface air temperatures. The 1-D EBMs are parameterized in terms of sea level air temperatures.

Sea level temperature is a rather arbitrary quantity in any location where the topography departs from sea level, since the choice of how to extrapolate back to sea level is somewhat arbitrary. For instance, one could use the moist or dry adiabiatic lapse rates, or standard atmosphere lapse rates, or observed lapse rates at similar latitudes, or other such measures of what the sea level temperature might be in the absence of the existing topography. Observed lapse rates are probably as reasonable a choice as any, and so we use observations of the lapse rate in the lower atmosphere (850-950mb) to extrapolate temperatures from surface air to sea level. The lapse rate data and topographic data is described below.

5.4.1 Lapse rate data

To calculate the observed lapse rate between 850 and 950 mb we used data on temperatures and geopotential heights from Oort (1983). The Oort data consists of zonal averages at 5° latitude spacing between the equator and 70°, with an additional observation at 80°. The Oort data is temporally averaged also, being an annual mean averaged over the years 1963 to 1973. Lapse rates (γ) were calculated as $\gamma = -dT/dz$, where T is temperature and z is geopotential height. The resultant 850mb - 950mb lapse rates are shown in figure 5.1. The lapse rate over 850-950mb varies quite a bit with latitude (justifying the additional work of using climatologically observed lapse rates rather than just picking a single value as representative of all latitudes), and is lowest in north polar regions, indicating the high degree of static stability there. A value for the lapse rate has not been calculated at 80° in the southern hemisphere (SH), since there is no 850mb or 950mb data there due to the presence of the Antarctic continent. In interpolating lapse rate values to model latitudes and to the Sellers temperature latitudes, for latitudes south of 70°S the value of the lapse rate at 70°S was used. For latitudes north of 80°N, the lapse rate was set to zero.

5.4.2 Topographic data

Topographic data for calculating sea level temperatures from the lapse rates and surface temperatures was available for each of the models. For the GISS model the topography was supplied by Reto Reudy on the GISS grid, and is plotted in figure 5.2. The major continental mountain ranges and Antarctica and Greenland are clearly discernable in the figure. For the spectral NCAR and GFDL models the equivalent gridded topography was supplied by Robert Black, and is shown in figure 5.3. Zonal average topography was calculated from the gridded data and is shown in figure 5.4. The curve labelled 'G' depicts the GISS zonal mean topography, and the curve labelled 'N' depicts the NCAR and GFDL zonal mean topography. Despite the differences in resolution between the GISS model and the other models, the zonal mean topography is relatively similar.

5.4.3 Sea level temperatures

Sea level temperatures were calculated from the lapse rate data, topographic data, and surface air temperatures via $T_{sealevel} = T_{surface} - \gamma z$. Figure 5.5 shows the derived annual sea level temperature for each of the models (plotted as dashed lines) and the corresponding surface temperatures (plotted as solid lines). The sea level temperatures depart most from the surface temperatures at latitudes corresponding to the presence of Antarctica, the Himalayas, and Greenland.

5.5 Satellite data

As part of parameterizing the 1-D EBM with observational data, it is necessary to have observations of the outgoing longwave flux of radiation at the top of the atmosphere, and of the albedo of the earth-atmosphere system. Such observations have been made from satellite data and tabulated in zonal mean form in Stephens et al. (1978). Note that the Stephens et al. data was also used as a source of data in producing estimates of the total meridional energy transport in Carissimo et al. (1985).

The Stephens et al. data was produced from a composite of 48 monthly mean radiation budget maps. We used annual mean values calculated in Stephens et al. from this data set. The latitude spacing of the annual mean zonally averaged Stephens et al. data is the same as that for the Sellers temperatures shown in table 5.1.

The satellites employed in the Stephens et al. study were all in sun-synchronous orbit sampling at near one local time during daylight hours. Stephens et al. discuss sources of error and uncertainties in the satellite data. To give an idea of the size of errors in the data Stephens et al. note that the annual global average outgoing longwave flux is $234\pm7\,W/m^2$, the albedo is 0.30 ± 0.01, and the net flux of radiation at the top of the atmosphere is $9\pm10\,W/m^2$.

5.6 Sea ice data

A value for the latitude of the sea ice limit, x_s, in each hemisphere is required in parameterizing the 1-D EBMs. The valus of x_s is incorporated into the representation of the albedo in the EBMs. When parameterizing the EBMs to the GCMs we estimated values for x_s from the graphical output for sea ice limits published by the model groups for the appropriate control runs (see figure 7.4). For NCAR the sea ice extent was plotted for DJF and JJA in Washington and Meehl (1984). We determined x_s from the summer map in each hemisphere, since the bulk of the insolation in polar regions is received during the summer months. For GFDL, February and August sea ice maps were supplied by Richard Wetherald, and the same procedure was followed. For GISS, x_s values were obtained from information given in Hansen et al. (1984).

For observations we used the sea ice maps compiled by Alexander and Mobley (1976) to determine the appropriate x_s values. Alexander and Mobley present monthly distributions of the main ice packs of the Arctic and Antarctic on a 1° global grid using data digitized from U.S. Fleet Weather Facility ice charts and Navy atlases. The sea ice limit was again estimated for each hemisphere on the basis of the summer month maps. In each case (models and observations), the estimate of x_s was taken as an 'eyeballed' zonal average from the global maps, and serves as an approximate estimate to within a couple of degrees latitude.

5.7 Journal literature and media accounts

In the chapter on presentation of theory and interpretation of the theory in the popular literature, we use conventional literature sources for data. On the presentation of greenhouse theory, journal literature is selected from the major climate related journals, or from well known journals publishing greenhouse theory articles: JGR, GRL, Science, Nature, etc.. For a data source on public interpretation of the greenhouse journal literature we have selected the news press (as opposed to television or other sources), because of it's impact and because it is documented, archived, and readily researched. The news press is quite vast and variable in coverage, so we adopt the New York Times as the major news source in order to obtain some sort of standard. We do not limit ourselves exclusively to the Times, but focus on their coverage since it is presumably among the most sophisticated of the presses in its discussion of environmental science issues.

TABLE 5.1

Latitudinal spacing of the zonal mean data in the NCAR, GFDL, and GISS models, in Carissimo et al. (1985) observations, and in Sellers (1965) observations (°N and °S).

NCAR GFDL	GISS	Carissimo et al.	Sellers
86.60	90.0	80	85
82.19	82.2	70	75
77.76	74.3	60	65
73.32	66.5	50	55
68.88	58.7	40	45
64.44	50.9	30	35
59.99	43.0	20	25
55.55	35.2	10	15
51.11	27.4	0	5
46.66	19.6		
42.22	11.7		
37.77	3.9		
33.33			
28.89			
24.44			
20.00			
15.55			
11.11			
6.67			
2.22			

6 VALIDATION OF MODEL FUNDAMENTALS : ENERGY TRANSPORT

6.1 Energy transport and its role in climate

The fundamental drive for atmospheric motions is differential solar heating and the resulting gradients of entropy and temperature (Stone, 1984). The 3-D fluxes of heat resulting from the differential solar heating can be viewed vertically and horizontally. In the vertical, radiation is preferentially absorbed at the surface so that large vertical temperature gradients would be produced by radiation acting in isolation. The resultant convection tends to reduce these gradients. In a similar way, the variations with latitude of the absorbed flux would lead to large horizontal temperature gradients if radiation acted in isolation. Again, fluid motion takes place that tends to reduce these gradients. The atmospheric and oceanic fluid motions transport energy from low latitudes to high latitudes. In the atmosphere, transient eddies, stationary eddies, and mean meridional circulations all make significant contributions to the energy transport, and the energy is transported in different forms, primarily as sensible heat, latent heat, and potential energy (Stone, 1984). In addition, the transport of sensible heat by ocean currents represents a substantial fraction of the total energy transport (Oort and Vonder Haar, 1976).

Since the fluid motions transport heat, they modify the horizontal temperature gradients and consequently play a fundamental role in determining the climate of the atmosphere-earth-ocean system. Similarly, the feedbacks between the dynamical heat transports and temperature structure play a crucial role in determining how sensitive the climate system is to external change (Stone, 1984), such as to increased greenhouse gas concentration. Held and Suarez (1974) speculated that the poleward transport of heat (by ocean currents) reduces the meridional temperature gradient, increases the latitudinal shift of the margin of snow covered area responding to a given change in surface temperature, and thus enhances climate sensitivity by increasing the contribution of the albedo feedback process. In the opposing sense, Spelman and Manabe (1984) note that we also expect oceanic heat transport to shift the margins of snow cover and sea ice poleward. This reduces the ice-albedo feedback and therefore the climate sensitivity.

In the following analysis, we will attempt to determine how well the NCAR, GFDL, and GISS models simulate the energy transport, and consider the implications of the results.

6.2 Procedure to calculate the energy transport in the models

The procedure to calculate the total energy transport in the models follows from the method outlined in Carissimo et al. (1985) using energy budget considerations. Energy balance requires that the difference at each latitude between the incoming solar radiation and outgoing infrared radiation at the top of the atmosphere be balanced by the sum of the divergence of energy that is transported across each latitude circle by fluid motion and the storage of energy in the system. The energy budget for a unit-width latitude band that includes both the atmosphere and oceans can be written as

$$S_A + S_O = F_{TA} - \frac{1\,\partial}{a\,\partial\theta}(T_A+T_O) \qquad (6.1)$$

where $S_A \equiv \frac{\partial E_A}{\partial t}$ is the rate of change of energy in the atmospheric part of the band, $S_O \equiv \frac{\partial E_O}{\partial t}$ is the rate of change of energy in the oceanic part, F_{TA} is the net downward flux of radiation at the top of the atmosphere (net shortwave plus net longwave), T_A and T_O are the transports of energy across a latitudinal wall in the atmosphere and oceans, respectively, a is the mean radius of the earth, and θ the latitude. In equation 6.1, the storage of energy in the land, snow and ice has been neglected (see Oort and Vonder Haar, 1976 for further discussion).

Integration of equation 6.1 over a polar cap yields

$$T_T(\theta,\phi) \equiv T_A(\theta,\phi) + T_O(\theta,\phi) = -\langle F_{TA}\rangle + \langle S_A\rangle + \langle S_O\rangle \qquad (6.2)$$

where $\langle(\;)\rangle = -\int_{\theta}^{\pi/2}\int_{0}^{2\pi} (\;)\, a\cos\theta'\, d\phi\, a\, d\theta'$, T_T is the total meridional energy transport (the T subscript may sometimes be dropped), and ϕ is longitude. The integration extends around a latitude circle ($a\cos\theta\, d\phi$) and over latitude ($a\,d\theta$).

For the present analysis we calculate only the annual transports. In the annual case, equation 6.2 can be simplified (Carissimo et al., 1985) by assuming that S_A and S_O are zero when taken over a 'normal' year or, in other words, that interannual variability can be neglected. In Carissimo et al. observations this is probably a reasonable assumption because the observed radiative fluxes and other energy parameters represent composites of measurements made over several years. Similarly in the models, the annual mean radiative fluxes represent averages over 10 years (GFDL and GISS) and 3 years (NCAR), so the assumption is again made. This reduces the expression for the total meridional energy transport to

$$\bar{T}_T(\theta) = -\int_{\theta}^{\pi/2}\int_{0}^{2\pi} F_{TA}(\theta',\phi)\; a\cos\theta'\, d\phi\; a d\theta'$$

or $$\bar{T}_T(\theta) = -2\pi a^2 \int_{\theta}^{\pi/2} F_{TA}(\theta') \cos\theta'\, d\theta' \qquad (6.3)$$

where the overbar denotes time average over a year.

Integration of equation 6.3 from pole to pole would result in zero if the net radiation at the top of the atmosphere was in balance (in Carissimo et al. observations, or in the model simulated radiative fluxes). In practice this is not completely true in either case, and a residual results reflecting the degree of imbalance in determination of the annual net radiation at the top of the atmosphere.[32] For each of the models we have integrated F_{TA} from pole to pole to gain an indication of the imbalance. In the GISS model, Hansen et al., 1984 note that there is a global annual net flux into the top of the atmosphere of $7.5 W/m^2$. Of this imbalance $5 W/m^2$ goes into the surface where it is absorbed by the specified ocean (which is an infinite sink of heat) and the rest is lost in the atmosphere through conversion to kinetic energy and computer

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[32] Actually, there are several sources of imbalance in the annual net radiation at the top of the atmosphere. With regard to observations, there are errors in measurement. In models, there are errors in calculation. In addition, there are interannual variations that result in imbalances from year to year, which presumably smooth out to balance on average over a number of years if the climate is in radiative equilibrium. The climate is not in radiative equilibrium however. It is being perturbed by the long term build up of greenhouse gases among other things. Even over longer period averages we would not necessarily expect annual radiative balance (though for most external forcings the radiative perturbation from equilibrium is small relative to the net fluxes of shortwave and longwave radiation).

truncation. In the GISS control run experiment the solar radiation absorbed at the ocean surface is multiplied by 0.96 in order to cancel the energy imbalance. The imbalance is equivalent to 0.04 times the solar radiation at the surface, so we subtract this amount from F_{TA} in the GISS transport calculations. In the NCAR model no attempt was made to correct radiative imbalances in the annual mean. The GFDL model is in balance. Note however that the GFDL model uses an incorrect value of the solar constant, and is unlikely to be in balance with the correct value of the solar constant (if the solar constant alone was changed). That the GFDL model is in balance with a solar constant 7% too large suggests the presence of other compensating errors or tuning.

To correct for the energy imbalance in determining the model simulated transport we have followed the 'constant correction' method principally adopted in Carissimo et al. (1985). In this method, if $T_1 = T_{NP}(-\frac{\pi}{2})$ represents the accumulated total northward transport $T_A + T_O$ at the south pole (from an integration beginning at the north pole as denoted by the NP subscript), the flux correction per unit area is $\frac{T_1}{(4\pi a^2)}$. The corrected transport at latitude θ is given by

$$T(\theta) = T^*_{NP}(\theta) - \int_{\theta}^{\pi/2} \frac{T_1}{(4\pi a^2)} 2\pi a^2 \cos\theta \, d\theta$$

$$\text{or} \quad T(\theta) = T^*_{NP}(\theta) - T_1(1-\sin\theta) \qquad (6.4)$$

where the star denotes uncorrected estimates.

The integration of equation 6.4 was carried out numerically. For the GISS model we employed the expended trapezoidal numerical integration method where the transport at latitude j is related to that at latitude j-1 by

$$\bar{T}_j = \bar{T}_{j-1} - 2\pi a^2[F_{TA}(\theta_j)\cos\theta_j + F_{TA}(\theta_{j-1})\cos\theta_{j-1}]/2 \qquad (6.5)$$

The NCAR and GFDL models do not have grid points at 90°, and so it is was more convenient to use a midpoint integration scheme for these models. For the midpoint scheme the transport at latitude j is related to that at latitude j-1 by

$$\bar{T}_j = \bar{T}_{j-1} - 2\pi a^2 F_{TA}(\theta_j)\cos\theta_j \, (y_j - y_{j-1}) \qquad (6.6)$$

where the y's are the midpoints (in radians) of the NCAR and GFDL latitudes.

The results of the transport calculations will be presented in a following section, but first the validity of the indirect method will be explored.

6.3 Comparison of direct and indirect transport calculation method

The method used to calculate the total meridional transport of energy in the models from the radiative balance at the top of the atmosphere has been labelled the 'indirect method'. A direct approach would entail calculating the energy transports directly from the model fluid motions in the atmosphere and ocean. Model output to calculate the transports directly was not saved or not available from the NCAR and GFDL models. This data was however provided by Reto Ruedy in zonal mean form for the GISS model. This enables at least one test to be made of how well the indirectly calculated total energy transport compares with that calculated directly.

The energy transport output supplied by Ruedy includes the annual northward global ocean energy transport and the annual northward energy transport in the atmosphere. The total atmospheric energy transport was calculated as the sum of the northward transports of sensible heat, geopotential energy, and latent heat in the model. The total energy transport was calculated as the sum of the atmospheric and oceanic transport.

The GISS direct transports have been plotted against the Carissimo et al. observations for the atmosphere, ocean and total energy transport in figure 6.1. The figure shows that the total energy transport agrees reasonably well between observations and the model, while the model transports relatively more energy in the atmosphere than the oceans than in observations (as noted by Covey, 1988). The ocean transport in the model is particularly small relative to the Carissimo et al. observations in middle latitudes in the Southern

Hemisphere. Miller et al. (1983) also find that the GISS model yields smaller transports between 40°S and 60°S than observations. The discrepancy may be partly related to observational errors as well as to model shortcomings. For observations, the radiosonde data used to derive atmospheric transports in the Southern Hemisphere is sparse and the atmospheric transports are tentative as a result. Since the ocean transports are inferred as a residual, any error in the atmospheric transports would affect the ocean transports as well.

In a test of the indirect method, the GISS indirect total energy transport calculated from equation 6.3 was plotted against the GISS directly calculated total energy transports as shown in figure 6.2. In general, the match of the magnitude and distribution of the flux calculated by the two methods is pretty good, and gives us confidence that the indirect method is useful and reasonably accurate. The match is not perfect however, and we note that the indirect method slightly overestimates the peak of the transport in the Northern Hemisphere. This is possibly a result of the fact that the numbers for the net radiation at the top of the atmosphere in the GISS model still do not yield a global annual balance. In any case, the spread between the curves gives an indication of the error in using the indirect method relative to the direct method. The error related to use of the indirect method is a few tenths of a terraWatt at the peak of the transport curve, and very small elsewhere.

6.4 Results

6.4.1 Annual radiative fluxes at the top of the atmosphere

The annually averaged vertical fluxes for each of the models (obtained from AER Climate Group, 1987) are plotted in figures 6.3.1 to 6.3.6. The figures show the major vertical fluxes of energy at the surface (sensible heat, latent heat, longwave, and shortwave) and top of the atmosphere (longwave and shortwave) for the $1xCO_2$ and $2xCO_2$ equilibrium runs in the models. Gutowski et al. (1988) have already analysed the model surface fluxes in detail. For calculation of the annual meridional energy transport we are interested in only the vertical fluxes at the top of the atmosphere (figs. 6.3.1 and 6.3.2). Clearly, there are differences between the model simulated vertical fluxes, and so we might begin to suspect that there will be differences in their energy transports (as outlined in the next section). We reserve discussion on why the models differ until after we have considered the transports.

One feature which can be noted from figures 6.3.1 - 6.3.6 is that the differences between the different model fluxes are greater than the differences between the $1CO_2$ flux and $2CO_2$ flux for each model. That is, the change in fluxes induced by the CO_2 perturbation for each model is smaller than the differences in flux between the models. Does this mean that the game is over for successful greenhouse change modelling? Not necessarily. A trivial explanation could be that one of the models might be right and the others wrong. This explanation is not required at all however, since there is a far more plausible view that can be taken. If the mean state in each of the models is off by a little bit (yielding different values for the fluxes), that does not necessarily mean that the predictions are off for doubled CO_2. If the models are linearized about a different mean state, they would have different coefficients for the equations describing the physics (but only slightly different), and could still yield consistent results for the predicted changes. Whether this is so or not depends on how nonlinear the system is. If the nonlinearity is strong, small differences or perturbations might lead to large differences between predicted states (though it depends on the time scale and the phenomenon of interest). Manabe and Stouffer (1988) demonstrate this sort of behaviour in a coupled atmosphere-ocean climate model. In an initial run with the model the ocean surface fluxes were close to observations, but a thermohaline circulation did not develop in the North Atlantic ocean and the simulated SST in the region was significantly lower in northern latitudes than the observed. In another model run with the surface ocean fluxes changed a little, a thermohaline circulation developed in the North Atlantic ocean, and the simulated SSTs were closer to observations. The question of how important, and what role the nonlinearities play is still an open one. On the basis of the Manabe and Stouffer results at least, we have some grounds to be suspicious of the consequences of the differences between the model simulated vertical fluxes noted above. On the other hand, we do not have sufficient grounding to dismiss the model predictions on the basis of the differences.

In calculating the energy transports we use the annual net radiative flux (shortwave plus longwave) at the top of the atmosphere (as represented in eqn. 6.3). The net flux at the top of the atmosphere is shown in figure 6.3.7 for each of the models and for observations. The observations are from Stephens et al. (1981), and are represented by the dashed line. At most latitudes the observations lie between the ranges displayed by the three GCM net flux curves. For all curves the broad pattern shows a net downward flux at low latitudes and a net upward flux in high latitudes. This pattern yields poleward energy transports as we shall see in the following section.

6.4.2 Energy transports

The annual northward total energy transports calculated for the control run for each model from equation 6.3 are shown as a function of latitude in figures 6.4.1 to 6.4.4. In figures 6.4.1 to 6.4.3, three curves are shown for each of the NCAR, GFDL, and GISS energy transports. The curves labelled 'S' and 'N' are for integrations beginning from the south and north poles respectively. The curve labelled 'C' is the corrected transport calculated from equation 6.4. The curve labelled 'O' is Carissimo et al. (1985) observations.

For the GFDL model, the difference between curves N, S, and C is negligible, indicating that the model is in annual radiative balance.[33] For the GISS model there is a small residual. This might be expected since there is no *a priori* reason why the constant correction technique used to balance the GISS model at the surface should yield a successful energy balance at the top of the atmosphere.

In the NCAR model there is a large residual at the poles. This is not unexpected, since the NCAR model control run is known not to have reached a quasi equilibrium. Schlesinger and Mitchell (1987) note that the NCAR model globally averaged surface air temperature was cooling at a rate of 0.4K per year in the control run. Using the NCAR transport residual it is possible to estimate an implied drift for the NCAR model to compare with the actual drift. We perform this calculation crudely below.

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[33] It also serves as a check on the implementation of the integration scheme used to calculate the transport, since it would be quite a coincidence for the scheme to be in error and still yield a perfect balance for one of the models.

The NCAR energy transport residual is about 5×10^{15}W. The earth's surface area is 5.1×10^{14}m^2, so the imbalance is 9.8W/m^2. The model drift is given by this number divided by the total heat capacity of the model. The NCAR model has a 50m deep ocean, land surfaces, and atmosphere. We will approximate the area of the earth's surface covered by the NCAR ocean as 70%, and we will neglect the heat capacity of the land relative to the ocean and atmosphere since it is small. Approximating the ocean as water[34] with a specific heat at 1 atm. pressure and 0°C ($C_p=4217\frac{J}{kgK}$) and density 1000kg/m^3, the ocean heat capacity is

$$0.7 \text{ x } 4217\frac{J}{kgK} \text{ x } 50m \text{ x } 1000kg/m^3 = 1.49\text{x}10^8\frac{J}{m^2K}$$

Approximating the atmosphere as dry air at constant pressure with specific heat $C_p=1004\frac{J}{kgK}$, density 1.225kg/m^3, and depth 8km, the atmosphere heat capacity is

$$1004\frac{J}{kgK} \text{ x } 8000m \text{ x } 1.225 \text{ kg/m}^3 = 9.8\text{x}10^6\frac{J}{m^2K}$$

The total model heat capacity is approximated as the sum of the ocean and atmosphere heat capacity, and is thus $1.6\times10^8\frac{J}{m^2K}$. Dividing this value into the energy imbalance yields a drift of 6.2×10^{-8}K/s, or approximately 2K per year (cooling).

If the model was perfectly energetically consistent and the above approximations were reasonable, then the drift calculated above should be the same as the figure quoted by Schlesinger and Mitchell (0.4K). The drift implied by the energy imbalance at the top of the atmosphere does not match the actual drift in the model; it is larger. This suggests that the energy imbalance in the NCAR model is partially compensated by something else; possibly the numerics.

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[34] A single heat capacity is used for the ocean in liquid form, and no attempt is made to include variations for sea ice or for the heat capacity of the polar ice sheets.

Because the transport residual is so large in the NCAR model, the corrected value is subject to greater uncertainty than for the other models. This should be born in mind in subsequent discussion referring to the corrected model transports.

Figure 6.4.4 shows the corrected model energy transports plotted against one another and against the Carissimo et al. observations. The figure shows differences between the different model transports, particularly at the peaks in the transport curve. The differences at the transport peak in the NH between the models are large compared to the error inferred in the technique for calculating the transport from figure 6.2. Only the NCAR transport curve is unreliable due to the global energy imbalance in that model. While the differences between models can be assumed to be real, the differences between the models and observations are not as reliable due to the uncertainty in observations at mid-latitudes of about 1×10^{15}W. The difference between models and observations is of this order or greater, so the differences between the models and observations are probably of marginal significance or better. The GISS model shows a relatively larger total energy transport than observations in the Northern Hemisphere, while the NCAR and GFDL models show relatively less transport than observations. The result is not completely symmetric in the Southern Hemisphere however. At the transport peak in the SH the GISS and NCAR curves are close to observations. The GISS transport is a little greater than observations, though not by more than the uncertainty in the observations. The NCAR SH transport peak is even less significantly different from observations due to the additional uncertainty in the NCAR curve. The GFDL SH transport peak is smaller than observations by a margin greater than the uncertainty in the observations.

Returning to discussion of the more significant NH transport differences, we note that the GISS model total energy transport includes model contributions from the atmosphere and ocean. The ocean transports follow from the prescription of SSTs using the 'Q flux' method. The NCAR and GFDL models do not include ocean transports, so all the energy transport must take place in the atmosphere. It is likely that the atmosphere does not pick up all the slack caused by the omission of ocean transport (Stone, 1984), leading to the smaller NCAR and GFDL transports. The implications of this will be discussed in the next chapter. For now we note that the NCAR model control run simulates overextensive sea ice relative to observations (Washington and Meehl, 1984). They attribute this as being most likely due to the lack of ocean heat transport in the model. In the GISS model, the annual mean sea ice cover is under extensive relative to observations. There is 15% less sea ice in the model than in observations (Hansen et al., 1984). This is consistent with the energy transport calculations in which the GISS energy transport is too strong relative to observations. In the GFDL model the simulated sea ice is overextensive relative to observations in the Northern Hemisphere winter, but slightly underextensive in both hemispheres at other times of the year (Wetherald, personal communication).[35] It is difficult to draw solid conclusions about whether these small differences are consistent with the GFDL energy transport curves, since the energy transport and sea ice extent are influenced by other processes as well. This will be elucidated in the following chapter when analysing the EBMs.

In terms of model validation, our major concern from figure 6.4.4 is whether the differences between the model simulated energy transports are important; if so why, and with what implications. The next chapter will be devoted to exploring this issue by considering how sensitive the climate system is to the energy transports.

6.4.3 Atmospheric energy transports

Before proceeding to the next chapter, we present here the atmospheric component of the energy transports. The results will be discussed in relation to the temperature structure displayed in the models to determine whether the errors in each quantity appear to be consistent with one another.

The atmospheric energy transports for models and observations are shown in figure 6.5. Observations are represented by the dashed curve, and are from Carissimo et al. (1985). For the NCAR (labelled N) and GFDL (labelled P) models, the atmospheric energy transports are the same as the total energy

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[35] The sea ice simulation in the GFDL model run corresponding to this data has not been described in any published documents, and these observations were taken from model sea ice maps supplied by Richard Wetherald.

transports calculated using the indirect method, since these models have no ocean energy transport. For the GISS model (labelled G), the atmospheric energy transports are from direct calculations from the model atmospheric motions as supplied by Reto Reudy.

Figure 6.5 shows that the model atmospheric transports are similar to one another, but much larger than observations. In low latitudes the GISS atmospheric energy transport is less than the NCAR and GFDL transports, consistent with the GISS model containing ocean transports (which are relatively more important in low latitudes), which are not represented in the other models. One might expect that the intensity of the Hadley cell in the NCAR and GFDL models would be stronger than that observed in the real atmosphere to make up for the omission of ocean transports in low latitudes. We have not been able to find published references to the strength of the Hadley cell circulation in the control run for either of these models to verify this however. Information on mean meridional circulation simulation is available for the GISS model (Stone, personal communication). The Hadley cell in the GISS model is approximately correct, being some 10-20% too weak. The atmospheric energy transport in low latitudes in the NCAR and GFDL models is between 15 and 50% greater than in the GISS model, suggesting that their Hadley cell circulations are too strong.

In midlatitudes the GISS simulated ferrel cell is too weak, being less than half of its observed strength. Starr et al. (1970) have published estimates for observations of Ferrel cell strength using a variety of techniques. The GISS simulated Ferrel cell is weak relative to the Starr et al. estimates. The Ferrel cell is a thermodynamically indirect reverse cell, and so an underestimate of the Ferrel cell will contribute to an overestimation of the poleward atmospheric energy transport. In the GISS model it is believed that the Ferrel cell is underestimated because simulated eddy momentum transports are in error relative to observations (Stone, personal communication). Covey (1988) has impugned the observations, and suggested that the model atmospheric transports are correct instead. The differences between model atmospheric transports and observations af the atmospheric transport are quite large however (between 50 and 80%), and it is unlikely that the observations could be off by this much. In the absence of good reasons why the observations should be systematically too low by such a large amount, we continue to believe the observations.

If the atmospheric energy transports are too strong relative to observations in the GISS model, then they are also too strong in the NCAR and GFDL models, which have similar values for the peak atmospheric energy transport. The peak of the atmospheric transports might be expected to be similar even though the NCAR and GFDL models do not contain ocean transport, since, where the total energy transport peaks, the atmospheric energy transport peaks too. Considering now figures 6.4.4 and 6.5 together, the atmospheric energy transports in the models are too large relative to observations; and the total energy transports, while not matching observations, are relatively much closer to observations. That the model total transports are approximately correct would suggest that their simulated meridional temerature gradients are about right. If this is so, then the atmospheric components of the models are being too efficient (relative to the real atmosphere) at transporting energy polewards. We can investigate this by considering the meridional temperature structure simulated by the models.

6.4.4 Meridional temperature structure

In our first attempt to describe the meridional temperature structure in the models we fitted the model zonally averaged temperatures to an equation of the form $T(x)=T_0+T_2P_2(x)$, where x is the sine of latitude, and P_2 is the second Legendre polynomial. We chose this form to match the form of the EBM temperature representation. In this representation, T_0 is the planetary average temperature, and T_2 gives an indication of the equator to pole temperature gradient. This approach was not very fruitful however, as it turned out that T_2 did not give a very good measure of the forcing of the energy transport flux. The probable reason for this is that T_2 is influenced by temperatures in high latitudes, whereas the transport flux maximum peaks near about 40°, and is not much influenced by high latitude temperatures.

Stone and Miller (1980) have shown that the total energy transport is well correlated with the temperature gradient at 1000mb between 25°N and 55°N. Following this approach, we take the temperature gradient ΔT, as the sea level temperature difference between latitudes 25° and 55° in the models. We also calculated ΔT as the surface temperature difference between these latitudes, and the results were quantitatively similar. The results for the sea level ΔT's are shown in table 6.1.

For the NH, the ΔT for each of the models is quite close to its value for observations. The observations are represented here by the sea level temperature derived from the Sellers (1965) surface temperature observations. Agreement on ΔT's is to be expected if the total energy transports are reasonably correct, because the total transport determines ΔT. In the SH, the GISS model ΔT is close to observations, while the NCAR ΔT is too large, and the GFDL ΔT is too small. The GFDL ΔT is small because the model simulates a relatively high zonal mean temperature at 55°S (6°C too warm). This latitude in the SH is mostly ocean, with few observing stations, and so observations themselves might be questionable there. Nonetheless, in the GFDL SH a weak ΔT is not consistent with an atmospheric transport stronger than observations as in figure 6.5. Similarly, a ΔT close to observations as in the GISS SH is not consistent with an atmospheric transport stronger than observations. The same also applies to the models in the NH, where the ΔT's are close to observations, yet the atmospheric transports are much larger than observations. From this we conclude that the atmospheric transports are too efficient in the models. To highlight or resolve some of the apparent inconsistencies between the models temperature structure and energy transports, we need to consider their energy balance in more detail. To this end we will parameterize 1-D EBMs from the output of the 3-D GCMs to analyse the energy balance implications of the errors in the GCMs. This approach is taken up in the next chapter.

TABLE 6.1

Sea level temperatures near 55° and 25° in each hemisphere derived from GCMs and Sellers (1965) observations. ΔT is the temperature difference per degree of latitude over this range of latitudes. The actual latitudes were chosen on the model grids as close as possible to the above nominated latitudes. They are: NCAR and GFDL (55.55° and 24.44°), GISS (58.7° and 27.4°), and Sellers (55° and 25°).

	T at 55°S (K)	T at 25°S (K)	ΔT (K)	T at 25°N (K)	T at 55°N (K)	ΔT (K)
NCAR	272.3	295.6	0.75	296.4	275.7	0.67
GFDL	280.2	295.6	0.50	296.6	274.9	0.70
GISS	274.3	293.9	0.63	295.6	275.1	0.66
SELL	274.4	292.6	0.61	295.0	274.8	0.67

7 MODEL CLIMATE SENSITIVITY

7.1 Introduction

In the previous chapters we noted various similarities and differences between construction of the three GCMs considered here, and between their simulated outputs. For instance, the model energy transports are different from one another (though not radically different from observations), and it is interesting to ask whether the differences are important or not. Of primary concern is whether the differences in model energy transport could yield different climate sensitivities. The term 'climate sensitivity' refers to how much the climate changes (as measured by say its global average temperature) in response to a perturbation or change in forcing (such as due to changes in incoming solar radiation or due to changes in concentration of greenhouse gases in the atmosphere, etc.).

For the doubled CO_2 experiments the three GCMs display similar sensitivities. Why should this be so when the models have obvious differences in the way they are formulated, and in the way they model certain processes? We know that their energy transports are different, and will show that the climate is sensitive to the energy transport. To test this sensitivity, we will parameterize a 1-D EBM with output from each of the three GCMs. This yields 'equivalent' EBMs, meaning that each EBM is set up to be an equivalent 1-D energy balance representation of the 3-D GCM. We hope that the essential physics relevant to energy balance contained in each GCM will be represented in its 1-D equivalent. If the three GCMs really do have similar sensitivities, then their equivalent 1-D EBM representations also ought to have similar sensitivities (assuming that the essential physics has been captured). If the sensitivities turn out to be different, then the fact that the models have approximately the right control climate, means this must be so for the wrong reasons. If the sensitivities turn out to be similar, then there must be compensating factors or processes at work (to account for the obvious differences between the models, some of which, we know, affect the sensitivity), and we ought to try to identify them.

7.2 Climate sensitivity to the energy transport

To illustrate how the climate is sensitive to the energy transport, we will follow the approach used in Stone (1978). Stone used an EBM to show that the energy transport flux is insensitive to the climate (except when the dynamical transport efficiency is low and the climate system is near local radiative equilibrium). The converse of this result is that the climate is very sensitive to the transport flux. We will rederive Stone's result using the same 'Northtype' two mode truncated EBM (as described earlier), but with albedo parameterization and temperature-longwave parameterization from Wang and Stone (1980). The rederivation will also serve as a further introduction to the EBM which will be parameterized from model output and observations in following sections.

Equation 4.15 in the EBM derivation defines the function $x_s(q)$, which describes how the extent of the polar icecap changes as the solar constant changes. The iceline latitude is plotted against the ratio of the solar constant to the current solar constant (taken to be 1365W/m^2) in figure 7.1. Where the solar constant has its current value, the iceline is at its NH value of $x_s = 0.95$ (72°N). A climate sensitivity for the EBM is readily found by changing the solar constant by a known amount and calculating the planetary average temperature, T_0, corresponding to that solar constant. The climate sensitivity, β, can be defined by $\beta = q\, \partial T_0 / \partial q$. In the 3-D GCMs, the climate sensitivity experiments were performed not by changing the solar constant, but by doubling the amount of CO_2 in the model atmosphere. Greenhouse gases are not included explicitly in the 1-D EBM, so we cannot perform the exactly analagous CO_2 doubling sensitivity test. We can however roughly approximate this sensitivity test by simply increasing the solar constant by 2% in the EBM, since the radiative forcings for each experiment are of similar magnitude. In experiments with the GISS GCM and a 1-D radiative-convective model, Hansen et al. (1984) note that a 2% solar constant change corresponds to a forcing of 4.8W/m^2. For a CO_2 doubling, the global mean radiative forcing is about 4W/m^2 (Hansen et al. 1981). The small heat capacity of the atmosphere means that the similar radiative forcings for CO_2 change and solar constant change lead to similar global mean heat fluxes into the planetary surface in the GISS experiments. The resultant equilibrium global mean warming of the surface air is about 4°C in both the solar constant and CO_2 GISS sensitivity experiments. In later sensitivity experiments with the equivalent EBMs we will use a 2% solar constant change as a proxy for a CO_2 doubling sensitivity test.

The meridional energy transport flux in the EBM is modelled by a simple diffusion law:

$$F = -2 \pi R^2 (1-x^2) D \, dI/dx$$

where R is the radius of the earth (we use a mean earth radius of 6.367×10^6m). From equation 4.7, $I(x) = I_0 + I_2 P_2(x)$. Substituting for I(x) and differentiating yields:

$$F = 6 D I_2 \pi R^2 (x^3-x) \qquad 7.1$$

The transport flux is plotted as a function of x in figure 7.2. The flux maximum occurs where $x=1/\sqrt{3}$ (≈35°), and has a value of about 5.5 terraWatts. This is very close to the value of the flux maximum in the NH in the Carissimo et al. observations, but this is not too surprising. In demonstrating the insensitivity of the flux to internal parameters, Stone (1978) notes that the "ability of a climate model to reproduce the observed flux is not a good test of the model's reliability."

To reproduce Stone's result with our parameterizations, we first calculated x_s as a function of D for D values between 0 and 1[36] by solving the seventh order polynomial for x_s represented by equation 4.15. The fully expanded polynomial is:

$$\begin{aligned}
&\{(12D+2)^{-1}\ 27/4\ \delta S_2\}\ x_s^7 + \\
&\{(12D+2)^{-1}\ (15/2\ \delta - 21/4\ \delta S_2\}\ x_s^5 + \\
&\{\ \delta S_2 [1/2 + 25/4\ (12D+2)^{-1}\ ] - 10\ \delta (12D+2)^{-1}\}\ x_s^3 + \\
&\{3(12D+2)^{-1}\ [S_2(1-\alpha_0-\delta) - 2/7\ \alpha_2 S_2 - \alpha_2]\}\ x_s^2 + \\
&\{\ \delta [1 + 5/2\ (12D+2)^{-1}] - \delta S_2 (1/2 + 5/4\ (12D+2)^{-1}]\ \}\ x_s + \\
&\{\ 1 - 1/5\ \alpha_2 S_2 - \alpha_0 - \delta - (12D+2)^{-1} [\ S_2(1-\alpha_0-\delta) - 2/7\ \alpha_2 S_2 - \alpha_2] - 4\ I_s/q\ \} = 0
\end{aligned}$$

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[36] D is a measure of the efficiency of the dynamical fluxes in transporting energy from low to high latitudes. When D=0, the system is in local radiative equilibrium, and as D increases the system deviates more and more from local radiative equilibrium.

The resultant x_s curve is shown in figure 7.3.3. The iceline latitude recedes polewards with increasing efficiency of the dynamical transports (D) until D reaches about 0.39. At this point the transports are so efficient that the ice has been eliminated, and so further increases in D are irrelevant as far as the iceline is concerned.

Figure 7.3.2 shows I_2 as a function of D, where I_2 has been calculated from eqn. 4.11. The flux maximum (F_{max}) can now be calculated as a function of D from eqn. 7.1. The result, shown in figure 7.3.1 is quantitatively similar to Stone's result (his figure 4). F_{max} is insensitive to the dynamical transport efficiency for D greater than about 0.17. Over the range $0.17 < D < 0.8$ F_{max} does not deviate by more than 15% from the value 5.5 terraW (note: D for the current climate in the EBM is about 0.355). While F_{max} is insensitive to the transport efficiency, note that conversely, the climate is very sensitive to the value of F_{max}. Moving along the curve there are three values of D which give an F_{max} of about the present NH value. These D values are 0.29, 0.355 (the current value) and 0.42, which correspond to radically different climates with iceline latitudes at 60°, 75° (the present) and an icefree earth respectively. The value of the flux maximum need change by no more than a few percent over the range of D values between about 0.2 and 0.5, and the resulting climate could be anywhere between an iceage and an icefree earth. For further discussion of the F_{max} - D curve refer to Stone (1978).

We conclude that the climate is potentially quite sensitive to the energy transports. In the following sections we will set up equivalent EBMs to test this sensitivity for the three GCMs.

7.3 Equivalent EBM parameterization

7.3.1 Parameterization technique

Equivalent EBMs were parameterized with output from each of the three GCMs and with observations. Because the hemispheres do not have symmetric climates, a NH and a SH equivalent EBM were set up for each of the above cases, resulting in eight cases. The values of the input parameters in the NH and SH were also averaged to give a global equivalent EBM for each case (four more cases), yielding twelve cases in all. Taking averages of hemispheric parameters to produce a global version is valid assuming linearity, and the extent to which the global EBM results are not bracketed by the hemispheric EBM results will give an indication of the non-linearity of the response. Jumping ahead to the results in table 7.1, it appears that the global equivalent results are roughly bracketed by each hemispheric result.

To specify the EBM in each case, the required input parameters are the meridional distribution of solar radiation (S_0,S_2 in eqn. 4.3), the albedo (α_0,α_2,δ in eqn. 4.4), the surface temperature - longwave coupling (A,B in eqn. 4.2), the sea level temperature at the iceline latitude (T_s), and either the dynamical transport efficiency (D), or the iceline latitude (x_s). The major outputs from the model are the longwave coefficients from eqns. 4.10-4.12 (I_0,I_2), and D or x_s.

Our initial approach was to specify D and calculate x_s on the basis of the specified D. The reasoning for specifying D was that we know the flux maximum in each model reasonably accurately from earlier calculations, and can use eqn. 7.1 and eqn. 4.11 to solve for I_2. Knowing I_2, we can then solve for D and use it as an input to solve for x_s. The problem with this approach should be apparent from the conclusion drawn in the previous section. The iceline latitude is too sensitive to the value of the flux maximum, and so any small errors in the value of the flux maximum lead to large errors in the derived value of the iceline latitudes.

The approach taken then was to specify the iceline latitude x_s, and use that to derive D for each of the models and observations. It is important to have the iceline correct in the equivalent EBMs if they are to represent the same climate states as simulated by the GCMs. The results of the parameterizations are described in following sections.

7.3.2 Mean annual meridional distribution of solar radiation and solar constant

The mean annual meridional distribution of solar radiation at the top of the atmosphere in the EBM, S(x), is represented by eqn. 4.3. i.e. $S(x)=1+S_2P_2(x)$.

With S_2=-0.482, S(x) is uniformly approximated within 2% (North, 1975). The distribution of S(x) is determined from astronomical calculations, and is the same in each EBM since all the GCMs incorporate this distribution correctly. We use the above value of S_2 in each case.

To calculate the latitudinal distribution of the albedo in each of the GCMs and observations it is necessary to know S(x). For this purpose we interpolated S(x) values to model latitudes from the values for S(x) tabulated in Chylek and Coakley (1975) since this is more accurate than using eqn. 4.3. Note that, in the annual mean case, S(x) is symmetric between the hemispheres.

The solar constant, q, used to multiply S(x) to obtain the net incoming solar flux, Q, at a given latitude is not the same in the GCMs. For NCAR it is 1370W/m^2, for GISS it is 1367W/m^2, and for GFDL it is 7% larger at 1467W/m^2. We use thes values in the equivalent EBMs. The observational value of the solar constant is not known to within better than about 5W/m^2. A reasonable choice given current measurements is 1365W/m^2, which is the value we use for the EBM parameterized to observations.

Note that the EBM climate is quite sensitive to the solar constant as displayed in fig. 7.1. With the EBM as parameterized in this figure, a 2% increase in solar constant is enough to eliminate the polar ice sheets. All other things being equal, the 7% larger solar constant used in the GFDL GCM should eliminate the ice caps in that model. This is not the case in the GCM however, since there are ice caps in the GFDL control climate. Compensating factors must be at work there.

7.3.3 Iceline latitudes and sea level temperatures

The technique for determining the iceline latitude in each of the models and observations was outlined in section 5.6. The data source for observations was Alexander and Mobley (1976). The iceline latitudes (accurate to within a couple of degrees) are plotted in figure 7.4. The first thing to note is that the iceline latitude in observations in northern and southern hemispheres is not the same. The NH iceline is at x_s=0.95 (72°) as conventionally specified in EBMs for the current climate, while the SH iceline is at x_s=0.89 (63°). The GFDL and

GISS icelines are fairly close to the observed, while the NCAR model simulates too much ice in both hemispheres as has been noted previously.

The sea level temperatures at the iceline, T_s, were interpolated from the meridional temperature profile for each of the models and observations in the respective hemispheres. The results are shown in figure 7.5. Since the meridional temperature profiles are fairly similar in each of the models and observations, the results for T_s mostly reflect the iceline latitude positions (except for the GFDL SH, which is warm in southern high latitudes).

7.3.4 Outgoing longwave radiation

The flux of outgoing longwave radiation at the top of the atmosphere, I, is the prime dependent variable in the EBM. I is parameterized as given in eqn. 4.2 as $I = A + BT$, where T is the sea level temperature in °C. For each of the models, output for I was available directly from the AER data. For observations, I is tabulated in Stephens et al. (1981). The latitudinal profiles of I are shown in figures 7.6.3 and 7.6.4 for each hemisphere for models and observations. In each of the curves there is a minimum in outgoing longwave radiation near the equator, and a maximum in subtropical latitudes. Commenting on this in the observations, Stephens et al. note that the near equatorial minimum is the result of "high topped clouds associated with the ITCZ," and that the subtropical maxima "are indicative of the subtropical dry zones (both over land and over ocean)." While all the models simulate this general profile, the GFDL model has significantly more outgoing IR at virtually all latitudes relative to NCAR, GISS, and observations. Remember that GFDL has more incoming solar radiataion than observations and the other models (its solar constant is 7% larger). Since the GFDL model is in energy balance the higher solar constant requires higher emissions to space, which can be shortwave, longwave, or both.

The sea level temperature was derived for each of the models from the model output surface temperatures. For observations the sea level temperature was derived from the Sellers surface temperature data. The resulting latitudinal profiles are shown in figures 7.6.5 and 7.6.6 for each hemisphere for models and observations. In the NH all the models match the observation profile fairly closely. In the SH the match is not quite so good, with a tendency for all of the models (especially GFDL in mid and high latitudes) to be warmer than observations.

The coefficients A and B were determined for each case by least squares fitting (with latitude area weighting) the temperature profiles to the outgoing longwave flux profiles. The values obtained for A and B are shown in figures 7.7.1 and 7.7.2 respectively. There is not much that stands out from these figures except that in each case (models and observations) B is larger in the SH than the NH.

To gain an indication of how well I is parameterized by eqn. 4.2 we calculated I from the A and B obtained above, using the sea level temperature profiles for T in each case. The results are shown in figures 7.8.1 and 7.8.2. Comparing these figures with figures 7.6.3 and 7.6.4 it is apparent that the relative magnitudes of I are preserved in each case, but that the equatorial minimum profile has been lost in every case. i.e. figs. 7.8.1 and 7.8.2 show I increasing steadily toward the equator. Equation 4.2 does not appear to be sophisticated enough to reproduce I accurately in low latitudes, and we should remember this in interpreting the EBM results.

7.3.5 Albedos

Observations of the planetary albedo in the annual mean as a function of latitude are tabulated in Stephens et al., and were used for parameterizing the observation EBM albedo. For each of the GCMs we did not have available direct output of the albedos. The albedo may be calculated however from the net flux of shortwave radiation at the top of the atmosphere, SW_{net}, which we do have for each of the models. The net shortwave flux is related to the albedo via

$$SW_{net}(x) = q/4\ S(x)\ [1 - \alpha(x)] \qquad 7.2$$

where q is the solar constant, α is the planetary albedo, and S is the meridional distribution of solar radiation. Equation 7.2 was used to calculate the albedo profiles for each of the models. The results are shown together with the Stephens et al. albedos for each hemisphere in figures 7.6.1 and 7.6.2. In both hemispheres there are fairly large differences between the albedos for the various cases. The profile of the GISS albedo is in general closest to the observed profile, increasing relatively smoothly polewards. The NCAR and GFDL albedo profiles are flatter through to high latitudes, then rise sharply.

In the EBM, the albedo is parameterized as in eqn. 4.4: $\alpha = \alpha_0 + \alpha_2 P_2(x) + \delta u(x-x_s)$. We pick α_0, α_2, and δ by solving a system of three simultaneous equations. The first two equations are obtained by equating the first two insolation components in eqn. 4.9 with their expansions in equations 4.13 and 4.14:

$$H_0 = \int_0^{90} S(x)\ [1- \alpha(x)]\ dx = (1/2\ \delta\ S_2)\ x^3 + (\delta -1/2\ \delta\ S_2)\ x - 1/5\ \alpha_2 S_2 + 1-\alpha_0-\delta$$

$$H_2 = 5 \int_0^{90} S(x)\ P_2(x)\ [1- \alpha(x)]\ dx = (9/4\ \delta\ S_2)\ x^5 + 5/2\ (\delta - \delta\ S_2)\ x^3 + 5/4\ \delta\ (S_2 -2)\ x + S_2\ (1-\alpha_0-\delta) - 2/7\ \alpha_2 S_2 - \alpha_2$$

The third equation we chose to be weighted to high latitudes to make sure that the parameterization represents the albedo reasonably well in latitudes higher than x_s (i.e. that the δ is well picked). The chosen integral and its expansion is:

$$H_h = \int_{x_s}^{1} S(x)\ [1- \alpha(x)]\ dx = 9/20\ \alpha_2 S_2\ x^5 - 1/2\ (\alpha_2 S_2 + S_2 - \alpha_2 - \alpha_0 S_2 - \delta\ S_2)\ x^3 - [\ 1/2(\alpha_2 - S_2 + \alpha_0 S_2 + \delta\ S_2) + 1- \alpha_0 - \delta - 1/4\ \alpha_2 S_2]\ x + 1 - \alpha_0 - \delta - 1/5\ \alpha_2 S_2$$

For each of the three equations above the left hand side was integrated numerically with albedo values taken from figs. 7.6.1 and 7.6.2, and with values of S interpolated at each model's latitudes. With S_2 set to -0.482, the three equations were then solved in each case for the unknowns α_0, α_2, and δ. The results are shown in figures 7.9.1, 7.9.2, and 7.9.3. α_0 is a measure of the global average planetary albedo, and does not vary much between models and observations as shown in figure 7.9.1. All α_0 values are close to 0.3. α_2 is shown in figure 7.9.2, and provides a measure of the meridional gradient of the albedo. The GISS α_2's are close to observations, while the NCAR and GFDL α_2's are only half as large as observations. This is to be expected from the relatively flat albedo profiles for NCAR and GFDL in figures 7.6.1 and 7.6.2 (except in high latitudes where there is less area).

The third albedo component, δ, is shown in figure 7.9.3, and gives an indication of the strength of the albedo in high latitudes (where there is ice present). δ is large relative to observations in the SH for the NCAR and GFDL models. δ is about the same in both hemispheres in observations, but not in the NCAR and GFDL models. The GISS δ's are small relative to observations in both hemispheres.

To test the albedo parameterizations we substituted the values of α_0, α_2, and δ obtained above into eqn. 4.4 and plotted the result in each case. Comparing the results obtained thus so in figures 7.10.1 and 7.10.2 with the albedos directly from models and observations that they were parameterized to (figs. 7.6.1 and 7.6.2), it is evident that the albedo parameterization was quite successful. The two sets of curves are relatively similar, and most of the features in the actual albedo profiles seem to have been captured in the parameterized profiles. We can thus be relatively confident of having a fair representation of the albedos in the EBMs.

7.4 Equivalent EBM results

Having parameterized the equivalent EBMS as outlined above, we then calculated outputs for each case. First, the value of I at the iceline, I_s was calculated from $I_s = A + BT_s$. Knowing I_s, the transport efficiency, D, can be calculated from eqn. 4.15.

For the sensitivity study, D was held fixed while the solar constant was increased by 2% in each case. The iceline latitudes, x_s, corresponding to the increased solar constant were determined by solving for x_s in the seventh order polynomial expression represented by eqn. 4.15. For both the current climate and the climate with increased solar constant, we then solved for the longwave radiation, temperature, and flux maximum via: $I_0 = q/4\ H_0(x_s)$ (eqn. 4.10); $I_2 = (I_s-I_0)/(P_2(x_s))$ (eqn. 4.12); $T_0 = (I_0-A)/B$; $T_2 = I_2/B$ and $F_{max} = 6\pi R^2 DI_2(x^3-x)$ where $x=1/\sqrt{3}$ is the latitude where the flux mamimum occurs. It was then possible to calculate the climate sensitivity, β, for each case:

$$\beta = q_1\ (T_{02}-T_{01})/(q_2-q_1) \qquad 7.3$$

where the 1 and 2 subscripts refer to cases with the current solar constant and with the solar constant increased by 2% respectively. The results, showing values for all inputs and outputs in the equivalent EBMs are tabulated in table 7.1.

The values of I_s are shown in figure 7.11. A relatively large I_s value is obtained for the GFDL model in the SH, which is related to the very small negative value for T_s in that case. All other T_s values are not too different from observations.

The D values for each case are shown in figure 7.12, and vary considerably. For observations D is about the same in both hemispheres with a value close to 0.26. The GISS D values are larger than this in both hemispheres, which is consistent with the large poleward energy transports simulated in the GISS model. The NCAR D values are less than observations, as is the case for the NCAR energy transport in the NH. Note however that the magnitude of the energy flux depends on I_2 also, and will be discussed below.

Figures 7.13 and 7.14 show values for I_0, I_2, and T_0, and T_2 for each case. For the planetary IR emission, I_0, the NCAR model shows asymmetry between the cases for the two hemispheres, whereas most other models and observations are relatively symmetric between hemispheres. For I_2 (a measure of the meridional gradient of I), NCAR is large relative to observations in both hemispheres, which will tend to offset the low D values in calculation of the energy transports. The GISS model I_2's are small relative to observations, which will tend to offset the relatively high GISS D values.

From values of T_0 and T_2 for each case we have plotted the EBM meridional temperature profile via $T(x) = T_0 + T_2P_2(x)$. These profiles are represented by the dashed lines in figure 7.15. The solid lines in the figures show the actual sea level temperature in the relevant model or observations for each hemisphere in each case. The EBM temperature profiles do not capture the levelling off of the actual temperature profiles at low latitudes, particularly for observations and the NCAR model cases. For instance, the large T_0 value displayed in figure 7.14.1 for the NCAR NH model is not realistic, since the EBM temperature profile is much too warm in low latitudes. The unrealistic T values in the EBM reflect shortcomings in the use of such a simple model, and not additional errors on the part of the GCMs. The surface temperature - outgoing IR parameterization in the EBM was not sufficiently sophisticated enough to accurately account for the drop in IR near the equator, and this could lead to both T_0 and T_2 being larger than they should be. For the global version of the equivalent EBM, the planetary average temperature, T_0, for NCAR and observations is 19.9°C and 16.6°C respectively, and the meridional gradient of the temperature, T_2, is 38.8°C and 34.6°C respectively, which is too high.

The energy transport maximum in the equivalent EBMs for each case is shown in figure 7.16 as the solid bars. The corresponding flux maximim from earlier calculations for the GCMs and observations is shown as the cross-hatched bars. For the global cases for the latter, the value of the flux maximum plotted is an average of the two hemispheric values. For the EBM global values however, the flux maximum is from the global EBMs parameterized with averages of the hemispheric parameter values. The results show reasonable agreement between the equivalent EBM flux maximum and the flux maximum from GCMs and observations, and this despite shortcomings in the EBM representation of I_2. In the EBM results, the GISS transports are large relative to observations, and NCAR and GFDL transports are small relative to observations as for the GCMs and observations in fig. 4.4.

The iceline latitudes input to the equivalent EBMs are shown in figure 7.17 (solid bars) alongside the iceline latitudes obtained from the EBMs when the solar constant was increased by 2% (diagonal pattern bars). The GISS iceline latitudes are closest to observations in both cases. For the control climate the GISS model has the advantage of representing ocean heat transports parameterized from observed SST data, which helps in simulating the iceline latitude. The equivalent GFDL EBM loses its polar ice caps for a 2% solar constant increase, which it did not do in the GCM doubled CO_2 experiment.[37] This suggests that whatever compensating mechanisms are

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[37] Wetherald (personal communication) notes that only about half the sea ice melted for doubled carbon dioxide in the GFDL model.

being represented in the EBM, they are still not as strong a compensating influence as in their representation and sum in the GCM.

The ultimate goal of the EBM experiments was to determine the climate sensitivity for each equivalent EBM to see how it compares with the sensitivity in the GCM. For the GCMs the climate sensitivity to doubled CO_2 can be calculated by assuming that doubling CO_2 is equivalent to a 2% increase in the solar constant. Given the temperature increase in the the CO_2 doubling experiment in each GCM, and the GCMs solar constant, the GCM sensitivities are as follows:

NCAR: $\beta = 1370\text{W/m}^2 \times 3.5\text{K} / 27.4\text{W/m2} \approx 175\text{K}$[38]

GFDL: $\beta = 1467\text{W/m}^2 \times 4.0\text{K} / 29.3\text{W/m}^2 \approx 200\text{K}$

GISS: $\beta = 1367\text{W/m}^2 \times 4.2\text{K}/ 27.3\text{W/m}^2 \approx 210\text{K}$

The climate sensitivity calculated for each equivalent EBM is shown in figure 7.18 (and listed in table 7.1). They are fairly similar in most cases, with values lying between about 150K and 200K. The major exceptions are the NCAR and GFDL values in the SH. In these cases the iceline shifted relatively further poleward than in other cases in response to the increased solar constant, which would affect the sensitivity. The NCAR and GFDL EBMs have the highest values for δ (which contributes to the albedo in icecovered latitudes) in the SH.

Considering the climate sensitivity for the global equivalent EBMs, not only are they relatively similar for models and observations, but they are also similar (for models) to the values obtained from the GCM CO_2 doubling experiments listed above. The respective equivalent EBM and GCM values are: NCAR(190,175), GFDL(205,200), and GISS(145,210).

The sensitivities are similar between models and between the EBM and GCM versions of the models in spite of the many obvious differences between the models. For instance, the energy transports differ, the solar constant is relatively larger in the GFDL model, the treatment of convection is different between the GISS and other models, the icelines differ, albedo profiles differ, etc. These differences should influence the model sensitivity. That the

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[38] The temperature in the NCAR doubled CO_2 run was still increasing (relative to the control run), and so the actual NCAR model climate sensitivity may be a little higher than this.

sensitivities are not so different suggests that either compensating factors are at work, or the GCM climate sensitivity is not sensitive to the differences between models. We reject the latter explanation on the grounds that the EBM climate sensitivity is quite sensitive to the solar constant (which differs between models), and the EBM climate is sensitive to the energy transport.

7.5 Discussion

Compensation in the EBMs and GCMs could be a coincidence (this is unlikely, but if so it would not give us much faith in the models, since their agreement would be meaningless then), or it could be related to the physics in the models. For the EBMs we have been unable to determine (at the time of writing) what factors are responsible for compensation leading to a lack of sensitivity to the energy transport differences. Potential candidates for sensitivity compensation include those related to ice-albedo feedback and water vapour / cloud feedbacks. It is possible that the relationship between D and x_s may be important. D influences the energy transport which influences the iceline position, x_s. The position of the iceline changes the strength of the ice-albedo feedback, which influences climate sensitivity. The iceline position is also influenced by T_s. The strength of the ice-albedo feedback is also influenced by δ (replacing no ice by ice). α_0 and α_2 are not important since they merely specify the albedo in the absence of ice and so do not relate to the ice-albedo feedback. For the cloud and water vapour feedback the only important parameter is B, which provides a direct measure to the feedback.

It is interesting to speculate at this point as to whether compensations are necessarily present in the GCMS (and their equivalent EBMs) because the GCMs are tuned to the current climate. Tuning is possible in the GCMs because all the relevant physics are not included with certainty and completeness. The meridional temperature structure is approximately right in each of the GCMs partly because they are tuned to the current climate. If this induces compensations, then the parameters B, δ, x_s, T_s, and D may be constrained (not set, since there is still much physics in the models, and these parameters depend on physics) by tuning to the current climate. This raises the possibility that the climate sensitivity is constrained by tuning to the current climate. A way to test this in the EBM would be to see whether it is possible to change the sensitivity of an EBM (by changing B, δ, x_s, T_s, and D) while still maintaining the correct current climate and consistency with observations. If it is possible, then we can be pretty certain that it is also possible to change feedbacks related to B, δ, x_s, T_s, and D in the more complex GCMs and change the sensitivity in the GCMs too. If it is possible to change these parameters in the EBM and change the sensitivity then we know that the errors in the GCMs are important, and would have some indication of where to look for them in each GCM. In future work we will try to answer the unresolved questions identified in discussion here.

7.6 GCM validation implications

In the next chapter we consider aspects of the greenhouse change problem relevant in science and policy frameworks together. Before moving to this level however (and by way of introduction) we follow on here with a brief discussion on validation of GCMs, considering implications of the energy transport validation results.

Theories of greenhouse change can not be properly validated without validation of the 3-D GCMs, since greenhouse change scenarios rely so heavily on GCM experiments in the absence of other tools to forecast climate change in detail. Attacks on the validity of greenhouse change scenarios have consequently concentrated to a considerable degree on uncertainties in the GCMs. Newsweek (1989) quotes U.S. White House Chief of Staff John Sununu as saying "you do not establish policies on the basis of incomplete models" in arguing against taking steps to mitigate greenhouse warming. Apart from the fact that the Government does this all the time where economic models are concerned[39], this begs the validation question. We really need to ask how incomplete is too incomplete, and how much uncertainty is too much, as there will always be some incompleteness and therefore uncertainty in any model of a complex system.

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[39] Handel (personal communication) notes that he would "place far more faith in forecasts of global warming from increases in greenhouse gases, than in the belief that we are on the high side of the Laffer curve so that decreasing the upper tax rates will lead to increases in government revenues."

For the sake of scientific research, the answer to these questions might not be too important, since, for the accumulation of scientific knowledge we might be more interested in learning about processes in the models than concerned with the actual predictions themselves. For policy purposes, the greenhouse change scenarios are important, since it is desireable to determine what the potential impacts on society might be, if policies are to be put in place. What then are the implications of model differences (such as in energy transport), model internal inconsistencies (such as between energy transports and temperature structure and iceline), model incompleteness (such as lack of ocean energy transports, or lack of potential for ocean transports to change as climate changes), and other model uncertainties for validation of the GCMs and their greenhouse change projections? The short answer is that the implications of model shortcomings depend on just what it is we want to determine from the models. If we want to know whether the regional results in a particular area for a particular GCM are reliable or not, then the answer is probably no.[40] The effects of ocean circulation are not included in the models, and energy transports are not correctly partitioned between atmosphere and ocean, and this would affect the atmospheric circulation, which in turn would affect regional climate (other factors such as poor representation of hydrology and clouds and moist convection would also be important in this regard). That model results are not reliable for the purpose of forecasting regional climate does not mean that the model results are not reliable period. There are other questions one can ask of the models for which the above shortcomings may not be crucial. That is, for some questions, the results may not be very sensitive to the remaining uncertainties, or at least not so much as to yield scenarios that would warrant a different policy response. In this case the model results must be acceded some degree of validity. Alternatively, it may not be clear how sensitive the model projections are to the remaining uncertainties, though the perceived risks associated with the projections may be high enough to warrant policy responses regardless of how much validity we associate with the models.

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[40] For most questions at this level the answer is certainly no. For particular regions like midlatitude continents and for questions related to drought occurrence, GCMs may provide some reliable information.

In either case, the question being asked of the GCMs at present is what the equilibrium global warming associated with a doubling of CO_2 in the atmosphere is. The results yielded by models are typically in the range 1.5-5.5°C (Dickinson, 1989). These results are widely enough quoted in the literature thay they have taken on a sense of validity by reinforcement, but we must ask: why believe them? From whence do they derive their validity? Are they sensitive to the sorts of errors and inconsistencies we have noted in the energy transport for instance?

The equilibrium doubled CO_2 response prediction can be broken down into three parts following Dickinson (1989). The first part is determining what the radiative forcing associated with doubling CO_2 is. While there are uncertainties in this determination, they are relatively small and not much argued about. The second part involves determining what the surface temperature response to the forcing is (and the third part involves determination of how long it takes the response to occur). The second part can be broken down into the response in the absence of feedbacks and the response with feedbacks present. From simpler 1-D radiative-convective models the response in the absence of feedbacks can be determined, and generally yields results at the lower end of the above range. i.e. about 1.2°C. When feedbacks to the 1.2°C temperature response are incorporated in GCMs they yield a net positive feedback and sensitivities (for the three models considered here) in the range of 4°C.

It is the representation of the feedbacks that is most contentious (particularly the cloud feedbacks) in the models, since the feedbacks are sensitive to model uncertainties. This is demonstrated by the results from the UKMO model (Mitchell et al. 1989) which displays sensitivities between 1.9K and 5.2K for various plausible representations of clouds (though these results are still not outside the range quoted by Dickinson[41]). As noted by Stone (personal communication), for changing the no feedback surface temperature response with feedbacks, positive feedbacks provide a greater response (warming) to a given feedback strength than the cooling tendency for a

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[41] They are also not outside the lower end of the range estimated by NRC (1979) for carbon dioxide doubling of 3°C±1.5°C.

negative feedback of the same strength. This can be understood by considering the profile of the feedback / no feedback response ratio versus the feedback plotted in figure 2 in Schlesinger (1989). This ratio goes as 1/(1-f), where f is the feedback, and is much more sensitive to positive feedbacks. This means that to reduce the sensitivity of the GCMs by a significant amount we must include processes not currently accounted for, that yield substantial negative feedback. While this does not prove that the GCM sensitivities are right, it makes the job of proving them wrong more substantial, since processes not included in the models that would yield only weak feedbacks will not suffice to change the results much when they are included.

The doubled CO_2 sensitivity results from the models cannot be tested for some time however (until greenhouse gases build up to an equivalent CO_2 doubling, and the surface temperature has time to respond), and so confidence in the models must be derived from other sources. A variety of validation tests are possible, and some of them were outlined in an earlier chapter. These tests are ultimately important as reasons why those who find GCM results credible find them credible, and so will be mentioned again here.

Firstly, the GCMs agree qualitatively in many respects in their CO_2 doubling simulations, and for their simulation of the current climate. This test ultimately yields a half glass of water though, in that one's perspective of whether the glass is half full (models agree) or half empty (models disagree) depends on what is being compared and what one is trying to show. Nevertheless, the GCMs do yield similar sensitivities and results despite differences in the way they have been formulated. The equivalent EBM model results suggest (barring coincidences) that the GCMs obtain similar sensitivities due to compensations between processes related to ice-albedo feedback and cloud and water vapour feedback, so that the outcome is perhaps not too sensitive to differences in model formulation.

Further evidence supporting GCMs comes from their ability to simulate the seasonal cycle (which involves larger temperature changes than CO_2 doubling results), past climates (including the last glacial maximum), and the climates of other planets such as Mars and Venus. Confidence in the GCMs is also obtained by noting consistency with simpler models and physics (which can be more easily understood), and with observations of temperature and CO_2 in past climate changes (on paleoclimatological time scales and over the last 100 years).

None of this evidence taken alone offers clear confirmation of GCMs and their climate sensitivity experiments. In validating a GCM there are lots of necessary tests that can be performed, but no sufficient ones (short of waiting until hindsight is available). It is therefore virtually impossible to validate a GCM (or any other model of a complex system) unequivocally. On the other hand, everybody has some confidence in GCM results at some level. For example, the basic physics of the greenhouse effect is reproduced in a GCM and is not disputed. How far one is willing to go in seeing utility in GCM output at a particular level depends on how much weight one gives to the various validation tests that are available to support the types of processes that the results depend on. Those that have some confidence in the GCM doubled CO_2 sensitivity results as reasonable generally do so, not on the basis of a particular validation test, but through a subjective judgement of all the information available to test the models and their representation of the relevant physics.

The GCM projections are at least plausible at some level, and are gaining attention in the policy community. In the next chapter we will outline a validation framework for greenhouse change (models and projections) in a science - policy context.

TABLE 7.1

Inputs and outputs from the twelve equivalent EBMs. In each case the first four letters denote the model or observations that the EBM was parameterized to. The last two letters denote the hemisphere (Northern Hemisphere or Southern Hemisphere) or average of the hemispheres (Global). Results are given for both the current solar constant and for the solar constant increased by 2%. The climate sensitivity calculated for each case is also shown.

CURRENT CASE VALUE SOLAR CONSTANT

	D	XS	IS	IO	I2	TO	T2	FM	A	B	ALO	AL2	DEL	TS	QO
NCARNH	0.167	0.920	185.64	245.93	-78.34	22.5	-39.5	3.85	201.4	1.98	0.280	0.079	0.209	-7.9	1370.0
NCARSH	0.207	0.839	188.60	235.30	-84.02	17.0	-36.7	5.12	196.3	2.29	0.283	0.076	0.364	-3.4	1370.0
GFDLNH	0.275	0.956	193.44	244.04	-58.10	15.0	-32.1	4.71	216.9	1.81	0.341	0.099	0.148	-13.0	1467.0
GFDLSH	0.353	0.899	208.97	244.68	-50.13	16.5	-24.2	5.20	210.5	2.07	0.322	0.041	0.251	-0.7	1467.0
GISSNH	0.415	0.957	188.55	231.92	-49.64	14.7	-29.4	6.06	207.1	1.69	0.340	0.208	0.079	-11.0	1367.0
GISSSH	0.365	0.906	192.89	233.68	-55.78	15.4	-28.6	6.00	203.6	1.95	0.330	0.202	0.101	-5.5	1367.0
OBSNNH	0.266	0.950	181.89	236.78	-64.29	17.6	-35.3	5.04	204.7	1.82	0.317	0.162	0.164	-12.5	1365.0
OBSNSH	0.253	0.891	190.48	239.76	-71.32	15.7	-33.9	5.30	206.8	2.10	0.303	0.168	0.168	-7.8	1365.0
NCARGL	0.177	0.880	186.77	241.45	-82.82	19.9	-38.8	4.30	198.9	2.14	0.282	0.078	0.286	-5.7	1370.0
GFDLGL	0.291	0.928	200.40	244.73	-56.09	16.0	-28.9	4.80	213.7	1.94	0.331	0.070	0.200	-6.8	1467.0
GISSGL	0.385	0.932	190.36	232.87	-53.03	15.1	-29.1	6.00	205.4	1.82	0.335	0.205	0.090	-8.2	1367.0
OBSNGL	0.258	0.920	185.85	238.31	-68.04	16.6	-34.6	5.16	205.8	1.96	0.310	0.165	0.166	-10.1	1365.0
WASTWH	0.355	0.950	191.85	236.46	-52.25	14.4	-32.1	5.46	213.0	1.63	0.316	0.146	0.186	-13.0	1365.0

2% INCREASED CASE SOLAR CONSTANT

	D	XS	IS	IO	I2	TO	T2	FM	A	B	ALO	AL2	DEL	TS	QO
NCARNH	0.167	0.956	185.64	252.44	-76.70	25.8	-38.7	3.76	201.4	1.98	0.280	0.079	0.209	-7.9	1397.4
NCARSH	0.207	0.952	188.60	249.49	-70.85	23.2	-30.9	4.32	196.3	2.29	0.283	0.076	0.364	-3.4	1397.4
GFDLNH	0.275	0.999	193.44	250.23	-56.96	18.4	-31.5	4.61	216.9	1.81	0.341	0.099	0.148	-13.0	1496.3
GFDLSH	0.353	1.000	208.97	255.16	-46.19	21.6	-22.3	4.79	210.5	2.07	0.322	0.041	0.251	-0.7	1496.3
GISSNH	0.415	0.990	188.55	237.06	-50.01	17.7	-29.6	6.10	207.1	1.69	0.340	0.208	0.079	-11.0	1394.3
GISSSH	0.365	0.940	192.89	239.10	-55.98	18.2	-28.7	6.02	203.6	1.95	0.330	0.202	0.101	-5.5	1394.3
OBSNNH	0.266	0.985	181.89	242.64	-63.59	20.8	-34.9	4.98	204.7	1.82	0.317	0.162	0.164	-12.5	1392.3
OBSNSH	0.253	0.923	190.48	245.76	-71.06	18.5	-33.8	5.28	206.8	2.10	0.303	0.168	0.168	-7.8	1392.3
NCARGL	0.177	0.930	186.77	249.56	-78.75	23.7	-36.9	4.09	198.9	2.14	0.282	0.078	0.286	-5.7	1397.4
GFDLGL	0.291	1.000	200.40	252.71	-52.30	20.1	-27.0	4.47	213.7	1.94	0.331	0.070	0.200	-6.8	1496.3
GISSGL	0.385	0.965	190.36	238.15	-53.28	18.0	-29.3	6.03	205.4	1.82	0.335	0.205	0.090	-8.2	1394.3
OBSNGL	0.258	0.954	185.85	244.25	-67.50	19.6	-34.4	5.12	205.8	1.96	0.310	0.165	0.166	-10.1	1392.3
WASTWH	0.355	1.000	191.85	242.98	-51.13	18.4	-31.4	5.34	213.0	1.63	0.316	0.146	0.186	-13.0	1392.3

CLIMATE SENSITIVITY PARAMETER : BETA QDT/DQ

NCARNH	164.20
NCARSH	309.73
GFDLNH	171.02
GFDLSH	253.20
GISSNH	152.20
GISSSH	139.10
OBSNNH	160.74
OBSNSH	142.62
NCARGL	189.77
GFDLGL	205.57
GISSGL	145.07
OBSNGL	151.17
WASTWH	200.03

8 GREENHOUSE CHANGE VALIDATION FRAMEWORK

8.1 Introduction

The purpose of this chapter is to present a framework for thinking about greenhouse change validation in science and policy contexts. While there are many commonalities between science and policy requirements in validating a scientific theory, there are also differences between these two cultures and between their requirements. By comparing and contrasting the validation process in these arenas, it might be possible to identify ways of facilitating effort between them to match their respective needs more closely.

We will argue that there are characteristics of the greenhouse problem which do lead to some degree of mismatch in requirements between science and policy validation systems (thus compounding the validation problem). We treat each system as a dynamic system that is capable of responding to changes in the other, and adjusting its requirements or effort accordingly. We do not venture to suggest just how dynamic each system is, but will try to identify questions that could be presented from one system to a responsive other. Some of the questions and problems identified in the framework will undoubtedly turn out (under deeper analysis) not to be problems at all. Conversely, we do not expect to exhaust the list of difficulties encountered in evaluating greenhouse science-policy issues in the context of the framework.

The validation framework relies upon notions of how science and policy systems operate. The validation philosophy to be outlined in the science domain views scientific theories as valid if they are largely consistent, and seem to explain more about the way the world might work than alternative views of the world. Validation in the policy domain will extend upon the scientific version to consider issues such as useability, defensibility, and acceptability. The policy system is viewed implicitly as a composition of interest groups with varying degrees of power and benevolence.

The validation framework is represented schematically in figure 8.1. In subsequent sections describing the framework we will describe what is being validated and in what context, and what definitions of validation have been settled upon and what the different components of the validation process are. In this work we will not go much beyond presenting the framework. The following chapter will address only one of the questions derived from the framework: that of how well validation information is being presented from science to policy arenas.

8.2 Framework outline

In broad terms, the validation framework shown in figure 8.1 involves the development, interpretation, transfer, reinterpretation, redevelopment and so on, of information relevant to assessing the credibility of greenhouse change theory. Representation of information flow by inputs, outputs, and feedbacks has been adopted for convenience, and the lack of a connecting line between any two boxes should not be taken to imply that no relationship exists.

A problem such as that posed by greenhouse change must come from somewhere. The origin of the problem is represented in the framework as inputs in the top left corner. The primary inputs are greenhouse gas concentrations in the atmosphere and their time evolution (the result of anthropogenic and natural contributions). As a result of changes in greenhouse gas concentration, we have climate changes and associated impacts. Building into the knowledge base on input is our understanding of paleoclimatic history and the historical response of societies to climate change and other social perturbations.

The problem at issue as a result of increases in greenhouse gases is greenhouse change or greenhouse warming. The validation framework is geared toward the issue of greenhouse change, not to the greenhouse effect. Definitions of these terms were given earlier. The greenhouse effect is reasonably well understood, and perfectly well validated by the fact that the global average temperature is about the predicted amount above the blackbody equivalent value for the earth of 255K. Greenhouse change refers here to changes in the greenhouse effect associated with the increase of greenhouse gases in the atmosphere over roughly the present and coming centuries. The theory of greenhouse change is called upon to project what the climate changes (temperature increases, precipitation changes and circulation changes) associated with the greenhouse gas buildup in the atmosphere might be. The development of the science toward this end has relied upon development of a hierarchy of models of increasing sophistication, culminating in the GCMs, and on the reconstruction and interpretation of past and present climate changes. The topic for validation is the projections (largely derived from the GCMs) of the magnitude and nature of the climate change expected over the coming century or two. Whether the projections are useful and valid for policy or not depends on what questions are asked of them. A reasonable level of detail in the projections would probably go no further than that articulated in Jaeger (1988). Jaeger outlines in broad terms what the range of response of temperature, sea level, and precipitation might be, and how they might change latitudinally. We do not try to validate whether or not climate changes of the order of a few degrees celcius or so (globally averaged) imply significant environmental and social impacts. We assume that they do. Validation is directed toward understanding whether or not the expectation of a climate change of about this order seems reasonable.

The validation process is framed here in science and policy communities at primarily the national level. Throughout the framework, policy refers mostly to national policy, and the science community is mostly thought of as those doing science based in the U.S. This is a guide only, and it is expected that the framework would be useful in thinking about the validation problem at the international level as well.

Once it is realized that a theory such as greenhouse change has potential policy implications, it is of interest to try to validate it in some manner. The effort expended by the scientific community in validating the theory must be communicated to the policy arena. Publicizing the issue in science and noting policy implications provides a trigger to the policy community. Once political interest has been garnered, the issue is on the political agenda. Whether the issue stays on the policy agenda or not depends in part on perceptions of how well the theory and its projected social and environmental impacts are validated. Many greenhouse commentators have noted the ability of phenomena such as a hot summer in keeping greenhouse change on the political agenda. In the validation framework a single hot summer (though essentially meaningless in validating greenhouse change) does strengthen the public perception of the validity of the theory. Whether people attribute causal relationship to the event or not, the event serves to reify the theory, which provides reinforcement to the more plausible evidence supporting the theory.

Once an issue is on the policy agenda there is at least some incremental social response/adjustment in the way we live and do business (even if only slow). The mere fact that an issue is on the policy agenda means that the policy process is relevant to the issue, since even if we do nothing about it, that is a policy response.

Validation inputs to the policy level come from more than just the scientific community. Once an issue has attention in the policy arena, then the political, social, economic, cultural, environmental, and personal dimensions of the issue will also be communicated in varying degree. While the scientific community might touch on each of these issues, other communities will provide much of this input. Each of the communities (science and policy related) provides information back and forth in seeking to evaluate the relative importance of an issue.

Each community is by no means homogenous. The science community is composed of researchers from government, industry, universities, public interest groups, non-government-organizations, and private organizations. Each sub-community is subject to common and unique factors that influence the type of science they do and the way they view it. Scientists may even see their responsibilities and duties differently, depending on where they work. Superimposed on this is their own personal world views and values, which can influence the way they approach the greenhouse validation problem.

Though scientists can be viewed as interest groups beholden to particular interests or world views, or to a particular sense of responsibility, they have instituted a widely agreed upon procedure for regulating the development of their research. Imperfect as it might be, that procedure is the process of peer review and publication in refereed journals. Scientists also share an adherence to the notion of 'the scientific method'; a process involving hypothesis, experiment, testing, verification, and repeatability where possible. The scientific method is ideally guided by a rationality (call it science rationality) based upon reasonable logical choices and evaluation (usually within a framework where measurement of quantifiable characteristics is possible and meaningful). The output of this whole process in science is science journal papers that represent formal descriptions of the state of knowledge and development of understanding of the issue (greenhouse change).

Scientific output is communicated through more than just the journal papers. Panels and committess are convened to report on issues, and documentaries, interviews, reports, and testimony frequently provide communication channels for scientists to the media, the public, industry, and government. Scientists are communicating across cultures to groups with differing working bases and differing conceptions of rationality to apply to the information received. For industry this may be an economic based rationality. For the public this may be a socially based rationality. In each case some form of reason is still the basis for evaluating information, but the factors considered and the bounds and constraints applied may be different. If the frameworks for evaluation are not the same between two communities in isolation, then where they intersect they will need to enlarge their frameworks to make shared information mutually comprehensible. For scientists this implies supplementing scientific notions of rationality with an understanding of the non-scientific dimensions of the issue in its communication.

After validation information is presented by scientists it is received and interpreted by the public. For the policy process it is important to determine the potential or likely impacts (social and ecological) associated with the projections from theory. While it is difficult to factor in potential extreme climatic outcomes and their impact, it is easier to set up an expression for thinking about evaluation of the likely impacts. The likely impact is essentially the mid-range scenario for how climate might change multiplied by the probability of this scenario occuring. This is then further influenced by the political acceptability of the outcome and perceptions of its policy implications.

The mid-range greenhouse scenario is determined from greenhouse change theory and modelling projections. Development of the mid-range scenario is in some ways determined as an average of the most influential results in the literature, and perhaps also by what those scientists comprising the consensus group will agree to. Scientists view of the certainties and uncertainties will influence how the scenario is constructed. The probability of the mid-range scenario is a measure of the credibility of theory and model projections as determined at the policy level. Credibility is influenced by perceptions of the consistency and plausibility and support of the models and theory, and by perceptions of the degree of consensus among scientists in supporting the projections (the relative standing of dissenting views would also be important). Views of the uncertainty, such as how critical they are, and in which direction they are likely to influence the results, would also play a role in determining how much probability to associate with the scenario.

Political acceptability is influenced in the presentation of the science by how effective the presentation has been in matching policy requirements. The presentation of the climate change projections must be meaningful at the policy level. The projection information must have clear policy implications and be translateable into concrete terms that people can understand and appreciate. An example of effective presentation in this regard is shown by Hansen et al. (1988). In presenting greenhouse change scenarios from the GISS GCM, rather than just showing results in terms of a 4°C or so temperature rise (which doesn't sound like much to most people), they calculated the increase in such things as the number of days per year with temperatures exceeding certain amounts in several major cities (which has more local meaning). Political acceptability is also influenced by views of the uncertainty and how they have been characterized in presentation. The policy community is interested in knowing what the potential to reduce uncertainties is, and how long it might take to do so. Uncertainties frequently act as a source of inertia in environmental policy[42], though there are also characteristics of uncertainty that can be construed as implying action as well as inaction. A perception that uncertianties can be reduced relatively quickly

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[42] Uncertainties have been framed to act this way by U.S. Chief of Staff John Sununu on greenhouse policy to date.

might bode for delay to resolve them before taking policy action. This view of the uncertainties is taken by Marshall Institute (1989) in urging delay on the basis that uncertainties can be reduced enough to make a difference in 3 to 5 years. If on the other hand the perception is that uncertainties cannot be reduced quickly, while there are costs to delaying, then the uncertainties might be less likely to bode for delay of short term policy responses. Discussion of the role of uncertainties in the validation process will be taken up again later.

Received views of the potential risks are also important in establishing political acceptability of a theory. i.e. if the risks associated with not acting seem high, and the worst case scenarios seem plausible, then the theory is likely to gain higher acceptability in the face of interests that find the costs of action unacceptable.

Major influences on political acceptability of the greenhouse change projections come from the economic, social, and environmenal realms. Whether greenhouse change projections and their implications are considered acceptable depends on the evaluation and considerarion of factors such as: the perceived cost and benefits of alternative policy responses; risk perception; perceptions of relative interests and distributive burdens; issues of justice, equity, morality; perception of the relative strength of coalitions and constituencies; the relationship of greenhouse change to other issues and policies and their constituencies; the political and legal structure; the role of lobbying, sanctions, protest, cooperation, capital etc. This is not supposed to be an exhaustive list. It is only intended to give an indication that a myriad of factors can potentially affect perceptions of the acceptability of greenhouse change scenarios.

In evaluationg the likely impacts of greenhouse change, it is relevant (to policy) to ask: Are the likely impacts worth worrying about yet? The answer to this question depends on who you are. Perspectives on the importance of the problem differ widely, and the problem itself is different for different people, depending on what part of the world they live in, how adaptive their social and physical infrastructure is, and what segments of society they come from. In a national context greenhouse change will implicitly or explicitly be given some degree of priority by whether or not it stays on the agenda and engages the policy process. If it is dropped or deferred from the policy agenda, then the implicit answer is that the scenarios are not worth prioritizing yet. If greenhouse change stays on the policy agenda with some priority, then either policies (of some kind) can be put in place or lip service paid. In either case, a place on the agenda yields legitimacy to the issue.

Where policies are articulated, we need to ask whether they are substantively just, procedurally correct, and have implementable goals. Further, do they make any difference to greenhouse warming or to other social and environmental problems (energy, pollution, quality of life, economic well being, resource depletion, stratospheric ozone, etc.).

As a result of greenhouse change policies (which could include 'do nothing'), greenhouse gas concentratons in the atmosphere will change. This in turn will lead to exacerbation or ameloration of greenhouse climate change trends. Climate will change (both in actuality, and as we measure and perceive the changes), and climate impacts and projections of the impacts will change (together with confidence in the projections). This in turn will feed back on the way we evaluate greenhouse change projections at each of the various levels throughout the coupled system outlined in the framework.

8.3 Validation components

In discussing validation of GCMS and greenhouse change projections we have not attempted to define validation according to a single criterion with a single yes/no answer. It is virtually meaningless to say that the models and projections are valid or invalid period, since that involves too many judgements over questions that come down to matters of faith, belief, and values. The answer depends on who you are and what your world view is, and ultimately ends up revealing more about the person giving the answer than it provides useful information to the question at hand. When phrased in this simple manner the question transcends science, since it implicitly asks whether present uncertainties justify a policy response or not. Schneider (1989) notes that this is not an issue resolvable by scientific methods.

We can learn more about the validation question by phrasing it instead in terms of related questions about subsets of the projections such as: Are particular greenhouse projections credible and plausible: More specifically, is there some level of aggregation of the results at which the results are not only credible, but also useful from the point of view of policy development? While it might be said that framing the validation problem in utilitarian terms avoids the question, we need to think about the context of the validation question. Strictly from the point of view of scientific research, a focus on utility in validating greenhouse projections doesn't make immediate sense, since we would like to think that we do not decide scientific questions simply on the basis of whether the results are useful or not. In science though, we probably wouldn't spend so much time worrying about the actual veracity of GCM results beyond the utility so derived in improving the models and our understanding of important climatic processes. Greenhouse science is coupled to policy, since the projections have social implications. If policy practitioners are to use the results as an input to policy considerations, then the results must be credible, defensible, and ultimately useful.

The problem still remains as to how credible and how useful particular greenhouse projections should be in a science-policy context. To this end, we have broken the validation process up into four components, and listed particular questions or tests under each component. The four components are labelled scientific, utilitarian, paradigmatic, and political.

For the scientific component the overriding question is whether the projections are credible. To answer this we must ask whether the theory from which the projections derive is consistent, whether it is supported by observations and other theories or models, and whether the uncertainties are crucial in undermining credibility and in formulating the policy response.

For the utilitarian component the main consideration is whether meaningful and useful information can be derived from the projections. Certain aspects of the projections will not be useful, either because they are not credible, or because they do not match the needs or ability of the policy system to comprehend them or translate them into suitable form. By 'suitable form' it is meant that information may be presented in various ways that make its contents appear more or less tangible, meaningful, and palatable to a system that, we will suppose, processes certain information forms more easily than others. For example, local scale interpretations of global scale changes and numbers may be more comprehensible to a policy audience.

For the paradigmatic component, focus returns to the theory and tools behind the projections to ascertain whether they have widespread support. Do we have a more powerful theory than greenhouse theory to explain relevant aspects of past, present, and potential climate changes? Are GCMs the best available tools to project climate changes as a result of perturbations by greenhouse gases? Is the theory defensible? Does it have consensus support? In short, is the greenhouse paradigm or research program intact?

The final validation component is the political, which essentially boils down to: are the projections (and perceptions of what they imply) acceptable in their current form?

Having identified what we see as important questions and tests relevant to validating greenhouse change theory, we will now discuss more specifically how the questions relate to the policy and science communities. Categorization of the discussion will follow under the terms of uncertainty, credibility, and useability.

8.3.1 Uncertainty

In the policy arena uncertainties are critical and strategic in whether theory is seen as credible and forceful or not. The uncertainty is strategic, since there are political constituencies opposed (and in favour) to the perceived policy implications of the theory who will exploit the uncertainty. This exploitation takes place in an environment where many people will not necessarily understand the context and relevance of the uncertainty, and so might be shaken into thinking that the whole theory is damned (or proved).

In the science community the uncertainties are still important, except that if there is no real competing theory, then there is no obvious strong science constituency in position to exploit the uncertainty. In the science community, to win the support of others it is generally not enough to simply critique the uncertainties. Scientists will want to know what alternative views or theories are being proposed to supplant the old ones being critiqued. Otherwise, why should they abandon a reasonably successful theory? If well known greenhouse change theory critics such as Lindzen[43] are to acheive support in the science community, then they must put forward progressive views to explain past observations on which we currently rely on greenhouse theory to some degree. In the science community it is easier to place the uncertainties in perspective in relative importance to the certainties in the theory, since the broad context of the theory and what supports it is generally understood (though of course the degree of ignorance in the science community is also fairly high at times).

For policy purposes it would ideally be nice to know how long it would take to reduce the uncertainties. This is also important information for scientific research programs, though there there would be less impetus to trying to come up with quantitative answers to just how important the uncertaintes are and how long it will take to reduce them. The exercise of trying to quantify the uncertainties leads to disagreement over the uncertainties (which by their very nature are difficult to quantify), thereby compounding the uncertainty.

In the science community there is an appreciation that some of the uncertainties can not be reduced. We are stuck with them. For example, the natural variability of climate and the behaviour of volcanoes lead to variations in the temperature record which make it more difficult to extract out a greenhouse signal, and more difficult to predict just how temperature will evolve in the future. The prospects for reducing uncertainties derived from these sources are quite slim. The categorization of uncertainties (as essentially irreducible or potentially reducible) and the task of placing them in context in validating the theory lends a level of complexity to greenhouse validation which makes it more difficult for the policy system to incorporate.

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[43] Richard Lindzen is an MIT meteorologist who has received media coverage in the U.S. on his critique of greenhouse change theory.

8.3.2 Credibility

In assessing the credibility of greenhouse theory and projections it is necessary to ascertain whether models and projections agree sufficiently with observations and simpler models. This is an exercise for which it is helpful to have a grasp of the way the climate system potentially works. A knowledge of the potential role of various forcing factors and of internal variability is useful in assessing whether the observed climate response matches that expected from theory and projections. Again, the broad context is quite important, and it may be difficult to communicate the breadth, depth and relevance of this to the policy domain. For example, how does one view the Newell et al. (1989) sea surface temperature (SST) observations? They indicate essentially no trend in measurements of SSTs over the last 130 years. Is that consistent with model projections for doubled CO_2 which would imply a warming to date of between 0.4K and 0.8K (Ramanathan et al., 1987)? In the science system there are a variety of Lakatosian type positive heuristics (Lakatos, 1970) which form a protective belt around the core of a research program, and serve to maintain support for the theory in the face of observations which apparently do not support theory.[44] Firstly, in a science context it is widely recognized that the SST data itself is open to question, since the measurement techniques used in collecting the data have changed considerably and are difficult to correct for. Secondly, it is also recognized that it is not even necessary for the oceans to have warmed at all so far, and not be inconsistent with the theory. The oceans may not have warmed, since the ability of the oceans to take up heat may be at the high end of the estimated range, and so we may not see the warming at the surface yet. Additionally, the greenhouse warming expected to be realized to date (0.4-0.8K) is still small relative to natural variability and influences of other potential forcing factors. These positive heuristics derive from knowledge of the

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[44] Lakatos (1970) notes that "few theoretical scientists engaged in a research programme pay undue attention to 'refutations'. They have a long-term research policy which anticipates these refutations. This research policy, or order of research, is set out - in more or less detail - in the *positive heuristic* of the research programme. The negative heuristic specifies the 'hard core' of the programme which is 'irrefutable' by the methodological decision of its protagonists; the positive heuristic consists of a partially articulated set of suggestions or hints on how to change, develop the 'refutable variants' of the research programme, how to modify, sophisticate, the 'refutable' protective belt. The positive heuristic of the programme saves the scientist from becoming confused by the ocean of anomalies."

broader theory and climate context and may not be well incorporated into the policy realm. This makes evaluation of the theory more difficult there.

Testing agreement and disagreement between GCMs is also important in establishing credibility. That models disagree, in the modelling community is viewed mostly as grounds for improving the models, since a perfect match in all detail is not expected. For the modelling community to stay happy, there must be reasonable consistency between the 3-D models, and appropriate agreement with simpler models. In addition, those disagreements that do exist should be traceable to differences in model formulation, and should not lead to drastic differences in model sensitivity. Where the energy transport is concerned at least, this seems to be the case. Outside the modelling community however (in science and policy domains) any model disagreement is sometimes seen more in terms of the inability of science to get its act together. This has the affect of making the theory less defensible.

Defensibility of the theory and projections is crucial, since if society is going to commit policy efforts to regulating and making changes, then policymakers want to be able to defend policy decisions against critics arguing the uncertainties. In the policy arena, to defend policy decisions and the credibility of theory, policymakers will at least want to fall back and demonstrate that there is a reasonable consensus among scientists supporting the theory, and that dissent is relegated to the fringes. Views of the relative degree of consensus and dissent can thus be quite important in evaluating theory in the public mind. At the present time, examples can be found characterizing the degree of dissent over greenhouse projections quite differently. Kerr (1989) in Science magazine characterizes Lindzen's position as part of the fringe under the heading "Greenhouse skeptic out in the cold. A prominent meteorologist says the greenhouse warming will probably be a bust; experts in and out of the climate community staunchly disagree with this latest iconoclast." By contrast, Stevens (1989) in the New York Times ("Skeptics are challenging dire 'greenhouse' views") characterizes both dissenters and advocates of the theory as on the fringes of a climate community that has for the most part not made up its mind on the issue. A policymaker wishing to defend their decision to support greenhouse response policies may point happily to the Science article, but not quite so happily to the Times article.

The weight accorded to dissenters is important in science and policy realms, though probably more of an issue in the latter. Whereas a modeller might respect scientists such as Lindzen, but dismiss his critique as yet incomplete (by general science standards) and unpublished, the policy community must deal with the fact that he is an emminent scientist. Nevertheless, it is important to ensure a "fair play of ideas" in evaluating theory, tolerating majority and minority viewpoints (Clark and Majone, 1985). Clark and Majone note that the "implications of how scientific inquiry is prepared to handle minority viewpoints when working in policy contexts has not been usefully explored at any deep conceptual level." If the science community does not have a conceptual framework for approaching such problems, then the policy community can hardly be blamed for resorting to essentially a 'head count' approach. The weighing up of consensus support and relative standing of dissent among scientists by the policy community is a proxy measure of the credibility base in science. It is an attempt to quantify via informal poll among influential researchers the questions of whether the theory represents the dominant paradigm (following Kuhn, 1962), or whether the research program has been successfully replaced by a progressive shift in view (following Lakatos). Perceptions of widespread support for the theory in science provide necessary grounding for defending the theory in policy. If the science community can develop frameworks for evaluating science-policy problems, then perhaps it will be possible for the policy community to respond with more sophistication and progressively[45] change the necessary conditions for defensibility.

The net result of relative differences in assessing credibility of theory in science and policy domains is relative differences in the degree of stability afforded theory. In science, protective heuristics and a validation philosophy that takes into account a gestalt of theory, models, observations, and knowledge of the climate system is more resilient in maintaining credibility for theory in the face of threats by uncertainties and perceived

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[45] As outlined in Clark and Majone (1985), a Lakatosian progressive shift in the policy arena "may be said to be progressing as long as it succeeds in disposing of issues, i.e., in moving them from the stage of contention to a class of issues which the actors in the policy process judge to be in a state of satisfactory, if temporary, resolution."

inconsistencies. In the policy community the validation philosophy appears to be less pervasive, and so relative stability of the theory against apparently contrary evidence is diminished. The science validation philosophy may be less pervasive in the policy community for a number of reasons. It is less widely known and not institutionalized into the policy process. Continuity and focus is also lacking in the policy community, which must deal with many issues, of which greenhouse change is only one.

While the science community has a system of buffers in place that protects and stabilizes theory, the policy community requires a system of buffers that entail skepticism toward accepting any science-policy issue and related theory that enters the policy agenda. The reasons for this can be found in the point made by Lindzen (1989) that "given a week or so, I would have no difficulty generating a long list of environmental problems which although objectively exceedingly unlikely cannot be rigorously proven impossible. If each called forth a statutory response, our society would be quickly paralyzed - tied into excruciating knots by its well meaning but misplaced enthusiasm".

8.3.3 Useability

If greenhouse change scenarios are to be useful as a basis for policy, then they must be believable (otherwise they would not be defensible), meaningful (have policy implications), and not too sensitive to the residual uncertainties (or at least not so sensitive as to yield results with different policy implications). Trimming GCM output down to a level of specificity at which the scenarios are justified by this type of reasoning is a difficult task. Jaeger (1988) is an example of an attempt to do this, specifying expected changes in temperature, sea level, and precipitation in broad terms, together with outer limits at the 1/10th probability on what might occur. Scientists will not easily agree as a community over what level of detail to accept the projections, some believing that one shouldn't engage in the exercise at all.

Where scenarios or guidelines are derived from GCMs, confidence in their validity is obtained on the basis of physical plausibility. For instance, Hansen et al. (1989) note that the frequency and severity of droughts increases in greenhouse change simulations with the GISS GCM. Though drought is a regional phenomenon and the GCMs are generally not regarded as being reliable for regional forecasts, the underlying physics behind drought intensification seems fairly robust. In a study with the GFDL GCM, Manabe and Wetherald (1987) note that warmer temperatures in continental midlatitudes lead to an earlier occurence of the snow melt season followed by a period of intense evaporation. This leads to a CO_2 induced reduction of soil moisture in summer. Enhanced summer dryness leads to a reduction in low cloud amount and precipitation, which increases both the solar energy reaching the continental surface and the potential evaporation. Both the decrease of precipitation and the increase of potential evaporation further reduce the soil moisture during early summer and help to maintain it at a low level throughout the summer. Hansen et al. note that there are other factors that influence the location and timing of droughts such as atmospheric longwave patterns and ocean temperature distribution. These factors are not necessarily well simulated in the GCMs, and introduce uncertainty into regional forecasts of exactly where drought behaviour will change. Nevertheless, the physical processes leading to intensification of droughts in a general sense (averaged over time and location in midlatitude continental areas) are manifestly plausible, and confidence is thereby associated with the prediction.

Greenhouse change theory and projections are used in science and policy communities slightly differently. In science the actual projections and the level at which they are accepted are not quite so important. What is important is whether we can learn more about the system with the theory (as conceptual and numerical models) than with alternative models. The theory is useful in the sense that it provides a superior tool for learning about the climate system and climate processes. In policy, the projections are useful at some specific level if they have broad support in the science community, and clear and meaningful implications whereby climate change may be related to social change.

Though many of the questions asked by the science and policy communities are common, the emphasis and extension eventually diverges. In science, validation in terms of the most successful paradigm or research program affords theory a degree of stability, while still leaving it quite open to attack and lively dissent. In policy, the projections are validated more in terms of whether they are defensible against attacks of being arbitrarily based. In the face of political opposition, it must be clearly demonstrated that the projections are reasonable and not arbitrary. A way of doing this seems to be via showing that the consensus view supporting the projections is dominant, and that the likely impacts have policy implications. If this is satisfied in substantial part, then the issue presumably remains on the policy agenda.

8.4 Greenhouse change characteristics

Assessing the validity of greenhouse projections according to criteria such as credibility and usefulness is made difficult by the fact that we as a society do not seem to know how to make decisions about determining credibility in a manner that takes into account the characteristics of complex issues like greenhouse change. The development of a critical capacity for the appraisal of scientific enquires with policy implications is still in its infancy (Clark and Majone, 1985) The nature of the uncertainties surrounding the greenhouse issue alone seems to defy our ability to assess the relative merit of the projections using conventional frameworks and notions about uncertainty and credibility. We will outline various characteristics of the greenhouse problem and consider how they might contribute difficulties for assessment and resolution of the problem at the policy level.

The greenhouse problem is global in cause and effects, but the contributions to the causes derive from many different social sectors (there is no one culprit) and countries. Cooperation between industry and government and between countries is required (unilateral action is insufficient), but this is made difficult when the effects and costs and benefits of action are different for different regions. The perception of 'winners' and 'losers' compounds this difficulty.

Greenhouse change involves large systems, both the socio-economic and the ecological, so the inertia of the systems becomes important. Lags in the climate response make it difficult to validate the projections and to prove that the issue is serious. Meanwhile, delay in industrial response means that greenhouse gases continue to build up. If the climate sensitivity is large, then the stored response will grow bigger with time, and it is largely irreversible.

In a methodological sense, Science (to avoid refuting all theories at all times) is forced to build resiliency and stability into the evaluation of theories. Policy (to avoid accepting all issues at all times for its agenda) is forced to build instability into the evaluation of theories so that only the most well supported issues survive by virtue of the overwhelming evidence supporting them, or by way of political acceptance of the implications of the issue.

The impacts of greenhouse change can appear both small and distant. The global temperature changes forecast sound like small numbers to the public, and impacts occuring next century and beyond are difficult to factor into current decisions. The results of the problem are potentially severe, but the activities causing the problem are not viewed as reprehensible. People in industrialized countries seem to view large scale fossil fuel energy consumption as quite normal, and the consequences of this behaviour are unintended.

There are no sufficient tests of greenhouse change, only necessary ones, which can be exasperating to those seeking certainty to act upon. The uncertainties are severe and will be slow to be reduced. The complexity and uncertainty of the issue allow respectable scientists to take opposing views. This is confusing to the public.

The greenhouse projections rely in part on complex climate models which are difficult to interpret and validate, and expensive to run. Few groups and nations have GCM modelling capability. This expertise is confined to a few small isolated groups. Policymakers are forced to rely on a relatively small community of modellers, which makes it harder to make the case that the projections are well tested.

The net result of these difficulties is an apparent mismatch between the problems faced by scientists in evaluating and presenting information on greenhouse change and its credibility, and the needs of the policymakers. The policymakers would like to defend their policies with some surety and support, describe the problem comprehensively, and give concrete indications of the implications of not acting. Meanwhile, they are operating in an environment in which the risk perception is low or distant, and the perception of the costs of action is high and action difficult to negotiate, while perceptions of benefit from effective policy responses are low. With much political inertia and a divided community, uncertainties are apt to be exploited rather than incorporated critically. Much of the uncertainty cannot be reduced however, so both scientists and policymakers are stuck with it, and must learn to incorporate it into issue evaluation.

Reappraisal of the evaluation of science-policy issues can draw upon potential inherent in both science and policy communities. For the part of the scientists, they can change the way they research and present material so that it more effectively matches the needs of policymakers, and is more effective in providing validation information in policy relevant terms. The science-policy community can work on setting critical evaluation standards that incorporate uncertainties, minority viewpoints, and other evaluation dimensions following Clark and Majone. The policy community must be geared up to playing a role in setting evaluation standards so that they will be useful once applied in the policy arena. Whether it will be necessary for the political framework to undergo some form of transformation or paradigm shift if it is to be able to come to terms with the challenges presented by greenhouse change is left as an open question.

In further discussion here, we are going to confine the focus to the role played by scientists in validating greenhouse change. We return to one of the problems identified in the framework of communicating validation information from science to policy communities. In the next chapter we will try to show that the scientists are not doing a good job uniformly in presenting validation information in a comprehensible manner.[46] We further suggest that this leads to unnecessary confusion in the public debate over validation of greenhouse change. What we would really like to know is

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[46] It should be stressed that the next chapter is not an attempt to illustrate the entire framework, but to address one of the communication questions identified in the framework (Is validation information being presented well?) in so far as it relates to one of the validation components in the framework (the scientific component viewing validation in terms of credibility).

whether this makes any difference. i.e. What is the marginal benefit of improving the way science validation information is presented to the policy arena? Would a clarification of uncertainties and validation evidence help in forcing a paradigm shift in the policy arena? In an extreme hypothetical case: if the evidence validating theory was persuasive for all scientists (dissent being non-existent among scientists with standing) and the uncertainties essentially zero (but political inertia and opposition were still present), would this make a difference to the policy evaluation and response?

For now we can only suggest avenues via which an improved presentation of the science might make a difference. In the validation framework we described the outcome of the policy validation process as an evaluation of likely impacts given by the mid-range scenario times the probability of the scenario times the political acceptability. By science presenting non-conventional, but possible outcomes as well as the mid-range scenario, and comprehensible stories of how they come about, the fragility of the conventional scenarios might be appreciated in context. By presenting validation evidence clearly and in context, and outlining sources and implications of real disputes in the scientific interpretation, the probability of the scenarios could be gleened more realistically. Less room would be available to needlessly diminish or enhance the probability estimates by carrying out debates over non-issues. If debate can be minimized to real issues only, then people are likely to be less confused, and the probabilities they associate with the scenarios are likely to fluctuate less. If the presentation of the science is more effective in supplying useable information to policymakers so that the impacts and policy implications are clearer, then the scenarios may be more acceptable.

If validation information can be presented clearly, comprehensively, and faithfully, then the risks and uncertainties associated with the scenarios can be appreciated in their appropriate context. This will be important information for the policy system if it is, or can be, sensitive to the relative degree of risk and uncertainty.

9 GREENHOUSE CHANGE VALIDATION PRESENTATIONS

9.1 Introduction

In this chapter we take up the question of how well information relating to the validation of greenhouse change is being presented from the scientific community to the policy community. We will not consider greenhouse validation presentations with regard to all validation components outlined in the previous chapter (scientific, utilitarian, paradigmatic, and political), though we should in time. For now we will consider validation mostly in terms of the scientific component, which involves testing whether greenhouse change theory and scenarios are credible.

The problem of how science in general relates to its broader social context is complex, and has been outlined by Ravetz (1971). To keep this study manageable in answering the validation question we are going to concentrate on subsets of science and policy which we see as integral to the translation of validation information from science to policy. The subsets chosen are the science journal literature (science output) and interpretation of this in the media (policy input). The original and most sophisticated of sources of scientific validation information is the science journal literature. Successive fora for communication beyond the scientific journals (popular articles, interviews, etc.) tend to become increasingly simplistic relative to the sources of information in the journals. If the validation information is not presented well in its most sophisticated form, then the validation problem will be made successively more difficult in other fora. With greenhouse change being a topic of current concern, the journal literature is frequently reported on directly in the press upon publication. Interpretation of the literature in the press is influential in shaping public perception of whether greenhouse change theory has been bolstered or eroded by the latest research results. Policymakers are part of the media audience, and the policy process is influenced by public perceptions.

Much of the recent scientific and public debate on validating greenhouse change has concentrated on whether or not it is possible to detect greenhouse change signatures in the climatic record to date. The role of the

1988 U.S. drought received attention in the public debate in this regard. Interpreting the 1988 U.S. drought in the context of greenhouse change validation is not conceptually difficult. In response to long term sustained forcing of the climate by greenhouse gases, events such as the 1988 U.S. drought are likely to happen more often. This does not say that the 1988 U.S. drought is conclusive evidence for greenhouse change, nor does it say that the 1988 U.S. drought is unrelated to greenhouse change.[47] Yet, misinterpretation of scientific information such as this is distressingly common. Schneider (1989) notes that the trial by media of greenhouse change and the drought was a non-scientific issue from the very beginning. By analysing some of the journal articles that were cited by the media in the 1988 drought non-issue debate, we hope to show here that misinterpretation of validation information is in part related to the way the scientific literature is presented. The scientific community is writing journal papers with policy implications that are being picked up in the press without providing adequate context for that audience. In short, greenhouse change validation information is not being presented well.

Scientists wield influence in the public domain by presentation of relevant results on contentious public issues, regardless of whether such influence is intended or desired. Being in such a position, the minimum obligation incumbent upon scientists is to inform the public debate in a manner consistent with the nature of the relevant body of scientific knowledge. We argue here that part of this obligation can be met by placing the implications of scientific research in context appropriate to a broader audience than that of the immediate field at hand.

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[47] For instance, in a particular region of the U.S., droughts of a particular severity (exceeding some arbitrary measured threshold) may occur say 10 times in a hundred years on average. If greenhouse warming increases the likelihood of drought occurence in the region, then severe droughts may now occur say 15 times in a hundred years on average. We cannot however separate out the 5 additional droughts from the 10 that would have occured anyway (in the absence of a change in climate forcing). It is thus not possible to conclude that a particular drought is caused by greenhouse warming, or that a particular drought is unrelated to greenhouse warming.

9.1.1 Approach

We will frame the problem of translating scientific knowledge from the scientific to public arena largely in terms of communication. With this construction, poor translation occurs due to communication barriers across or within the different arenas. Scientists, science journalists, and external influences on their communication channel are all involved to varying degrees in contributing to poor translation of scientific results. Further, scientists, the media, politicians, and the general public all interact in a complex manner in the formulation of public policy. In this work, we do not attempt to articulate what the role of each group is, nor the extent of their influence or interaction, though these are certainly interesting questions to take up. We will however, begin to address the role of scientists through the expression of their research results in the journal literature, and look particularly towards how these results are interpreted in the media. Most of the examples used here will be of journal papers that elicited direct coverage in the press. The papers have been chosen for their relevance in clarifying the themes presented. We have not attempted to do a survey of the greenhouse change literature. Media interpretation of the papers has been considered mostly in the U.S. We leave open the question at this stage as to whether science presentation and media coverage of science is significantly different in other countries.

To simplify analysis initially, we will make the assumption that science journalists and the media are reasonably competent and faithful in their attempts to interpret scientific information. We will not consider in any depth here problems at the media end of the communication funnel that hinder or distort translation of information. The structure and organisation of the media can lead to significant distortion of information. Herman and Chomsky (1988) outline this with respect to communication of U.S. foreign policy. Potential sources of constraints on the media, and the media's role in shaping the communication of scientific information will be left for later work. By focusing here largely on scientists, we intend to make a first cut on the problem of communicating scientific results effectively. This is not intended to be wholly conclusive, but to provide a basis for further consideration.

9.1.2 Background

Detection and validation of any greenhouse induced climate change presents a conundrum. Wigley and Jones (1981) articulate this as follows: "The effects of CO_2 may not be detectable until around the turn of the century. By this time, atmospheric CO_2 concentration will probably have become sufficiently high (and we will be committed to further increases) that a climatic change significantly larger than any which has occurred in the past century could be unavoidable. To avert such a change it is possible that decisions will have to be made (for example to reduce anthropogenic CO_2 emissions) some time before unequivocal observational 'proof' of the effects of CO_2 on climate is available." In light of this, establishing high confidence in detection of greenhouse change may well be irrelevant to the formulation of policy. That is, a point may be reached at which policy decisions do not depend on resolution of residual scientific uncertainty. For now at least, the policy debate on greenhouse change has not reached this level (whether it should have or not), and communication of increments of scientific knowledge on greenhouse change is relevant to the policy response.

The presentation of evidence within a framework in which response may be required prior to conclusive resolution of the science is bound to be contentious. Furthermore, until modelling studies of the transient response to greenhouse gas build up over the last one hundred years are carried out with three-dimensional climate models including realistic representations of ocean circulation and mixing, good quantitative indicators of the expected strength of greenhouse change to date will not be available. Without good quantitative numbers, there is plenty of scope for disagreement over whether the change expected from theory has occurred or not. Nevertheless, some guidance for presentation of theory is still possible.

In recent years, greenhouse change has received increased attention, not only in the scientific community, but by the general public, and in the political sphere as well. This widening of attention from within the scientific community to without has meant that greater import is now attached to the presentation of scientific results in the area. Two of the recent papers bearing on the detection or lack thereof of greenhouse effects in climatic data

(Hanson et al., 1989, and Trenberth et al., 1988)[48] have received high profile coverage in the U.S. national press (New York Times, 1989a, and New York Times, 1989c). Earlier papers, such as those by Hansen et al. (1981) and Kukla and Gavin (1981) also received prominent newspaper coverage (New York Times, 1981a, and New York Times, 1981b) in bringing the greenhouse issue to the public.

That the general public and policymaking community will be digesting papers on greenhouse change like those cited above, or more correctly, interpretations of the papers, places an additional burden on the papers to provide the appropriate context for the work therein. Scientific results and their manner of presentation do influence public perception and policy makers opinion in the area of climate change.[49] Greenhouse climate change scenarios have serious social and environmental implications, and it is vital that the public debate on the issue be an informed one. This requires an understanding of the relevant context of the research. While much of the context is known to the community of climatologists, it is generally not well known (as yet) outside this community, and the failure to provide it creates unnecessary confusion in the public sphere. The papers cited above are representative of many papers in the literature that have created such confusion for the lack of appropriate context. As such, it may be instructive to go through them in turn and consider where or how confusion was created, with a view to avoiding this in future. Note that this is not an attempt to challenge the research or essential conclusions in these papers, for there appears little reason to conclude that it isn't satisfactory. In fact, the groups presenting the research in these papers are among the most thorough and careful in the field. Rather, it is the presentation of the results that will be

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[48] Our selection of examples is skewed toward papers interpreted to be in conflict with greenhouse change theory, or at least interpreted as not supporting present expectations. We can speculate on why these examples may be easier to find than examples supporting theory. Clark (personal communication) notes that the greenhouse change journal literature has been subject to fairly good peer review over quite some decades now, resulting in tight and careful presentation. When the peer review process has had sufficient time to critique theory, then presentations advancing the theory may be fairly refined, and less subject to incautiousness.

[49] For example, it is unlikely that the U.S. ban on chlorofluorocarbons as aerosol propellants would have occured as early as 1978 without the presentations of Rowland and Molina and their peers in the atmospheric chemistry field.

considered; that which was presented and how, that which was not, and how it was interpreted in the press.

9.2 Discussion of scientific journal papers on or implicated in detection of greenhouse change

With some waxing and waning of attention, scientific papers bearing on detection of greenhouse change have received coverage in the press throughout the 1980's. Illustrative of earlier papers this decade outlining greenhouse change theory is Hansen et al. (1981) in a paper entitled 'Climate impact of increasing carbon dioxide'. In describing the potential consequences of global warming, this and other papers quite rightly outlined the probable consequences of global warming, and the less probable, but sometimes potentially catastrophic impacts. It is crucial that less probable, but potentially disastrous consequences be factored into the policy debate on greenhouse warming, lest we have the hubris to defy Murphy's Law, and assume that only the most probable outcome will occur.[50] However, in doing so, it is also important to be careful about how such possibilities are presented. Lack of attention to detail in presentation can result in an overemphasis on 'gloom and doom' scenarios to the detriment of the overall debate.

For instance, Hansen et al. (1981) present the possibility of sea level rise associated with warming in the vicinity of the West Antarctic ice sheet as follows. "Danger of rapid sea level rise is posed by the West Antarctic ice sheet, which is grounded below sea level, making it vulnerable to rapid disintegration and melting in case of general warming. The summer temperature in its vicinity is about -5°C. If this temperature rises ~5°C, deglaciation could be rapid, requiring a century or less and causing a sea level rise of 5 to 6m. If the West Antarctic ice sheet melts on such a time scale, it will temporarily overwhelm any sea level change due to growth or decay of land-based ice sheets. A sea level rise of 5m would flood 25 percent of Louisiana and Florida, 10 percent of New Jersey, and many other lowlands throughout the world." If the West Antarctic ice sheet were to contribute to sea level rise, the

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[50] From Clark (1985), "it is generally accepted that among the greatest blunders of military and political analysis is focusing on what one's adversary will probably do, to the exclusion of what he might."

important points are how much, and how fast. Both pieces of information must go together to be meaningful in impact evaluation. This information is in the paper. The paper tells us that the sea level rise would be 5 or 6m over a time period of a century or less. The West Antarctic ice sheet would take of order one hundred years to melt if conditions for melting were attained.[51] It is thus quite important to distinguish the above statement from one that implies that the sea level could rise 5 or 6m at any time within the next century.

Unfortunately however, scientists occasionally use certain terms with particular meaning in a scientific context that can lead to confusion in other contexts where the particular meaning is not understood. For example, Hansen et al. (1981) use the word 'rapid' three times in the above quote to refer to deglaciation and sea level rise. On geological time scales, a change in sea level or glaciation over a period of one hundred years is indeed incredibly rapid. The general public however, do not generally think on such time scales, and rapid in the public context implies time scales probably a couple of orders of magnitude less than one hundred years. In the press, this inevitably led to initial confusion, and eventual decoupling of the potential change with the associated rate of change in sea level. New York Times (1981a) interpreted the paper of Hansen et al. in a way that already made the association between the amount of sea level rise and its rate of change ambiguous, as follows. "The seven atmospheric scientists predict a global warming of 'almost unprecedented magnitude' in the next century. It might even be sufficient to melt and dislodge the ice cover of West Antarctica, they say, eventually leading to a worldwide rise of 15 to 20 feet in the sea level. In that case, they say, it would 'flood 25 percent of Louisiana and Florida, 10 percent of New Jersey and many other lowlands throughout the world' within a century or less." Does this imply that the flooding would occur over the course of a century, or simply sometime within the next century? Two months later, in a New York Times article covering another paper in the journal Science on greenhouse change (Kukla and Gavin, 1981), the association between 'how much rise' and 'how fast' was already lost in the public arena. New York Times (1981b) closes

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[51] More recent estimates since Hansen et al. were writing have relaxed the time required to melt the West Antarctic ice sheet (if it were to melt) to closer to two hundred years. Semi-apocalyptic references to the West Antarctic ice sheet have now virtually receded from discussion, though this took quite some time to come about.

with the paragraph "The consequences of rising carbon dioxide concentrations have been a matter of intense debate. Some contend that the resulting warming would melt and dislodge the West Antarctic ice sheet, raising sea levels 15 to 20 feet."

Up until recently, the public debate on greenhouse change has been characterized by similar references to possible rapid sea level rise, without qualifying statements on the period over which it would take place. Of course, a 5 or 6m rise over 100 years would still be disastrous, but it does not quite engender the same gloom and doom as perceptions of an instantaneous 'rapid' rise. We do not imply that the paper of Hansen et al. (1981) is responsible for this shaping of the debate, but that it is illustrative of the type of presentation that could contribute to it. Similarly, we turn now to more recent papers, illustrative not of the potential for creation of unwarranted gloom and doom, but of the potential for the unwarranted toning down of greenhouse change theory.

The paper by Hanson et al. (1989) titled "Are atmospheric 'greenhouse' effects apparent in the climatic record of the contiguous U.S. (1895-1987)?", pertains directly to the detection of greenhouse change. The paper makes an implicit leap into the policy realm by writing on this issue, and a more explicit one by choice of title, and by setting up NASA climatologist James Hansen as a 'straw-man' in the introduction. Hansen's testimony to U.S. Congress is quoted in the introduction to the paper as follows: "the global warming is now sufficiently large that we can ascribe with a high degree of confidence a cause and effect relationship to the greenhouse effect..." Whether one agrees with this or not, Hanson et al.'s paper alone (or without supporting references) is not of sufficient breadth to refute Hansen's statement. As climatologists well know (but not necessarily the press), and as Hansen pointed out in response to the paper in the press, the contiguous United States cover only 1.5% of the surface area of the earth, and we would not expect such a small area to be even close to representative of the globe. Only one extra sentence would have been required to point this out. By quoting Hansen and then failing to place the paper's research findings in the proper context, unnecessary confusion was created in the public sphere (see for example Reuters coverage of the paper [Boston Globe, 25 Jan 1989]). Reuters were so unaware of the relevance of the paper to the detection of greenhouse change that they printed the following: "Their findings contradict suggestions by other scientists that the global climate is gradually warming and rainfall is declining because of the so-called greenhouse effect." New York Times (1989a) did not confuse the meaning of the paper in the same way, but it was clear that they had spoken in some detail with one of the authors about its implications. As Reuters showed on this occassion, not all reporting agencies will trouble themselves to do that, and Reuters influence on public opinion is probably more pervasive than that of Geophysical Research Letters (where Hanson et al. was published).

A further source of distress in Hanson et al. is that the paper makes no reference back to the areal distribution of temperature changes in those works whose interpretation it casts implicit aspersions on, namely Hansen and Lebedeff (1987) and Jones et al. (1986,1987).[52] While both these global data sets show a global warming trend over the last one hundred years; for the contiguous United States, Hansen and Lebedeff (1987) show an insignificant trend, and the Jones et al. (1987) data for the past 40 years shows cooling for the eastern U.S. and warming for the western U.S.[53] Thus, not only is the data of Hanson et al. not representative of global trends, but it doesn't even contradict the available indications of regional U.S. trends in the global data sets that do indicate global warming. This is important contextual information that should be included in the paper.

Along similar lines, the GFDL model predictions for precipitation changes over the United States are referenced in Hanson et al., and then the conclusion of no significant precipitation trend in the observations is presented. This represents a failure to place the theory in proper context, resulting in the creation of a straw-man of theory (or 'straw-theory' if you will). The reader is not told, but deserves to know, that regional precipitation forecasts from current models are highly provisional and uncertain, and that

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[52] In this case, even from a purely scientific point of view, one has an obligation to note recently published data covering the same region.

[53] In addition, but less representative, Angell (1989) has analysed global temperatures over the last thirty years. At the surface, Angell's data shows a marginally significant warming trend for the globe, but no trend for the north temperate region (within which the U.S. lies).

there are substantial differences between predictions from different models (Schlesinger and Mitchell, 1987).[54] Quoting predictions of regional precipitation trends for the U.S. for comparison is fine. It is important to recognize however, that the degree of precision offered in the paper goes well beyond the present consensus. The Villach and Bellagio workshops for instance, offered a consensus scenario of "perhaps, a decrease in summer rainfall in the mid-latitudes" (Jaeger, 1988). Note further that that was for a doubling of carbon dioxide in the atmosphere at equilibrium; a situation we are yet to attain, and which is not expected until about the middle of next century. Without some form of caveat on the comparison of one model group's detailed predictions for doubled carbon dioxide levels with pre 1988 climatic data, a much stronger refutation of greenhouse theory is presented to the policy level discussion than is justified.

A paper concerned with whether greenhouse change is apparent in the data should treat the expected greenhouse change from theory with the same sophistication that is afforded the analysis of the data. The paper by Hanson et al. is systematically deficient in this respect. Both the implicit comparison with Hansen's data, and the explicit comparison with the GFDL precipitation predictions presented the theory in an overly simplistic manner, and failed to provide context for the work. Moreover, the systematic deficiencies in the paper all have the affect of impugning greenhouse change theory. It is healthy to be critical of any theory, but critical debate is not strengthened by setting up straw-men. In short, the approach to the policy implications of the research in the Hanson et al. paper is misleading.

Trenberth et al. (1988) presented a plausible mechanism of causally related events that were associated with the 1988 North American drought. The development of the Midwest United States drought in particular is discussed in relation to tropical sea surface temperature (SST) patterns, and displacement of the jet stream over North America. In setting this work in context, the authors point out that "the greenhouse effect may tilt the balance

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[54] Though speculative, there may be simple physical mechanisms that support the GFDL model predictions for a decrease in precipitation in the U.S. If such mechanisms are relied upon here, then they should be mentioned, and any reliance upon them made explicit. Note also, that even for different runs with the same model, there may be significant differences in precipitation distribution.

such that conditions for droughts and heat waves are more likely, but it cannot be blamed for an individual drought." Though seemingly adequate in setting context for the study relative to greenhouse theory, this provided only half the picture as we shall see. On the one hand, an individual drought does not represent conclusive evidence that greenhouse change is with us. On the other hand, the fact that we can explain any drought on the basis of antecedent land, atmosphere and ocean conditions means that, on the basis of this evidence, we cannot dismiss the 1988 U.S. drought and greenhouse change as unrelated.

The greenhouse reference in Trenberth et al. served only to attract people's attention, and then confusion reigned again. New York Times (1989c) for instance, gave the paper an emphasis that belied an inability to interpret the paper's relevance to greenhouse theory. Their 'Science Times' section ran with the heading "Scientists link '88 drought to natural cycle in tropical Pacific," subheading "Greenhouse effect was not the culprit this time, researchers agree," and introductory paragraph "Last year's killing drought in the United States was caused by massive, naturally occurring climatic forces in the tropical Pacific Ocean and had little to do with global warming caused by the greenhouse effect, according to new evidence." The Scientific American (Burnham, 1989) presented Trenberth et al. similarly to the Times, attributing an unwarranted role in greenhouse change detection to the paper.

Though the paper of Trenberth et al. doesn't actually say anything wrong, it was set up to be misinterpreted in the above manner. The leap from Trenberth et al.'s explanation of the 1988 U.S. drought to the notion that the drought is unrelated to greenhouse change occurs in two steps. First, Trenberth et al. offered an explanation for the drought in terms of natural phenomena, but didn't say that such explanations always hold and that one cannot separate the 1988 U.S. drought and greenhouse change on this basis. By failing to provide the latter information, the second step follows whereby the leap is made that the U.S. drought and greenhouse change are unrelated. This leap occurs on interpretation by others, while Trenberth et al. never actually make it themselves. In effect, Trenberth et al.'s presentation leads science journalists and other interpreters to the edge of a cliff, and then the interpreters do the work of throwing themselves and their readers off.

Following Trenberth et al.'s paper in Science, Palmer and Brankovic (1989) published a similar paper in Nature titled 'The 1988 US drought linked to anomalous sea surface temperature.' The style of Palmer and Brankovic is identical to Trenberth et al. The 'greenhouse effect' is mentioned in the first line of the paper, but no subsequent attempt is made to place the research in the paper in full context relative to greenhouse change. Quite predictably, the notion propagated on interpretation of Trenberth et al. that the 1988 U.S. drought and greenhouse change are unrelated received a further boost from Palmer and Brankovic's paper. The American Meteorological society (AMS, 1989) commented on Palmer and Brankovic under the heading "Don't blame 1988 drought on greenhouse effect, study says." The AMS short piece concluded with the following. "Their study supports the conclusions of many U.S. climatologists, expressed in such forums as the National Climate Program Office's Strategic Planning Seminar held last November, that the drought probably arose from natural causes, a simpler and more logical explanation than the greenhouse effect." Namias (1989), writing in Nature on the Trenberth et al. and Palmer and Brankovic papers articulated the nonsense leap made from these papers more clearly than most. Namias closes with the sentence "One thing, however, is absolutely clear: the drought was a consequence of normal atmospheric variability, and has no connection whatever with the greenhouse effect."

To stem the tide of confusion, it was left to James Hansen (In These Times, 1989; New York Times, 1989b) to place the paper of Trenberth et al. more directly in context thus: "Every drought can be related to concurrent and antecedent climatic factors such as the position of the jet stream, soil moisture, snow cover, and ocean temperature patterns. That will be true even if the greenhouse effect greatly increases the frequency and severity of drought." In a strictly scientific sense, the papers of Trenberth et al. and Palmer and Brankovic are virtually irrelevant to the detection of greenhouse change, since greenhouse change or not, one could always explain droughts in the manner of Trenberth et al. It is possible that the reference in Trenberth et al. to greenhouse change was an attempt to lay to rest the public belief that the 1988 U.S. drought represented unequivocal evidence for the arrival of greenhouse change. While that belief is indeed an unfortunate distortion of the public debate over greenhouse change, so too is the misinterpretation of Trenberth et al. that the 1988 U.S. drought and global climate change are unrelated. If global climate is changing in response to long term changes in greenhouse gas forcing, then this will be manifest in shifting statistical outcomes by changes in the frequency or intensity of some events in some places. Simply put, drought will be a more likely occurrence in some places. One cannot rule out the plausibility of relationships between greenhouse climate change and particular drought occurrences (even the 1988 U.S. drought) on the basis of Trenberth et al. and Palmer and Brankovic.

A more direct statement on the research of Trenberth et al. and its implications for greenhouse theory such as those above, if contained in the paper, might have prevented the confusion that the New York Times, Scientific American, AMS, and Nature interpretations propagated.[55] Of course, a science journalist could choose to ignore the relevant contextual information, but they deserve at least the information to make that decision for themselves.

Knowing what level of context is required in scientific papers that reach a broad audience is difficult. In the aforementioned Trenberth et al. and Hanson et al. papers, the confusion the papers created was probably foreseeable with some thought, so the simple answer is to provide more context to avoid the common or likely sources of confusion or misinterpretation. In many cases however, it will be unclear as to whether the press will view the paper as relevant, such that one needs to include much contextual information. Even then, it might still be unclear as to where confusion might stem, and what would be required to address it.

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[55] In placing the research more directly in context in papers, there is no reason why one shouldn't use in addition alternative methaphors (Schneider, 1988) that are more easily understood by a broader audience. In Trenberth et al. for instance, one might liken the relationship between the drought, the greenhouse problem, and jet streams to that between nuclear reactor accidents, lack of adequate safety standards, and cracks in the containment vessel. The lack of adequate safety standards in nuclear plants leads to conditions more conducive to the occurence of accidents, however, any given accident can always be explained in terms of cracks in the containment vessel and such like. Similarly, greenhouse change might lead to conditions more conducive to the occurence of droughts in some areas, however any given drought can always be explained in terms of jet streams, SSTs, and such like.

Angell (1989) is probably an example of a paper in this grey area. This paper, "Variations and trends in tropospheric and stratospheric global temperatures, 1958-87," has relevance to detection of greenhouse change. In the concluding section of the paper, the evidence for and against greenhouse change as analysed in the paper is laid out. The author then concludes that "In view of the pros and cons listed above, I believe it is premature to state categorically that a greenhouse effect is already being observed." The question presents itself as to whether this statement and the paper required further context outlining the limitations of what one can conclude from a thirty year data set. Predicted warming due to greenhouse gas increases will appear as a climatic variation on approximately this time scale and longer, and one probably doesn't have sufficient information on natural fluctuations or climatic noise to separate out greenhouse effects in a thoroughly convincing manner. Kelly et al. (1982) note that "variables for which only 20 or 30 years of data exist are of little value on their own as candidates for detection of CO_2 effects." Thus, on the basis of a thirty year data set, it is unlikely that one could ever make the categorical statement that the author was not prepared to make. Rather, the paper could provide supporting evidence, albeit fairly strong, and should be looked at in conjunction with other longer period data sets. The big picture might suggest conclusions not implied by the smaller one. These sorts of considerations are self-evident to a climatologist, but those outside the field need more information to begin thinking in this manner.

This attention to Angell (1989) may seem to give unfair emphasis to an isolated statement in the paper. However, it would be naive to assume that such statements directly addressing the greenhouse issue would not be picked up in the press and perhaps subject to overly simplistic interpretations other than those intended. At the level of the scientific literature it is far better to err on the side of sophistication and too much context, than to oversimplify too early, as with the present trend in papers. If this trend continues, it will become increasingly difficult to conduct an informed policy debate.

9.3 Characterization of the media coverage

Before proceeding to consider implications of the above examples for the writing of science journal articles, we will say a few things about the media coverage in general, since it also bears on this.

In coverage of the science journal papers reviewed here (and elsewhere on other topics), the media display a particular salient characteristic with several common manifestations that scientists should be aware of in writing journal papers on issues undergoing current public scrutiny. That characteristic is the tendency to present coverage of a paper attaching greater import to the story contained in the paper than is probably justified. The media seem to want to validate theory by applying what Lakatos (1970) calls 'instant rationality'. The media frequently interpret new results as 'crucial experiments' that can refute (or confirm) a theory instantly. In Lakatos' view of science "*there are no such things as crucial experiments*, at least not if these are meant to be experiments which can *instantly* overthrow a research programme." For those papers that might be classified as in some way at odds with conventional theory, the story of the conflict with the theory tends to be overemphasized. Thus Hanson et al. and Trenberth et al. are presented as significant setbacks for greenhouse change theory. Similarly, the media also tend to overemphasize individual observations that are consistent with the theory. In doing so, the media sometimes creates the impression that the single observation proves the theory. This occurs as a media response to observations such as a hot summer locally, or record global warmth in one year. In these cases, the misinterpretation usually stems from channels of communication other than directly from the science journal literature.[56] That is, through weather service summaries, interviews with scientists, or even just plain every day observation and experience of local conditions. We have been unable to find good examples of direct coverage of science journal literature whose presentation resulted in misinterpretation such that a single observation was interpreted as proving the theory.

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[56] For instance, Schneider (1989) notes that no atmospheric scientist he is aware of has ever made a statement attributing the 1988 U.S. drought to greenhouse change.

For those papers that might be classified as supportive of theory, the story of the potential impacts or consequences of theory tends to be over emphasized. Thus, greenhouse change is associated with rapid sea level rise, new drugs with miracle cures, superconductivity with the promise of a transport revolution, cold fusion with an end to greenhouse problems, and so on.

Since our concern here is on the science journal literature, we will be content to simply note the above for now without further exploration. Furthermore, we will not consider more subtle characteristics of the media and their effects on science coverage beyond the few salient features covered in this section, since that is beyond the scope.

9.4 Science presentation standards

9.4.1 General guidelines

In determining guidelines for the presentation of scientific research on issues relevant to the formulation of public policy, we pose several questions for scientific authors to consider. The author must first decide whether the issue being presented has significant public policy implications or not, and if so, whether the policy debate is still sensitive to the presentation of relevant scientific material. If both these criteria are satisfied, then additional care in presentation is warranted. We have argued that the issue of greenhouse change satisfies both these criteria, but this is also true of many other issues. On some issues, the second criterion is not met, and the standards advocated here for communication with the public sector are not as important. One might argue for example that studies of the health effects of leaded petrol have public policy implications, but that the public policy with respect to leaded petrol is essentially in place now (in the U.S.), and unlikely to be changed by anything but radical new results.

The assessment of whether the above criteria are met or not may not always be clear. In such cases it probably makes sense to err on the side of supplying additional appropriate context for communication with a broader audience, since the cost of doing so is minimal, and the insurance gained against possible creation of confusion in the public sphere is worth having.

In considering whether a particular piece of research is relevant to public policy issues or not, it is important to keep in mind whether it might be construed as relevant, even if it is not. In such cases, one should be clear in presentation that the material is not relevant to the issue, particularly if the issue is mentioned at all in the paper.

Once the decision to provide context appropriate to a broader audience is made, one must decide just how much additional context is necessary. This point was broached earlier. A loose guideline is that the competent reader from outside the field at hand should understand the intended implications of the research (no more, and no less) from a single careful reading of the paper. This guideline is set in this way to attempt to describe the position of the majority of journalists or science journalists who might interpret the paper for the public. This does not mean that such readers should understand all the details of the research; only that they be clear about its implications.

The decision as to how much context to give is also moderated by the choice of journal of publication. The simplest guidelines are probably the best, and these would be that the more widely read the journal, the more context for a broader audience should be supplied. In particular, the journals Science, and Nature are closely scrutinized by the science press, and so more attention should be given to context in presentation of articles appearing therein.

In reference to the characteristic of media coverage to generate a story, scientists should consider whether their paper will be considered as supportive of theory or at odds with theory. This is a somewhat artificial construction, since most papers usually contain results that work both ways to some degree. What is meant here then, is to consider that information in the paper which might be spotlighted by the media, and how it would be construed in relation to theory.

If the paper is supportive of theory, then the potential for the media to overemphasize the impacts or consequences of the theory exists, and more attention should be given to supplying context in this area. Scientists describing theory should also be aware of the tendency of the media in subsequent coverage to present individual observations as proof of theory. To help the media overcome this tendency, it might be useful to devote further discussion in the paper to clearly outlining the role that observations play in confirming theory.

If the paper might be viewed as at odds with conventional theory, then the potential for the media to overemphasize the conflict exists, and more attention to contextual information relevant to the real or apparent conflict is warranted. Papers should be quite clear as to whether a conflict exists or not between conventional theory and the observations or theory presented. If a conflict does exist, then it would be instructive to outline the source of the conflict and the implications of the conflict for conventional theory. While it might be viewed that this is standard practice anyway, it has not been done in the papers of this nature discussed here.

More specific guidelines for presentation of greenhouse change theory will be offered in the following section.

9.4.2 Scientific standards pre and post presentation

The argument to be developed in this section is that the failure to provide adequate context in presenting science validation information leads to inappropriate evaluation of the information in the public arena. Critical evaluation of theories and observations following the scientific method requires an understanding of the broader context of the research topic and program in which the theory is embedded. While the public cannot be expected to understand intricacies of research programs or the full implications of every new result, they are not being helped at all by scientists who don't try to provide basic context. We suggest that scientists think about applying critical evaluation standards to validation material after the information they present has been interpreted by a competent lay person. i.e. think about implications of the scientific method both before presenting the material and also after it has been interpreted. We do not wish to imply that a good following of the scientific method in science and policy analysis will resolve uncertainties and make policy evaluation much easier or clearer. Clark (personal communication) points out that adherence to the scientific method alone isn't enough. The scientific method is not enough since the interpretation of information relevant to validating greenhouse change and placing it in context is not an objective value-free exercise. Nevertheless, the scientific method provides useful guidelines. It may be that thinking about critical evaluation in a broader context in presenting validation information can reduce incidences of "trial by the media of the greenhouse effect" (Schneider, 1989) over non-scientific issues such as that involving the 1988 U.S. drought.

Authors of recent papers such as those of Hanson et al. (1989) and Trenberth et al. (1989) have jumped into the fray of the greenhouse issue with direct or implicit comparisons between greenhouse change theory and observations. Unfortunately however, such papers have not confronted the theory directly and systematically. They have addressed greenhouse change theory in the title or text of the paper, deliberately calling attention to the paper in the context of that subject. At this point, people are taking notice, and obligations are incurred. Appropriate context is then required, which is not given. Taking side shots at theory is a fair practice only when context is given. Using Hanson et al. (1989) as an example, suppose that Hansen and Lebedeff (1987) did show a significant temperature trend for the U.S., and Hanson et al. (1989) did not. It would then be quite appropriate to give context, and then impugn Hansen et al.'s global data set on the basis of disagreement with Hanson et al. over the U.S. Though one might disagree with the conclusion, it would be a fair attack, and one would not be calling Hanson et al. to task for their handling or lack thereof of contextual issues.

The issue of confronting theory appropriately is important enough that it is worthwhile to outline it in more detail. A summary of examples from the papers reviewed here will be presented with regard to scientific standards for confronting theory.

The scientific method is based in part on the presentation of theories, and critical evaluation of the theories, particularly in light of observations or alternative theories. Greenhouse change theory provides certain predictions about the manner in which the climatic system might be altered over time. Critical evaluation of this theory requires determination of whether the response predicted by theory can be detected and explained in a manner consistent with the theory. For the process of critical examination of theory via observations to be valid and successful, it is necessary to compare theory and observations in a manner that does justice to the sophistication and nature of each. Though none of the papers discussed here have strictly fallen short in this regard by the standards of the scientific method, there are additional relevant standards that should be applied. That is, these papers are being interpreted in the policy realm, but in varying degree, have failed to provide adequate context for that realm. As a result, when one applies the techniques of critical evaluation of theory in that realm (without adequate context), the critical comparison process fails to do justice to the theory, and is therefore invalidated. In the examples from the more recent papers presented earlier, from the perspective of the policy realm, one sees observations that appear to be inconsistent with theory, and so doubt is cast on the theory. This can be misleading if the conclusions are different from those in the scientific realm (where context is known, but not necessarily given) where doubt (or support) is not necessarily implied. The lack of context leaves policy makers in the position of the six blind men examining the elephant in the Indian proverb (Saxe, 1955). Each examines a different part of the elephant's anatomy, and believes that they are encountering different creatures. None has sufficient information to identify the elephant.

In the process of critical evaluation of greenhouse change theory, the failure to provide context adequate for transference from the scientific to policy realm can be related to several shortcomings or pitfalls. These include: comparing observations with something not well established in or representative of greenhouse change theory; comparing observations not representative of relevant climatic behaviour or scale to greenhouse change theory; comparing theory or observations not relevant to greenhouse change theory to greenhouse change theory; failing to describe consequences of theory accurately; failing to reference real or potential contradictions of the theory; and combinations of the above. Very often the comparison that is made in the policy realm in one of the above categories will not be the one that should have been made, nor might it be the one that was intended or implied by the authors of the scientific paper. The successful science journal paper in this regard would be the one that provides context appropriate to prevent this occurence by well intentioned interpreters. This is the sense in which the papers reviewed here contain shortcomings.

Explicit or implicit presentation of material in the recent papers reviewed here generated comparisons at the policy level illustrative of the above pitfall categories as follows:

--- Comparing observations with something not well established in or representative of greenhouse change theory: comparison of U.S. precipitation data with particular GFDL model predictions (Hanson et al., 1989).

--- Comparing observations not representative of relevant climatic behaviour or scale to greenhouse change theory: comparison of U.S. temperature data with global temperature data (Hanson et al., 1989); comparison of U.S. precipitation data for the past one hundred years with model predictions of precipitation changes for a doubled CO_2 climate (Hanson et al., 1989); comparison of a short period temperature data set (thirty years) with greenhouse change temperature predictions (Angell, 1989).

--- Comparing theory or observations not relevant to greenhouse change theory to greenhouse change theory: comparison of a physical mechanism for the 1988 U.S. drought to greenhouse theory (Trenberth et al. 1988, Palmer and Brankovic 1989).

Other examples of pitfalls that fall into these categories exist elsewhere in the literature. Note that the ratio of those in the popular press to those in the scientific journals is high. It takes only one journal article lacking in appropriate context to generate a plethora of misleading articles in the popular press. Early misinterpretation of a journal article by one news source is repeated over and over in coverage by later sources who base their information on the misinterpretation.

Frequently, the pitfalls in the literature relate to implicit comparison between theory and observations using inappropriate time or space scales. i.e.

too small an area, or too short a time series. Sometimes the scales are appropriate, but the variable selected for comparison on those scales is compared with the greenhouse signal in a different variable from theory. e.g. comparing SST data in isolation (where the greenhouse response time is lagged) with greenhouse theory estimates of global temperature change for land and sea combined. Comparison with theory unrepresentative of greenhouse theory is another common trap. This relates principally to a failure to distinguish between that which is reasonably well grounded in the theory, and that which is not. The global warming of surface air temperature is well grounded as a piece of theory; the exact time scale over which it might happen is not. That the warming at the surface should be greater (in the long run) in high latitudes than low latitudes has near consensus in theory; that precipitation changes over the central U.S. will increase or decrease does not.

Examples of failure to reference real or potential contradictions of the theory have not been described here, but it is easy enough to imagine their effect on the public debate. They would have the effect of presenting the theory in a more confident light than was justified. For example, to the extent that paleoclimatic data present a reasonable analogue between past warmer climates and predictions for a greenhouse change warmed earth, consistency between them should be checked. Hansen et al. (1981) have noted potential inconsistencies between reconstructions of regional climate patterns in the altithermal period with model predictions of greenhouse change scenarios. It appears that the climate over some land areas in past warmer periods was wetter than greenhouse change scenarios would indicate. This may not be an inconsistency, but deserves referencing and further exploration.

Much of the theory on greenhouse change stems from climate model predictions of doubled CO_2 scenarios. Where model results agree with one another, or with what one would expect from simpler models and simple physical descriptions, grounding of that part of theory is strengthened. Confidence in the predictions as theory is also increased when consistent with observations. Until particular climate model predictions attain reasonable grounding or consensus, support or refutation of theory based on comparison of observations with these predictions can not readily be assumed, and context pertinent to this should be included in presentations of results.

The real climatic system is likely to contain significant behaviour that is not included in the climate models, nor represented by their predictions. Surprises will occur.[57] In testing theory with observation, it will be vitally important to consider and note whether the surprises or inconsistencies contradict well grounded theory, or those more speculative aspects of the predictions that relate to shortcomings in the models. Failure to do this will result in a debate over 'straw-theory'. Establishing confidence in detection or refutation of greenhouse change relies on a broad coverage of observations (surface temperatures, upper air temperatures, radiation budget measurements, precipitation and hydrological measurements, circulation changes, sea level, ocean mixing, ice and snow cover, cloud distribution, etc.), and their comparison with relevant parts of the theory. Establishing confidence also requires better understanding of the more speculative aspects of the theory. In particular, the role of cloud feedbacks and the modelling of changes of cloud distribution and type, the role of oceans and ocean circulation, and the internal behaviour of the climatic system and the way it interacts with external environmental changes. The synthesis of a mix of observations with an evolving theory is an enormous task, and individual studies are likely to pertain to only a small part of this. To prevent misinterpretation of a subset of the required information in the public sector, it is important that studies be placed in context relative to the overall detection schema. The implications of research that is likely to be construed as relevant to greenhouse change should be stated directly and clearly.

9.5 The science-media interface

The short set of examples presented here is illustrative of the type and degree to which scientific journal papers on greenhouse change are misinterpreted in the press. How is this to be explained? One could blame the media solely. From that point of view then, there was no confusion created by the scientific papers. It could be that newspapers cannot resist sensationalizing issues and then downplaying them, and frame everything as

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[57] Surprises in the climatic response to greenhouse forcing will include amplifying and damping actions, and there is little reason to presume that either action is more likely to predominate than the other.

a story, which gets played out as we have seen (overemphasis on conflict or consequences). Perhaps they are constrained from acheiving faithful interpretation by structural factors or other means. On the other hand, one could blame scientists solely. Perhaps scientists are not able to communicate effectively, or are not aware that their audience transcends traditional disciplinary bounds. Perhaps scientists are occasionally content to write papers that could easily be misinterpreted for whatever reason.

These cases allocating blame represent extremes, of which the truth probably lies somewhere in between most of the time. If this is so, then scientists are not doing as well as they could in presenting validation information, and have some role to play in addressing this. We have suggested that relatively simple measures like supplying additional context and explanation might help reduce incidences of misinterpretation and dispute over non-issues. This is all very well except that we have not considered how scientists might be motivated to change or supplement current practices. For instance, what is motivating researchers to present results as they do now? Where could change be motivated - through scientists, or the editors of science journals, or perhaps through science journalists? Would structural change in science and policy arenas be necessary to motivate change? These questions are beyond the scope of this work, though the inability to answer them means that we cannot here go beyond our original goal of simply addressing the question of whether validation information is being presented well or not. Analysis of attempts to validate or illuminate greenhouse change incorporating the 1988 U.S. drought and U.S. temperature trends have been used as examples. They show that at least in terms of the scientific component of validation (establishing credibility), validation information is not being presented as well as it could be.

9.6 Conclusions

We began with an assumption that scientific journal articles relevant to validating greenhouse change have an effect in shaping the public debate on greenhouse validation. Scientific journal papers represent the initial point of communication between scientists and the press. As a result of the influence of scientific papers in such cases, scientists have an obligation to present their results in such a way that the results have a reasonable chance of being interpreted faithfully in the public domain. This requires the supplying of context describing the research and its implications that is appropriate to a broader public audience as well as to specialists in the field. It was noted that the scientific journal articles represent probably the most sophisticated forum for presentation of scientific results, and that loss of context and sophistication in this forum leaves greenhouse science-policy related discussions without complete sources of information.

Recent published scientific papers relevant to greenhouse validation have not supplied context appropriate to a broader audience. When the press have reported on scientific journal papers with greenhouse policy ramifications, considerable confusion and misinterpretation has taken place in the course of translation. This is detrimental to the public discourse, since a major aim of the communication of scientific information should be the yielding of an informed public debate. If we assume that the press is reasonably competent and faithful in its attempt to translate scientific papers, then some blame must be placed upon writers of the scientific literature for failing to supply appropriate context. The failure to supply appropriate context can lead to the unwarranted support or erosion of theory relevant to the scientific and public greenhouse debates.

The translation of scientific results from the scientific to the public domain depends upon the context offered. When we apply the standards of comparison of theory and observations in science to the scientific research which itself has been translated into the public domain, we find that inappropriate comparisons occur when context is lacking. Theory may be misrepresented so badly in translation (by failing to include pertinent information) that straw theory is presented in the public arena. Knocking down straw theory then has the effect in public of damning the whole canon of the theory, when this may not be implied in the scientific domain (where context is known) at all. The converse also applies to misinterpretation yielding unwarranted bolstering of theory. Unwarranted support for action based on theory in the public arena is created indirectly when the consequences of theory are allowed to take on unnecessary shades of gloom and doom. In either case, the result is an unfortunate skewing of the public debate on which the formulation of public policy depends.

The simplest guideline for avoiding unnecessary misinterpretation of scientific journal papers is to write them so that the competent reader outside the specialty field has a reasonable chance of interpreting clearly that which is implied by the paper, and that which is not. Scientific authors should think critically of how their comparison between theory and observations will be evaluated after interpretation by such a reader has taken place. If conclusions other than those warranted are reached in such a comparison, then further context is required. Since misinterpretation is frequently manifest as an overemphasis on consequences of theory or conflict with theory, scientific authors have some information on where to direct their attention.

An improvement in presentation of validation information relevant to establishing credibility of greenhouse theory could make policy level validation of the theory less confusing by reducing incidences of public debate over non-issues (such as the debate over the role of the 1988 U.S. drought). It is likely that there is also significant potential for improvement in greenhouse science presentation with respect to the utilitarian and paradigmatic validation components as well as the scientific component considered here. For the utilitarian component more useable information can be presented, and for the paradigmatic component scientists can examine science community views and how they are formed, and work on incorporating and presenting minority viewpoints critically and in context. For their part, the scientists appear to have scope for adapting their work and presentation to facilitate policy validation. Such adaption can not be expected however without attention to the factors motivating scientists.

10 SUMMARY AND CONCLUSIONS

In the thesis we set out to identify problems in validating greenhouse change in a science-policy context. We focused on GCMs, noting the heavy reliance upon 3-D GCMs for developing greenhouse change scenarios. In particular, we chose for study the three major GCMS used in greenhouse climate studies of NCAR, GFDL and GISS. We acknowledged that it is not possible to test everything in a GCM, and noted that there are no sufficient tests of greenhouse change. We chose the energy transports for a validation case study of the GCMs since the energy transports are fundamental to climate.

To calculate energy transports in the GCM annual mean control climates it was necessary to follow an indirect technique of deriving the total energy transport from the net flux of radiation at the top of the model atmosphere. For the GISS model only, it was possible to compare the indirectly calculated energy transport with that calculated directly from the model motions. The agreement was quite good. We conclude that the indirect method is a satisfactory technique for deriving the total energy transport in a GCM.

We identified differences between the meridional profile of the total energy transport for the three GCMs and observations from Carissimo et al. (1985). In the Northern Hemisphere where the differences were more significant, the GISS GCM showed more total energy transport than observations, and the NCAR and GFDL models showed less total energy transport than observations. The GCM atmospheric energy transports (which for NCAR and GFDL models are the same as the total energy transport since there is no ocean energy transport in these models) were similar to each other, but were much larger than observations of the atmospheric energy transport. On the basis that it is unlikely for the observations to be in error by the large amount (50-80%) required to bring them up to the model values, we argued that the observations are more believeable and that the GCM atmospheric transports are too large. We compared the models meridional temperature structure with an observed meridional temperature structure derived from Sellers (1965). They were similar, which would be inconsistent with the models having a larger atmospheric energy transport than observations. We conclude that the atmospheric energy transport is too efficient in the GCMs.

We then set out to consider implications of the differences in total energy transport between the three GCMs. We used a 1-D EBM to demonstrate that the climate is potentially quite sensitive to the energy transport. We asked the question: given that the GCMs have significantly different transports, why do they display similar climate sensitivities (≈4°C) to doubled CO_2? We investigated by using a simplified 'North-type' 1-D EBM of the climate system, where it is easier to trace implications of differences in energy transport. To set up 1-D EBMs that would be in some sense equivalent to the 3-D GCMs (hopefully capturing the essential physics) we parameterized 1-D EBMs with output from the GCMs. We also parameterized the EBM with observational data. Because the climate is not symmetric across the hemispheres we parameterized each hemisphere separately, and also set up global equivalent EBMs where the parameter values were averages of the hemispheric values. The major limitation found in fitting the EBMs to the GCMs and observations was related to the temperature - outgoing IR parameterization. The simple linear fit between temperatures and outgoing IR was too simplistic and did not capture the local minimum in outgoing IR in the tropics noted in observations and the models. However, this did not appear to be a major limitation of the ability of the EBMs to perform climate sensitivity experiments. The albedo parameterization was quite good, and showed the significant differences between albedo profiles in the GCMs, with GISS matching observations more closely.

To test the sensitivity of the equivalent EBMs we used a 2% increase in solar constant as a proxy for doubling CO_2. The results yielded essentially similar sensitivities between the equivalent GCMs (for models and observations). The equivalent global EBM sensitivities for the models were similar to the 3-D GCM sensitivities obtained for doubling CO_2 in the atmosphere. This agreement cannot be because the EBMs are not sensitive to the differences in parameter values between them, because their solar constants and energy transports were significantly different. EBM climate sensitivity is sensitive to the value of solar constant, and the EBM climate is sensitive to the energy transport. The fact that the equivalent EBM sensitivities are similar implies either an unlikely coincidence, or that compensating mechanisms are at work. Whatever compensating factors are represented in the EBM, they are not as efficient as in the GCMs however. In the GFDL equivalent EBM the polar icecaps were eliminated for a 2% solar constant increase, which was not the case in the GCM CO_2 doubling experiment.

The energy transports obtained from the equivalent EBMs were similar in magnitude to the values in the GCMs and observations (though a little smaller). It is not clear why the equivalent EBM climate sensitivities were not sensitive to differences in the energy transport. The main candidates for compensation of the energy transport differences in the EBM are the parameters B, D, x_s, T_s and δ, related to water vapour and cloud feedbacks and ice-albedo feedback. If compensation is occurring, and if these parameters are constrained by tuning the GCMs to the current climate, then this raises the possibility that the climate sensitivity is constrained by tuning to the current climate.

We concluded that errors and inconsistencies related to the energy transport do not necessary invalidate the GCMs since compensating factors may be at work. We noted that confidence in GCM greenhouse change projections is derived from a consideration of all relevant theory, models, and observations. The GCMs do not stand alone, but are part of a hierarchy of models used to test observations in developing greenhouse change projections.

As a next step in evaluating greenhouse change in a science-policy context we described a validation framework incorporating science and policy communities. The purpose of setting down the framework was to aid in comparing and contrasting the way science and policy systems approach the validation problem. In science, greenhouse change theory was said to be valid in the sense that it is embedded within the most progressive research program (that capable of explaining the most about present, past and potential future climates). In policy, greenhouse change theory was said to be evaluated by whether it is defensible against attacks of being arbitrarily based. This requires showing that the theory is consistent, has credibility, and is useful. We noted the lack of development of adequate evaluation procedures for science policy issues. We suggested that this is perhaps one reason why the policy community tend to measure credibility in terms of whether there is a consensus supporting theory in science. Defensibility in policy was also linked to the ability to demonstrate that the likely impacts have policy implications. We noted that the priority assigned to greenhouse change on the policy agenda also depends on the political acceptability of the theory and its implications.

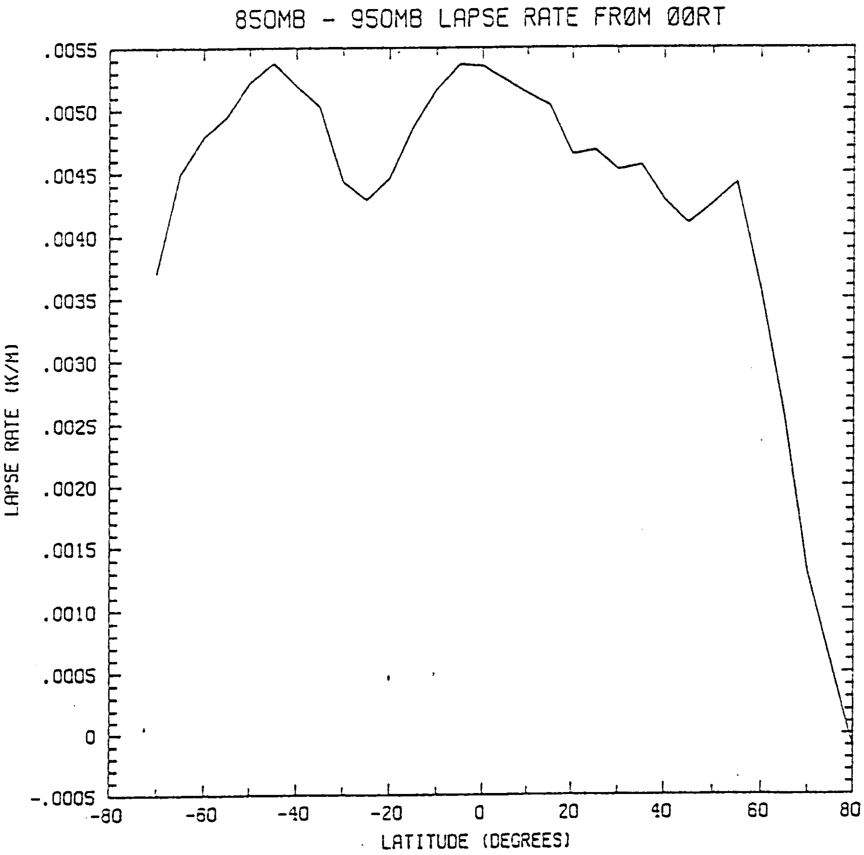
We argued that some of the characteristics of the greenhouse issue lead to a mismatch between science and policy requirements (related to factors such as time scales, source diffusivity, physical manifestation, and coordination requirements). This compounds the difficulty of incorporating uncertainties into issue evaluation. In addition there are methodological differences between the two communities that lead to diverging evaluation requirements. In science it is necessary to develop a set of protective heuristics that lend a research program a degree of resiliency in the face of difficulties. In policy it is necessary to raise the costs of uncertainty to allow only well supported or acceptable issues on the agenda.

We remained open to the possibility that both science and policy communities have potential to change and adjust to requirements of the other community in facilitating critical evaluation of issues. Of the various questions identified in the validation framework we chose to study the question of how well validation information is presented from science to policy arenas. If scientists can improve the way they present information then there may be some marginal benefit. The policy community will be able to make its decisions with enhanced knowledge of the inherent risks and uncertainties of greenhouse theory. We outlined potential mechanisms by which the improved presentation of validation information might change perceptions of risks and uncertainties. We noted that the current policy system requires high risk and low uncertainty to act when political acceptability is low.

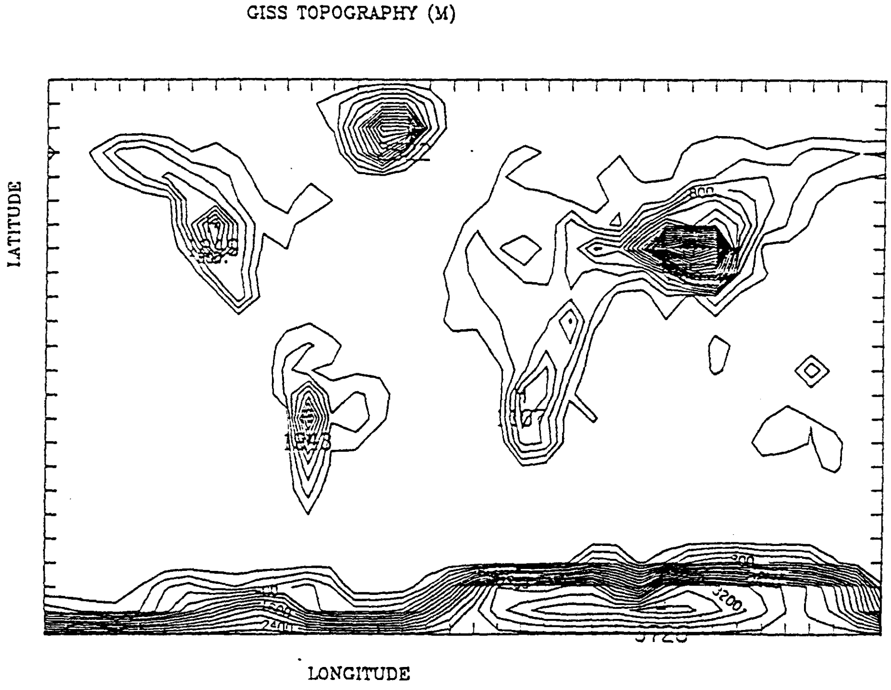
To answer the question of how well validation information is presented now we considered just the science journal literature and its media interpretation. These two components are integral to the wider science-policy commmunication network. We demonstrated by example that there is considerable misinterpretation of science journal literature in the media. As a direct result of media coverage of science journal papers there is even occasional debate over issues that are essentially irrelevant to validating greenhouse change in the media. For the case involving the 1988 U.S. drought we argued that this is partly due to a failure by scientists to provide context appropriate to a broader audience in presenting validation information. We note finally that improvements in communication of validation information might prove to be useful if greenhouse change unfolds within the ranges expected, and if the policy system can become more sensitive to the relative degree of risk and uncertainty.

FIGURES

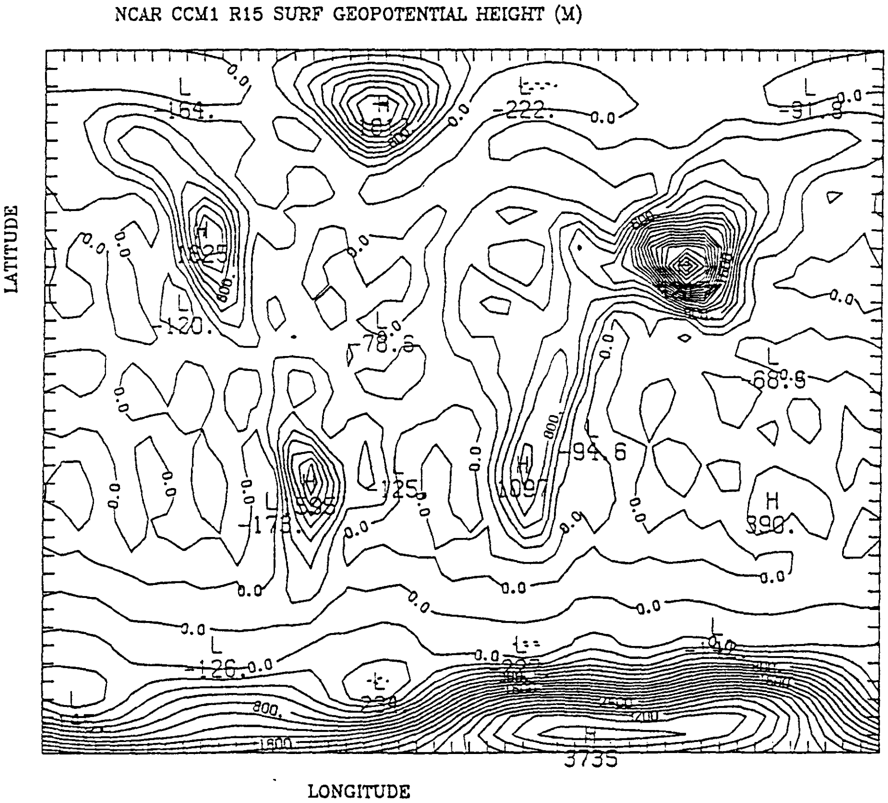
5.1 Zonal mean 850mb - 950mb lapse rate from Oort (1983) data



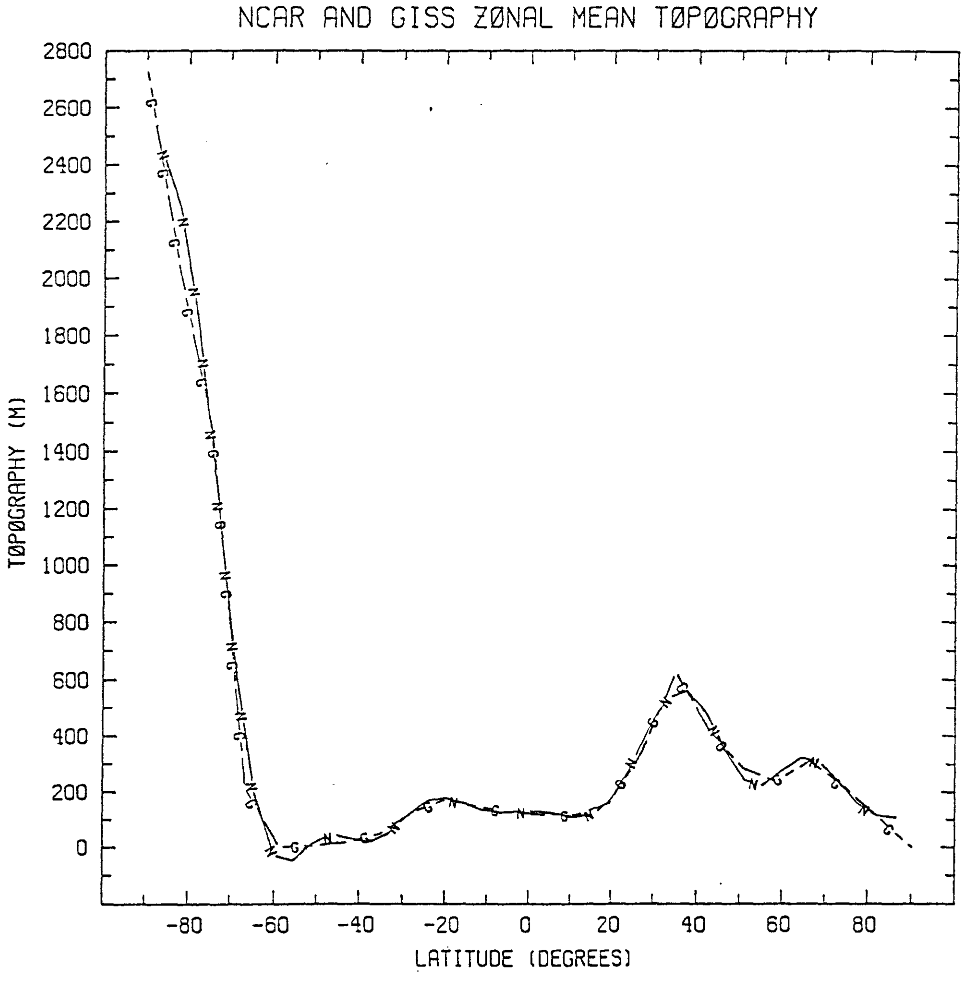
5.2 GISS topography on the GISS grid



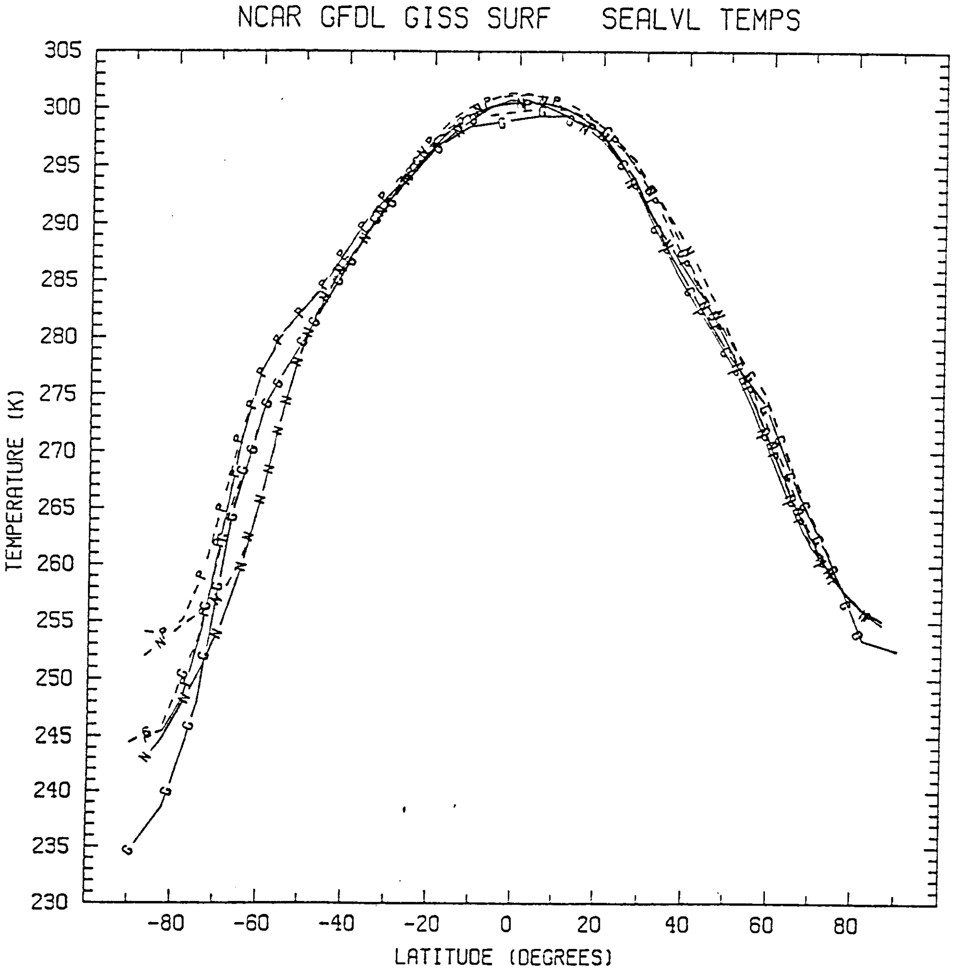
5.3 NCAR CCM1 R15 surface geopotential height



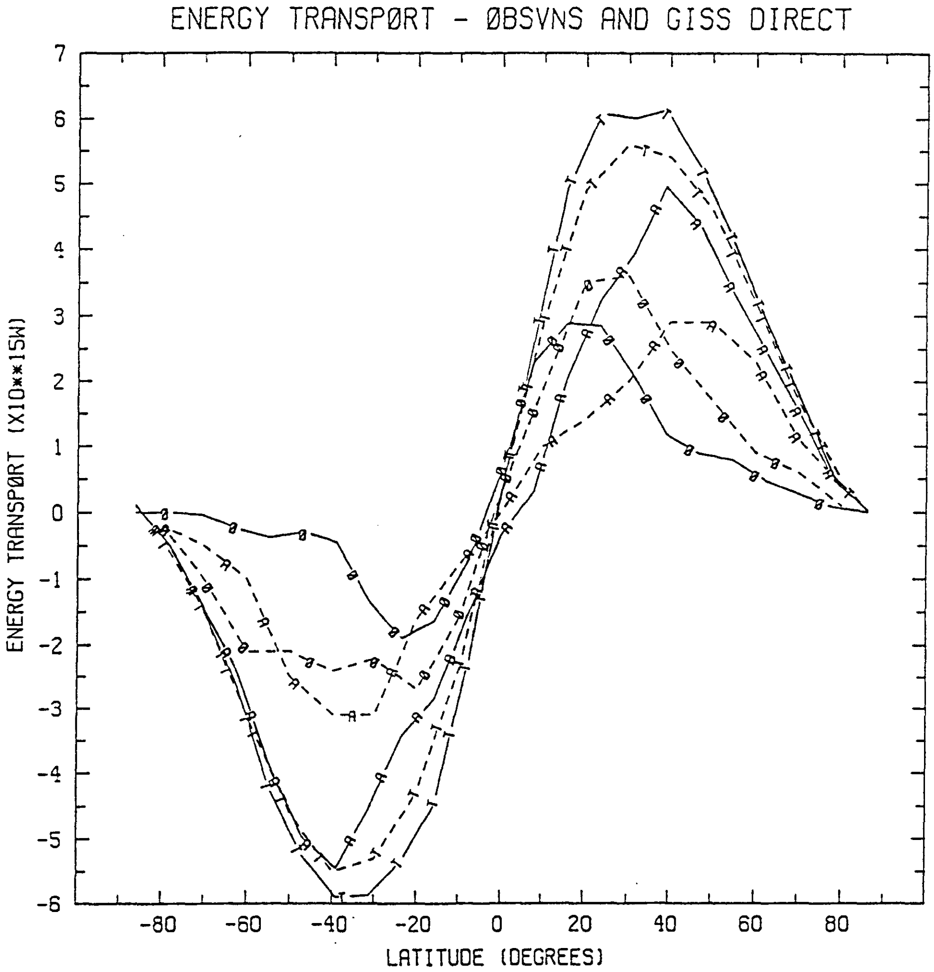
5.4 NCAR (N) and GISS (G) zonal mean topography



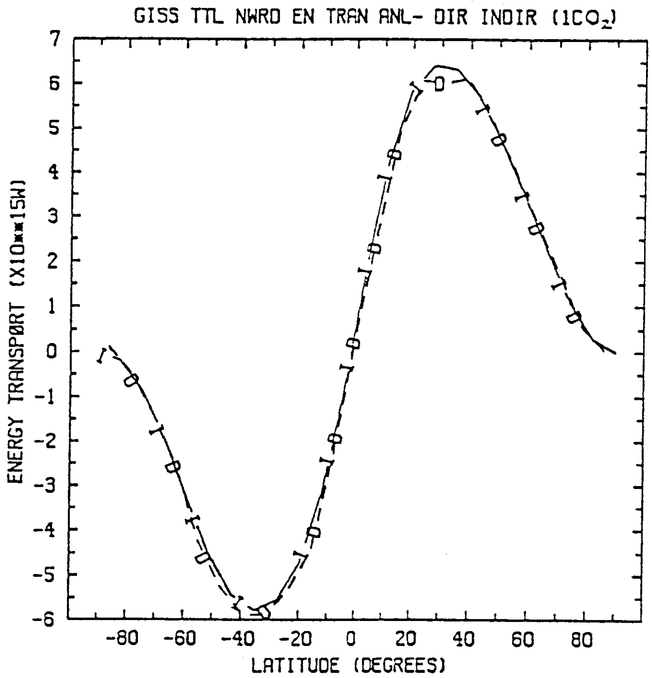
5.5 NCAR (N), GFDL (P), and GISS (G) control run surface temperatures (solid lines) and sea level temperatures (dashed lines)



6.1 Annual mean zonal mean total meridional (northward) energy transport (T), atmospheric energy transport (A), and ocean energy transport (O) for Carissimo et al. (1985) observations (dashed lines), and calculated directly from the GISS control run model motions (solid lines)

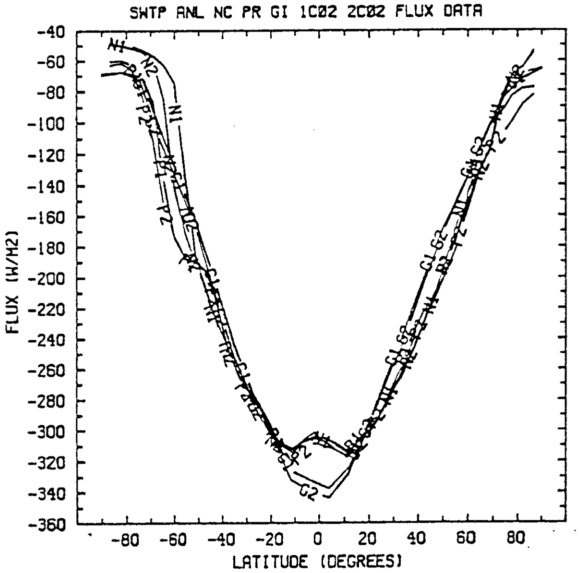


6.2 Annual mean zonal mean total northward energy transport calculated directly from the GISS model motions (D), and indirectly from the net flux of radiation at the top of the atmosphere in the GISS model (I) (control run)



6.3.1- 6.3.6 Annual mean zonal mean shortwave flux at the top of the atmosphere (SWTP), longwave flux at the top of the atmosphere (LWTP), shortwave flux at the surface (SWSF), longwave flux at the surface (LWSF), sensible heat flux at the surface (SHSF), and latent heat flux at the surface (LHSF) for the NCAR (N1,N2), GFDL (P1,P2), and GISS (G1,G2) models. The numbers '1' and '2' refer to equilibrium results for 1 x CO_2 and 2 x CO_2 simulations

6.3.1



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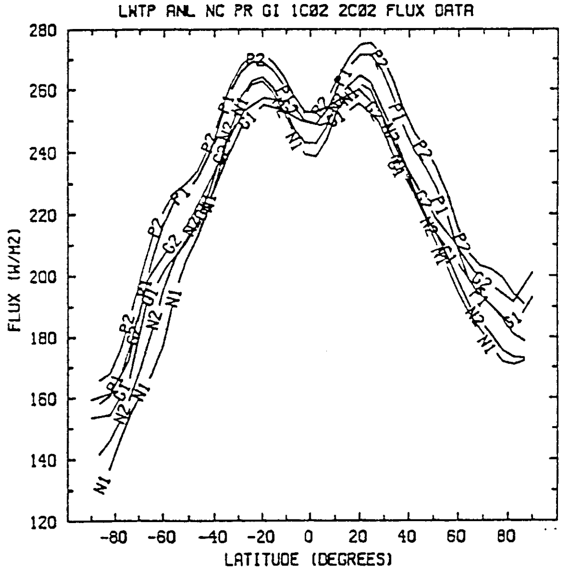
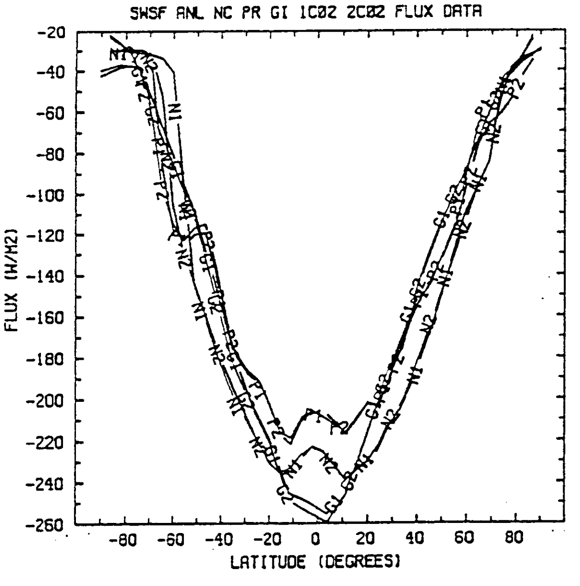
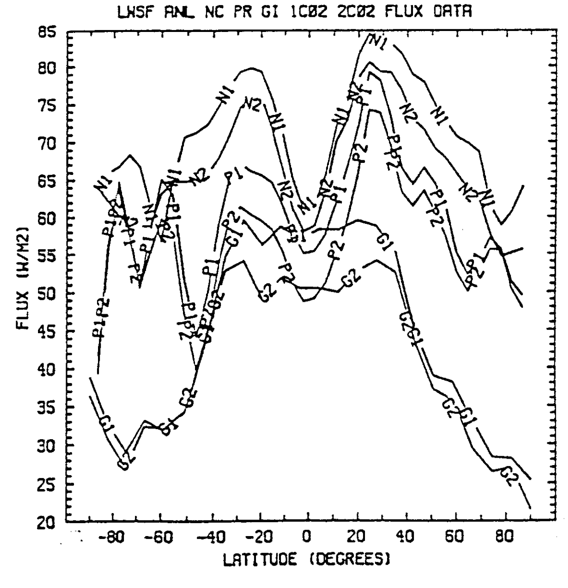




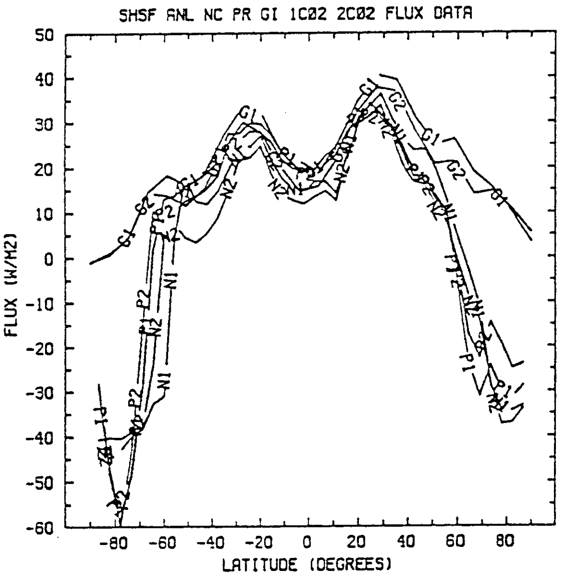
6.3.3



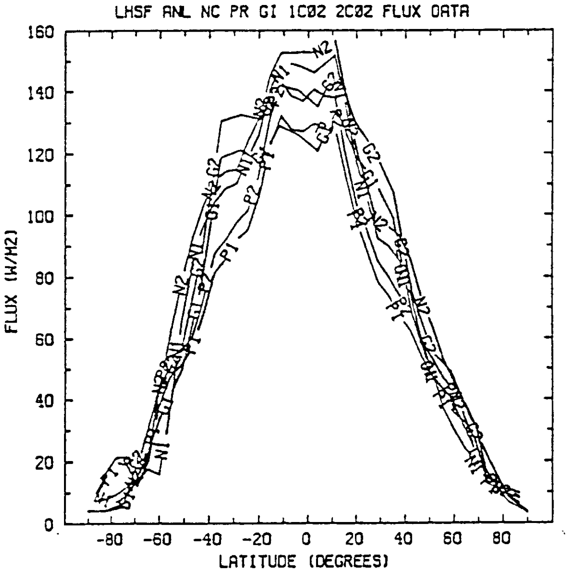
6.3.4

6.3.1- 6.3.6 Annual mean zonal mean shortwave flux at the top of the atmosphere (SWTP), longwave flux at the top of the atmosphere (LWTP), shortwave flux at the surface (SWSF), longwave flux at the surface (LWSF), sensible heat flux at the surface (SHSF), and latent heat flux at the surface (LHSF) for the NCAR (N1,N2), GFDL (P1,P2), and GISS (G1,G2) models. The numbers '1' and '2' refer to equilibrium results for 1 x CO_2 and 2 x CO_2 simulations

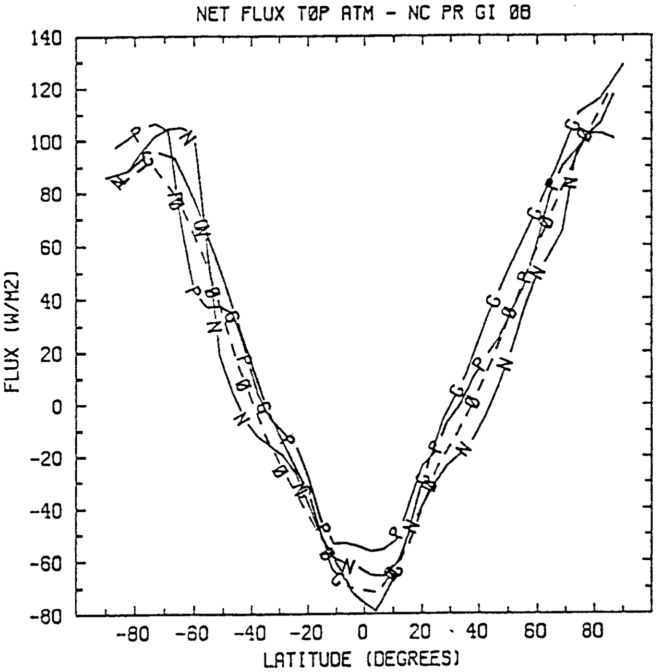
6.3.5



6.3.6

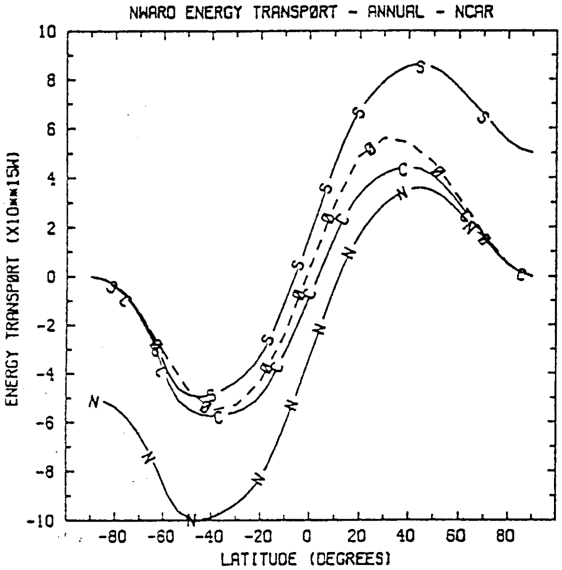


6.3.7 Annual mean zonal mean net radiative flux at the top of the atmosphere from NCAR (N), GFDL (P), and GISS (G) model control runs, and from Stephens et al. (1981) observations (O)

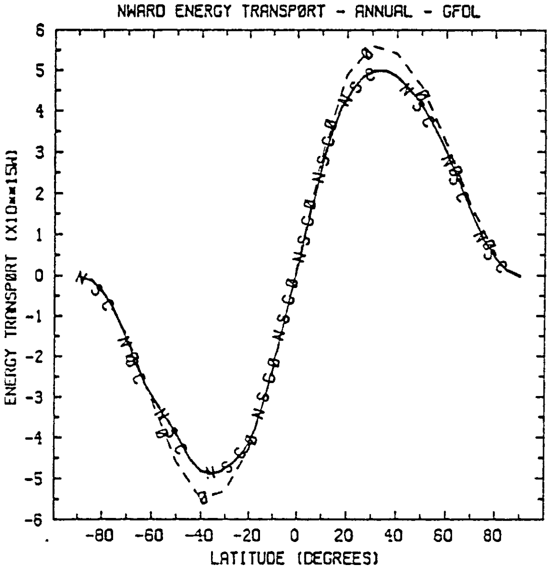


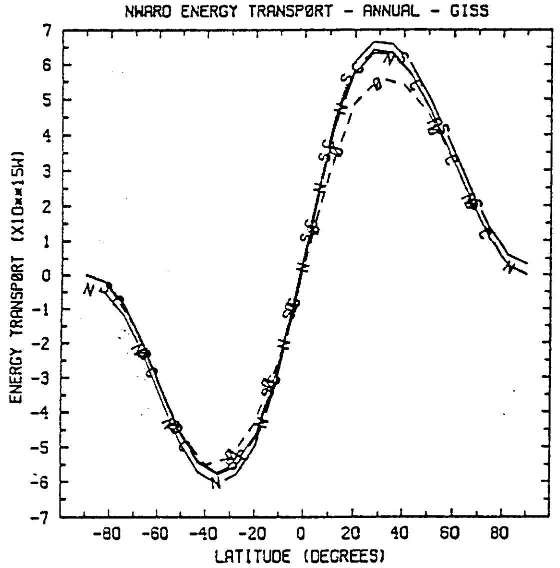
6.4.1- 6.4.3 Annual mean zonal mean total northward energy transport for NCAR, GFDL, and GISS models (control run). The curves labelled 'N' are for integrations starting at the north pole, the curves labelled 'S' are for integrations starting at the south pole, and the curves labelled 'C' are corrected curves. The dashed curve is from Carissimo et al. (1985) observations (O)

6.4.1

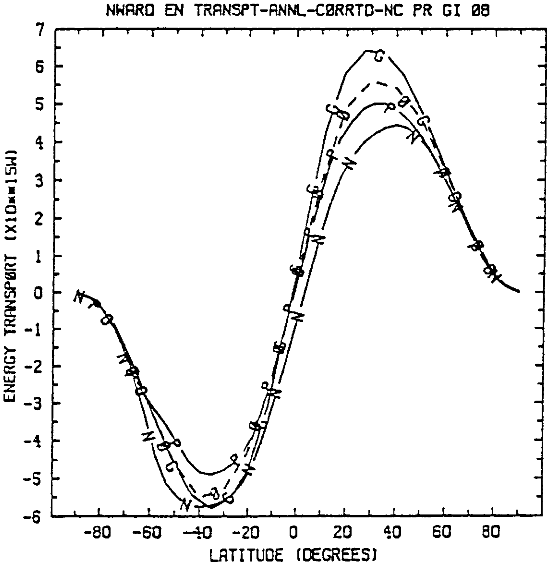


6.4.2





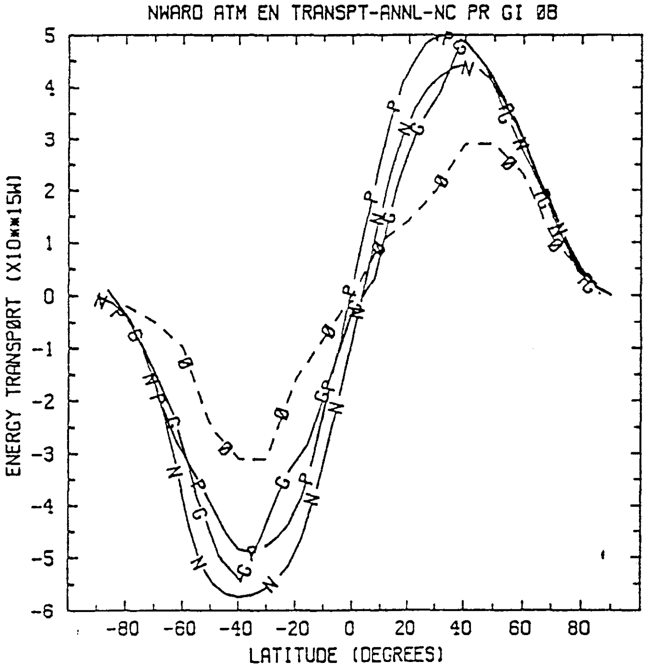
6.4.3



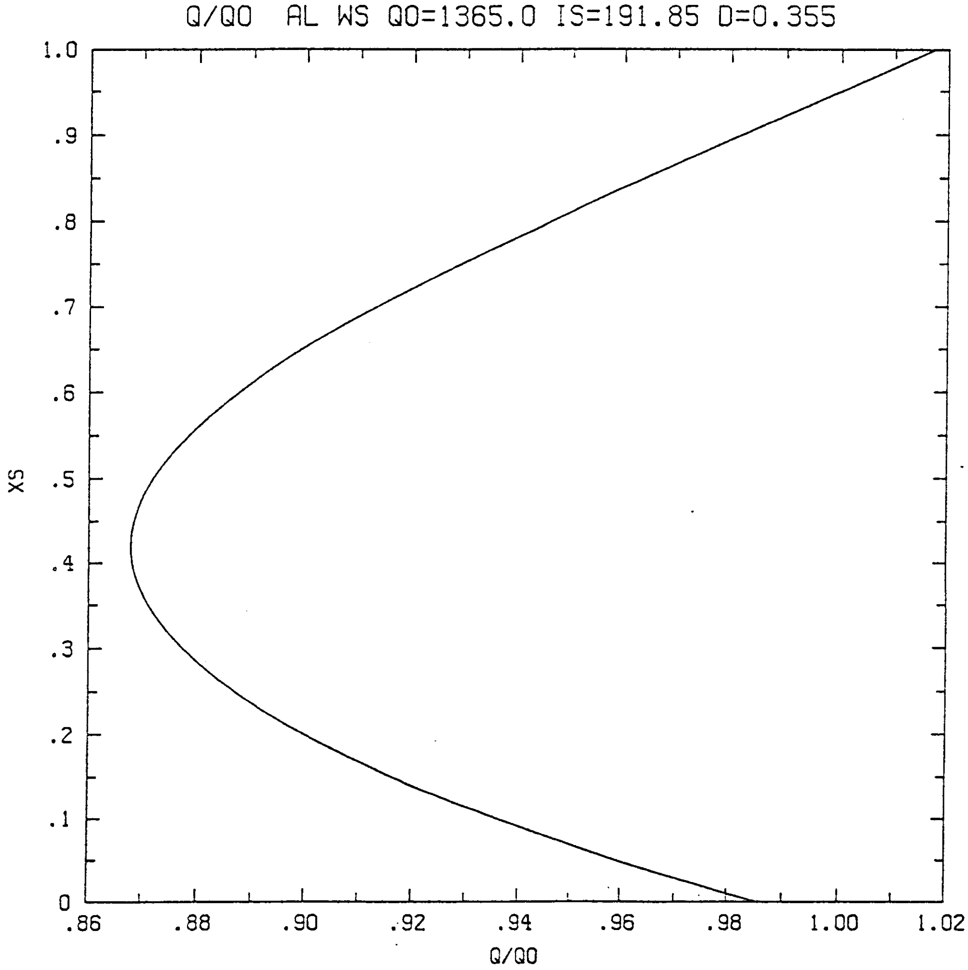
6.4.4

6.4.4 Corrected annual mean zonal mean total northward energy transport for NCAR (N), GFDL (P), and GISS (G) models (control run), and for Carissimo et al. (1985) observations (O)

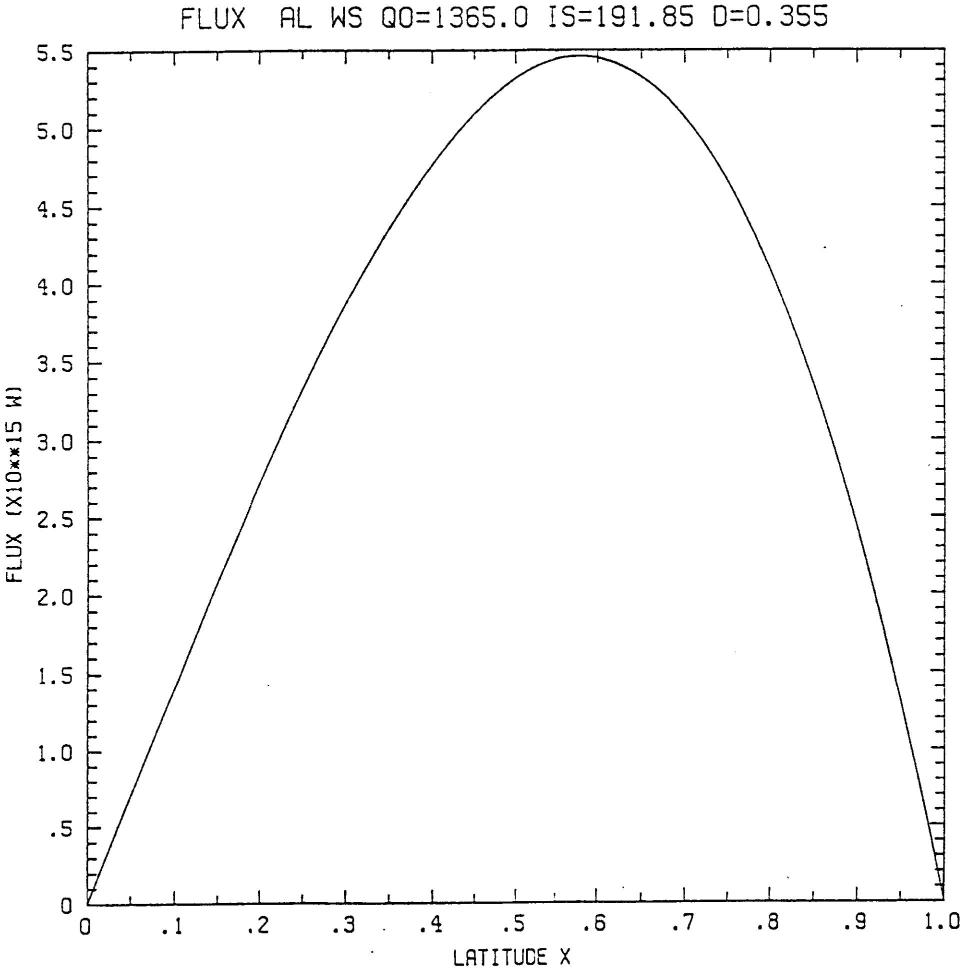
6.5 Annual mean zonal mean northward atmospheric energy transport for NCAR (N), GFDL (P), and GISS (G) models (control run), and for Carissimo et al. (1985) observations (O)



7.1 Iceline latitude (x_s) versus ratio of the solar constant to the current solar constant (q/q_0) for the 1-D EBM



7.2 Meridional energy transport flux as a function of the sine of latitude (x) for the 1-D EBM



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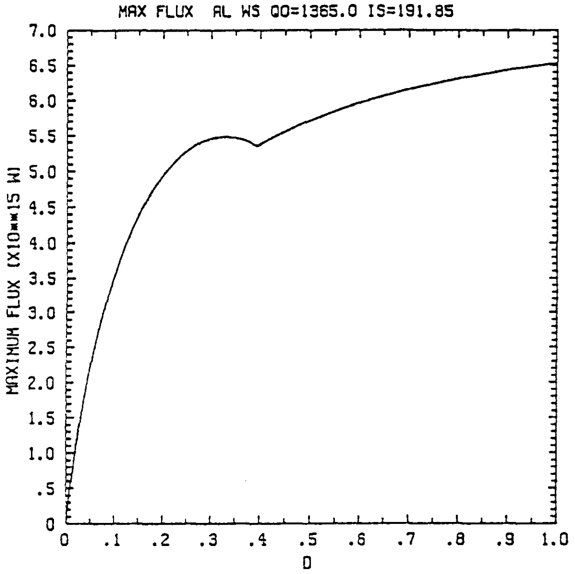
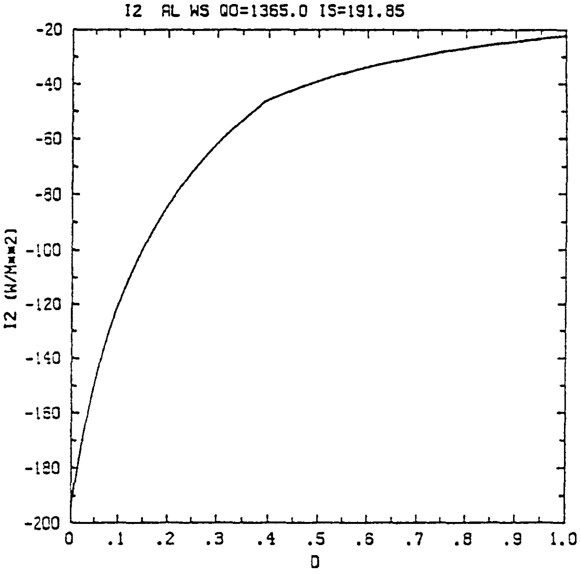
7.3.1- 7.3.3 Maximum energy transport flux, second component of outgoing longwave radiation (I_2), and iceline latitude (x_s) as functions of the dynamical transport efficiency (D) for the 1-D EBM

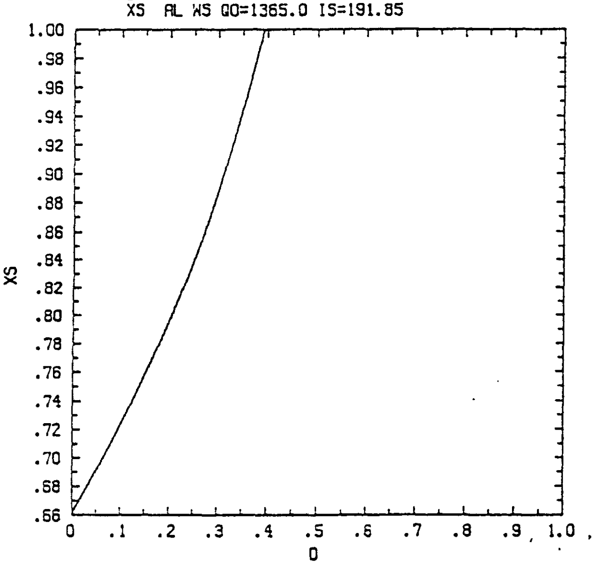
7.3.1



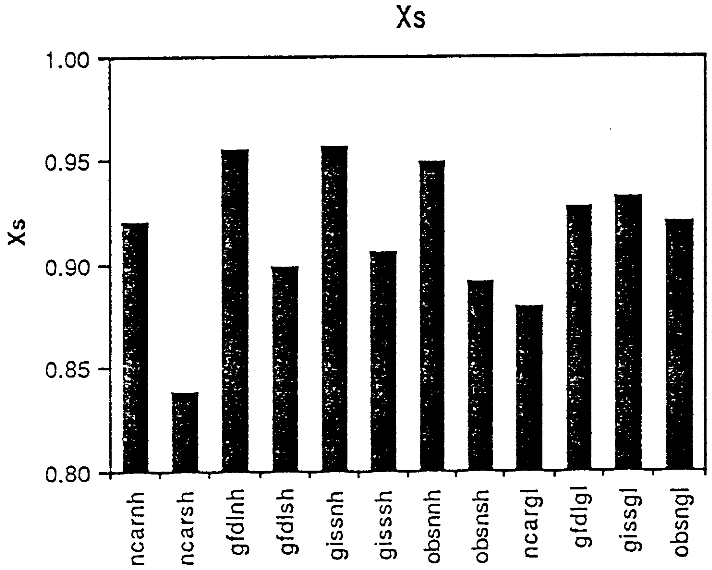
7.3.2



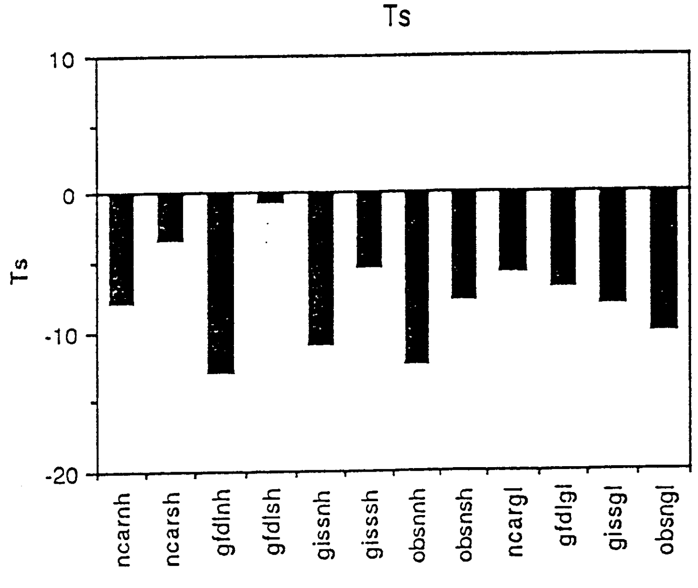
7.3.3



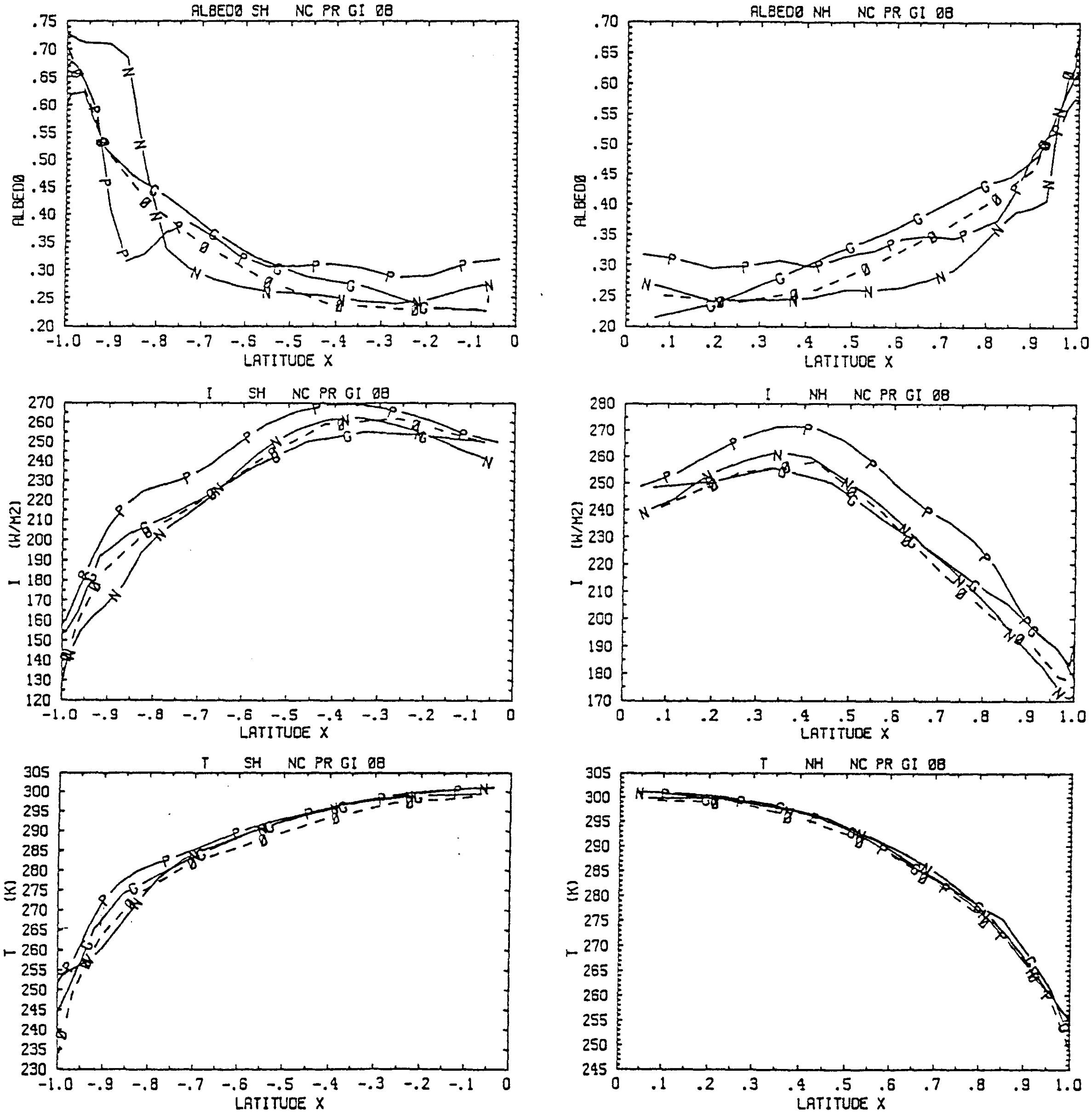
7.4 Iceline latitude (x_s) for NCAR (ncarnh, ncarsh, ncargl), GFDL (gfdlnh, gfdlsh, gfdlgl), and GISS (gissnh, gisssh, gissgl) models (control run), and from Alexander and Mobley (1976) observations (obsnnh, obsnsh, obsngl). In each case, 'nh' is for the Northern Hemisphere, and 'sh' is for the Southern Hemisphere. The global (gl) values are averages of the hemispheric values in each case



7.5 As in fig. 7.4, but for the sea level temperature at the iceline latitude. Temperature observations are from Sellers (1965)

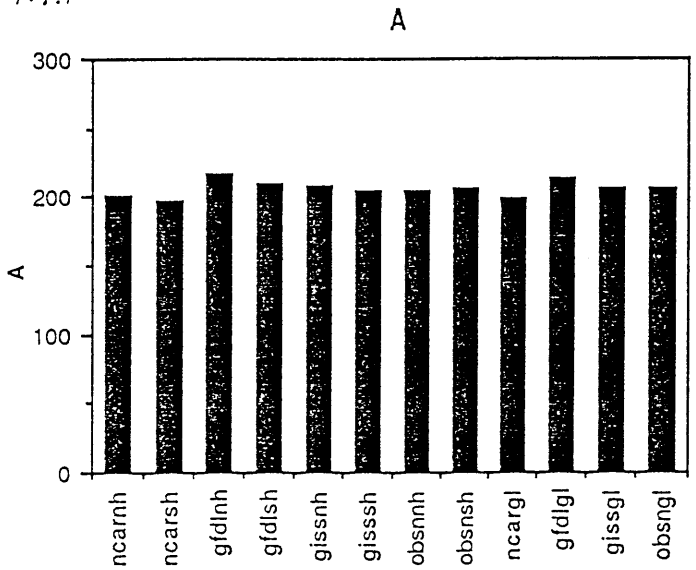


7.6.1- 7.6.6 Annual mean zonal mean planetary albedo (ALBEDO), net flux of longwave radiation at the top of the atmosphere (I), and sea level temperature (T) for NCAR (N), GFDL (P), and GISS (G) models (control run), and from Stephens et al. (1981) observations (O for Albedo and I) and Sellers (1965) observations (O for T). The curves are plotted separately for the Southern Hemisphere (SH) and Northern Hemisphere (NH) as a function of the sine of latitude (x)

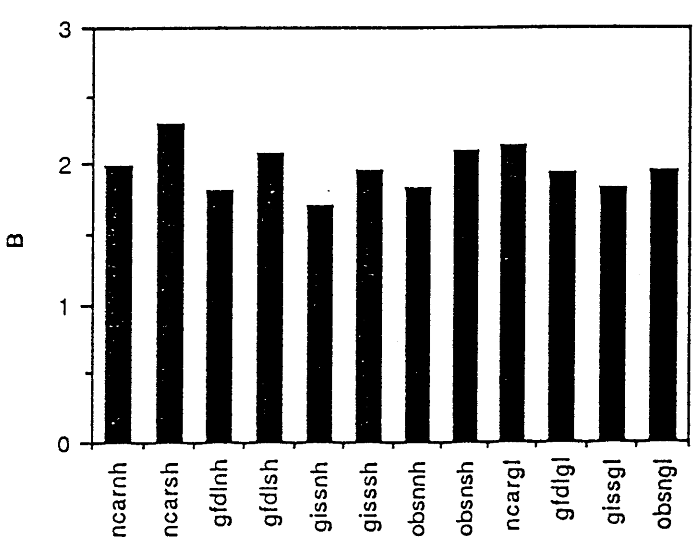


7.7.1-
7.7.2 As in fig. 7.4, but for parameterized values for A and B from the temperature - outgoing longwave radiation parameterization I = A + BT in the equivalent EBMs.

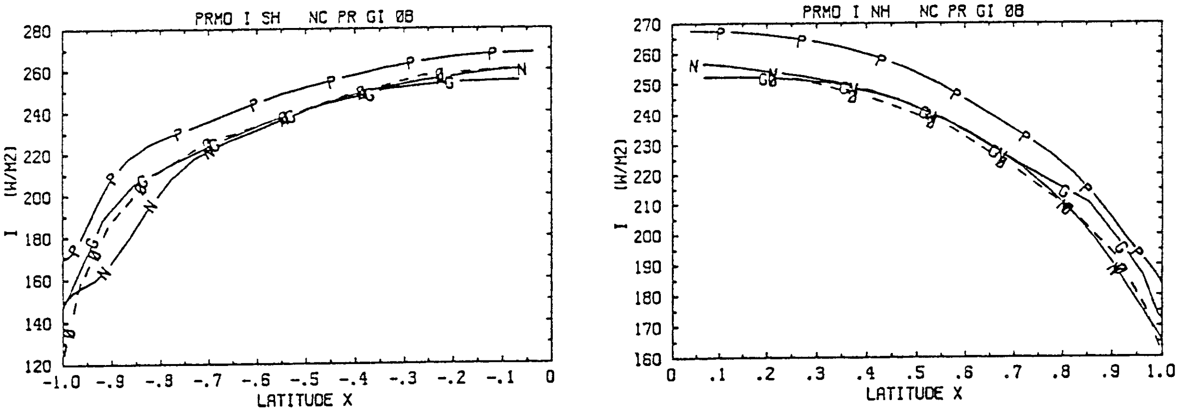
7.7.1



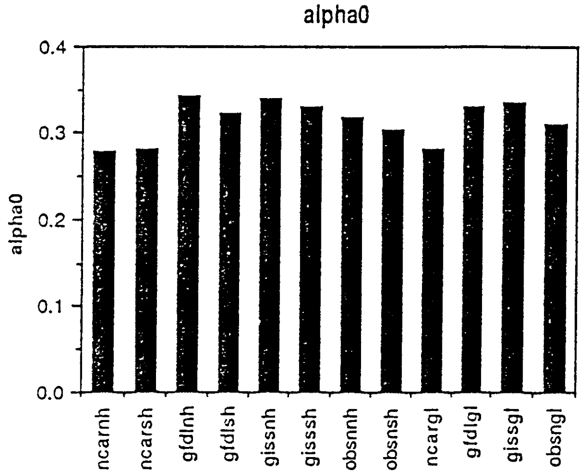
7.7.2



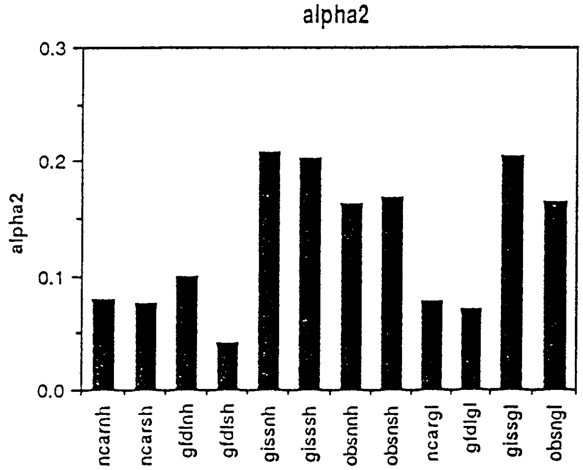
7.8.1-7.8.2 Parameterized outgoing longwave radiation (I) as a function of the sine of latitude in Southern and Northern Hemispheres (SH and NH) for NCAR (N), GFDL (P), and GISS (G) models, and for observations (O). I is calculated via I=A+BT with the parameterized A and B values and with the actual zonal mean temperatures for each case (not with the two mode Legendre representation of the temperature)



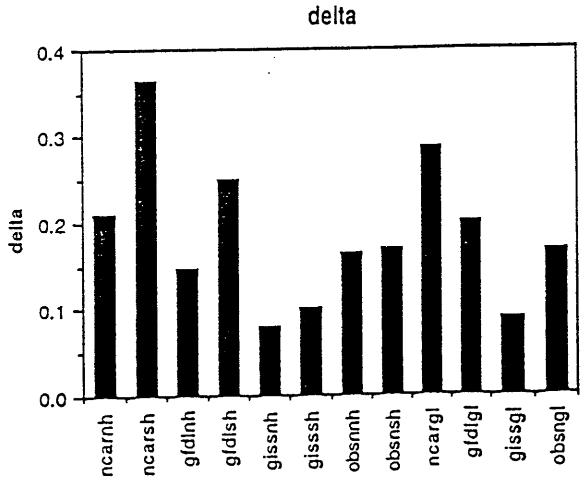
7.9.1- 7.9.3 As in fig. 7.4, but for parameterized values for the albedo components α_0, α_2, and δ in the equivalent EBMs



7.9.1

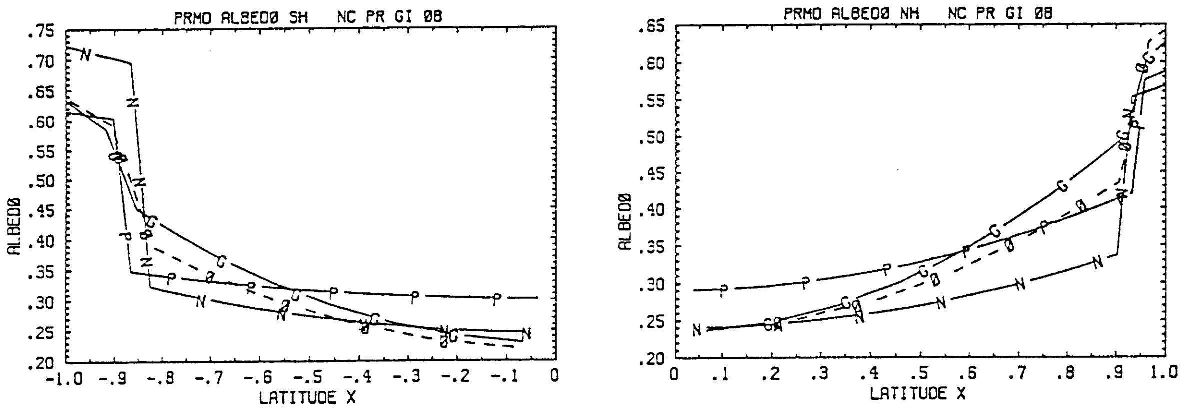


7.9.2

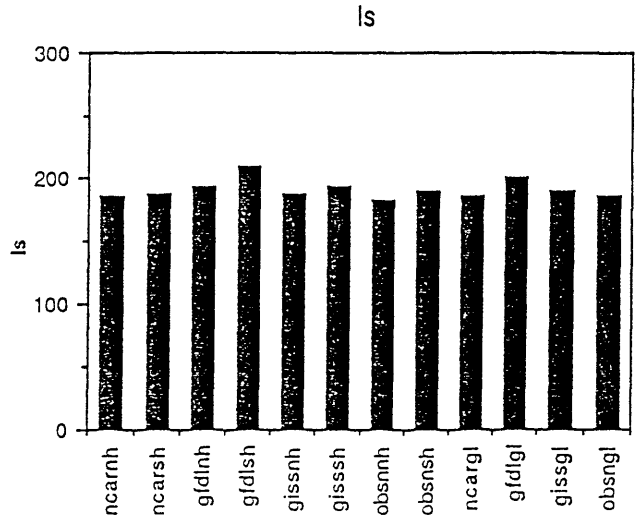


7.9.3

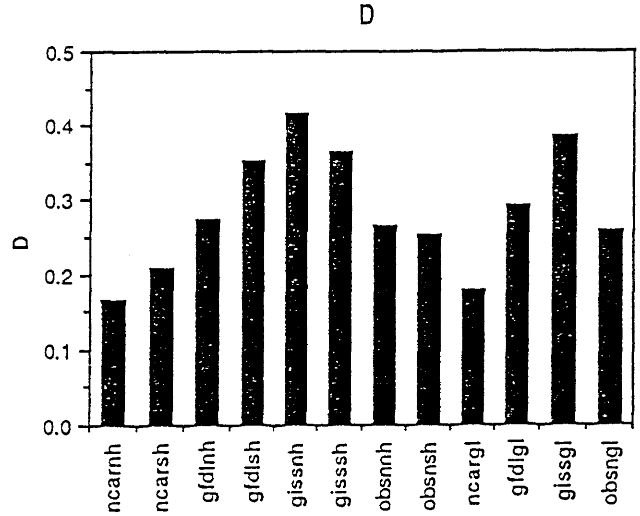
7.10.1- 7.10.2 Parameterized albedo as a function of the sine of latitude (x) in Southern and Northern Hemispheres (SH and NH) for NCAR (N), GFDL (P), and GISS (G) models, and for observations (O)



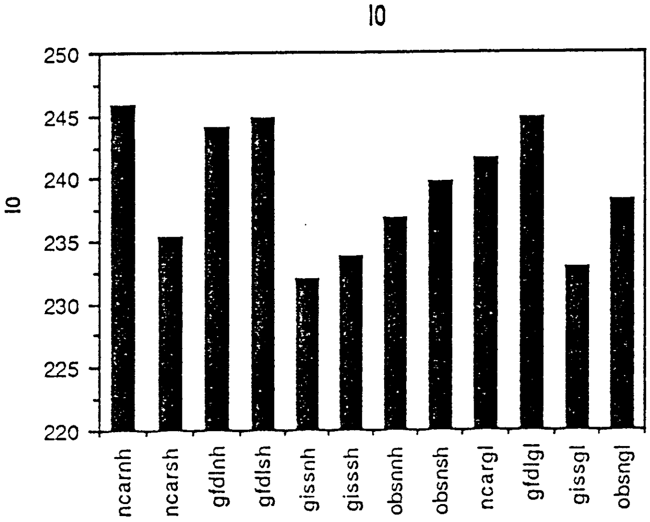
7.11 Outgoing longwave radiation at the iceline latitude (I_s) from each of the twelve equivalent EBMs



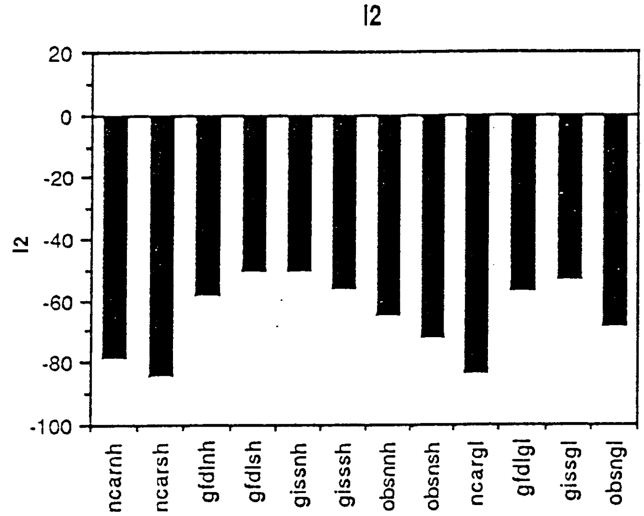
7.12 Dynamical transport efficiency (D) from each of the twelve equivalent EBMs



7.13.1- Outgoing longwave radiation components (I_0) and (I_2) from
7.13.2 each of the twelve equivalent EBMs

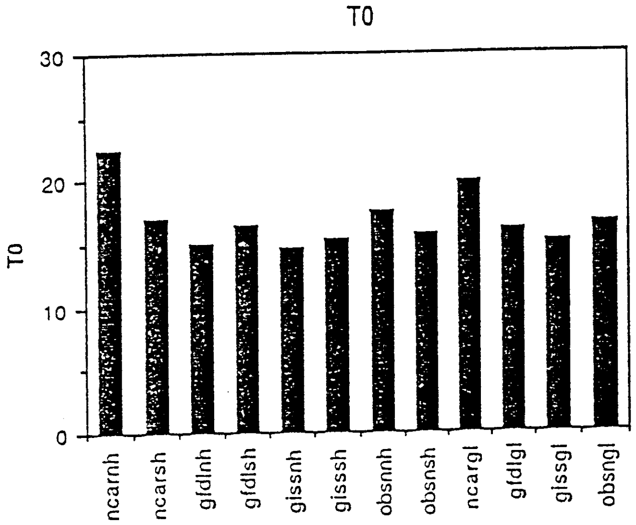


7.13.1

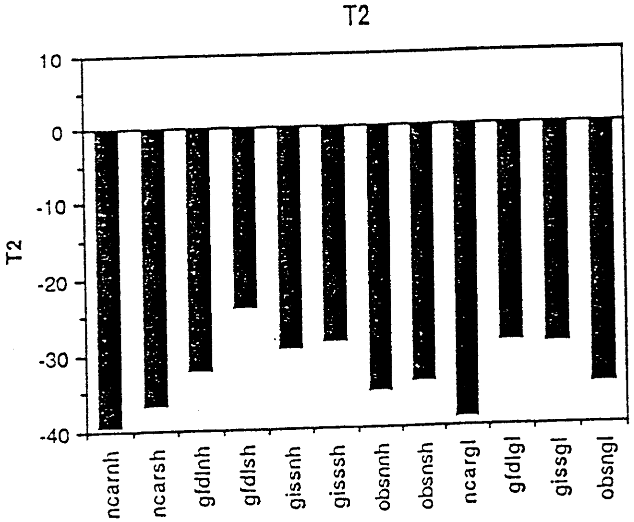


7.13.2

7.14.2 equivalent EBMs

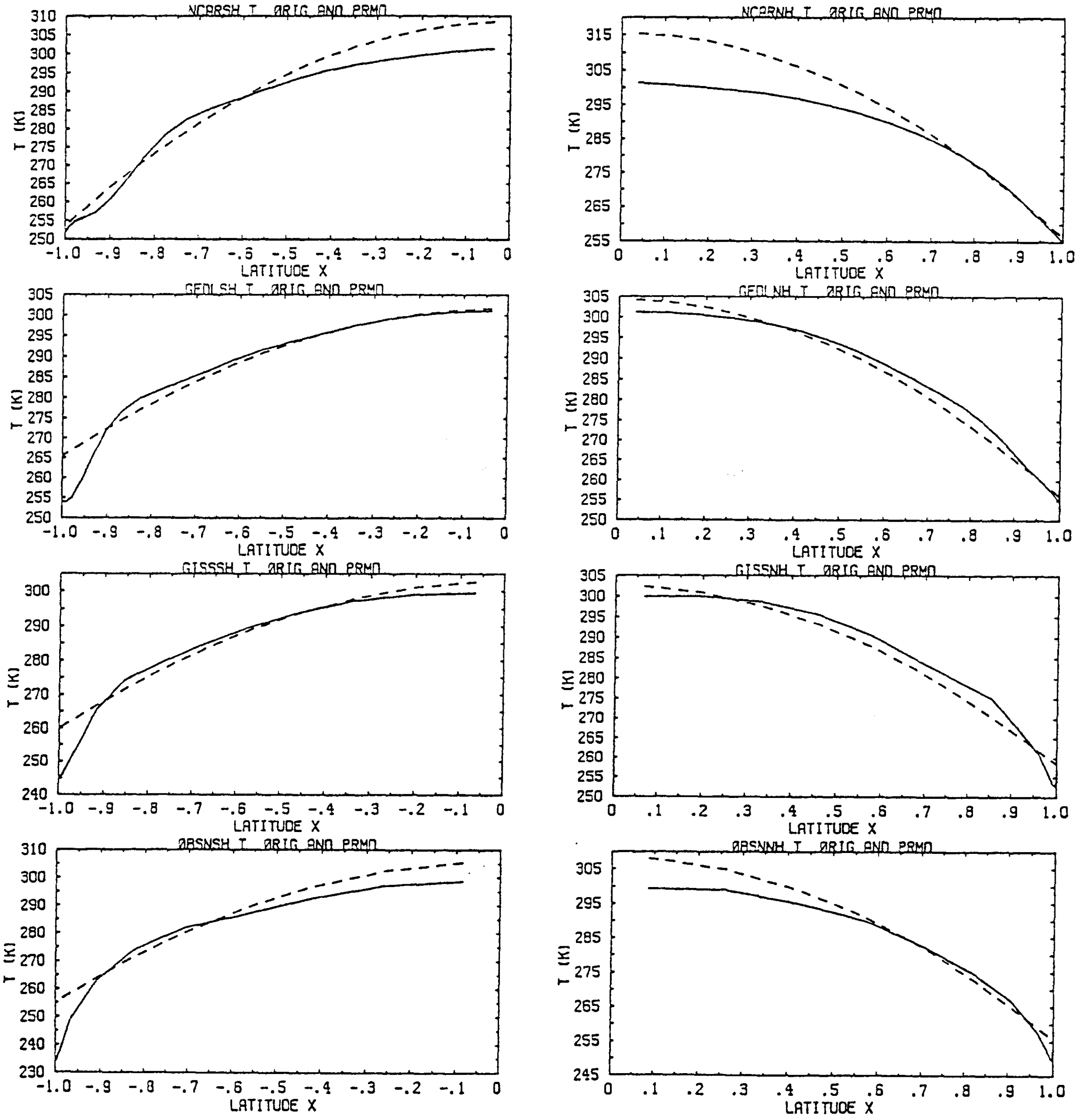


7.14.1

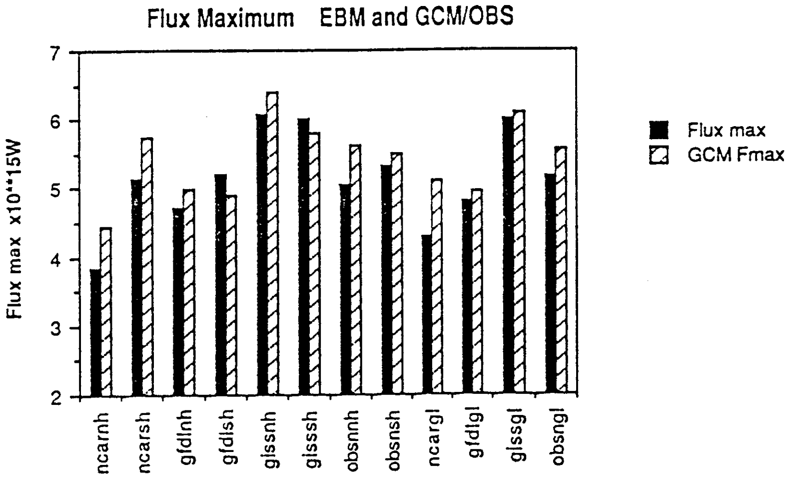


7.14.2

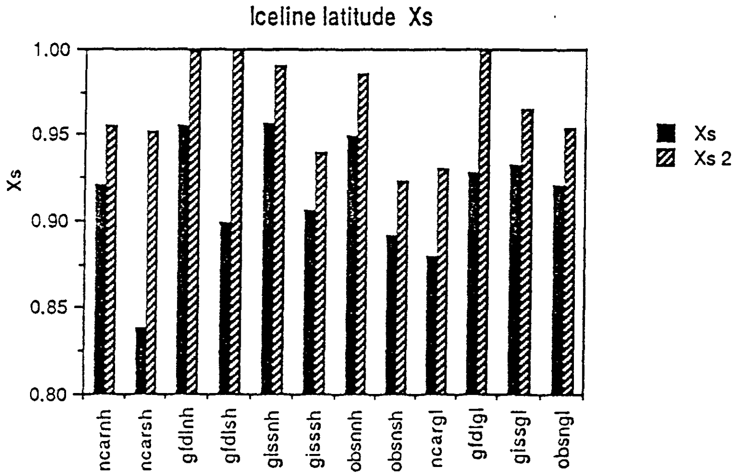
7.15.1- 7.15.8 Sea level temperature from the original NCAR, GFDL, and GISS GCMs and observations (solid lines), and calculated via the equation $T(x)=T_0+T_2P_2(x)$, where the T_0 and T_2 have been taken from the equivalent EBMs (dashed lines). The results are plotted as a function of the sine of latitude (x) for each hemisphere



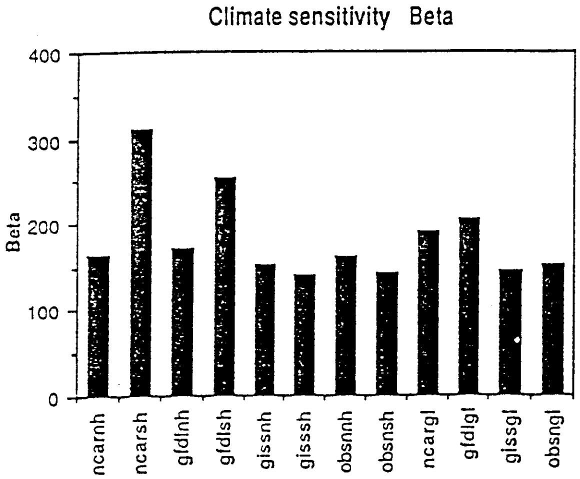
7.16 Energy transport flux maximum from the equivalent EBMs (solid bars) and from the original GCMs and Carissimo et al. observations (cross hatched bars). For the latter the global 'gl' values are averages of the hemispheric values



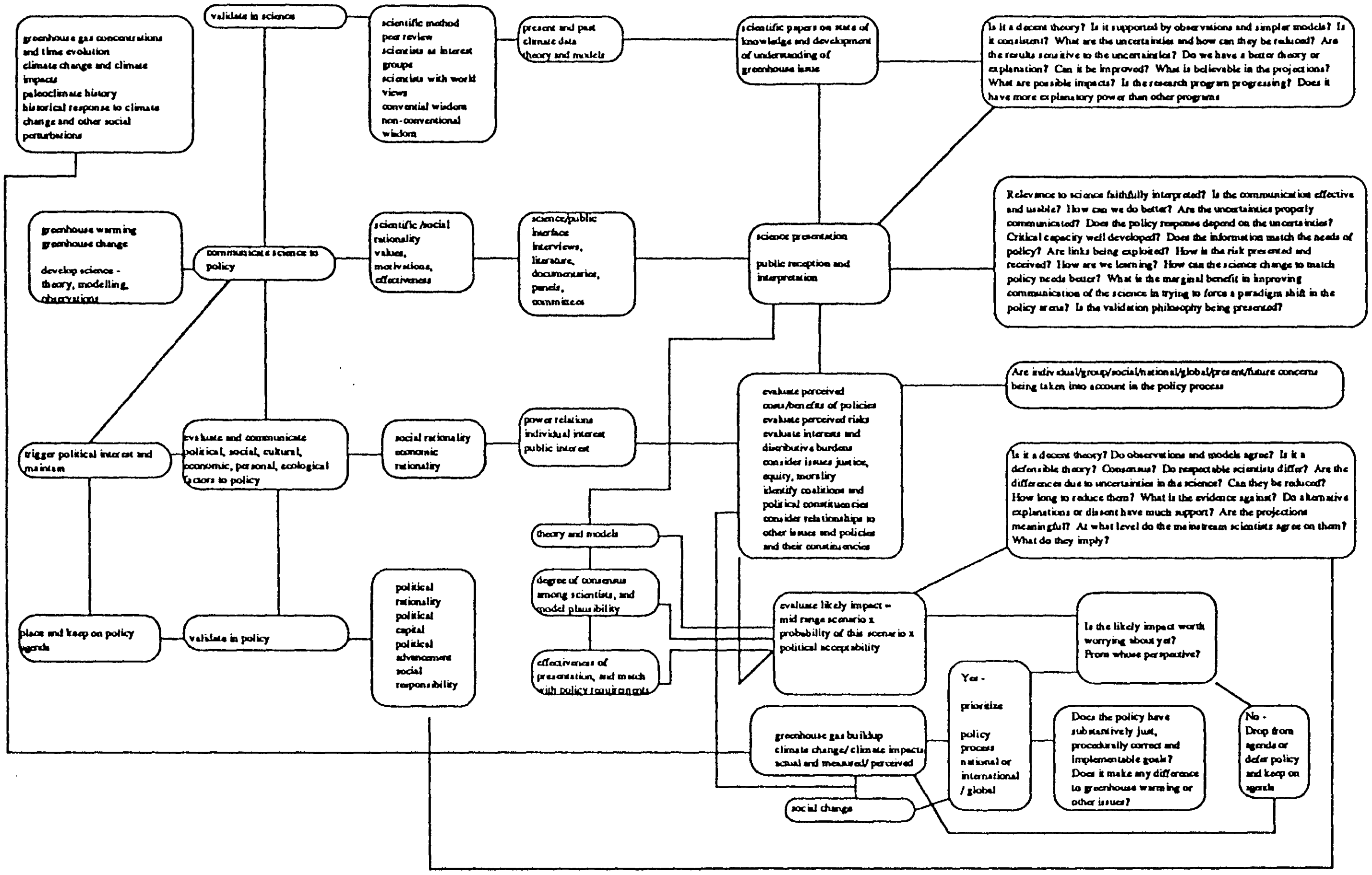
7.17 Iceline latitudes (x_s) as input to the equivalent EBMs in each case (solid bars), and as calculated in the equivalent EBMs when the solar constant was increased by 2% in each case (cross hatched bars)



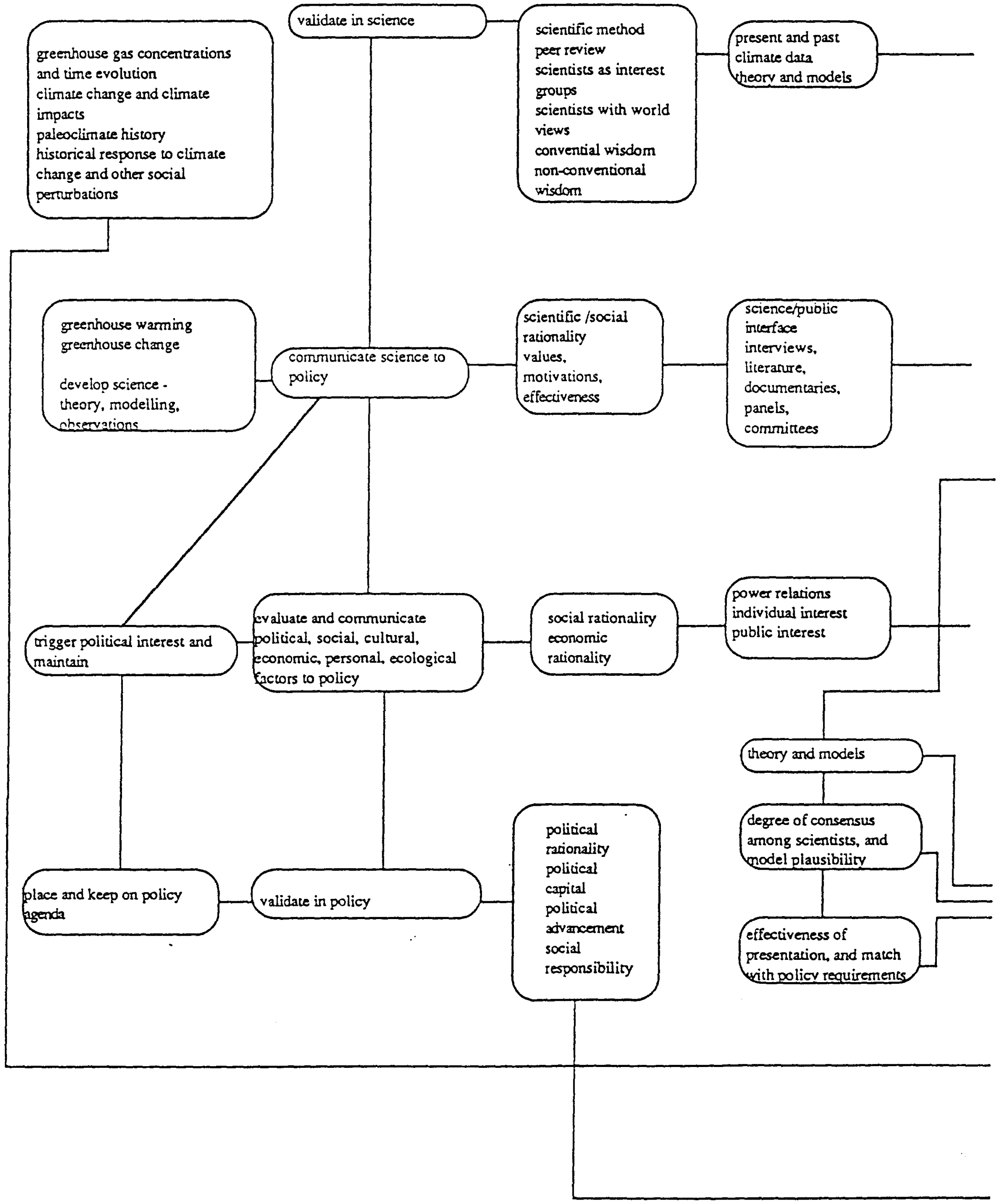
7.18 Climate sensitivity calculated from the equivalent EBMs



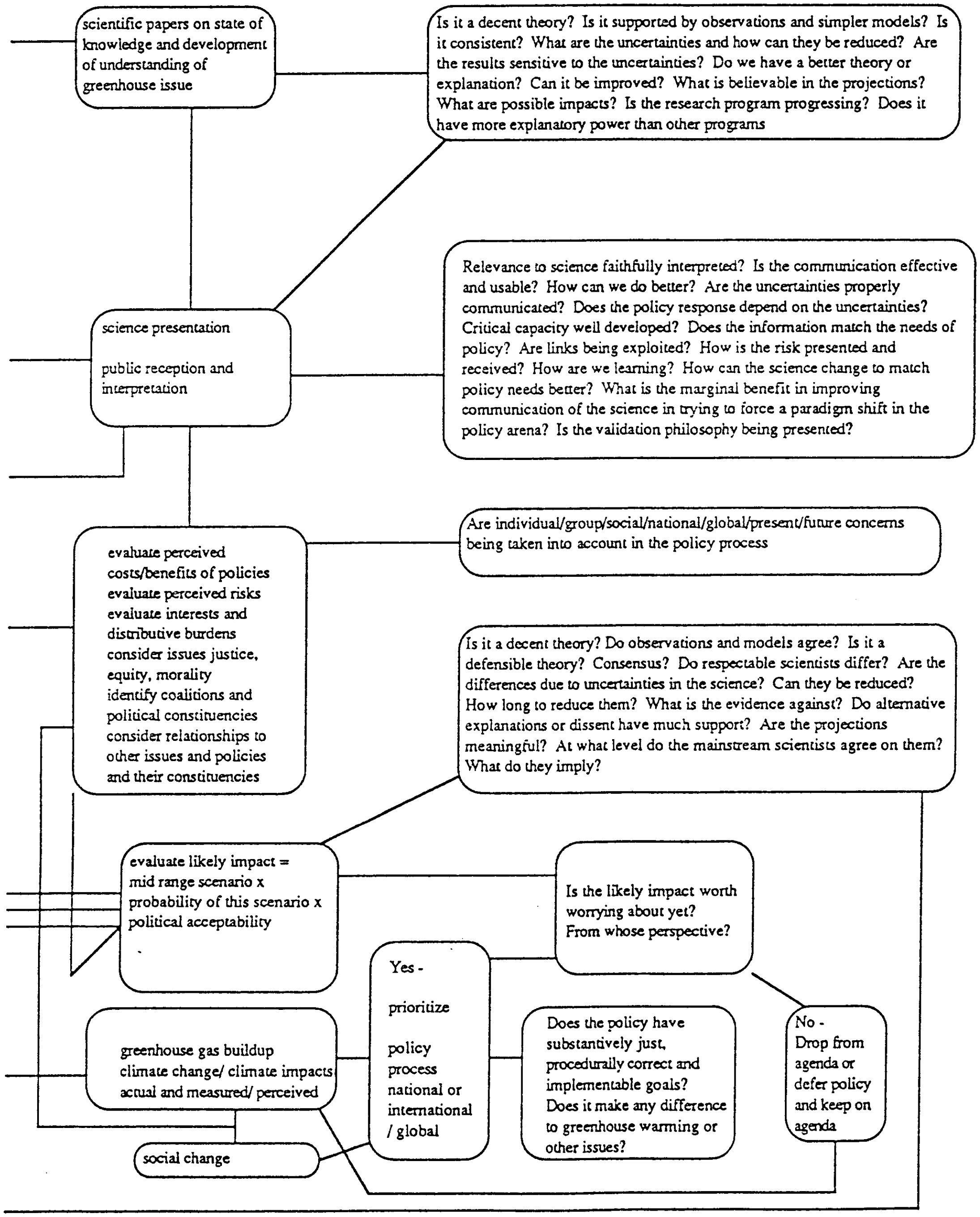
8.1 Greenhouse change validation framework



8.1 Greenhouse change validation framework



8.1 Greenhouse change validation framework



Is it a decent theory? Is it supported by observations and simpler models? Is it consistent? What are the uncertainties and how can they be reduced? Are the results sensitive to the uncertainties? Do we have a better theory or explanation? Can it be improved? What is believable in the projections? What are possible impacts? Is the research program progressing? Does it have more explanatory power than other programs

Relevance to science faithfully interpreted? Is the communication effective and usable? How can we do better? Are the uncertainties properly communicated? Does the policy response depend on the uncertainties? Critical capacity well developed? Does the information match the needs of policy? Are links being exploited? How is the risk presented and received? How are we learning? How can the science change to match policy needs better? What is the marginal benefit in improving communication of the science in trying to force a paradigm shift in the policy arena? Is the validation philosophy being presented?

Are individual/group/social/national/global/present/future concerns being taken into account in the policy process

Is it a decent theory? Do observations and models agree? Is it a defensible theory? Consensus? Do respectable scientists differ? Are the differences due to uncertainties in the science? Can they be reduced? How long to reduce them? What is the evidence against? Do alternative explanations or dissent have much support? Are the projections meaningful? At what level do the mainstream scientists agree on them? What do they imply?

Is the likely impact worth worrying about yet? From whose perspective?

Does the policy have substantively just, procedurally correct and implementable goals? Does it make any difference to greenhouse warming or other issues?

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